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WISSEN, UM ZU LEBEN: POPULARIZATION AS EPISTEMIC PRACTICE IN AUSTRIAN NATURAL SCIENCE, 1865-1916

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This work is dedicated to my parents.

Über dem Atlantik befand sich ein barometrisches Minimum; es wanderte ostwärts, einem über Rußland lagernden Maximum zu, und verriet noch nicht die Neigung, diesem nördlich auszuweichen. Die Isothermen und Isotheren taten ihre Schuldigkeit. Die Lufttemperatur stand in einem ordnungsgemäßen Verhältnis zur mittleren Jahrestemperatur, zur Temperatur des kältesten wie des wärmsten Monats und zur aperiodischen monatlichen Termperaturschwankung. Der Auf- und Untergang der Sonnes, des Mondes, der Lichtwechsel des Mondes, der Venus, des Saturnringes und viele andere bedeutsame Erscheinungen entsprachen ihrer Voraussage in den astronomischen Jahrbüchern. Der Wasserdampf in der Luft hatte seine höchste Spannkraft, und die Feuchtigkeit der Luft war gering. Mit einem Wort, das das Tatsächliche recht gut bezeichnet, wenn es auch etwas altmodisch ist: Es was ein schöner Augusttag des Jahres 1913.

—Robert Musil, *Der Mann ohne Eigenschaften*

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Introduction

In looking back on the trajectory of intellectual life in Central Europe after the First World War, the philosopher of science Karl Popper declared that the conflict not only destroyed

> the commonwealth of learning; it very nearly destroyed science and the tradition of rationalism. For it made science technical, instrumental. It led to increased specialization and it estranged from science what ought to be its true users –the amateur, the lover of wisdom, the ordinary, responsible citizen who has a wish to know.[1]

Like many other former citizens of the defunct Habsburg state, Popper was prone to idealize certain aspects of the fin de siècle period.[2] Scientific inquiry was already highly specialized and technical by 1914, for example, and efforts to instrumentalize scientific research for industrial purposes were partly responsible for, rather than the result of, the war's unprecedented savageness.[3] But his claim that pre-war science had not yet become "estranged from ordinary citizens" was corroborated by other high-profile intellectuals who either came of age or began their careers during the last decades of the Dual-Monarchy. Perhaps most famously, the philosophers of the Vienna Circle held that the intellectual *Zeitgeist* of late nineteenth-century Austria was not only defined by thinkers like Franz Brentano and Ernst Mach but by scientists' efforts to engage with the lay public. Indeed, Hans Hahn, Otto Neurath, and Rudolf Carnap were so impressed by those efforts that they made explicit reference to them in the Circle's manifesto, where they noted that the "scientifically-

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[1] Karl Popper, *Realism and the Aim of Science* (New York: Routledge, 1993), 260.

[2] See, for example: Stefan Zweig, *Die Welt von Gestern: Erinnerungen eines Europäers* (Frankfurt: S. Fischer, 1955).

[3] The career of Fritz Haber, co-inventor of the Haber-Bosch process and an important figure in the development of chemical weaponry, is instructive in this regard. See: Timothy Lenoir, *Instituting Science: The Cultural Production of Scientific Disciplines* (California: Stanford University Press, 1997), 203-239.

oriented adult education (*Volksbildung*)" and popularization programs of the 1890's and 1900's were as integral to their conception of the "scientific worldview" as logicism and positivism.[4]

Although Popper and the Vienna Circle philosophers agreed that fin de siècle Austrian scientists were peculiarly interested in leaving the ivory tower to engage with ordinary citizens, they offered differing explanations as to why. From the perspective of Neurath, Carnap, and their comrades, the scientific community at the time was uniquely devoted to "the spirit of the Enlightenment," and therefore uniquely committed to the task of educating the ignorant masses.[5] According to this disciplinary view, the main goal of the *Volksbildungs* movement and other mechanisms of expert-lay interaction was not to facilitate dynamic exchange between the two groups but to create a more rational citizenry. Popper, by contrast, suggested that pre-war scientists were interested in cultivating relationships with the general public for the same reason they tended to champion exchange between experts in different academic fields, namely: they saw "the narrow-mindedness of the specialist" as an epistemic vice akin to religious dogmatism and sought to incorporate a variety of perspectives into their research.[6]

This dissertation will offer a third, and in many respects conciliatory account of the fin de siècle Austrian scientific community's interest in ordinary citizens based on an analysis of the popular-scientific work produced by a small group of academics and intellectuals who were active between 1864 and 1916. Its argument is threefold. First, that this group, which included the physicist Ernst Mach, the botanist Richard von Wettstein and several of his students, and a small cadre of social democratic activists and theorists, not only used their popularizations to edify

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[4] Hans Hahn, Otto Neurath, and Rudolf Carnap, "Wissenschaftliche Weltauffassung: Der Wiener Kreis," in *Otto Neurath: Empiricism and Sociology*, ed. Marie Neurath and Robert Cohen (Boston: D. Reidel Publishing Company, 1973), 301-302.
[5] Ibid.
[6] Popper, 260.

laypersons but to intervene in scientific discussions and debates, and often to do both things at the same time. Second, that they understood the scientific value of popular representation to be a function of its capacity to mediate between the experiences and ideas of the "average man" and those of the professional researcher. And third, that their belief in the epistemic salience of this form of expert-lay mediation was a reflection of the idiosyncratic nature of intellectual life in late nineteenth-century Austria, as well as broader conceptual, institutional, and methodological transformations in scientific inquiry after 1860.

The Austrian perspective on popularization circa 1900 was only one among many different conceptions of what was still a relatively novel mode of scientific communication. Although one could conceivably classify Bernard Fontenelles *Eintretiens sur la pluralite des mondes* (1686) or the entries in Diderot's Enlightenment-era *Encyclopedie* as popular texts, most historians now agree that "popular science" did not become a coherent and recognizable actors' category until the early nineteenth-century.[7] According to Andreas Daum, the first self-styled work of popular science—or more accurately, popular natural philosophy—to appear in the Germanophone literature was Jakob Fries' 1813 *Populäre Vorlesungen über die Sternkunde*.[8] An astronomer, Kantian, and mathematician by training, Fries framed his text as a way of providing educated but innumerate readers with a basic, non-mathematical overview of recent astronomical findings and, in a move that would presage later popularizers like Mach, to tease out the "philosophical meaning of the science of the stars."[9]

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[7] Ralph O'Connor, "Reflections on Popular Science in Britain: Genres, Categories, and Historians," *Isis* Vol. 11, No. 2 (June 2009), 333-345.
[8] Andreas Daum, *Wissenschaftspopularisierung im 19. Jahrhundert: Bürgerliche Kultur, naturwissenschaftliche Bildung, und die deutsche Öffentlichkeit, 1848-1914*. (Munich: R. Oldenbourg Verlag, 1998) 268.
[9] Jakob Fries, *Populäre Vorlesungen über die Sternkunde* 2nd edition (Heidelberg: Christian Friedrich Winter, 1833), iii-v.

In the decade following the publication of Fries' *Vorlesungen*, Germanophone researchers not only produced a great deal more popular-scientific work but increasingly used the medium to circulate scientific information to colleagues and provide synthetic accounts of recent research to specialists in other fields. In 1822, the *Naturphilosoph* Lorenz Oken organized the first *Versammlung Deutscher Naturforscher und Ärtze* so that researchers and members of the lay public would have a venue to meet, network, and deliver popular lectures that either described important happenings in their respective fields of inquiry or addressed matters of general scientific interest. By 1830, this gathering would be one of the most important annual events in the Central European academic calendar. Around the same time that Oken was organizing the first *Versammlung*, the polymath naturalist Alexander von Humboldt also fastened onto the idea that popular lectures and texts could be useful vehicles for simultaneously educating laypersons and publicizing his research. In addition to helping administer the *Versammlungen*, in the winter of 1827-1828 he delivered a series of public talks that described his recent findings in physical geography to "grandstand audience(s) of thousands" in Berlin.[10] These lectures would later form the backbone of his later *Kosmos* (1845), which remains among the most widely read popular-scientific texts of all time.

Although Humboldt's Berlin talks would inform the way that Germanophone scientists understood the form and function of the popular genre well into the twentieth-century, they were also characteristic of a form of science, and of expert-lay relations, that would largely disappear by the late-1840's. Specifically, for much of the first half of the nineteenth century the scientific enterprise remained integrated with, rather than distinct from and peripheral to, public culture and

[10] Laura Walls, *The Passage to Cosmos: Alexander von Humboldt and the Shaping of America* (Chicago: The University of Chicago Press, 2009), 110.

discourse, just as it had been during the eighteenth-century.[11] As a result of this state of affairs, *Vormärz* scientists were far less likely to draw rigid boundaries between work intended for colleagues and work intended for general audiences than their post-1848 successors. The fluidity of this boundary was particularly evident in Humboldt's *Kosmos*, which unabashedly mixed scientific argument with travelogue; technical detail with poetic description; and vernacular speech with extended footnotes in multiple languages. The Scottish geologist Robert Chambers' *Vestiges of the History of Natural Creation* (1844), which perhaps exceeded *Kosmos* in terms of public and scientific influence, exhibited the same admixture of familiar and technical modes of communication. Indeed, historian Bernard Lightman has remarked that Chambers sought to make *Vestiges* as accessible to the average reader as possible as an explicit rebuke to the idea that scientific reasoning was divorced from public judgment and to uphold "the right of the layperson to speculate in matters scientific."[12]

One reason that Chambers, Humboldt, and Oken were disinclined to make absolute distinctions between scientific and non-scientific audiences was that no such distinction existed between scientific and non-scientific media. This is not to say that *Vormärz* researchers did not recognize that there were important differences between the proceedings of a scientific society and a newspaper article, but that this recognition did not stop them from using the latter, as well as other forms of mass media, to make original knowledge claims or engage in scientific debate. The porousness of the boundary between publications intended for specialist readerships and those intended for the general public was particularly palpable in Britain, where scientists frequently

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[11] For more on Enlightenment science as "public culture," which is to say as a form of cultural practice that is intimately connected to public life, see: Jan Golinski, *Science as Public Culture: Chemistry and Enlightenment in Britain, 1760-1820* (Cambridge: Cambridge University Press, 1999).
[12] Bernard Lightman, *Victorian Popularizers of Science: Designing Nature for New Audiences* (Chicago: The University of Chicago Press, 2007), 26.

used London-based literary magazines like *Athenaeum*, *Literary Gazette*, and the *Times* to announce important discoveries; circulate scientific intelligence; and resolve disputes.[13] As Alex Csiszar recently demonstrated in his pathbreaking history of the scientific journal in Britain and France, *Athenaeum* and its ilk in fact provided the model for later commercial scientific periodicals like *Nature*, which initially adopted a similar strategy of interspersing technical material with "more entertaining and readable matter as well."[14]

Hybrid literary-scientific publications were less common in Central Europe, where a robust specialist literature developed earlier than in Britain. Many of the journals that would dominate scientific discourse in Habsburg Austria and Germany into the twentieth-century, including the *Annalen der Physik und Chemie*, the *Journal für die reine und angewandte Mathematik*, and the *Archiv für Anatomie, Physiologie und Wissenschaftliche Medecin*, were already well-established by the 1830's. But Germanophone scientists also occasionally used *Athenaeum*-like periodicals to publish their research or engage with their colleagues.[15] The most important of these was Oken's *Isis*, an "encyclopedic magazine" which hosted original academic work on topics ranging from art history to mechanics for an audience comprised of "academics, artists, engineers, economists," and natural philosophers, among other members of the educated public.[16] Less important but still relevant were newspapers and literary reviews like the *Augsbürger Allgemeine Zeitung*, *Leipziger Zeitung*, and *Deutsches Vierterljahrsschrift*, which were more important for researchers of Goethe's generation but continued to publish scientific material, reprints, and extensive reviews

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[13] Alex Csiszar, *The Scientific Journal: Authorship and the Politics of Knowledge in the Nineteenth Century* (Chicago: The University of Chicago Press, 2018), 205.
[14] Ibid, 206.
[15] Germanophone scientists also published in *Athanaeum* itself. See, for example: Karl von Baer, "Berichte über die neuesten Entdeckungen an der Küste von Nawaja Semlja," republished in *Athenaeum* No. 535 (1836), 57-59.
[16] Lorenz Oken, "Isis, oder Encyclopaedische Zeitung," *Isis, oder Encyclopaedische Zeitung* 1, Vol. 1 (Jena: 1817), 1-2.

of new scientific monographs into the 1840's.[17]

When *Isis* ceased publication in 1848 it was not only symbolic of the decline of a certain kind of hybrid publication but of a form of scientific life. It also heralded the dawn of a new era in the communicative, institutional, and social organization of scientific research. One of the defining characteristics of this new era, as Max Weber famously remarked, was the pursuit of "science as a vocation," which is to say the reorientation of scientific inquiry around the attitudes and practices of credentialed experts working in salaried positions at a small number of privileged institutions.[18] Two of the processes underlying this wide-ranging disciplinary reconfiguration, specialization and professionalization, were already well underway in the German states in the first decades of the nineteenth-century. In physics, for example, German researchers had followed their French counterparts in adopting a thoroughly mathematical approach to the study of heat and light, thereby making extensive training in quantitative methods a prerequisite of participation in nearly every branch of physical research. By the mid-1820's, the Germans had coupled the French mathematical approach with a unique style of precision experiment, meaning that aspiring Germanophone physicists not only had to learn complex mathematical techniques to take their place in the disciplinary community but to master delicate technical procedures as well.

Perhaps more importantly, throughout the 1820's and 1830's physicists and their colleagues in other scientific disciplines began to secure professorships and institutes at the newly reformed universities in Prussia and several other German states, which they used as platforms to train students in field-specific ideas and practices; award credentials which indicated mastery of

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[17] Alexander von Humboldt, "Übergang über den Isthmus von Panama," *Augsbürger Allgemeine Zeitung* Nr. 90 (1846); and Alexander von Humboldt: "Über die Hochebene von Bogota," *Deutsche Vierteljahrschrift*, Vol. 1 (J.G. Cotta'schen Buchhandlung, Stuttgart und Tübingen, 1839), 97-120.

[18] Max Weber, *The Vocation Lectures* ed. David Owen and Tracy B. Strong, trans. Rodney Livingstone (Indiana: Hackett Publishing, 2004).

those ideas and practices; and to further their own research programs. One pioneer in this regard was the chemist Justus von Liebig, who established a mixed research and teaching laboratory at the University of Giessen in 1826 that sociologist J.B. Morrell aptly characterized as "a knowledge factory" because it manufactured both standardized facts and knowers.[19] Another pioneer was the University of Königsberg physicist Franz Neumann, who established an enormously influential seminar in 1834 that not only trained generations of students in the ideals and techniques of precision measurement but, as Kathryn Olesko has argued, helped define the very meaning of "physics as a calling" in Germany.[20] And in the 1840's, the physiologist Johannes Müller and naturalist H.G. Bronn used their university positions to transform the study of organic form and development from something that amateurs and "gentlemen of science" could do in their living rooms or country houses into a professional discipline that was firmly embedded "within the matrix of teaching and research on animal life" in German academia.[21]

The institutionalization and professionalization of scientific training and research happened at a far slower pace in Austria, which remained something of an academic and intellectual backwater into the 1860's.[22] As historian Jan Surman recently argued, claims about the underdevelopment of Habsburg scholarship during the *Vormärz* have tended to miss the mark by

[19] J.B. Morrell, "The Chemist Breeders: the research schools of Liebig and Thomas Thomson" *Ambix* 19 (1972), 1-46.
[20] Kathryn Olesko, *Physics as a Calling: Discipline and Practice in the Königsberg Seminar for Physics* (Ithaca: Cornell University Press, 1991).
[21] Lynn K. Nyhart, *Biology Takes Form: Animal Morphology and the German Universities, 1800-1900* (Chicago: The University of Chicago Press, 1995). See also: Laura Otis: *Müller's Lab* (Oxford: Oxford University Press, 2007).
[22] There were of course notable individual and institutional exceptions to this backwardness. The University of Vienna's medical school was one of the finest in the world throughout the nineteenth century. See: Erna Lesky, *The Vienna Medical School of the 19th Century* trans. L. Williams and I.S. Levij (Baltimore: The Johns Hopkins University Press, 1977). With respect to individuals, the philosopher Bernard Bolzano, the physicist Christian Doppler, and the botanist Franz Unger, among others, all made pioneering---if not always appreciated---contributions to their fields prior to 1850.

anachronistically imposing a late nineteenth century model of knowledge production on the period. That is, the lack of scientific activity at the pre-1848 Habsburg universities was not necessarily evidence of their backwardness, according to Surman, but of a peculiar division of labor in which they were tasked with "the production of loyal subjects, while the primary place for the production of scientific knowledge in the empire included museums, state collections, libraries, botanical and zoological gardens, and a number of more or less formal societies and clubs."[23] Even so, many contemporaries felt that this relatively disorganized and decentralized institutional arrangement, coupled with the state's aggressive censorship laws, had rendered Austria scientifically uncompetitive in comparison with many of its other European neighbors. Indeed, it was not uncommon for Austria's most talented researchers to decamp to the German states during this period—and for several decades after—in the hopes of finding better laboratories, mentors, and opportunities for career advancement. [24]

The dire situation in which Austrian scientists found themselves in the 1820's and 1830's began rapidly change for the better in the mid-1840's after mounting political pressure forced Metternich's government to relax its opposition to scientific centralization and academic freedom.[25] Having been granted more autonomy, researchers quickly moved to establish the kind of associations and institutions that were not only essential to organizing scientific work but to establishing a sense of professional identity, including the country's first scholarly society

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[23] Jan Surman, *Universities in Imperial Austria, 1848-1918: A Social History of a Multilingual Space*. (Indiana: Purdue University Press, 1919), 20.
[24] In 1861, Ernst Mach made plans to move to Königsberg to finish his training at the Neumann seminar because he found the faculty and facilities of the University of Vienna wanting but was ultimately unable to scrape together the funds to go. See: John Blackmore, "Three Autobiographical Manuscripts by Ernst Mach," *Annals of Science* 35 (1978), 401-418.
[25] Surman, 29.

dedicated exclusively to natural science in 1845 and the Austrian Academy of Science in 1847.[26] One year later, the revolutionary wave that swept over Europe toppled Metternich's government and sparked a movement to completely restructure Austria's educational system and scientific apparatus.

Critically, the so-called Exner-Thun reforms of 1848 made several changes that helped modernize scientific education and research in the Monarchy. First, they made science education into an integral part of the curriculum of the Austrian *gymnasia*, thereby assuring that students were better prepared to take on more advanced scientific studies at the university-level. Second, they transformed the Austrian universities along broadly Prussian lines by granting the professoriate greater control over university affairs, including highly important administrative functions like the appointment of new professors, as well as greater freedom to teach and research what they wanted. And third, they sought cultivate and retain homegrown scientific talent by dramatically increasing government support for scientific research. The positive effects of these changes were evident almost immediately. Between 1849 and 1861 the University of Vienna alone created new professorships in chemistry and zoology; updated existing scientific collections and instrumentaria; and founded several new research institutes and laboratories that quickly became obligatory points of passage for students seeking careers in science or even just to teach scientific topics in the Monarchy's lower schools.[27] And just as the reformers had hoped, these new institutes and professorships quickly brought Austrian science to a level of competence approaching that of its German counterpart.

In short, by the early 1850's Germanophone science was no longer the sort of thing that a

[26] Karl Fritsch, "Geschichte der Intitute und Corporationen," in *Botanik und Zoologie in Österreich in den Jarhen 1850 bis 1900* (Vienna: Alfred Hölder, 1901) 19-21.
[27] Fritsch, "Geschichte der Intitute und Corporationen," 17-127.

gentleman-savant could do in his spare time but a highly technical form of cultural production that was practiced almost entirely by credentialed experts occupying stable, salaried positions at institutes of higher learning, and that was reproduced through relatively standardized training regimes. This dramatic transformation in how and where scientists did their research also had dramatic effects on how they communicated with one another. That is, as the production of scientific knowledge became localized in small professional communities dedicated to the study of increasingly abstruse topics, the members of those communities began to call for communicative forms that were more attuned to their particular interests and needs, chief among them the rapid circulation of field-specific information. The primary result of this demand was an avalanche of new and highly specialized scientific journals, which had the advantage of appearing more frequently than the proceedings of major scientific societies and containing more relevant content than "encyclopedic" magazines like *Isis*. Granted, the explosive growth of the periodical literature after 1840 was also related to the declining cost of paper, the advent of new printing technologies, and other advances on the production side, but as the editors of the *Botanische Zeitung* noted in 1843, the expanding market for these publications was driven by "scholars by profession" and their desire for media which allowed for the "rapid communication of new experiences, observations, and discoveries."[28]

As scientific discourse and practice receded into the conceptually and spatially inaccessible confines of specialized journals, university seminars, and laboratories, its practitioners also began to place greater emphasis on their duty to communicate their results to the lay public. Granted, many scientists were only interested in public communication insofar as they recognized that

[28] Hugo Mohl and D.F.L. von Schlechtendal, "Prospectus," *Botanische Zeitung* Vol. 1 (Jan. 6, 1843), 1-2.

success or failure to demonstrate the value of their work could mean success or failure to secure necessary facilities and research funds.[29] But many also felt a genuine sense of responsibility for the edification of their fellow citizens. The eminent physicist Hermann von Helmholtz captured both sentiments in his preface to John Tyndall's *Fragments of Science*. It was not just the case that efforts to put the fruits of scientific research at the disposal of the public fulfilled a duty to "further the intellectual development of the people," he explained, because they also furthered "the intellectual development of the sciences themselves," which were inextricably tied to the fortunes of society at-large.[30]

Put another way, while the new class of professional scientists that emerged out of the Germanophone universities in the 1850's preferred to do their research in publicly inaccessible sites, and to communicate new discoveries, observations, and other bits of scientific information through specialized scientific journals, they were not content to remain out of the public eye. If anything, the growing chasm between scientific and lay communities stoked the former's desire to apprise the latter of the benefits of what they were doing. Conversely, the growing presence of scientific ideas and technologies in various domains everyday life, ranging from education and industry to politics and the domestic sphere, heightened public demand for media that explained scientific knowledge and research in comprehensible terms. This confluence of desire and demand sparked a veritable explosion in the number of popularizations and the advent of numerous new venues for the circulation of popular-scientific work. Between 1849 and 1860, German editors, publishers, and scientists helped found no fewer than eleven new popular-scientific magazines and

[29] Lenoir, *Instituting Science: The Cultural Production of Scientific Disciplines*, 75-131.
[30] Hermann von Helmholtz, "Vorrede," in John Tyndall, *Fragmente aus den Naturwissenschaften: Vorlesungen und Aufsätze* trans. Anna Helmholtz (Braunschweig: Viewig und Sohn, 1874), X-XI.

four popular-scientific book series.[31] These years were also seminal for the emergence of a new, if not controversial type of knowledge worker: the professional popularizer.[32]

Most of the individuals, media outlets, and organizations that took it upon themselves to produce or distribute popular-scientific work in the 1850's, whether they were working scientists or professional popularizers, understood the medium as a means of translating scientific ideas and terms into forms that were accessible to the average person. Indeed, a German publisher first coined the neologism "popular-scientific" in 1849 specifically to refer to lectures and texts that attempted to render specialist research "generally understandable" and "appropriate for common people (*Volksmässig*)."[33] The widespread identification of popularization with comprehensibility and *Volksmässigkeit* solidified its role as an educational and sociopolitical tool but also marginalized it as a mode of scientific communication. Whereas Chambers and Humboldt felt that their popular works could competently address both lay and scientific concerns, in other words, many post-1848 scientists felt that popular representation merely provided a simplified gloss on what occurred in laboratories and specialist journals, and that it was therefore unrelated to the production and refinement of scientific knowledge.

The chemist von Liebig's "Chemical Letters," a series of hugely influential popular articles that he published in the *Augsbürger Zeitung* between 1841 and 1844, provide an early example of a scientist suggesting that popularization was politically and socially useful but epistemically irrelevant. Specifically, Liebig maintained that he wrote the "Letters" to familiarize lay readers

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[31] Daum, *Wissenschaftspopularisierung im 19. Jahrhundert*, 324-358.
[32] Ibid, 373-453. Daum identifies four varieties of popularizer active in Germany after 1848: the professional popularizer; the occasional popularizer; the university popularizer; and the academic "thought leader." For more evidence of 1848 as "the starting point of a bourgeois culture of science popularization" in Germanophone Central Europe, see: Arne Shirrmacher, "Popular Science as Cultural Dispositif: On the German Way of Science Communication in the Twentieth Century," *Science in Context 26:3* (2013), 473-508.
[33] Ibid, 35-36.

with the elementary facts and principles of chemistry and

> for the especial purpose of exciting the attention of governments, and an enlightened public, to the necessity of establishing Schools of Chemistry, and of promoting, by every means, the study of a science so intimately connected with the arts, pursuits, and social well-being of civilized nations.[34]

And while he sought to provide a better class of popular representation by avoiding the "vulgarity" and mere "sense-making" that he saw as characteristic of many other popular texts, he did not count his fellow chemists among his intended readers, nor did he indicate that the "Letters" were in any way relevant to ongoing research in the discipline.[35]

Liebig's conception of the popular genre as socio-politically useful but devoid of scientific interest was mirrored by the Leipzig zoologist Emil Adolf Roßmäßler. One of the nineteenth-century's most influential and prolific popularizers, by 1860 he had not only published several successful popular books on natural history but helped co-found a number of popular-scientific clubs and magazines, including *Aus der Heimath* and *Gartenlaube*, the latter of which would become one of the most widely read publications of any kind in nineteenth century Germany.[36] He was also a vocal critic of the scientific community's growing intellectual distance from everyday life. At the 1865 *Versammlung Deutscher Naturforscher und Ärtze*, he delivered an impassioned speech to his colleagues imploring them to remember Oken's commitment to creating a gathering that also incorporated members of the lay public.[37] But his rebuke of the ivory tower and conviction in the importance of popularization stemmed more from his belief in the genre's power to create a

[34] Julius von Liebig, "Preface," in *Familiar Letters on Chemistry and its Relation to Commerce, Physiology, and Agriculture* 2nd edition, ed. John Gardner (London: Taylor and Walton, 1844).
[35] Justus von Liebig, "Vorrede zur Ersten Auflage," in *Chemische Briefe* 3rd edition (Heidelberg: Akademische Velagshandlung von C.F. Winter, 1851), X-XI.
[36] Alfred Kelly, *The Descent of Darwin: The Popularization of Darwinism in Germany, 1860-1914* (North Carolina: The University of North Carolina Press, 1981), 15.
[37] Emil Roßmäßler, "Über naturgeschichtliche Volksbildung," *Amtlicher Bericht über die Vierzigste Versammlung Deutsche Naturforscher und Ärtze* ed. C. Krause and K. Karmarsch (Hannover: Hahn'sche Hofbuchhandlung, 1866), 71-72.

better class of citizen than his belief that it could contribute to expert science itself. As he explained to the readers of his *Populaiere Vorlesungen aus dem Gebiete der Natur* in 1852, his popular lectures were not supposed to initiate them into the world of the laboratory and seminar but to provide them with "educational material" and inculcate "a taste and love" for nature and scientific inquiry.[38]

The generation of Germanophone scientists that succeeded Liebig and Roßmäßler in the 1870's largely maintained their predecessors' understanding of popularization as a tool for transmitting simplified accounts of science to the lay public, as well as their belief that these accounts did not actively contribute to scientific discourse. In fact, many of the scientists that took up professional positions in the last quarter of the nineteenth century made even stronger distinctions between scientific and popular communication. One factor in the increasing polarity between the two communicative forms after 1870 was the continued growth and specialization of the periodical literature, which compelled bibliographers, publishers, and scientists to place tighter restrictions on what counted as a scientific article and journal.[39] Another was the consolidation of popularization as a professional enterprise that was primarily practiced by people who were either outside or on the margins of the scientific community, which made the genre easier to dismiss as part of a distinct, and distinctly non-scientific, tradition.[40] And lastly, scientists' growing recognition that "popularization and scientific specialization were two essentially opposed

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[38] Emil Adolf Roßmäßler, "An den Leser," *Populaire Vorlesungen aus dem Gebiete der Natur* Vol. 1 (Leipzig: Hermann Costenoble, 1852), VII.
[39] Csiszar, *The Scientific Journal*, 199-241. For Mach's complaint about this phenomenon, see: Ernst Mach, *History and Root of the Principle of the Conservation of Energy* trans. Philip E. B. Jourdain (Chicago: The Open Court Publishing Co., 1911), 10, 80.
[40] A common refrain among scientists who were critical of non-academic popularizers was that they propagated a harmful brand of "half-knowledge" that not only harmed listeners and readers but science itself, although many of the staunchest critics of the latter were scientists who saw popularization as their exclusive purview. See: Anton Lampa, "Über die von der Universität Wien veranstalteten auswärtigen Kurse," *Zentralblatt für Volksbildungswesen* Vol. 8 No 5-6 (July 1908), 67-74.

processes which…required their own resources" became institutionally and physically manifest when researchers began to establish professionally run popular science organizations that were administratively and spatially distinct from their research institutes, a la *Urania* and the *Physikalisch-Technischen Reichsanstalt* in Berlin.[41]

Although post-1848 Germanophone scientists would gradually come to understand popularization as a mechanism for translating "real science" into forms that were appropriate for lay consumption and nothing more, several high-profile researchers continued to use the genre as a means to scientific ends. The biologist Ernst Haeckel, Darwin's great champion in Central Europe, simultaneously used his popular work to spread the gospel of natural selection and to make original knowledge claims, intervene in scientific debates, and engage with his colleagues on technical matters.[42] The physicist Helmholtz did much the same, circulating several of his most important arguments concerning the nature and epistemological limits of physical inquiry through work that he explicitly labeled popular.[43] Even researchers who were active in fields that were so highly specialized as to seemingly preclude any epistemic use for popular communication at all, ranging from developmental mechanics to theoretical physics, occasionally found the genre useful for making certain kinds of arguments.[44]

Fin de siècle Austrian scientists were particularly prone to continue to use popular media

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[41] Daum, 178.

[42] Nick Hopwood, *Haeckel's Embryos: Images, Evolution, and Fraud* (Chicago: The University of Chicago Press, 2015), 67. As Hopwood notes, Haeckel famously claimed that the lectures of his *Natürliche Schöpfungsgeschichte* were "popular and scholarly" at the same time.

[43] Gregor Schiemann, *Hermann von Helmholtz's Mechanism: The Loss of Certainty* trans. Cynthia Klohr (Dordrecht: Springer, 2009), 70-73. See also: Jutta Schickore, "The Task of Explaining Sight—Helmholtz's Writings on Vision as a Test Case for Models of Science Popularization," *Science in Context* 14, no. 3 (2001), 397-417.

[44] As Paul Forman demonstrated in his classic analysis of Weimar physics, popular lectures and texts would remain integral to specialist discourse in theoretical physics into the late 1920's. See: "Weimar Culture, Causality, and Quantum Theory, 1918-1927: Adaptation by German Physicists and Mathematicians to a Hostile Intellectual Environment," *Historical Studies in the Physical Sciences* Vol. 3 (1971), 1-115.

to produce and refine scientific knowledge. That popularization would take on a unique character in Austria unsurprising, given the peculiar features of intellectual and social life in the Habsburg state. The University of Vienna zoologist Carl Brühl was among the first to explicitly suggest that his countrymen should design their public-oriented work in ways that spoke to local circumstances, interests, and needs rather than "North German" ones. "The natural sciences have a different meaning for the people of Austria than they do for the people of others lands," he remarked in one of his influential "Sunday lectures" of the mid-1860's, and it was therefore incumbent on Austrian scientists to design "worldly" work that reflected that meaning.[45] He was particularly interested in counteracting the German biologist Rudolf Virchow's conception of popularization, and of natural science in general, as part of a German nationalist political project. Rather than suggest that there was a "special relationship…between the natural sciences and a particular tongue," he insisted that Austrian scientists should represent their research and knowledge as something that bound different communities together regardless of language or ethnicity.[46]

In addition to creating intellectual common ground between the Habsburg state's different ethnic groups, Brühl suggested that popularization could help establish a more dynamic and productive relationship between Austria's scientific and lay communities, which exhibited cultural and linguistic differences that rivaled, if not exceeded those of its Czechs and Germans. The mutual benefit of greater contact between scientists and laypersons was a prominent theme of a series of popular lectures he delivered at the newly founded Zoological Institute in 1868. On the one hand, he maintained that the millions of Austrian adults who had been left completely ignorant of science by the state's inadequate school system would be able to fulfill their curiosity about the

[45] Carl Brühl, "Professor Brühl's erste diesjährige Sonntagsvorlesung," in *Wiener Medizinische Wochenschrift* Vol. 16, ed. L. Wittelshoefer (Vienna: Seidel and Son, 1866), 148-150.
[46] Ibid.

Naturwissenschaften and become more informed, rational citizens.[47] On the other hand, he claimed that scientists stood to benefit from popularization themselves because the process of rendering their research tractable to the average person would help purge it of "learned obscurity" and clarify its "intellectual and material essence," by which he meant its basic empirical and theoretical content.[48]

For many of the students and scientists who heard Brühl's lectures, including the botanist Richard von Wettstein, his remarks on the benefits and uses of popularization were revelatory.[49] He made a considerable impression on the lay members of his audience as well. When the founders of the University of Vienna extension courses asked participants why they decided to make use of the new adult-education program in 1895, several responded that they first became interested in science after attending the "Sunday lectures."[50] But Brühl was not the only Austrian *Wissenschaftler* to make waves with his popular work in the 1860's, nor was he alone in articulating the epistemic virtues of the genre. By the time the zoologist gave his first public talk in 1866, the physicist Ernst Mach had already been delivering popular lectures on a range of scientific topics and theorizing about the relationship between popular and specialist discourse for several years. Like Brühl, he suggested that popular representations, when properly executed, could make valuable contributions to knowledge. He articulated one of the earliest iterations of the latter point in his *Einleitung in die Helmholtz'sche Musiktheorie: Populär für Musiker dargestellt*

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[47] Carl Brühl, "Universität und Volksbildung, Priesterthum und Naturwissenschaft," *Wiener Medizinische Wochenschrift* Vol. 8 no. 10 (Feb. 1, 1868), 167-170. As an avid supporter of the new 1868 constitution and "compromise" (*Ausgleich*) between Austria and Hungary, he was also convinced that a scientifically literate citizenry was more capable of recognizing the benefits of continued liberalization

[48] Ibid.

[49] Erwin Janchen, *Richard Wettstein: Sein Leben und Wirken* (Vienna: Springer, 1933), 13.

[50] "Antworten auf die von dem Wiener Auschusse für volksthümliche Universitäts-Vorträge veranstaltete Umfrage über den Nutzen der Universitäts-Kurse," *Zentralblatt für Volksbildungswesen* Vol 4., No 6/7 (May 5th, 1904), 89.

(1866), where he argued that creating and reading popular texts could help scientists achieve a measure of linguistic and empirical clarity about their own research because the genre revealed the relationship between abstract scientific ideas and the commonplace thoughts and experiences at their core.[51]

In the five decades following the publication of *Einleitung in die Helmholtz'sche Musiktheorie,* Austrian scientists in a variety of disciplines would continue to reiterate Brühl and Mach's claim that popularization not only aided in the production of scientific knowledge but that it did so by mediating between the experiential and intellectual world of the "average man" and that of the specialist. Indeed, the group of scientists and social scientists under consideration in this dissertation maintained that dynamic engagement with the everyday was integral to the intellectual health of their respective disciplines and natural science as a whole. As Mach became increasingly critical of "mechanical worldview" and concerned about overspecialization in the 1870's, for example, he became more adamant that the most effective way to rid science of metaphysics and promote interdisciplinary cooperation and understanding was to analyze the "substantial sameness of every-day and scientific thought."[52] Wettstein and his followers were similarly convinced that they could not assure the long-term viability of their research program without producing a robust popular literature that adequately represented their methodological and theoretical commitments and mobilized public opinion in their favor. And finally, the Austromarxists and naturalists associated with the Social Democratic Party of Austria (SDAPÖ) contended that the only way to create a truly free scientific culture, and to assure the triumph of

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[51] Ernst Mach, *Einleitung in die Helmholtzsche Musiktheorie: Populär für Musiker dargestellt* (Graz: Leuschner and Lubensky, 1866), V-VII, 2-4.
[52] Ernst Mach, "Introduction," *Popular Scientific Lectures* 5th ed., trans. Thomas McCormack (Chicago: The Open Court Publishing Company, 1943), vi.

socialism more generally, was to cultivate a proletarian research community that embraced their unique views on scientific knowledge and inquiry.

The origins of the Austrian perspective on the scientific salience of the commonsensical and quotidian were complex, and in certain respects idiosyncratic, but in all three of the aforementioned cases both local and global factors were at work. Mach's belief that "every-day thought" was relevant to the status of mechanical concepts and thermodynamic laws was a direct result of his embrace of Darwinism, which began to take root in Central Europe the same year he received his doctorate in 1860, but it was also heavily influenced by his experiences as a student and *Privatdozent* at the University of Vienna. Wettstein and his students' conviction in the scientific importance of public opinion was also reflective of large-scale changes in biological discourse after 1860. Specifically, their understanding of the constitutive relationship between popularization and biological research grew out of their recognition that the mass-market literature on Darwinism and ecology had helped spread and legitimate concepts like natural selection and "living community" amongst their colleagues. They were uniquely positioned to grasp this development, however, because it had negatively affected their research program, which represented a peculiarly Austrian admixture of neo-Lamarckian theory, phylogenetics, and systematics. Lastly, the Austromarxists' belief that the proletarian mind was the key to liberating science and society grew out of the "revisionism controversy" of the 1890's, which sparked debate about the scientific status of Marxism among socialists across Europe, but it was also rooted in the SDAPÖ's unique ideological emphasis on working-class education and consciousness.

To return to Popper and the Vienna Circle, this dissertation's analysis reveals that both of their accounts were in certain respects correct but in need of significant modification. Popper's claim that fin de siècle Austrian scientists had epistemological reasons for producing public-oriented work was essentially accurate, but these reasons were not only rooted in a deep suspicion of disciplinary myopia and “narrow-mindedness” but in the belief that there was something scientifically valuable, if not essential, embedded in everyday experiences, ideas, and opinions. And the Vienna Circle was correct that many Austrian scientists did not understand “engagement” to mean extended dialogue or parity with the lay public but rather disciplining and mobilizing popular opinion in ways that supported their scientific agendas. Even Mach, who was among Central Europe’s most vocal supporters of integrating amateurs, artisans, and laborers into the scientific community, preferred to construct highly idealized theories of what the “average man” was biologically and historically predisposed to think over finding out what actual workers had to say about the phenomena they encountered in factories and on shop floors.

Historiography

This dissertation makes three major historiographical interventions, although each individual chapter will make smaller, more targeted contributions to the scholarly literatures on Machian philosophy, the history of biology, and the history of Austrian socialism. First, its arguments are intended to contribute to a growing and rapidly evolving body of academic work on popularization. For much of the twentieth century, historians either ignored popular-scientific forms entirely or denied that they had any real relevance to how scientists produced and refined facts. In Alfred Kelly’s pathbreaking *The Descent of Darwin: The Popularization of Darwinism in Germany, 1860-1914*, for example, one can find in-depth analyses of how German Darwinians used popular texts on natural selection to further various cultural and sociopolitical agendas, ranging from regulating the body politic to “extending the radical democratic spirit of 1848,” but virtually nothing on how those texts contributed to biologists' understanding of evolution.[53]

While the origins of this historiographical indifference towards the epistemic contributions of popularization are difficult to pin down, it was in many respects a straightforward reflection of how nineteenth-century scientists themselves described the relationship between their technical and non-technical representations. Another important, if unintentional cause was the so-called "constructivist turn" heralded by Thomas Kuhn's *The Structure of Scientific Revolutions* (1962). Briefly, Kuhn contended that the logical positivists' schematic conception of science as a set of abstract methods for building up objective knowledge out of neutral "protocol statements" overlooked the constitutive role of "paradigms," or communally held conventions, models, rules, and values, in establishing the kinds of questions that were worth answering; the range of phenomena that could be considered scientifically meaningful; and how researchers were to go about investigating them. "Normal science" was essentially a form of historically contingent "puzzle-solving," in other words, because the fundamental purpose of research was to resolve the problems posed by a given paradigm using the conceptual tools, instruments, and rules that it supplied.[54]

In the decades following Kuhn's enormously influential book, academics across a variety of disciplines would incorporate his notion of paradigm, as well as kindred concepts drawn from cultural anthropology, ethnomethodology, and Wittgensteinian philosophy, into their own work. The primary result of this theoretical paradigm-change, so to speak, in scholarly attitudes toward science was a growing emphasis on scientific inquiry as a form of practical reasoning that was conditioned by the intellectual and methodological commitments and material resources of local

[53] Kelly, *The Descent of Darwin: The Popularization of Darwinism in Germany, 1860-1914*,
[54] Thomas Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 2012).

scientific cultures. In the numerous "laboratory studies" that appeared in the late-1970's and 1980's, for example, historians, philosophers, and sociologists sought to demonstrate that knowledge of phenomena like peptides and quarks was inextricably tied to the contingent conventions, interactions, and day-to-day practices of small groups of scientists working at individual or closely connected research sites.[55] Similarly, in their classic *Leviathan and Air-Pump* (1985) Steve Shapin and Simon Schaffer argued that Robert Boyle's experimental program of the 1650's was not just a set of methodological prescriptions but a "form of life" that "rested upon the acceptance of certain social and discursive conventions" as well as forms of social organization that were particular to the Royal Society and Restoration England.[56]

While the constructivist turn represented a welcome corrective to the ahistorical, highly idealized, and often Whiggish accounts of earlier historians and philosophers of science, its practitioners' focus on the highly contextual nature of scientific reasoning obscured and problematized other historical phenomena, including how and why local facts were able to become general knowledge. Or as Shapin and Adi Ophir remarked in a prescient essay in 1991:

> the success of a program dedicated to displaying the situatedness of knowledge generates its successor problem. How is it, if knowledge is indeed local, that certain forms of it appear global in domain of application? Is the global—or even the widely distributed—character of, for example, much scientific and mathematical knowledge an illusion? If it is the case that some knowledge spreads from one context to many, how is that spread achieved, and what is the cause of its movement?[57]

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[55] See, for example: Bruno Latour and Steve Woolgar, *Laboratory Life: The Construction of Scientific* Facts (New Jersey: Princeton University Press, 1986); Andrew Pickering, *Constructing Quarks: A Sociological History of Particle Physics* (Chicago: The University of Chicago Press, 1984); Karin Knorr-Cetina, *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science* (New York: Pergamom Press, 1981); and Ian Hacking, *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science* (Cambridge: Cambridge University Press, 1983). Although for Hacking, the study of the laboratory revealed the poverty of Kuhn's "theory-first" approach.
[56] Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* (Princeton: Princeton University Press, 1989).
[57] Adi Ophir and Steven Shapin, "The Place of Knowledge: A Methodological Survey," *Science in Context* 4 (1991), 15-16.

To his credit, Shapin had already provided a potential answer to these questions in *Leviathan*, where he and Schaffer suggested that literary technologies like the experimental report helped assure global assent to locally-produced matters of fact by enabling far-flung readers to "virtually witness" the experiment itself.[58] The sociologist and philosopher Bruno Latour offered a different, albeit no less compelling answer to the problem of globality in his *Science in Action* (1987), where he argued that local knowledge was able to become universal by virtue of "actor-networks" that extended material and social elements of the laboratory into other spaces.[59] But as James Secord remarked in a speech to the History of Science Society in 2004, these two texts are still merely preludes to a broader project on the circulation of knowledge that remains in its infancy.[60]

Given the relative novelty of circulation as a topic of historical analysis, it is unsurprising that the historical literature on popularization—a circulatory mechanism par excellence—remained relatively small and explanatorily anemic until the late 1990's. As Stephen Pumfrey and Roger Cooter declared in their "Separate Spheres" essay of 1994:

> still shrouded in obscurity are the effects of even the most obvious mechanisms for the transmission of scientific knowledge and culture: the popular press, radio and television….As for the historical significance of such efforts, or the meanings of the various discourses involved, these have hardly begun to be contemplated, let alone explored, explained and compared across cultures, classes and chronologies.[61]

Like Shapin and Ophir, they maintained that this historiographical problem was not merely an oversight but intrinsic to the constructivist position itself, insofar as its emphasis on the "social permeability of science" and elite sites like the laboratory "tended…to close off the space for

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[58] Shapin and Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life*, 60-66.
[59] Bruno Latour, *Science in Action: How to Follow Scientists and Engineers Through Society* (Cambridge, MA: Harvard University Press, 1987).
[60] James Secord, Knowledge in Transit," *Isis* Vol. 95, No. 4 (December 2004), 654-672.
[61] Roger Cooter and Stephen Pumfrey, "Separate Spheres and Public Places: Reflections on the History of Science Popularisation and Science in Popular Culture *History of Science* 32:3 (Sept. 1994), 237-242.

considering the dissemination and cultivation of science in popular culture."[62] Historian Deborah Coen has provided more recent support for this position in a 2012 analysis of Kuhn which argued that his influential identification of "normal science" with paradigms and specialized research communities made it difficult for twentieth-century Anglophone historians of science to even "bring technical and popular science into the same frame of analysis," much less explore the latter's influence on the former.[63]

By the early 2000's, however, historians had begun to take the popularization's contributions to technical science more seriously. There were many reasons for this change in historical perspective, including the "Separate Spheres" essay itself, which quickly became a classic in the field, but several other, older texts also bore some responsibility for laying its conceptual and empirical foundations. As early as 1986, the collected essays of *Expository Science: Forms and Functions of Popularisation* put forth several withering critiques of the "traditional view" of popular media as mechanisms for transmitting simplified information to passive audiences. They also articulated a number of compelling, albeit ahistorical arguments against the assumption that "there is no feedback from popularization to scientific research."[64] Several years later, Stephen Hilgartner provided a similar argument in his "The Dominant View of Popularization: Conceptual Problems, Political Uses" (1990), where he claimed that the "two-stage model" of popularization, which held that scientists first "developed genuine knowledge" and subsequently relied on popularizers to disseminate what were at best simplified accounts of that knowledge, was a distortion that scientists cultivated in order to preserve their cultural

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[62] Ibid.
[63] Deborah Coen, "Rise, Grubenhund: On Provincializing Kuhn," *Modern Intellectual History* Volume 9, Issue 01 (April 2012), 109-126.
[64] *Expository Science: Forms and Functions of Popularization* ed. Terry Shinn and Richard Whitley (Dordrecht: D. Reidel, 1985).

authority.[65] And in in 1994, Anne Secord's analysis of the artisan botanists of Lancashire county in "Science in the Pub" offered further evidence that amateur and lay communities were not merely receptacles for elite scientific knowledge but participants in its construction.[66]

If Cooter, Hilgartner, Pumfrey, and Secord's texts were responsible for severely destabilizing the "traditional" or "two-step" model of popularization, Andreas Daum's *Wissenschaftspopularisierung im 19. Jahrhundert* (1998) and James Secord's *Victorian Sensation* (2000) delivered its deathblow. At first glance, Daum's and Secord's monographs do not have much in common. Aside from focusing on different halves of the nineteenth century and different geographic regions, *Wissenschaftspopularisierung* systematically analyzed popularization in all its various guises over the course of sixty-five years while *Victorian Sensation* focused on the production and reception of a single popular text over approximately two decades. But the lesson that both authors hoped to impart was much the same: since its inception in the early nineteenth-century the popular genre has played an important role in shaping the form and content of scientific discourse, and any serious historical analysis of knowledge production after 1800 could ill-afford to dismiss popular representation as a distortion or mere simplification "real science."

By 2010, the academic literature on popularization looked completely different than it did in 2000 or 1994. For one thing, historians had turned so aggressively against the "traditional view" that it was difficult to find anyone who was still willing to openly defend it.[67] The historiography had also become much larger and more comprehensive, encompassing a diverse array of case

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[65] Stephen Hilgartner, "The Dominant View of Popularization: Conceptual Problems, Political Uses," *Social Studies of Science* 20:3 (1990), 519-539.
[66] Anne Secord, "Science in the Pub: Artisan Botanists in Early Nineteenth-Century Lancashire," *History of Science* Vol 32, Iss. 2 (Sep. 1, 1994), 269-315.
[67] Andreas Daum, "Varieties of Popular Science in the Transformation of Public Knowledge: Some Historical Reflections," *Isis*, Vol. 100, No. 2 (June 2009), 320.

studies on the individuals and groups that have historically popularized science; the sites where lay audiences have traditionally encountered popular-scientific productions; and the various communicative forms that popularizers have used to circulate their message. And finally, historians had articulated a variety of new theories which purported to better explain the popular genre's role in scientific discourse. Most notably, between 2009 and 2012 Deborah Coen and Jonathan Topham proffered models based on Habsburg-Polish immunologist Ludwik Fleck's *Genesis and Development of a Scientific Fact*, which suggested that popular science acted as a kind of common intellectual soil for all the particular sciences by "provid(ing) the background that determines the general traits of the thought style of an expert" and establishing a baseline of "common sense" ideas against which expert knowledge could be judged.[68]

Despite what could justly be called a renaissance in the academic study of the popular genre over the last two decades, a number of serious historiographical gaps and problems remain. One of the most pressing issues in the current literature is the dearth of empirical work on the concrete ways that popular forms influenced scientific discourse. This is not to say that historians have ignored this topic altogether, but that they have been more concerned with articulating models and theories of how popular media contribute to the construction of knowledge in the abstract than with examining the relationship between specific popular representations and specific scientific

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[68] Ludwik Fleck, *Genesis and Development of a Scientific Fact* ed. Thaddeus Trenn and Robert Merton, trans. Fred Bradley and Thaddeus Trenn (Chicago: University of Chicago Press, 1979), 112-115; Coen, "Rise, Grubenhund," 109-126; and Jonathan Topham, "Rethinking the History of Science Popularization/Popular Science," in *Popularizing Science and Technology in the European Periphery, 1800-2000*, ed. Faidra Papanelopoulou, Agusti Nieto-Galan, and Enrique Perdiguero (VT: Ashgate, 2009), 1-20. Fleck is undergoing something of a renaissance in the history of science, particularly among scholars who see *Genesis and Development of a Scientific Fact* as a useful corrective to Kuhn's *Structure*. See: Lorraine Daston, "On Scientific Observation," *Isis,* Vol. 99, No. 1 (March, 2008), 97-110; Michael Friedman, "History and Philosophy of Science in a New Key," *Isis* Vol. 99, No. 1 (March 2008), 125-134; and Bruno Latour, "A Textbook Case Revisited: Knowledge as a Mode of Existence," *The Handbook of Science and Technology Studies,* 3rd Edition, ed. Hackett, Amsetrdamska, M. Lynch, and J Wacjman (Mass.: MIT Press, 2006), 83-112

ideas. Another crucial gap in the literature stems from Anglophone historians' overwhelming emphasis on popularization in a small number of national settings.[69] There are numerous English-language analyses of popularization in Victorian Britain, for example, and a sizeable number of monographs and articles on popularization in Germany and the United States over the same period, but virtually no work on Habsburg Austria, which remains understudied in the history of science in general.[70]

This dissertation seeks to address these historiographical issues in several ways. First, it partially fills the empirical lacuna on the relationship between popular and scientific discourse by providing several novel examples of biologists and physicists using their popular work to make original knowledge claims, intervene in specialist discussions, and settle scientific disputes. In articulating precisely how these scientists deployed their popular work, it also seeks to clarify the genre's niche in the broader ecosystem of fin de siècle epistemic practices. On the one hand, the arguments laid out in chapters one, two, and three support several existing generalizations concerning the way that popularization contributed to knowledge production at the end of the long nineteenth-century, including the theory that it facilitated a productive form of conceptual and linguistic translation;[71] that it presented a synthetic picture of the research landscape;[72] and that it helped marginalized researchers or scientists in marginalized fields to circulate their ideas.[73] On the other hand, chapters one and four suggest that Austrian scientists' understanding of the

[69] Daum, "Varieties of Popular Science in the Transformation of Public Knowledge: Some Historical Reflections," 319-332.
[70] Allan Janik, "Vienna 1900 Revisited: Paradigms and Problems," in *Rethinking Vienna 1900* ed. Steven Beller (New York: Berghahn Books, 2011), 47-48.
[71] Fleck was responsible for one of the original formulations of this point. See: Fleck, *Genesis and Development of a Scientific Fact*, 112-125.
[72] Lightman, *Victorian Popularizers of Nature*, 487.
[73] Lynn Nyhart, *Modern Nature: The Rise of the Biological Perspective in Germany* (Chicago: The University of Chicago Press, 2011).

epistemic value of popular media was also a function of biological and evolutionary considerations that have heretofore escaped historical attention. In drawing attention to these considerations, this dissertation not only aims to shed light on popular forms and practices in an overlooked corner of the European scientific community but to suggest that historians need to make further distinctions between the popular genre's meaning and use in different national contexts. This is not to say that Austrian popularizers completely departed from their British, French, or German colleagues, but that their emphasis on popular representation as a tool for bringing everyday experiences and ideas to bear on specialist science was reflective of a unique set of assumptions, concerns, and interests.

This dissertation's second major historiographical intervention concerns another relatively novel area of inquiry: historical epistemology. According to Hans-Jörg Rheinberger, the essential goal of this nascent field is to analyze "the historical conditions *under* which, and the means *with* which, things are made into objects of knowledge. It focuses thus on the process of generating scientific knowledge and the ways in which it is initiated and maintained."[74] The historical epistemologist is less interested in the genesis and development of particular facts, in other words, than with the history of reason itself. Like the scholarly study of popularization, this project has deep roots in fin de siècle and interwar Austria, and in the work of Edmund Husserl, Fleck, and Mach in particular. It also has roots in Foucault's theory of "epistemes," Kuhn's conception of "paradigms," and Paul Feyerabend's arguments for "epistemological anarchism," which all provided different ways of thinking about the sociohistorical contingency of scientific method and thought. But the field did not take on its modern form until scholars began to reframe scientific reasoning as a form of situated practice in the 1980's.[75]

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[74] Hans-Jörg Rheinberger, *On Historicizing Epistemology: An Essay* trans. David Fernbach (Stanford: Stanford University Press, 2010), 3.
[75] Nicholas Jardine, *The Scenes of Inquiry: On the Reality of Questions in the Sciences* (New York: Oxford

Among the most important and influential—if not problematic—templates for the new, practice-oriented approach to historical epistemology has been the philosopher Ian Hacking's "styles of reasoning" project, a series of essays published in roughly ten-year intervals between 1982 and 2011.[76] Although Hacking's perspective has changed in various ways over the years, his fundamental point in these essays has remained consistent: "there are neither sentences that are candidates for truth, nor independently identified objects to be correct about, prior to the development of a style of reasoning," or what he has also called "a way of thinking and doing" and "a genre of inquiry."[77] This contention about the relationship between truth and style is not altogether different from the logical positivists' infamous claim that "the meaning of a proposition is the method of its verification," insofar as both maintain that the positivity of a statement is a function of the cognitive and material practices that scientists use to assign truth-values. But Hacking departs from the positivists by claiming that truth-making practices are neither timeless nor universal but emerge out of "micro-social interactions and negotiations," follow distinct historical trajectories, and occasionally vanish.[78] When a new style of reasoning comes into being, it therefore introduces new kinds of sentences, entities, types of evidence, and possibilities for truth-or-falsehood. To use one of Hacking's clearest examples: the advent of the statistical style in the early nineteenth-century enabled scientists to formulate novel propositions concerning distributions and frequencies, for example, and to speak intelligibly about theretofore unknown

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University Press, 1991).

[76] Two of the project's most important essays, "Language, Truth and Reason" and "'Style' for Historians and Philosophers," are included in Ian Hacking, *Historical Ontology* (Cambridge: Harvard University Press, 2004). See also: Ian Hacking, "'Language, Truth and Reason' 30 years later," *Studies in the History and Philosophy of Science* 43 (2012), 599-609. As Hacking notes, the notion "style" has a long pedigree in the history of science, going back to Fleck's notion of "thought styles."

[77] Hacking, "'Style' for Historians and Philosophers," in *Historical Epistemology*, 189.

[78] Ibid, 194.

objects like "the average man" and "gross national product."[79] Conversely, when a style dies out it forecloses certain ways of thinking about and engaging with the world. The decline of Paracelsian alchemy did not just preclude natural philosophers from seriously entertaining a range of statements about alchemical properties and relations, it rendered those statements meaningless, i.e., not even candidates for being true-or-false.

In terms of the number of styles active in European science over the last four centuries, Hacking tends to hew closely to the canonical list proposed by historian A.C. Crombie, which includes: (1) the simple method of postulation exemplified by the Greek mathematical sciences; (2) deployment of experiment to control postulation and explore by observation and measurement; (3) hypothetical construction of analogical models; (4) ordering of variety by comparison and taxonomy; (5) statistical analysis and the calculus of probabilities; and (6) the historical derivation of genetic development.[80] At least initially, Hacking attributed the longevity and stability of these six styles to their capacity to "self-authenticate" and "self-stabilize," but in more recent years he has suggested that their persistent hold on the scientific imagination is a reflection of biological and "ecological" factors.[81] He first suggested ecology as an explanans in a 2012 retrospective on the styles project, writing that

> the larger grounds for the canon are not self-consciously rational, and might better be described as ecological. Ecological? On the one hand, scientific thinking—and doing—exploit human capacities, both mental and physical, which in the course of human history we have learned to use and hone…On the other hand, the discovery of how to use those capacities has happened in highly specific local settings at individual moments in time…Why is this 'ecological'? Because a creature with our bodies, including our hands and brains, has discovered how to use its endowments to interact with the world in which

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[79] Hacking, *The Taming of Chance* (Cambridge: Cambridge University Press, 1990).
[80] Hacking, "'Style' for Historians and Philosophers," 181-184. Hacking posited the existence of a seventh "laboratory" style in 1992 but called this move "unwise" in 2012. See: Ian Hacking, "The Self-Vindication of the Laboratory Sciences," in *Science as Practice and Culture*, ed. Andrew Pickering (Chicago: The University of Chicago Press, 1992), 29-64.
[81] Hacking, "'Language, Truth and Reason' 30 years later," 599-609

it finds itself.[82]

Viewed from this perspective, the basic elements of the canonical styles were always latent in the species' cognitive and physical faculties but did not "crystallize" into coherent genres of inquiry until small groups of people situated in particular historical contexts discovered their utility for accomplishing certain kinds of tasks.

For all its importance and influence among historical epistemologists, the "styles project" has also drawn significant criticism.[83] Philosopher Martin Kusch registered three especially salient objections in a 2010 essay for *Studies in History and Philosophy of Science*, arguing that Hacking failed to adequately purge his work of Crombie's archaic "continuist" perspective on the history of science; that his rejection of epistemological relativism is unconvincing; and that his conception of the nature and number of styles is untenable.[84] This dissertation addresses Kusch's third objection in particular. On the one hand, it supports his contention that the Crombie-Hacking list is far too restrictive by demonstrating that the fin de siècle Austrian approach to popularization was, according to Hacking's own criteria, characteristic of a unique "way of thinking and doing." In Mach's popular corpus alone one can find attempts to introduce novel scientific objects ("every-day ideas"); forms of evidence (instinctive thought; public opinion); sentences ("scientific views arise directly out of popular ones…and then gradually develop away"); and techniques of self-authentication and self-stabilization.[85] This is not to say that Mach and his colleagues were the first group of European intellectuals to express an interest in the cognitive world of the average

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[82] Ibid, 600.
[83] Simon Schaffer, "Opposition is True Friendship," *Interdisciplinary Science Reviews* 35:3-4 (2010), 277-290.
[84] Martin Kusch, "Hacking's historical epistemology: a critique of styles of reasoning," *Studies in History and Philosophy of Science* 41 (2010), 158-173.
[85] One way that Mach et. al. consistently sought to preserve their popular analyses from criticism was by casting opposition to them as the product of specialist myopia.

person—indeed, the persistence of this interest throughout the history of human thought suggests the existence of a broader, longue-durée style—but that their peculiar way of reasoning about and with commonplace experiences and ideas was distinct from what came before, as well as the other canonical genres of inquiry.[86]

On the other hand, the Austrians' understanding and use of popularization bears out certain aspects of Hacking's "ecological" conception of how and why certain styles "crystallize" in particular historical contexts. Close examination of the popular work produced by Mach, Wettstein, et. al. reveals that they began to reflect on the meaning and epistemic value of everyday thought in response to seismic changes in their intellectual environment, analogous to the way that organisms develop new adaptations or refine existing ones in response to major changes in their physical environment. As noted above, the argument here is not that European intellectuals were disinterested in commonsense ideas and experiences prior to the late nineteenth-century, but that the rapid transformation of scientific and social life after 1848 imbued them with a new epistemic potential that the Austrian scientific community was uniquely situated to grasp and exploit.

This dissertation's final historiographical intervention concerns a longstanding question in modern intellectual history: is there an Austrian philosophy? Perhaps the most influential answer to this query is the Neurath-Haller thesis ("NHT"), which Otto Neurath articulated in basic form in the *Wissenschaftliche Weltauffassung* and Rudolf Haller later elaborated in a series of essays between 1968 and 1986. The NHT makes three essential claims. First, that a distinct philosophical tradition emerged in Austria between roughly 1848 and 1934 in the work of Ludwig Boltzmann,

[86] Steve Shapin, "Science and the Public," in *Companion to the History of Modern Science* ed. R.C. Olby, G.N. Cantor, J.R.R. Christie and M.J.S. Hodge (New York: Routledge, 1990), 990-1006. According to Shapin, the fifteenth-century alchemist Paracelsus and his followers maintained that "the sequestration of official intellectual from everyday empirical experience and their socialization into esoteric ways of knowing and speaking guaranteed that what they claimed to know was defective."

Bernard Bolzano, Franz Brentano, Edmund Husserl, Ernst Mach, Alexius Meinong, the philosophers of the Vienna Circle, and the early Ludwig Wittgenstein, and that the latter two represented its apotheosis.[87] Second, that this tradition was fundamentally anti-metaphysical, anti-Kantian, empiricist, and realist in orientation; and that its practitioners focused primarily on issues within the philosophy of language and science. And third, that to understand why the Austrian philosophy revolved around these particular commitments and interests one has to look at the historical development of Austrian liberalism, the nationalities conflict, and other features of sociopolitical life in the Habsburg state over the latter half of the nineteenth century.

The NHT has drawn a fair amount of criticism over the last five decades, particularly from scholars who reject the idea that Austrian philosophy was, in Neurath's words, "spared the Kantian interlude."[88] As historian Johannes Feichtinger wrote in his recent *Wissenschaft als Reflexives Projekt*, it was not only the case Kantian thought was alive and well in Vienna circa 1900 but that it also greatly informed the philosophical views of many of Neurath's own logical positivist comrades.[89] The NHT has also come under fire for its attempt to link disparate thinkers like Bolzano, Brentano, and Mach together as part of a common tradition. In philosopher Michael Stöltzner's 1999 analysis of the indeterminist tendencies of Boltzmann, Franz Exner, and Mach, for example, he acknowledged that it was tempting to fit the three physicists within the NHT framework but refrained from doing do because they uniformly rejected "Brentano's version of

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[87] Rudolf Haller, *Fragen zu Wittgenstein und Aufsätze zur Österreichischen Philosophie* (Amsterdam: Rodopi: 1986) 31-44. See also: Rudolf Haller, *Studien zur Österreichischen Philosophie*, (Amsterdam: Rodopi, 1979).
[88] For more on the relationship between neo-Kantianism and logical positivism, see: Alan Richardson, *Carnap's Construction of the World: the 'Aufbau' and the Emergence of Logical Empiricism* (Cambridge, Cambridge University Press, 1998); Friedrich Stadler, *Studien zum Wiener Kreis: Entwicklung und Wirkung des Logischen Empirismus im Kontext* (Vienna: Springer, 1997); Michael Friedman, *Reconsidering Logical Positivism* (New York: Cambridge University Press, 1999); and Nancy Cartwright, Jordi Cat, Lola Fleck, and Thomas E. Uebel, *Otto Neurath: Philosophy between Science and Politics* (New York: Cambridge University Press, 1996).
[89] Johannes Feichtinger, *Wissenschaft als Reflexives Projekt. Von Bolzano zu Freud zu Kelsen. Österreichische Wissenschaftsgeschichte 1848-1938.* (Bielefeld: Transcript-Verlag, 2010)

realism and a priori knowledge."[90]

In spite of strenuous opposition from some academic quarters, the NHT has also attracted supporters. The philosopher Barry Smith has been among the theory's most prominent advocates, albeit with some qualification. In *The Austrian Philosophy: The Legacy of Franz Brentano* (1994), Smith sought to defend all three aspects of the thesis while also centering Brentano's contributions over and above those of the other figures identified by Haller and Neurath.[91] One can also find independent corroboration for aspects of the thesis in work by scholars who study the historical figures that it singles out. In their now-classic *Wittgenstein's Vienna* (1973), Allan Janik and Stephen Toulmin argued that Wittgenstein's philosophical project was less a reflection of his encounters with Englishmen Bertrand Russel and G.E. Moore than of his experiences in pre-war Austria, where Boltzmann, Karl Kraus, Mach, and Fritz Mauthner were formulating bold new critiques of language and anti-metaphysical theories of knowledge. Like Neurath and Haller, Janik and Toulmin also maintained that there was a constitutive connection between these novel critiques and theories and the Dual-Monarchy's sociopolitical issues, including the "failure of liberalism" and "problems of identity and communication" among the state's competing ethnic groups.[92] More recently, Deborah Coen articulated a NHT-esque argument for the constitutive connection between the "habitus" of pre-war Austrian liberals and the Habsburg scientific community's adherence to a form of "translational" empiricism, as well as its peculiar interest in linguistic issues.[93]

The analyses presented over the next four chapters are not intended to definitively prove

[90] Michael Stöltzner, "Vienna Indeterminism: Mach, Boltzmann, Exner," *Synthese,* Vol. 119, No. 1 / 2 (1999), 81.
[91] Barry Smith, *Austrian Philosophy: The Legacy of Franz Brentano* (Chicago: Open Court Publishing, 1994).
[92] Allan Janik and Stephen Toulmin, *Wittgenstein's Vienna* (New York: Sim and Schuster, 1973), 33-67.
[93] Deborah Coen, *Vienna in the Age of Uncertainty: Science, Liberalism, and Private Life* (Chicago: University of Chicago Press, 2007). See also: Deborah Coen, *The Earthquake Observers: Disaster Science from Lisbon to Richter* (Chicago: University of Chicago Press, 2013), 141-162.

or disprove the NHT but to further clarify its advantages and disadvantages. In terms of the former, close scrutiny of the "popular style of reasoning" common to Mach, the Wettstein Circle, and the Austromarxists reveals a deep commitment to the empiricist idea that knowledge stemmed from the accumulation and synthesis of different forms of experience, as well as an abiding interest in the relationship between scientific communication and knowledge. Indeed, nearly seventy years before Wittgenstein's *Philosophical Investigations*, Mach and Wettstein used their popular work to draw attention to the role that communicative practice and linguistic convention played in determining the form and content of scientific facts. It also reveals a fundamental antipathy towards metaphysics. This antipathy was most evident in Mach's popular corpus, which was explicitly intended to rid physics of transcendental speculations and superstitions, but it was also present in Karl Renner and Angelo Carraro's conception of popularization as a means of cultivating a true natural science that was untainted by liberal ideology.

But analysis of the popular work produced by Mach, et. al. also calls several planks of the NHT into question. For example: it is difficult to establish a direct causal connection between their epistemic interest in the everyday and the sociopolitical factors identified by Haller and Neurath. This is not to say that liberalism and the nationalities conflict exerted no influence on Mach or Wettstein's approach to popularization, but that other institutional and intellectual factors, both local and local, were more responsible for convincing them of the scientific value of commonsense. Further, Mach's insistence that there was a "substantial similarity" between everyday and scientific thought brought him into open conflict with other thinkers that the NHT identifies as part of the Austrian philosophical tradition, including Husserl, who accused Mach of "vulgarizing" the scientific enterprise; and Boltzmann, who felt that Mach's insistence that all the elements of a scientific theory be reducible to direct experience was unrealistic.[94] And perhaps most importantly, Mach and his fellow popularizers' belief that sociohistorical factors influenced the form and content of scientific knowledge belies the NHT's claim that Austrian philosophy "rejected Kantianism and the various sorts of historicism and relativism which came in its wake in favor of realisms and objectivisms."[95] In fact, if there was one philosophical conviction that bound Mach, Wettstein, Renner, and Carraro together it was that science was a fundamentally human project and therefore subject to various forms of extra-scientific influence, ranging from individual prejudices to intellectual predispositions rooted in biology and culture.[96]

Outline of Chapters

The first chapter of this dissertation focuses on Mach. It begins by arguing that his decision to use popular lectures and texts to do scientific work was informed by the communicative conventions of the period, as well as his personal belief that popular representation was uniquely suited to convincing his physicist colleagues of the biological nature and adaptive function of knowledge. The chapter then track's Mach's changing conception of the phylogenetic relationship between "every-day" and scientific ideas, highlighting an important shift in his understanding of the concept of substance. Whereas he initially construed physicists' attachment to ideas like matter as a harmful vestige of the discipline's origins in ancient Greek natural philosophy, by 1892 he had come to see substantialism as an instinctive, and oftentimes useful part of human cognition, although he maintained his earlier view that it had no place in a mature science. The chapter closes

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[94] Ernst Mach, *The Science of Mechanics: A Critical and Historical Account of its Development*, 5th ed., trans. Thomas McCormack (La Salle: The Open Court, 1942), 596.
[95] Smith, *The Austrian Philosophy*, 3.
[96] In this respect, there is much to recommend in Feichtinger's claim that the defining characteristic of Austrian philosophy "from Bolzano to Freud and Kelsen" was a reflexive awareness of the contingency of human cognition and knowledge.

by examining the two-fold way that he applied his new conception of substance in his late-career popular work on thermodynamics. First, he sought to persuade his colleagues that two of the dominant theories of the period, energetics and the mechanical theory of heat, misrepresented thermodynamic phenomena by framing them in terms of recondite substantial entities like the atom and energy. He then attempted to position his own "phenomenological" approach to physics, which aimed to provide mathematical descriptions that were ostensibly devoid of ontological baggage, as a more suitable alternative.

Chapter two examines Mach's attempt to use his popular work to foster mutual cooperation and understanding among representatives of different forms of expertise, ranging from university-affiliated professors to industrial laborers who had little formal education but possessed broad bodies of practical experience. The first part of the chapter shows that his belief in the epistemological importance of interdisciplinary exchange, like his critique of substance, was rooted in his biological theory of knowledge, or more accurately, in his theory that ideas evolved over time in response to their application in different contexts. The chapter then examines how he set about building conceptual and empirical connections between the domains of art, labor, the humanities, and natural sciences in popular texts like *Analysis of Sensations* and *Knowledge and Error*. His short-term goal in establishing these connections was to help resolve several issues in physics and physiology, including what he pejoratively called the "pseudoproblem" of consciousness. But as the final part of the chapter argues, he also had two longer-term goals in mind: to unify the sciences into a coherent whole and advance the cause of social democracy by establishing a more dynamic and equitable relationship between scientists and workers.

This dissertation's third chapter analyzes a group of Austrian biologists affiliated with the esteemed botanist Richard von Wettstein. Like the first two chapters, it begins by examining the circumstances surrounding the Wettstein and his followers' interest in popularization, arguing that their acute sensitivity to the genre's influence on biological discourse reflected the marginality of their methodological and theoretical commitments. More specifically, at the end of the nineteenth century Wettstein and his associates found themselves defending a controversial form of neo-Lamarckism and an unfashionable approach to plant systematics. A key part of their defense strategy was to argue that these positions had not gained widespread scientific acceptance because they had been misrepresented in the popular literature. The chapter then analyzes how the Wettstein Circle used their own popularizations to go on the offensive against their critics, focusing on their efforts to build public and scientific support for their research and to forge stronger relationships with amateur botanists, who represented a critical source of labor, and students, who could assure the survival of their research programs.

Chapter four describes how a small cadre of Austrian socialists used popularization to cultivate their novel conception of the scientific worldview among the working classes. The first part of the chapter argues that the primary architects of this worldview were the so-called "Austromarxists," a group of theorists who took it upon themselves to revamp the SDAPO's philosophy of science in response to the revisionism controversy of the 1890's. This same group of theorists also helped establish the network of institutions and voluntary organization that would be charged with transmitting it to the proletariat, including *Die Naturfreunde*, a hiking club that would eventually command tens of thousands of members across Europe. Although the *raison d'etre* of the latter was to cultivate class-consciousness among its members by enabling them to read the socialist lessons that were implicit in the natural world but ignored in bourgeois science, the Austromarxists and club officials also hoped that it could establish a working-class foothold in the scientific community, and ultimately lay the groundwork for a new scientific culture that reflected socialist ideas and values.

The concluding chapter of this dissertation examines popularization in interwar Austria, focusing on Wettstein and his former students August Ginzberger and Friedrich Vierhapper; the physicists Erwin Schrödinger and Philipp Frank; the sociologist and philosopher Otto Neurath; and the Nazi paleontologist Othenio Abel. It argues that the First World War destroyed the sociopolitical world in which they lived by did not alter their conception of the popular genre as a mode of scientific communication and knowledge production. For Wettstein, popularization remained a key tool in the fight to save the inheritance of acquired characteristics from oblivion, while for Frank and Schrödinger it was an integral part of a broader discussion amongst specialists concerning the meaning of new discoveries in the nascent field of quantum mechanics. Abel and Neurath, for their part, shared the Austromarxists vision of popular-scientific work as a means of altering the scientific enterprise to better reflect their ideological and political convictions. The Austrians also maintained elements of their fin de siècle predecessors' belief in the epistemic salience of everyday experience and thought, although from Schrödinger and Frank's perspective the quantum revolution had demonstrated that familiar concepts and intuitions were almost completely inapplicable to the atomic realm. This dissertation closes by posing several historiographical and normative questions for future research, namely: how did historians come to believe that the popular genre was divorced from knowledge production in the first place, and what value does the Austrian approach to popular representation hold for scientists in the twenty-first century?

Chapter One:

Ernst Mach's Popular Science, Part One

Ernst Mach (1838-1916) was not only one of fin de siècle Austria's great scientific talents but one of its most important and influential philosophers. Although his philosophical work was not always well-received and frequently sparked controversy among intellectuals who resented its epistemological and scientific implications, it left an indelible mark on the physics and physicists of the period, including Albert Einstein, Ludwig Boltzmann, Max Planck, Pierre Duhem, and Wilhelm Ostwald. By the time of his death in 1916, his ideas were so deeply embedded in the way that his contemporaries thought about causality, explanation, metaphysics, scientific method, and phenomenology that Einstein could confidently assert that "even…the people who consider themselves opponents of Mach, scarcely know how much of Mach's way of thinking they have absorbed, so to say, with their mother's milk."[1] Given the afterlife of Machian thought in the work of the Vienna Circle, Wittgenstein, Karl Popper, and Paul Feyerabend, it seems plausible to say that philosophers and historians of science continue to grapple with his ideas, if unwittingly, to this day.[2]

For the most part, Mach chose to circulate his philosophical reflections through popular texts and public lectures rather than scientific papers, memoirs, treatises, society proceedings, and

[1] Albert Einstein, "Ernst Mach," in *Ernst Mach -- A Deeper Look: Documents and New Perspectives*, ed. John Blackmore (Dordrecht: Kluwer Academic Publishers 1992), 155.
[2] Hans Hahn, Otto Neurath, and Rudolf Carnap, *Wissenschaftliche Weltauffassung: Der Wiener Kreis*, ed. Friedrich Stadler and Thomas Uebel (Vienna/New York: Springer, 2012); Henk Visser, "Wittgenstein's Debt to Mach's Popular Scientific Lectures," *Mind* Col. XCI, Issue 361 (January 1982), 102-105; Malachi Hacohen, *Karl Popper-- The Formative Years, 1902-1945* (Cambridge: Cambridge University Press, 2002); Paul Feyerabend, "Mach's Theory of Research and Its Relation to Einstein," *Studies in History and Philosophy of Science* 15, 1-22.

other formats more commonly associated, at least historiographically, with the production and dissemination of scientific knowledge.[3] Put in the words of his longtime editor and translator Thomas McCormack, it was Mach's body of popular work, not his technical scholarship, that contained the "first discussion" of the ideas that "afterwards, under other names and other authorship, became rallying cries" in fin de siècle science.[4]

The first part of the present chapter aims to clarify why Mach used popular forms in the way that he did, arguing that his understanding of popularization as a means of advancing his philosophical agenda within the scientific community rather than merely diffusing settled facts to laypersons was informed by two factors.[5] First, it reflected the norms and conventions of scientific communication and publication in late nineteenth-century Central Europe, which ceded a limited legitimacy to the popular genre as a forum for specialist discussion and debate, particularly with respect to matters that scientists deemed philosophical in nature. And second, it was rooted in his belief that popular exposition offered a novel perspective on the evolutionary relationship between popular and scientific reasoning and was therefore uniquely suited to demonstrating the biological nature and adaptive function of knowledge. Or as he remarked in 1895, the fundamental aim of his popular corpus was to show the "substantial sameness of scientific and every-day thought," and thus to convince his colleagues that their research and its products were governed by the same

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[3] On the historiographical invisibility of popularization as an aspect of knowledge production, see: Jonathan Topham, "Rethinking the History of Science Popularization/Popular Science," in *Popularizing Science and Technology in the European Periphery, 1800-2000*, ed. Faidra Papanelopoulou, Agusti Nieto-Galan, and Enrique Perdiguero (VT: Ashgate, 2009), 1-20.

[4] Thomas McCormack, translator's note to *Popular Scientific Lectures*, 5th ed., by Ernst Mach, trans. Thomas McCormack (Chicago: The Open Court, 1943), viii.

[5] On the "diffusionist" model of popularization and its discontents, see: Andreas Daum, "Varieties of Popular Science and the Transformations of Public Knowledge: Some Historical Reflections," *Isis* Vol. 100, No. 2 (June 2009), 319-332; and Ralph O'Connor, "Reflections on Popular Science in Britain," *Isis* Vol. 100, No. 2 (June 2009), 333-345.

"general developmental processes (*allgemeinen Entwicklungsprozesses*)" that ruled over every other aspect of organic life.[6]

As the second part of this chapter will argue, however, Mach's relatively consistent understanding of popularization as a tool for tracing the evolutionary arc of knowledge was accompanied by a persistent uncertainty about the phylogenetic connection between "every-day" and scientific ideas. This uncertainty was particularly evident in his shifting conception of the role of substance, i.e. the naive notion of "an absolutely permanent nucleus, to which cling the other more variable elements" of nature, in the intellectual evolution of physics.[7] In 1871, he maintained that the concept was an arbitrary convention whose pervasive and largely negative influence on physical reasoning and explanation was rooted in "history and custom," but over the course of the late 1870's and 1880's he came to believe that its ubiquity in the history of physics was a result of the human mind's instinctive predisposition to think in substantial terms.[8] By 1892, he was convinced that physicists reflexively used substantial analogies and "pictures" to render novel phenomena "formally" intelligible, and that the concept had played an essential role in enabling legendary researchers like Newton and Huygens to make unfamiliar or obscure aspects of nature amenable to further investigation.[9]

Despite ceding that substantial pictures were integral to the formal development of mechanics and optics, among other fields, Mach nevertheless continued to caution against their

[6] Ernst Mach, "Vorwort," *Populär-Wissenschaftliche Vorlesungen*, 5th ed., ed. Elisabeth Nemeth and Friedrich Stadler (Berlin: Xenomoi, 2014), XVI.
[7] Ernst Mach, *Principles of the Theory of Heat: Historically and Critically Elucidated*, ed. Brian McGuinness, trans. P.E.B. Jourdain and A.E. Heath, (Dordrecht: D. Reidel, 1986), 384.
[8] Ernst Mach, *History and Root of the Principle of the Conservation of Energy*, trans. Philip Jourdain (Chicago: The Open Court, 1911), 56-57.
[9] Ernst Mach, "Zur Geschichte und Kritik des Carnot'sche Wärmegesetzes," *Sitzungsberichte der Kaiserlichen Akademie der Wissenschaften: Mathematisch-Naturwissenschaftliche Classe* Vol. 100 (Vienna: Tempsky, 1892), 1609.

use at the more advanced stages of physical research, arguing that their initial utility and intuitiveness did not change the fact that they were highly artificial, misleading, and liable to obstruct further progress.[10] In keeping with his overarching biological perspective on knowledge, he sometimes described substantialism as a vestigial trait that would gradually be selected against as physicists began to encounter more complex phenomena. Viewed from this perspective, Newton's theory that an object's mass was a function of the quantity of matter that it contained was analogous to the human appendix or hind limbs of a whale, insofar as all three adaptive forms had long since ceased being useful to their bearers.

The third part of this chapter examines how Mach applied his mature understanding of the explanatory function and limit of substance-thinking to the field of thermodynamics in his popular works of the mid-1890's, arguing that it was central to his critique of the two dominant theoretical orientations in the discipline at the time: energeticism and mechanism. Specifically, he used his popularizations to demonstrate that physicists' understanding of heat and its behaviors had been conditioned by the mind's instinctive substantialism "from the very outset," and that harmful vestiges of this instinct were still active in the "obscure and metaphysical" energeticist and mechanist theories of the fin de siècle.[11] When proponents of these theories insisted that thermodynamic processes were functions of transformations of recondite quantities of energy, for example, he maintained that they were not doing so based on disinterested reasoning from the facts but as a result of the same cognitive reflex that led an earlier generation of physicists to picture heat as a fluid-like entity called caloric.[12]

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[10] It is worth noting that Mach began speaking of scientists' mental predispositions as "obstacles" to progress before Gaston Bachelard made the idea famous in *The Formation of the Scientific Mind*.
[11] Mach, *Principles of the Theory of Heat*, 6.
[12] Ernst Mach, "On the Principle of Comparison in Physics," *Popular Scientific Lectures*, 245-246.

Having demonstrated the psychobiological origin and explanatory insufficiency of his colleagues' substance-based theories of heat, Mach then sought to position his own "phenomenological" style of physics, which avoided reference to any entity or process that was not potentially accessible to direct experience, as a more rational and productive approach to describing natural phenomena, thermal or otherwise. He nevertheless stopped short of suggesting that his phenomenological perspective should be imposed on popular thought. The chapter will close by arguing that the ostensible tension between Mach's views on "the kinship between common-sense thinking and that of science" and his desire to destroy a significant conceptual commonality between them reflected his belief that it was provisionally better for scientists and laypersons to simply inhabit different intellectual worlds—one substantial and one not—than to force the average person to abandon a notion that remained highly useful outside of the laboratory and seminar.[13]

The academic literature on Mach is curiously small given his tremendous influence on the intellectual culture of fin de siècle Europe and North America, and of the scholarly works that do treat him in any depth, most ignore his epistemological interest in every-day thought in favor of highlighting other features of his philosophy. One particularly heated debate among Mach scholars has revolved around the nature and extent of his phenomenalism and whether it would be better to classify him as a neutral monist.[14] Another prominent set of discussions has centered on his relationship with other thinkers and intellectual movements. Indeed, perhaps the most enduring topic of conversation among historians concerns his influence on Einstein, but there is also a

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[13] Ernst Mach, *Knowledge and Error: Sketches on the Psychology of Inquiry*, trans. Thomas McCormack (Dordrecht: D. Reidel, 1976), xxxv. Paul Feyerabend was a notable exception to this trend. See: Paul Feyerabend, "Mach's Theory of Research and its Relation to Einstein," *Studies in the History and Philosophy of Science* Volume 15, Issue 1 (March 1984), 1-22.
[14] Erik Banks, "Sympathy for the Devil: Reconsidering Ernst Mach's Empiricism," *Metascience* 21 (2012), 321-330.

formidable amount of scholarship concerning his impact on the gestalt psychologists and logical positivists.[15] More recently, several historians have become interested in his connection to William James and a diffuse movement that Thomas Uebel has dubbed "European pragmatism."[16]

Of the very limited number of historical analyses that have addressed the role of popular thought in Mach's philosophy, the two most important are contained in Steven Fuller's *Thomas Kuhn: A Philosophical History for Our Times* (2000) and Deborah Coen's *The Earthquake Observers* (2013). Specifically, Fuller and Coen have argued that Mach's attempts to integrate everyday ideas into expert science were evidence that adhered to an "epistemology of translation," or the view that scientific knowledge was the product of exchange between many different perspectives rather than reduction to one. In Fuller's account of his polemical exchange with the German physicist Max Planck, for example, he argued that in "Mach's idiosyncratic…vision" of research "the tractability of science to common modes of experience should constrain the development of science nearly as much as science should revise and discipline common modes of experience. Translation, not reduction, was his principle of scientific unification."[17] Coen offered a similar assessment in her examination of Mach's work for the 1895 Imperial Earthquake Commission, arguing that his efforts to bring lay experiences and ideas to bear on seismological

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[15] Friedrich Stadler, *Studien zum Wiener Kreis: Ursprung, Entwicklung und Wirkung des Logischen Empirismus im Kontext* (Franfurt am Main: Suhrkamp, 1997); Gereon Wolters, *Mach I, Mach II, Einstein und die Relativitätstheorie: eine Fälschung und ihre Folgen* (Berlin: De Gruyter, 1987); *Logical Empiricism and Pragmatism* ed. Sami Pihlstroem, Friedrich Stadler, Niels Weidtmann (Switzerland: Springer International, 2017).
[16] Thomas Uebel, "Ernst Mach's Enlightenment Pragmatism: History and Economy in Scientific Cognition," in *Interpreting Mach: Critical Essays* ed. John Preston (Cambridge, Cambridge University Press, 2021), 84-102. See also: Alexander Klein, "On the Philosophical and Scientific Relationship between Ernst Mach and William James," in *Interpreting Mach: Critical Essays*, 103-122.
[17] Steven Fuller, *Thomas Kuhn: A Philosophical History for Our Times* (Chicago: University of Chicago Press, 2000), 130.

problems was reflective of a deeper, if only metaphoric, commitment to “translation” between “all possible perspectives” as a precondition to achieving complete knowledge of a phenomenon.[18]

Although this chapter’s analysis of Mach’s popular work certainly supports Coen and Fuller’s contentions about his belief in the epistemological importance of translation, it also suggests that he was highly wary of the “philosophical views of the average man” and desired to keep scientific reasoning at least partly insulated from their influence.[19] Indeed, in seeking to purge the concept of substance from thermodynamics he was in many ways seeking to remove one of the field’s last bridges to the cognitive world of everyday life, thereby rendering dialogue and exchange between physicists and members of the general public at least provisionally more difficult. It would therefore be more accurate to say that Mach understood translation as an essential but nevertheless limited epistemic practice, and that at times he embraced a weak and temporary form of conceptual incommensurability as a necessary feature of scientific research and precondition of scientific progress.[20]

Mach’s Philosophy of Popularization

Born in Habsburg Moravia in 1838, Ernst Mach (1838-1916) enjoyed a long and successful, albeit controversial scientific career, which not only saw him make important advances in physics and physiology but to help foment an “epistemological movement,” as his ally Pierre Duhem called it, that fundamentally altered physics at the end of the nineteenth century.[21] He first

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[18] Deborah Coen, *The Earthquake Observers: Disaster Science from Lisbon to Richter* (Chicago: University of Chicago Press, 2013), 30.
[19] Ernst Mach, *Analysis of Sensations and the Relation of the Physical to the Psychical*, trans. C.M. Williams (Chicago: Open Court Publishing, 1914), 25.
[20] Mach’s understanding of incommensurability as a contingent outcome of popularization lends credence to Simon Schaffer’s contention that “island universes and distinct systems of classification are not best seen as the preconditions of translation but as among its more significant consequences.” See: Simon Schaffer, “Opposition is True Friendship,” *Interdisciplinary Science Reviews,* Vol. 35, No 3-4 (2010), 277-290.
[21] Pierre Duhem to Ernst Mach, Bordeaux, August 10, 1909, in *Ernst Mach’s Influence Spreads*, ed. John Blackmore, Ryoichi Itagaki, and Setsuko Tanaka (New Hampshire: Sentinel Open Press, 2009), 408.

began articulating the basic framework of his revolutionary epistemology during his years as a doctoral student and *Privatdozent* at the University of Vienna (1859-1863), when he formulated two general positions that would guide his thinking on the nature of scientific knowledge for the rest of this career. The first of these positions emerged from his physiological research on spatial and auditory perception, and posited that human cognition and its products were empirical in origin and ineluctably shaped by the dictates of human biology.[22] Or as he remarked in an 1863 lecture, his studies had led him to believe that the objects of physics, physiology, and psychology existed in "unbreakable connection to one another," and that one could only understand each individual domain in light of its relationship with the others.[23]

That an aspiring physicist like Mach would dedicate much of his time and energy to investigating physiological problems and exploring their philosophical implications had much to do with his childhood interest in Kant. As he recalled in a late-career autobiography, his first encounter with the *Prolegomena to Any Future Metaphysics* at age fifteen "destroyed his naïve realism" and sparked a lifelong interest in the relationship between perception, cognition, and knowledge.[24] His interest in this relationship would only deepen after he arrived at the University of Vienna in 1855 and immersed himself in an intellectual culture that was highly invested in exploring the philosophical aspects of scientific research. One especially lively and portentous dispute within the physics faculty at the time concerned the explanatory legitimacy of analogies

[22] John Blackmore, "Three Autobiographical Manuscripts by Ernst Mach," *Annals of Science* 35 (1978), 401-418.
[23] Ernst Mach, "Vorträge über Psychophysik," *Österreichische Zeitschrift für Praktische Heilkunde* 9 (Vienna: Veit, 1863), 365.
[24] Ernst Mach, "Autobiography," in "Three Autobiographical Manuscripts by Ernst Mach" by John Blackmore, *Annals of Science* 35 (1978), 401-418.

and models.[25] It was also an intellectual culture in which physiology was an area of emphasis and strength. In 1860, he came to know the great physiologists Ernst Brücke and Carl Ludwig personally, and with their encouragement began a physiological research project that related to his extent interest in Johann Herbart's mathematical psychology and Gustav Fechner's new "psychophysics," which sought to provide a quantitative account of the relationship between stimuli and sensation.[26]

The second philosophical position that Mach developed during his early-career tenure at the University of Vienna was rooted in his experiences giving public addresses, private tutorials, and lecturing. Because he was perpetually impoverished, he spent far more time engaged in these remunerative pedagogical activities than he would have liked, complaining that he "naturally lost much valuable time" that would have been better spent pursuing his own research.[27] But he also ceded that the practice of summarizing a vast number of scientific developments and facts in the space of a single lecture sparked his interest in the history of science. It also convinced him that the aim of scientific inquiry, as well as the guiding logic of its historical progression, was not the discovery of immutable truths but the search for "the simplest, most economical, most goal-oriented" ideas.[28]

By the time Mach left Vienna to assume his first professorship at the University of Graz in 1864, he had synthesized his theories about the biological nature of cognition, the unity of physical

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[25] Wolfram Swoboda, "Physik, Physiologie, und Psychophysik — Die Wurzeln von Ernst Machs Empiriokritizismus," in *Ernst Mach: Werk und Wirkung* ed. Rudolf Haller and Friedrich Stadler (Vienna: Hölder-Pichler-Tempsky, 1988), 356-403.
[26] Mach, "Autobiography," 409-410. Mach also had financial reasons for doing physiology. In addition to piquing his interest, he was also attracted to the field was that it was far less expensive to do than experimental physics.
[27] Ibid, 415.
[28] Ernst Mach, "Die Leitgedanken meiner naturwissenschaftlichen Erkenntnislehre und ihre Aufnahme durch die Zeitgenossen," *Scientia* Vol. 7 (1910), 225-226.

and psychical phenomena, and the economizing function of ideas into the unified "biological and economic" epistemology that he would maintain for the rest of his career. According to this new synthetic view, the *raison d'etre* of human thought was the production and refinement of heritable "mental adaptations" that simplified and organized experience in ways that were useful for survival and orientation, meaning that scientific principles and concepts had the same biological origins, and were subject to the same evolutionary pressures, as any other organic trait.[29] In 1865, he began to apply his new epistemological framework to a critical project he had first mentioned in 1861 but only tentatively pursued up to that point: bringing about a "total reformation of (physicists') views on the foundations of physics" and destroying the "one-sided mechanical view" which held that all natural phenomena could be reduced to the "equilibrium and movement of molecules and atoms" in space and time.[30]

One of the first targets of Mach's naturalistic crusade against the mechanical worldview was the concept of space, which he subjected to extensive critique in his 1866 "Über die Entwicklung der Raumvorstellungen." In keeping with his biological and economic theory of knowledge, the primary aim of the paper was to show that space was not a real and necessary feature of nature, as many physicists and philosophers assumed, but a mental construct that reflected the "psychical" organization and evolution of the human organism.[31] Two years later, he directed similar arguments against Newton's concept of mass and the law of inertia, attempting to show that the latter was neither a priori nor "self-evident" but derived from basic sensory experience, and that one could give a more economical definition of former by jettisoning the

[29] Ibid.
[30] Mach, *History and Root of the Principle of the Conservation of Energy*, 49, 86. For Mach's earliest reservations about atomism, see: Ernst Mach, *Compendium der Physik für Mediziner* (Vienna: W. Braumüller, 1863).
[31] Ernst Mach, "Über die Entwicklung der Raumvorstellungen," in *Zeitschrift für Philosophie und Philosophische Kritik* Vol. 49, ed. Fichte, Ulrici, and Wirth (Halle: Pfeffer, 1866), 227-232.

nebulous concept of matter and merely describing the observable relations of bodies.[32] And finally, in his 1871 *History and Root of the Conservation of Energy* he extended his biologico-economic theory of knowledge to encompass the principles of excluded perpetual motion and the conservation of energy, explaining that these cornerstones of contemporary physics were merely highly developed cognitive adaptations to primitive, non-mechanical experiences, and that the atomic interpretations of energy preferred by the mechanists were a form of "mental notation" rather than a reflection of nature *an sich*.[33]

Although these papers "found small sympathy, and indeed were often contradicted" by Mach's colleagues upon their release, if they paid attention to them at all, he remained undeterred, and spent much of the rest of his career clarifying, developing, and expanding on their basic arguments and ideas in a variety of lectures, essays, and monographs, including: "On the Economical Nature of Physical Inquiry" (1881); *The Science of Mechanics* (1883); *Analysis of Sensations* (1886); "On the Principle of the Conservation of Energy" (1894); and *Principles of the Theory of Heat* (1896).[34] These later works received far more acclaim and recognition than their predecessors, and were instrumental in facilitating Mach's promotion to the newly-created chair in the "History and Philosophy of the Inductive Sciences" at the University of Vienna in 1895.[35] They were also far more influential among physicists, and helped place his previously marginalized epistemological views center-stage in the discussions and debates concerning the nature of physical knowledge and limits of mechanical explanation that unfolded in the discipline

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[32] Mach, *History and Root*, 80-86.
[33] Ibid.
[34] Ernst Mach, *The Science of Mechanics: A Critical and Historical Account of its Development*, 5th ed., trans. Thomas McCormack (La Salle: The Open Court, 1942), ix.
[35] Josef Mayerhofer, "Ernst Machs Berufung an die Wiener Universität, 1895" *Clio Medica* 2 (1967), 47-55.

around 1900.[36] By 1905, his ideas were so widespread among his colleagues that the philosopher Philipp Frank, who was then completing his doctorate under Ludwig Boltzmann, would later recall that nearly all the "productive physicists" in Vienna at the time were "more or less followers of Mach in the philosophical sense."[37]

Mach would attribute much of his late-career success to the fact that the release of *Science of Mechanics* in 1883 coincided with the physics community's rising interest in epistemological issues and dissatisfaction with classical mechanics after the mid-1870's; an interest which was closely tied, he noted with some frustration, to the influence of works that had independently recapitulated several of the core arguments of his little-read *History and Root of the Conservation of Energy*. He found the tremendous popularity of Gustav Kirchoff's 1876 *Vorlesungen über Mathematische Physik* to be particularly painful, because his colleagues often mistakenly assumed that his critiques of mechanics and arguments for the economical function of scientific knowledge were indebted to Kirchoff's later ones.[38] But he also tended to exaggerate the novelty of his objections to atomism and other aspects of the Newtonian paradigm. Michael Faraday and several other physicists involved in the field of optics had already questioned the universal applicability of the Newtonian model long before 1871, and in 1865 Faraday's student J.C. Maxwell demonstrated that one could create a mathematically satisfying explanation of electromagnetic phenomena that used ether and contact-forces instead of classical conceits like action-at-a-

[36] Mach counted Ludwig Boltzmann, Heinrich Hertz, Wilhelm Ostwald, Pierre Duhem, Henri Poincare, W.K. Clifford, Karl Pearson, Frantisek Wald, and J.B. Stallo, among others, as intellectual allies.
[37] Philipp Frank, quoted in *Ernst Mach's Vienna, 1895-1930: Or Phenomenalism as Philosophy of Science*, ed. J. Blackmore, R. Itagaki, and S. Tanaka (Dordrecht: Kluwer, 2001), 64.
[38] Mach, *The Science of Mechanics*, xxi. See, for example: James Clerk Maxwell, "A Dynamical Theory of the Electromagnetic Field," *Philosophical Transactions of the Royal Society* 155 (1865), 459-512.

distance.[39] Faraday and Maxwell also suggested that the mechanical elements of their explanations should not be understood as depictions of reality but as artificial models, thereby joining a small but growing chorus of scientists who were convinced that the mechanical worldview was not completely isomorphic with nature, and that Laplacian ideal of grasping the world as one giant mechanical system was a chimera.[40] It would therefore be more accurate to say that Mach was a pivotal figure in an anti-Newtonian avant-garde rather than the tip of the spear.

Another important, if unacknowledged factor in the rapid acceptance of Mach's ideas after 1883 was his decision to communicate them in popular form, which helped create broad readerships both within and outside the scientific community and among younger and non-German physicists in particular.[41] This is not to say that historian Martin Klein's contention that he "did not follow any existing model of historical or philosophical or scientific exposition" in his epistemological corpus is entirely wrong.[42] But it is also clear that he adopted many formal and stylistic qualities that he considered characteristic of popular exposition in order to reach a wider audience, including accessibility, simplicity, and engagement with the everyday. To take two prominent examples: in both *Science of Mechanics* and *Analysis of Sensations* he declared at the outset that he had intentionally avoided mathematics and expressed his arguments in "the language of everyday life" specifically to appeal to all "students of nature" rather than narrow groups of specialists.[43]

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[39] James Clerk Maxwell, "A Dynamical Theory of the Electromagnetic Field," *Philosophical Transactions of the Royal Society* 155 (1865), 459-512.
[40] Hermann von Helmholtz, "Zum Gedächtniss an Gustavus Magnus," *Vorträge und Reden* 4th ed. Vol. 2 (Braunschweig: Viewig und Sohn, 1896), 33-53; and Emil du Bois-Reymond, *Über die Grenzen des Naturerkennens* 2nd ed. (Leipzig: Veit und Comp, 1872).
[41] John Blackmore, introduction to *Ernst Mach's Vienna*, 6.
[42] Martin Klein, introduction to *Principles of the Theory of Heat*, by Ernst Mach, xi.
[43] Mach, *The Science of Mechanics*, xii-xiv. See also: Mach, *Analysis of Sensations and the Relation of the Physical to the Psychical*, vii-ix; Ernst Mach to Wilhelm Schuppe, December 16th, 1902, in *Wissenschaftliche Kommunikation: Die Korrespondenz Ernst Machs*, ed. Joachim Thiele (Kastellaun: A. Henn Verlag, 1978), 88.

Although Mach's decision to use popular media to spread his theory of knowledge was in part a circulation strategy, it also reflected the norms and conventions of communication and publication in late nineteenth-century Central European physics, which upheld the genre as a legitimate vehicle for scientific discourse.[44] He encountered and internalized these norms and conventions in a variety of ways throughout his career. For one thing, he often found himself enmeshed in controversies and discussions that were notable for being anchored in both popular and specialist forums. When he published his "Conservation of Energy" essay in 1895, for example, he noted that the text was part of a broader debate over the energy laws that included a fair number of non-technical and popular representations, including J.P. Joule's key "On Matter, Living Force, and Heat" (1847).[45] Although he did not mention it at the time, much the same could be said of his hostile public exchange with Max Planck over the validity of atomism in 1909 and 1910, which was merely one iteration of a protracted dispute over materialism that physicists had been pursuing in and through popularizations since the 1850's.[46]

Mach was also involved in a variety of scientific associations and organizations that acknowledged popularization as a valid mechanism of scientific communication and sponsored it as part of their institutional missions. The most high-profile of these organizations was the *Gesellschaft Deutscher Naturforscher und Ärtze*, which supported popular lectures as platforms for scientists to synthesize new results and discuss issues of common interest as early as 1822 and

[44] For a general overview of popularization in the German context between 1848 and 1914, see: Andreas Daum, *Wissenschaftspopularisierung im 19. Jahrhundert: Bürgerliche Kultur, naturwissenschaftliche Bildung, und die deutsche Öffentlichkeit, 1848-1914*. (Munich: R. Oldenbourg Verlag, 1998).

[45] Mach, "On the Principle of the Conservation of Energy," *Popular Scientific Lectures*, 137-186. See also: Hermann von Helmholtz, foreword to *Fragmente aus der Naturwissenschaften: Vorlesungen und Aufsätze*, by John Tyndall (Braunschweig: Vieweg und Sohn, 1874), V-XXV; Ludwig Boltzmann, "Der zweite Hauptsatz der mechanischen Wärmetheorie," *Populäre Schriften* (Leipzig: J.A. Barth, 1905), 25-51.

[46] For more on the materialism controversy, see: Frederick Beiser, *After Hegel: German Philosophy, 1840-1900* (Princeton: Princeton University Press, 2014), 53-128.

continued to host them at its annual conference even after placing more emphasis on specialist discussion and exchange in the 1880's.[47] He was also involved in several regionally-significant scientific organizations that mirrored the *Gesellschaft*'s sentiments. In 1862, the Vienna-based *Society for the Spread of Natural Scientific Knowledge* announced that it would regularly host popular lectures as a means for scientists to edify the lay public and present new research to their colleagues. By the end of the decade, it was not only responsible for hosting the first scientific discussion of Darwinism in Austria but for circulating some of Mach's original research on projectiles and shockwaves.[48] The Austrian Academy of Science and Prague-based *Lotos*, while less geared towards public edification than the *Gesellschaft* or *Society for the Spread of Natural Scientific Knowledge*, also hosted scientifically-oriented popular addresses, including Mach's famous "On the Economical Nature of Physical Enquiry" lecture and a number of other talks he would later include in his *Popular Scientific Lectures*.

Given the prominence of popular lectures and texts in fin de siècle scientific discourse and associational life, it is unsurprising that German-language reference works and scientific journals granted them a certain amount of scientific credibility, either by cataloguing and reviewing them as elements of the specialist literature or by publishing them as original contributions to knowledge. Prominent review organs in physics like *the Beiblätter zu den Annalen der Physik und Chemie* frequently evaluated new popular texts alongside more technical works, for example, and bibliographic services like the *Fortschritte der Physik* and *Biographisch-literarisches*

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[47] Lorenz Oken, "Versammlung der Deutschen Naturforscher," *Isis: encyclopädische Zeitschrift, vorzügl. für Naturgeschichte, vergleichende Anatomie und Physiologie* (1821), 198; and Yvonne Steif, *Wenn Wissenschaftler Feiern: Die Versammlungen deutscher Naturforscher und Ärzte 1882 bis 1913* (Stuttgart: Wissenschaftliche Verlagsgesellschaft, 2003).

[48] Eduard Suess, "Über die Entstehung und die Aufgabe des Vereines," *Schriften des Vereines zur Verbreitung naturwissenschaftlicher Kenntnisse in Wien* I (1862), 5.

Handwörterbuch elected to list popularizations as contributions to the scientific literature even after peer publications like the British *Catalogue of Scientific Papers* largely ceased to do so.[49] More importantly, many of Central Europe's flagship venues for original physical research, including the *Annalen der Physik* and the *Sitzungsberichte* of the Austrian Academy of Science, continued to publish popular articles alongside technical papers well into the 1900's.[50] Mach had far more luck getting his popular work accepted in the latter, but his colleague Boltzmann was able to successfully publish at least four texts that he categorized as popular in the *Annalen*.[51]

The *Annalen* and *Gesellschaft*'s willingness to circulate popularizations alongside technical works was partly related to the fact that there were no universal criteria for defining the popular genre or demarcating it from more specialized forms of communication, which allowed physicists to create popular works that were far more characteristic of scientific papers than magazine articles or feuilletons.[52] Indeed, the lack of consensus on precisely how technical a popularization could be was often a source of friction between Mach, his publishers, and his

[49] J.C. Poggendorf, "An unsere Leser," *Beiblätter zu den Annalen der Physik und Chemie*, Vol. 1, ed. J.C. Poggendorff (Leipzig, J.A. Barth, 1877), xvii-xviii. For reviews and references to Mach's *Popular Scientific Lectures* in these sources, see: *J.C. Poggendorff's Biographisch-Literarisches Handwörterbuch zur Geschichte der Exacten Wissenschaften*, Vol. 4, ed. Arthur Oettingen (Leipzig: J.A. Barth, 1904), 937; and *Beiblätter zu den Annalen der Physik*, Vol. 27, ed. Walter Koenig (Leipzig: J.A. Barth, 1903), 403-404. For more on the *Catalogue*, see: Alex Csiszar, *The Scientific Journal: Authorship and the Politics of Knowledge in the Nineteenth Century* (Chicago: University of Chicago Press, 2018), 199-281. Despite its relatively strict criteria for inclusion, the *Catalogue* nevertheless listed Mach's popular "Economy" lecture, perhaps because he originally delivered it at the Austrian Academy of Science.

[50] Mach's "Über die Wissenschaftliche Anwendungen der Photographie und Stereoskopie" originally appeared in the *Sitzungsberichte der kaiserliche Akademie der Wissenschaften. Mathematisch-naturwissenschaftliche Classe*, Vol. 54 (1866) and was eventually incorporated into the third German edition of his *Populär Wissenschaftliche Vorlesungen*.

[51] Boltzmann's "Über die Unentbehrlichkeit der Atomistic in der Naturwissenschaften," "Zur Energetik," "Ein Wort der Mathematik an die Energetik," "Nochmals über die Atomistik," and "Über die Frage nach der objektiven Existenz der Vorgänge in der unbelebten Natur" all first appeared in the *Annalen* or the Austrian Academy's *Sitzungsberichte* and were later reprinted in his *Populäre Schriften* (1905).

[52] Nineteenth-century Germanophone science in general was littered with popular works whose content shaded off into the scholarly. Alexander von Humboldt's *Kosmos* and Ernst Haeckel's *Die Natürliche Schöpfungsgeschichte*, for example, were both written in a mixed scholarly-popular mode. See: Daum, *Wissenschaftspopularisierung*, 273-279; and Nick Hopwood, *Haeckel's Embryos: Images, Evolution, and Fraud* (Chicago: University of Chicago Press, 2015), 67.

reviewers. His *Principles of the Theory of Heat* shifted so dramatically between generalized and highly technical, heavily-footnoted exposition that his contact at the publishing house J.A. Barth, Arthur Meiner, eventually suggested that the work be split into two separate texts, one marketed to physicists and one to more general audiences.[53] The same tension was evident in his *Popular Scientific Lectures*, which was intended for both lay and specialist readerships but contained chapters that were so inaccessible to non-scientists that his colleague Anton Lampa, a physicist and prominent advocate of adult science education in Austria, suggested that the work should not be labeled a popularization at all.[54]

For the most part, however, physicists tended to value popular exposition not for its ability to mimic scientific papers and treatises but for its capacity to address issues and raise arguments that were difficult to address within the strictures of the latter, and which were lacking in specialist discourse more generally. In one of his own collections of popular essays, Lampa argued that physicists' focus on a progressively smaller and more technical range of problems had hindered their ability to pursue the creation of "a true *Naturphilosophie*" and held up popularization as a means for his colleagues to re-engage with the philosophical aspects of their research.[55] Boltzmann raised similar points in a popular lecture that he delivered at the *Versammlung Deutscher Naturforscher und Ärtze* in 1899, arguing that the "exceedingly minute division of labor" which had come to characterize contemporary science, while immensely productive, had obscured "the

[53] Arthur Meiner to Ludwig Mach, June 17, 1918, NL 174, Konstanz Abgabe, No. 66, Deutsches Museum.
[54] Anton Lampa, "Bücherbesprechung," *Zentralblatt für Volksbildungswesen* Vol 3., No. 4 (March 19th, 1904), 56. Helmholtz and his editors noted similar problems with respect to his own collection of popular lectures and essays, and eventually opted to drop the word "popular" from its title altogether, adopting the more accurate *Lectures and Speeches* in 1884. See: Hermann von Helmholtz, *Vorträge und Reden* 4th ed., Vol. 1 (Braunschweig: Friedrich Viewig und Sohn, 1896), VII-VIII.
[55] Anton Lampa, "Einleitung," *Naturkräfte und Naturgesetze. Gemeinverständliche Vorträge* (Vienna: Ignaz Brand, 1895), XV-XVI.

broad view of the whole which is an indispensable requisite of ideal scientific research." Like Lampa, he suggested that scientists could "offset this drawback" of specialization by using popular expositions to "survey...the development of the special provinces of knowledge" in which they worked."[56] Or as he noted some years earlier in a popular address delivered to the Austrian Academy of Science, the practice of "treating a narrowly circumscribed specialist topic before a wider public should not be entirely without interest" to the scientific community itself, because it offered a means of establishing connections and commonalities between increasingly isolated fields and subfields.[57]

Mach also stressed the importance of popularization as a means of examining ideas and issues that were omitted from, or obscured in, specialist discourse. His understanding of the popular genre as a corrective to expert myopia was partly rooted in the difficulties that he had encountered in getting his philosophically oriented work published in physics journals in the 1860's and 1870's. In 1871, he dedicated several paragraphs of *History and Root* to complaining about his colleagues' inveterate intellectual conservatism and assailing the *Annalen*'s tendency to reject any paper which was "not wholly written" in disciplinary jargon before noting that these circumstances had compelled him to circulate his ideas via popular lectures and articles published in venues that were "little read by physicists."[58] But he continued to hold up popularization as a valuable counterbalance to the overly technical specialist literature even after his work began to

[56] Ludwig Boltzmann, "The Recent Development of Method in Theoretical Physics," *The Monist* Vol. 11, No. 2 (January 1901), 226-227.
[57] Ludwig Boltzmann, "The Second Law of Thermodynamics," *Theoretical Physics and Philosophical Problems*, ed. Brian McGuinness, trans. Paul Foulkes (Dordrecht: D. Reidel, 1974), 13-33.
[58] Mach, *History and Root*, 71, 80. Mach held this to be a relatively common occurrence in the history of physics, noting that many of the most important preliminary works on energy conservation, including Mayer's first paper and Helmholtz's "Über die Erhaltung der Kraft," were rejected by specialist journals. See: Mach, *Principles of the Theory of Heat*, 230.

gain more widespread acceptance among physicists in the 1880's and 1890's. In the introduction to *Science of Mechanics*, for example, he explained that the text was intended to draw attention to aspects of the field that were "of greatest and most general interest" but were "completely buried and concealed beneath a mass of technical considerations" in existing treatises.[59] He would later frame *Popular Scientific Lectures* in much the same way, writing that he hoped the collection would force his colleagues to consider the meaning and implications of their research from the perspective of "the collective whole" of humanity rather than that of a small and inward-looking circle of experts.[60]

In some respects, Mach's valorization of popular exposition was an extension of his advocacy for work of any kind, popular or otherwise, that challenged and undermined the "learned secrecy-mongering" of professional physics.[61] He was a frequent champion of texts by autodidacts and academic outsiders like Viktor Hüber and J.B. Stallo because he felt that their freedom from disciplinary constraints allowed them to explore scientific topics with the kind of "idiosyncrasy and independence" of thought that was essential to intellectual progress but uncommon among orthodox physicists.[62] He was also deeply interested in science pedagogy, and over the course of his career he not only helped found a journal on the topic—namely, *Zeitschrift für den Physikalischen und Chemischen Unterricht*—but co-authored several physics textbooks based on the belief that reforming the way that the discipline was taught was an essential component of waking his colleagues from their dogmatic slumber. But his wide-ranging interest in the scientific potential of other marginal genres did not keep him from ascribing a special importance to

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[59] Mach, *Science of Mechanics*, xi

[60] Mach, *Popular Scientific Lectures*, i.

[61] Mach, *Knowledge and Error: Sketches on the Psychology of Inquiry*, 165.

[62] Ernst Mach, foreword to *Die Begriffe und Theorien der Modernen Physik*, by J.B. Stallo (Leipzig: J.A. Barth, 1901), I-XI.

popularization, which he claimed had a unique epistemic function that differentiated it from technical articles, textbooks, and any other medium then available, namely: it analyzed the evolutionary relationship between scientific and everyday thought.

Mach first indicated that his popular works were intended to offer this kind of evolutionary analysis in the mid-1860's, around the same time he began formulating his early critiques of space and inertia. Specifically, in his 1866 *Introduction to Helmholtzian Music Theory: Represented Popularly for Musicians* he argued that popular expositions were "not indifferent to the development of science itself" because they served to relate ideas that were sealed away in the scientific literature to ideas that were "generally widespread, or popular," and therefore helped to "complete" the former by (re)familiarizing scientists with the primitive notions and experiences that were at their core.[63] He added another layer to this claim in a popular lecture that he delivered on optics that same year, where he argued that the point of examining the developmental relationship between primitive and scientific ideas was not only to clarify the content of specialized concepts and facts but to demonstrate that

> new thoughts do not spring up suddenly. Thoughts need their time to ripen, grow, and develop...like every natural product; for man, with his thoughts, is also a part of nature. Slowly, gradually, and laboriously one thought is transformed into a different thought, as in all likelihood one animal species is gradually transformed into new species.[64]

The aim of the popularizer was like that of the Darwinian biologist, in other words, because both sought to show that certain classes of phenomena did not come into being *ex nihilo* but developed over time from common ancestors.

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[63] Ernst Mach, *Einleitung in die Helmholtzsche Musiktheorie: Populär für Musiker dargestellt* (Graz: Leuschner and Lubensky, 1866), V-VII, 2-4.
[64] Mach, "The Velocity of Light," *Popular Scientific Lectures*, 63.

Mach would take virtually the same position on the epistemic function of popularization nearly thirty years later in his *Popular Scientific Lectures*, a collection that initially contained twelve hand-chosen talks and essays spanning from the mid-1860's to 1894 but would eventually encompass thirty-three pieces spread out over three English and five German editions. Although each of these editions served a slightly different purpose, he made clear that the fundamental aim of the text's ever-expanding roster of lectures and essays remained the same, namely:

> to exercise a favorable influence by showing the substantial sameness of scientific and every-day thought. The public, in this way, loses its shyness towards scientific questions, and acquires an interest in scientific work which is a great help to the inquirer. The latter, in his turn, is brought to understand that his work is a small part only of the universal processes of life (*allgemeinen Entwicklungsprozesses*).[65]

For critics like Edmund Husserl, these attempts to associate scientific reasoning with "vulgar thinking" were degrading to the former, but from Mach's perspective his phylogenetic analyses served to "exalt" the scientific enterprise because they showed that knowledge was not the exclusive province of an aloof scholarly caste but "deeply rooted in the life of humanity and reacting powerfully upon it."[66]

Rethinking Substance, 1871-1892

Although Mach's position on the overarching epistemic function of popular representation remained relatively consistent from the 1860's onward, his conception of the relationship between certain elements of "every-day thought" and the concepts, principles, and theories of specialist science changed over the course of the 1870's and 1880's. Some of these changes were relatively minor and merely reflected his efforts to keep abreast of new biological terminology, but others betokened more significant alterations in perspective. Critically, he gradually abandoned his initial

[65] Mach, "Preface," *Popular Scientific Lectures*, I.
[66] Mach, *Mechanics*, 596; Mach, *Principles of the Theory of Heat*, 5-6.

theory that the commonsense concept of substance was a socio-historical convention that exerted an entirely negative influence on the scientific mind in favor of an account that stressed its biologico-evolutionary origins and constructive, if not essential role at the early stages of scientific reasoning about natural phenomena.[67]

Mach put forth his clearest argument for the conventional status of the concept of substance in his 1871 *History and Root*. He began his analysis by reiterating the same general point he had made in his work on Helmholtzian music theory and optics vis-a-vis the importance of understanding how the "lower stages of knowledge" structured the form and content of the "higher" stages of scientific thought. He then sought to show that several cornerstones of the scientific worldview, including the principle of excluded perpetual motion, the principle of the conservation of energy, and the kinetic theory of heat, had evolved over time from ancient, non-mechanical intuitions about the natural world.[68] Put briefly, he held that the principles of excluded perpetual motion and energy conservation were ultimately scientific restatements of the commonsense idea that everything has a cause, and that the kinetic theory of heat was merely a more abstract and developed iteration of the basic idea that reality is composed of substantial things.

But Mach also drew an important distinction between the status of the primitive causal and substantial intuitions that were at the core of the energy principle and kinetic theory. Whereas he maintained that causal thinking was an inherited and instinctive feature of human cognition, he dismissed substantialism as an arbitrary "memoria technica" whose fundamental role in scientific explanation was less a function of the human organism's psychic organization than of historical

[67] Mach, *Principles of the Theory of Heat*, 388.
[68] Mach, *History and Root of the Principle of the Conservation of Energy*, 50.

accident and convention.[69] Put another way, he argued that the ideas that human beings were predisposed to use to make sense of the world around them were not always rooted in evolutionarily-acquired tendencies, a la causality, but could also reflect contingent factors that varied "with the standpoint of...culture," a la substance. He was even able to identify approximately when the "metaphysical custom" of framing nature in terms of atoms and corpuscles began, arguing that one could trace it back to ancient Greek thinkers like Democritus.[70] The goal for physicists in the present, he concluded, was to recognize that their attachment to obscure substantial entities like matter was unnecessary and to get rid of the notion entirely.

While Mach was satisfied with many aspects of his argument in *History and Root*, he would nevertheless gradually abandon his theory that substance was an arbitrary socio-historical convention in favor of a biological account of its origin and cognitive function. This change in his perspective was first clearly in evidence in his 1881 lecture on "The Economical Nature of Physical Enquiry," in which he posited that nature had "impelled" human beings to form the notion of substance as an adaptive response to recurrent "permanencies" that the mind perceived in the flux of nature, and that this adaptation had become an instinctive part of human cognition, much like cause and space.[71] While this new theory did not lead him to soften his position on the unsuitability of substance for much beyond "the crude purposes of common life" in the present, it did lead him to suggest that the concept had played a constructive role in the past by granting researchers intellectual purchase on unfamiliar phenomena.[72] The contemporary scientist might laugh at the naïve substantialism of a child's inquiries about whether heat entered and exited objects like water

[69] Ibid, 49.
[70] Ibid, 56-57.
[71] Mach, "The Economical Nature of Physical Enquiry," *Popular Scientific Lectures*, 203.
[72] Ibid, 191.

being poured into and out of a cup, he remarked, but the same naïve, instinctively arising question had enabled the early pioneers of thermodynamics to formulate testable hypothesis about thermal phenomena and establish the scientific foundations of the field.[73]

Mach would continue to expand on these thoughts about the instinctive role of substance in scientific reasoning over the next several years in "On Transformation and Adaptation in Scientific Thought" (1883) and *Analysis of Sensations* (1886). Taken together, these texts provided a more thorough description of the psychophysical and evolutionary factors that he claimed had driven the human mind to adopt the concept of substance in the first place and articulated a clearer account of the psychological processes that informed its persistent use in physics. His arguments in both pieces not only drew heavily on his biological and economic epistemology but his "*Elementenlehre*"; an ontological theory that held that the world was composed of a continuous fabric of ontologically neutral, a-spatial, and a-temporal "elements" that existed in functional relationships with one another.

At the beginning of *Analysis*, for example, Mach argued that cognition began when the elemental complexes we call "objects" first came into contact with the elemental complexes that comprised the human nervous system and mind, thereby producing the primitive "colors, sounds, temperatures, pressures, spaces, times, and so forth," that formed the empirical bedrock of experience.[74] He then contended that over long periods of time the human cognitive apparatus reflexively developed heritable mental adaptations, i.e., ideas, that instinctively organized and "economized" recurrent patterns in this tapestry of "colors, sounds, temperatures," in ways that

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[73] Ibid, 189, 200.

[74] Mach, *Analysis of Sensations*, 2, 25. For more on Mach's understanding of "elements," see: Erik Banks, *Ernst Mach's World Elements: A Study in Natural Philosophy* (Dordrecht: Kluwer, 2003).

were helpful for survival.[75] Substance was likely among the first of the species' primitive mental adaptations, he continued, because it captured an immensely useful and omnipresent feature of our perceptual experience: elemental complexes that seemed to remain "unconditionally constant" amidst the flux of nature. He cautioned that this unconditional constancy was an illusion but ceded that the assumption that a permanent substantial nucleus existed behind the stable network of elemental relations denoted by terms like "moon" or "tree" was nevertheless economical and enormously helpful for simplifying and manipulating the environment.

Having established that substance was part of the species' instinctive conceptual apparatus, Mach then sought to show how it was operative in science. As in his 1881 "Economy" essay, he maintained that its effects were at their most palpable and useful when researchers were attempting to make sense of novel phenomena, writing that

> where we cannot at once follow a new fact, the strongest and most familiar thoughts press forward to mould it into a richer, more definite shape...Thus we think of planets as projectiles, we figure to ourselves an electric body as covered with a fluid that acts a distance, we think of heat as a substance that passes from one body to another, until finally the new facts become as familiar and intuitive as the older ones, which we have used as mental helps.[76]

The many substances that populated the history of physics were products of the psychological fact that "the ideas that have become most familiar through long experience are the very ones that intrude themselves into the conception of every new fact observed," in other words, and had played a critical role in enabling scientists to initially explain obscure physical processes like planetary motion and conduction.[77] Even the great Newton, who was otherwise so careful not to feign

[75] An essential point of reference in Mach's understanding of mental adaptation and the heritability of ideas was Ewald Hering's 1870 *Über das Gedächtnis als allgemeine Funktion der organisierten Materie* (Leipzig: Engelmann, 1905).
[76] Ibid, 327.
[77] Mach, "On Transformation and Adaptation in Scientific Thought," *Popular Scientific Lectures*, 228.

hypotheses or to speculate beyond the bounds of what experience permitted, posited that mass was a function of some recondite "quantity of matter" because he could not help but think about moving bodies and their interactions in substantial terms.[78]

The final turn in Mach's understanding of substance and its role in scientific reasoning came in his 1892 essay "History and Critique of Carnot's Theorem," in which he further clarified his contention that there was an intrinsic relationship between the mind's instinctive substantialism and the specific conceptual forms that physicists imposed on their objects of study. The key to his new argument in the "Carnot" piece was an updated theory of the research process which held that the path from the initial investigation of a phenomenon to the formulation of scientific principles and laws passed through three developmental stages: the formal, the experimental, and the quantitative. Inherited mental categories like substance exerted the most influence on the first stage, he argued, because they provided researchers with ready-to-hand conceptual tools for rendering novel or poorly understood phenomena intelligible, and thus amenable to experimental and quantitative investigation.[79] What scientists called "discovery" usually referred to this process of unconscious formalization. The physician J.R. Mayer's initial construal of the interconversion of heat and work as the transfer of a conserved quantity of some recondite third entity therefore "had little to do with the discovery of (new) facts," he explained, but with the "discovery of a formal conception" or "form of viewing" the facts that gave him firmer intellectual purchase on them.[80] Despite acknowledging the utility of substance at the formal stage of inquiry, he continued to maintain that the concept was harmful at the experimental and quantitative stages because it hindered physicists' capacity to understand physical properties and processes that were not

[78] Mach, *Science of Mechanics,* 265.
[79] Mach, "Carnot'sche Wärmegesetze," 1606-1609.
[80] Ibid.

explicable in substantial terms, similar to the way that the categories of Aristotelian physics had prevented the scholastics from grasping those aspects of mechanical phenomena that were not explicable in terms of natural and violent motion.

Mach's Popular Critiques of Thermodynamics, 1894-1896

Mach did not see his arguments in "History and Critique of Carnot's Theorem" as a significant departure from the core positions that he had first articulated in *History and Root of the Conservation of Energy* nearly twenty years earlier.[81] And in many respects his stance on the continuity between the two works was warranted: both sought to show that highly abstract and seemingly a priori principles like the conservation of energy were rooted in primitive, non-mechanical experiences, and both argued that scientific investigation was shaped by a host of instinctive intuitions of which the investigator was largely unaware. But his detailed analysis of the psychological processes driving the research of pioneering investigators like Mayer in the 1892 text nevertheless marked the endpoint of a long transformation in his understanding of the concept of substance and its role in physical reasoning.[82] It also signaled the beginning of a period of renewed critical interest in thermodynamics, and in the years following the "Carnot" essay's publication he would expand on its basic arguments in a series of popular works intended to articulate his definitive position on the nature of heat and the energy laws, on the one hand, and to sketch out his vision for the future of the field on the other.

Mach's decision to focus his attention almost entirely on thermodynamics in the mid-1890's stemmed more from personal and professional considerations than any one incident in the

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[81] Ibid, 1596.

[82] As further evidence of his change of heart on substance, one might turn to a paragraph he appended to the 1897 edition of *Science of Mechanics*, where he noted that Newton's "the notion of mass as quantity of matter was *psychologically* a very natural conception…The concept developed quite instinctively; it is discovered as a datum perfectly complete, and is adopted with absolute ingenuousness." See: Mach, *Science of Mechanics*, 239.

field. He wrote "On the Principle of the Conservation of Energy" (1894) partly at the behest of his friend and the editor of *The Monist* Paul Carus, who convinced him that publishing a new piece on the energy laws would be a good way to recirculate ideas that he had first expressed in the long since out-of-print *History and Root*.[83] His monograph-length *Principles of the Theory of Heat* (1896), by contrast, emerged out of his university lectures, which impressed upon him the many "intolerable logical anomalies" present in the received view of thermodynamics, as well as his longstanding plan to write a sequel to *Science of Mechanics*.[84] But he also positioned the 1894 and 1896 texts, as well as his "On the Principle of Comparison in Physics" lecture, as a response to the issues raised by the high-profile debate over "energetics" that played out at the 1895 *Versammlung Deutscher Naturforscher und Ärtze* in Lübeck and in the *Annalen der Physik* the following year.[85]

Energetics, or the view that energy is the fundamental building block of reality and that the proper aim of physics is the mathematical description of macroscopically observable relations between different forms of energy rather than reduction to molecular motion, was not new when it came up for discussion in 1895. Indeed, from Mach's perspective the Lübeck controversy was merely the latest iteration of a longer-running conflict between competing "phenomenological and mechanical" conceptions of physics that stretched back to Hooke and Newton.[86] But it was only in the mid-1890's, as historian Robert Deltete has noted, that the energeticist position became "sufficiently coherent and widespread enough to have gained a measure of recognition" and to be "at least vaguely recognized to be a possible alternative to a mechanics-based natural science by

[83] Paul Carus to Ernst Mach, Nov. 13th,1893, NL 174, no. 858, Deutsches Museum.
[84] Mach, *Principles of the Theory of Heat*, 2-3.
[85] Mach, *Populär-Wissenschaftliche Vorlesungen*, XVI; Brian McGuiness, "Editor's Note to the English Edition," *Principles of the Theory of Heat*, xxi.
[86] Mach, *Principles of the Theory of Heat*, 333.

competent observers not involved in its promotion."[87] Ludwig Boltzmann, who was then one of Central Europe's staunchest defenders of atomism, described the situation circa 1895 in the more dire terms of internecine war, and characterized energetics as one among several "secessionist" movements that posed a legitimate—if epistemologically and scientifically unwarranted—threat to the hegemony of the mechanical worldview.[88]

The Lübeck conference was thus an occasion for the defenders and opponents of the newly ascendant theory to plead their case to the broader Germanophone scientific community, and although Mach was not there to offer his opinion directly, many of the physicists who were present took it for granted that his views aligned with those of the energeticist camp. The German theoretician Arnold Sommerfeld spoke for many when he remarked that Georg Helm's "report on energetics" was heavily influenced by the work of Wilhelm Ostwald, and that both thinkers were deeply indebted to "the *Naturphilosophie* of Ernst Mach."[89] Although Sommerfeld was generally hostile to Mach, and therefore not particularly well-suited to judge his ideas, his assumption was well-supported by the many surface-level similarities between Helm, Mach, and Ostwald, including their mutual antagonism towards atomism and belief in the fundamentally descriptive nature of scientific knowledge. It was also supported by Mach's own description of his intellectual relationship with Helm in the 1889 edition of *Science of Mechanics*, where he claimed that the latter's recently released *Die Lehre von der Energie* (1887) so agreed with his own investigations that he had "seldom read anything that, without the obliteration of individual differences, appealed in an equal degree to my mind."[90]

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[87] Robert Deltete, "Helm and Boltzmann: Energetics at the Lübeck Naturforscherversammlung," *Synthese* Vol. 119, No. 1-2 (1999), 48.
[88] Boltzmann, "Entwicklung der Methoden der theoretischen Physik," *Populäre Schriften*, 219.
[89] Deltete, "Helm and Boltzmann," 56.
[90] Mach, *Science of Mechanics*, 608.

When Mach finally offered his assessment of the Lübeck controversy in his popular works of the mid-1890's, however, he painted a more complicated picture of his philosophical kinship with Helm and the energeticists, arguing that both the latter and their mechanist opponents continued to operate within a primitive and misleading substantialist framework. The primary engine of his argument was a historical analysis of the evolution of thermodynamics that focused heavily on three case studies: Joseph Black's discovery of latent and specific heat in the 1750's; Sadi Carnot's theoretical work on heat engines around 1810; and J.R. Mayer and J.P. Joule's independent formulation of the mechanical equivalent of heat and principle of the conservation of energy in the 1840's. His emphasis on historical figures like Black and Carnot in the context of a contemporary debate that was in large part about ontology must have seemed unusual to his colleagues, given that the discipline had long since rejected the older physicists' conception of heat as a universally conserved, fluid-like entity called caloric. But as his analysis sought to show, this rejection was only apparent, insofar as many of the caloric theory's core features continued to live on in the energy-based thermodynamics that first emerged in the research of Mayer and Joule and later evolved into the energeticist and mechanist theories of the fin de siècle.

The context in which Black launched his investigations, which primarily concerned phase-change processes like condensation and vaporization, was not so much one of complete ignorance of these phenomena but of what Mach called "an incompatibility of the facts with the current opinion."[91] Black once observed that if the prevailing theory of vaporization of his time were correct, one should expect the simple act of bringing water to a boil in a kettle to always result in a violent explosion of steam. He also noted that many of the same absurdities were present in the

[91] Mach, *Principles of the Theory of Heat*, 164.

mid-eighteenth-century theory of melting, which implied that the normal springtime warming of ice and snow would inevitably unleash apocalyptic torrents of water upon mountain villages and river cities. The resolution of these conflicts thus required a new formal framework in addition to the production of further experimental data, and it was precisely in his pursuit of former aim, Mach argued, that Black was "powerfully struck" by the resemblance of thermal processes to "the motion of a substance" that he would later call caloric.[92]

To better understand the psychological mechanisms underlying Black's initial arrival at this particular substantial analogy, Mach asked his readers to recall that

> when we give an explanation of some extraordinary phenomenon or property…we always do it by showing that, in reality…a connection exists between it and other things with which under more familiar circumstances we are very well acquainted, either on account of the resemblance which it has with them in certain particular or on account of its origin from the same cause.[93]

In more developed research programs, the explanatory process was largely conscious and involved the methodical. But as he had previously shown in his "Carnot" paper, explanation at the initial stages of an investigation was primarily guided by inherited mental concepts that "push(ed) themselves forward unsought" as explananda.[94] He argued that it was therefore unsurprising that substance, as one of the species' most powerful and intuitive cognitive adaptations, would "completely dominate" Black's attempts to understand what was going on when a kettle boiled, or when a patch of snow melted. To drive his point home, he asked his readers to put themselves in Black's shoes and to try to imagine that they knew little of heat save what they could feel and see,

[92] Mach, "On the Principle of Comparison in Physics," 244.
[93] Mach, *Principles of the Theory of Heat*, 169.
[94] Mach, "On the Principle of Comparison in Physics, 244.

confident that they too "must observe, even without purposefully following the facts," that in many cases it behaved like the liquids they were familiar with from everyday life.[95]

Mach argued that many of the same psychological processes that were at work in Black's investigations also guided the research of French engineer Sadi Carnot (1796-1832). Like many other pioneering intellects in the history of physics, Carnot was not fully appreciated in his own time, but by the 1890's physicists universally acknowledged that his analysis of the motive power of heat through the so-called "Carnot cycle" was one of the cornerstones of thermodynamics. It provided the first attempt to demonstrably "exclude…from the province of a general physics the possibility of a perpetual motion," for example, thereby extending the principle beyond the domain of mechanics for the first time and laying the groundwork for the principle of the conservation of energy.[96] It also suggested that thermal processes were fundamentally irreversible, thereby setting the stage for Rudolf Clasius' and William Thomson's more formal articulations of the second law of thermodynamics some three decades later.

Mach's account of how Carnot arrived at these discoveries mirrored his analysis of Black's discovery of latent and specific heat in two key respects. First, he noted that the Frenchman also worked on a class of phenomena that was widely familiar but only poorly understood, and that his investigations thus aimed at the provision of a new formal framework in addition to empirical clarification. Put in Carnot's own words:

> notwithstanding the work of all kinds done by steam-engine, notwithstanding the satisfactory condition to which they have been brought to-day, their theory is very little understood, and the attempt to improve them are still directed almost by chance…The phenomenon of the production of motion by heat has not been considered from a sufficiently general point of view.[97]

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[95] Ibid.
[96] Mach, "On the Conservation of Energy," 162.
[97] Sadi Carnot, *Reflections on the Motive Power of Heat*, ed. R. H. Thurston (London: Chapman and Hall, 1897), 42-43.

And second, he claimed that Carnot's "first great step" in providing this "general point of view" lay in his recognition of "an analogy between water which, by falling, performed work; and heat which, by sinking in temperature, performed work."[98] Just like Black before him, in other words, Carnot's "need for scientific lucidity" about an obscure thermal process led him to instinctively picture that process in terms of substantial motion, which provided the formal clarity required to pursue his investigations in more depth.[99]

Mach's third case study in the history of thermodynamics concerned the "simultaneous discovery" of the mechanical equivalent of heat by the German physician J.R. Mayer and the English physicist J.P. Joule in the 1840's, and their subsequent formulation of the principle of the conservation of energy.[100] This discovery was among the most important and hotly contested events in all of nineteenth century physics not only because of its immense scientific and philosophical ramifications but because the physics community was deeply divided on how to assign priority, with the Germans generally backing Mayer and the English supporting Joule. Mach, for his part, was far less interested in adjudicating the latter issue, which he dismissed as senseless and suffused with "odious national and personal questions," than he was in exploring the energy principle's meaning. Unlike most of his colleagues, he denied that Joule and Mayer had "finally demolished Black's notion of heat as a substance" but rather "re-introduced the same notion of substance in a more abstract and modified form."[101]

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[98] Mach, *Principles of the Theory of Heat*, 306.
[99] Mach, "On the Principle of the Conservation of Energy," 160.
[100] Mach, *Principles of the Theory of Heat*, 227. The notion of the "simultaneous discovery" of energy conservation was made famous by Thomas Kuhn, but Mach also credited a number of different inquirers with articulating some version of the principle around the same time, noting that "all investigators share in the common convictions of their time and consequently are more or less accessible to the same ideas."
[101] Ibid, 300, 367.

As in his other examples, Mach's explanation for why Mayer and Joule had dispensed with the caloric theory only to end up reproducing one of its defining features centered on the fact that their research initially set out to resolve a "formal" problem stemming from the incompatibility of the prevailing theory with the evidence. In Joule's case, he found it difficult to square the caloric theory's stipulation that the quantity of heat in nature remained constant with the fact that electromagnetic and mechanical processes seemed capable of producing new quantities of heat, while Mayer felt that assumptions about the constancy of caloric did not align with his observations on the consumption and generation of heat in animal locomotion and respiration. And just like Black and Carnot before them, their first step towards resolving these discrepancies involved the instinctive construction of a new formal framework that better accounted for the facts in question. Specifically, Mach held that a "powerful need" for formal clarity led Mayer and Joule to reflexively picture the consumption of heat in the generation of work and vice versa in terms of the transfer of a third entity they subsequently called "force" or energy, which functioned as a universal substrate linking thermal and non-thermal processes together.[102] Having arrived at this "very convenient and lucid" formal conception, they were then able to undertake the experimental investigations that would eventually yield the precise conversion value known as the mechanical equivalent of heat as well as their respective versions of the principle of the conservation of energy.[103]

By the 1890's, the Mayer-Joule conception of heat as a form of energy, and of energy as a conserved quantity that was universally present in natural processes, was almost unanimously accepted in the scientific community, although as the Lübeck controversy demonstrated, physicists

[102] Mach, "On the Principle of Comparison in Physics, 246.
[103] Mach, "On the Principle of the Conservation of Energy," 183.

still disagreed on ontology. From the energeticists' perspective, it was clear that energy was a fundamental entity in its own right, while for the mechanists it was equally clear that energy was a function of the mass and velocity of atomic particles.[104] But for Mach both sides of this disagreement were wrong, insofar as they continued to cling an overarching "substance-like view" of thermal processes that had its immediate origins in the caloric theory of Black and Carnot and was ultimately rooted in cognitive instinct.[105] And while he acknowledged that this substantial view had been useful as a means of facilitating a "simple, clear, and living grasp" of the relationship between qualitatively different phenomena like heat, mechanical work, electromagnetism, etc., he also held that it was an active obstacle to continued scientific progress.[106]

One of the primary ways in which the new energy-based theories obstructed progress, according to Mach, was by making it difficult for physicists to grasp certain features of thermodynamic processes, much like the caloric theory had made it difficult for earlier generations of researchers to grasp the generation of heat by friction. He was particularly concerned that framing heat as merely one form of energy among others would lead his colleagues to ignore or explain away important differences between thermal, mechanical, and electrodynamic phenomena, like the fact that the first could "suffer a fall in potential without experiencing a loss of energy—at least according to the usual way of measuring it," but the latter two could not.[107] He also expressed concern that physicists' belief that energy was a universally conserved quantity very

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[104] As Robert Deltete has noted, Georg Helm was not as convinced of the substantial reality of energy as Wilhelm Ostwald, and "vacillated between ascetic phenomenalism and some form of energetic realism." See: Robert Deltete, introduction to *The Historical Development of Energetics*, by Georg Helm, trans. Robert Deltete (Dordrecht: Springer, 2000).
[105] Mach, *Principles of the Theory of Heat*, 205.
[106] Mach, "On the Principle of the Conservation of Energy," 138, 183.
[107] Mach, *Principles of the Theory of Heat*, 310.

quickly led to theoretical extravagance and metaphysics. There was very little scientific sense in "still considering as energy a quantity of heat that can no longer be transformed into mechanical work," he wrote in 1895, yet that was precisely what adherents of the principle of the conservation of energy demanded, mirroring "the liberty which Black took when he regarded the heat of liquefaction as still present but latent" and which Kant took when he postulated the existence of the thing-in-itself.[108]

Mach's solution to these problems was not to jettison the energy laws altogether, as he recognized that they were still valid "within very wide limits," but to dispense with the faulty "substance-like view" of them. In place of the latter, he suggested that physicists adopt a more economical and flexible view of energy and entropy as fundamentally metrical notions, which is to say as ways of describing and quantifying the interrelationship between various physical processes rather than real features of the natural world. Adopting a metrical conception of energy and entropy was only one step, however, in what he saw as the discipline's longer-term transformation into a "phenomenological" enterprise that dispensed with all "intuitive notions by means of which we obtain and facilitate our grasp of facts" in favor of "the accurate study of the facts themselves."[109] Or as he wrote near the end of *Principles of the Theory of Heat*, he felt that physics had matured to the point where it was necessary to "purify the exposition of the results of research from the unessential ingredients which have become mixed with them by working with hypotheses" and to focus exclusively on the direct, quantitative description of the perceptible relations between phenomena.[110]

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[108] Mach, "On the Principle of the Conservation of Energy," 177.
[109] Mach, *Principles of the Theory of Heat*, 200.
[110] Ibid, 390.

To clarify more precisely what a physics oriented around “facts themselves” entailed, Mach directed his readers to his prior popular works on mechanics, which had shown that one could adequately account for mass or the laws of motion without resorting to metaphysical notions like matter. He also held up the French physicist Joseph Fourier’s theory on conduction as exemplary in two respects. First, it was founded “not upon a hypothesis but upon an observable fact,” which meant that it was far less likely to produce the empirical inconsistencies and conceptual blind-spots that were characteristic of theories that began with the postulation of a substance. And second, it “really only consists in a consistent, quantitively exact, abstract conception of the facts of conduction of heat—in an easily surveyed and systematically arranged inventory of facts” that was highly economical and devoid of potentially misleading elements, and thus better able to orient its users with respect to the relevant phenomena.[111]

Although Mach was convinced that in the context of physical research “the more conditions of a phenomenon become known, the further the impression of materiality passes into the background,” he also recognized that the concept of substance would remain important in other areas of human activity. He made this point clear near the end of *Principles of the Theory of Heat*, remarking that

> the natural philosopher is not only a theorist, but also a practician. In the latter capacity he has operations to perform which must processed instinctively…In order to grasp a body, to lay it upon the scales, in short, for hand-use, the natural philosopher cannot dispense with the crudest substance-conceptions, such as are familiar to the naïve man and even to the animal. For the higher biological step, which represents the scientific intellect, rests upon the lower, which ought not to give away under the former.[112]

[111] Ibid, 113.
[112] Ibid, 390.

This was a curious assertion to make, given the extensive argument against substantialism that preceded it. It was also curious because it implied that the “the scientific intellect” should operate within a conceptual framework that was fundamentally different from that of the “naïve man,” which belied his repeated assertions that it was incumbent on physicists to understand the two cognitive domains as “substantially the same” and as part of an intellectual continuum. But his desire to cultivate a divide between expert and lay reasoning, at least with respect to the status of substance, ultimately made sense for two reasons.

First, Mach’s analyses were primarily aimed at the physics community, and therefore did not aim at “eliminating the vulgar concepts which are practical and worthwhile” outside the spaces in which physicists worked, or at least not in the near-term future.[113] And second, he maintained that scientific research always involved a delicate balance between adapting one’s concepts to new facts and vice versa.[114] At times, the latter course of action was not only desirable but a prerequisite of progress, as in the “discovery” that novel thermal phenomena could be subsumed under the familiar and intuitive concept of substance. At other times, however, he held that it was necessary for physicists to abandon “fixed habits of thought” and to embrace ideas that broke with the tried-and-true intellectual categories of everyday life. Abandoning the realm of commonsense of course carried the risk of spectacular failure, just like any enterprise which dispensed with conventional wisdom, but he was also adamant that it had the potential to yield notions which were better suited to describing the facts, a la Copernican astronomy, Galilean mechanics, and his own phenomenological thermodynamics.

Conclusion

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[113] Ernst Mach, draft foreword to the Russian translation of *The Analysis of Sensations*, in *Ernst Mach -- A Deeper Look: Documents and New Perspectives*, 116.
[114] Ernst Mach, “On Mental Adaptation,” *Popular Scientific Lectures*, 232.

Principles of the Theory of Heat did not achieve the same level of critical acclaim or commercial success as *Science of Mechanics* or even *Popular Scientific Lectures*, and in the years following the publication of the first English edition of the text in 1901, Mach would gradually shift his attention back to the physiology and psychology of cognition. Around 1907, he began to publicly distance himself from the physics community itself, in no small part because he had become even more disillusioned by his colleagues' continued "revulsion…towards considerations that go out of the closest disciplinary circles."[115] And in 1913, he symbolically left what he called the "church of physics" altogether by quitting the Austrian Academy of Science and taking the unusual step of preemptively refusing any honorary affiliation with the organization. In his letter of resignation, he explained that his departure had much to do with the fact that his "biological epistemological theory" had so alienated him from other physicists that he could "neither understand their speech nor they mine, which is why they have used means to proceed against me after the fashion of Pius X."[116]

Around the same time that Mach went into self-imposed exile, several prominent scientists began taking concrete steps to assure that his contributions to physics, including his ostensibly maligned "biological epistemological theory," would receive lasting professional recognition. In 1912, the physicist and recent Nobel laureate Ferdinand Braun wrote several letters to the Nobel Committee putting forth Mach as a candidate for the prize, claiming that *Science of Mechanics*, *Principles of the Theory of Heat*, and *Popular Scientific Lectures* were, "from a purely objective standpoint," fundamental to the intellectual development of scientific thought in its contemporary

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[115] Ernst Mach to Friedrich Adler, Vienna, March 1st, 1905, in *Ernst Mach: Werk und Wirkung*, 265.
[116] Ernst Mach to the General Secretary, Vienna, April 28th, 1913, in *Ernst Mach -- A Deeper Look: Documents and New Perspectives*, 147-149.

form.[117] H.A. Lorentz, another former laureate in physics, expressed similar sentiments in a letter nominating Mach for the Nobel that same year, claiming that the Committee could produce "a lively and general satisfaction throughout the scientific world" by honoring the "profound and salutary influence" of that Mach's work mechanics and thermodynamics had exerted on modern physics.[118] And in 1914, former chemistry laureate Wilhelm Ostwald submitted a nomination letter drawing attention to the "great service" that Mach's historical and epistemological critiques had rendered to science, graciously overlooking the fact that many of those critiques had been aimed at his own energeticist ideas.[119]

The Swedish Academy was unmoved by Braun, Lorentz, and Ostwald's entreaties, but their appearance in close succession spoke to Mach's continued influence on powerful corners of academic physics. They also spoke to the respect that his popular works still commanded as contributions both to public knowledge and to specialist discourse. Why, then, was he so convinced that his epistemological views had irreparably estranged him from his colleagues? One potential answer is that his contemporaries embraced certain aspects of his philosophy but nevertheless rejected many of the core lessons that he had hoped that *Popular Scientific Lectures*, *Principles of the Theory of Heat*, and other works in his popular corpus would impart. He repeatedly argued that one of the fundamental aims of these texts was to demonstrate the "substantial sameness of

[117] Ferdinand Braun to the Nobel Committee, November 24th, 1911, in *Ernst Mach als Aussenseiter* ed. John Blackmore and Klaus Hentschel (Vienna: Braumüller, 1986), 88-89; and Ferdinand Braun to the Nobel Committee, January 24th, 1912, in *Ernst Mach als Aussenseiter*, 93-94.
[118] Hendrick Lorentz and Wilhelm Julius to the Nobel Committee, January 29th, 1912, in *Ernst Mach als Aussenseiter*, 95-96.
[119] Wilhelm Ostwald to the Nobel Committee, January 27th, 1914, in *Ernst Mach als Aussenseiter*, 129-130.

scientific and every-day thought," for example, but even physicists who were generally sympathetic to his ideas tended to gloss over or ignore this aspect of his philosophy entirely.[120]

More importantly, few of Mach's contemporaries were willing to accept his contention that the concept of substance was an artificial "picture" which the mind instinctively imposed on novel phenomena in order to render them formally intelligible, and that researchers would do well to adopt a phenomenological approach that dispensed with the concept altogether. If anything, his failure to convince the physics community of this point became even more apparent in the decade after the publication of *Principles of the Theory of Heat* as more and more of his colleagues accepted the existence of atoms and subatomic particles like the electron. But he did not completely accept defeat until his polemical exchange with Max Planck in 1910, which solidified his belief that the discipline was on its way to becoming an atomistic religion, replete with its own unshakable dogmas and fanatics.[121]

As the next chapter of this dissertation will show, Mach's dispute with Planck not only brought an end to his quest to rid physics of substance but dealt a heavy blow to a different set of goals that he had been pursuing in and through his popular work, namely: to destroy the barriers that scientists had erected between their disciplines, and between academia and the lay public, and to set the stage for the unification of the scientific enterprise into a coherent whole.

[120] This absence is particularly noticeable in retrospective analyses of Mach's work by close friends and intellectual allies like Friedrich Adler. See, for example, Friedrich Adler, *Ernst Machs Überwindung des mechanischen Materialismus* (Vienna: Wiener Volksbuchhandlung, 1918).
[121] There is some evidence that Mach accepted the existence of atoms as early as 1903, but this evidence is contradicted by the fact that he continued assail the notion in both public and private until his death. See: Stefan Mayer, "Mach Looks through a Spinthariscope," in *Ernst Mach -- A Deeper Look: Documents and New Perspectives*, 151-152.

Chapter Two

Ernst Mach's Popular Science, Part Two

In the introduction to his 1895 *Popular Scientific Lectures*, Ernst Mach offered a rare precis of his approach to popular exposition; a mode of communication that he had used for almost thirty years but rarely took the trouble to explicitly theorize. Many aspects of his account were utterly conventional. He noted that his popular corpus was intended to familiarize laypersons with the "charm and poetry" of scientific inquiry, for example, and to inculcate public interest in contemporary research questions and programs.[1] But other aspects of his account were more unique. Most notably, he held that his popularizations were meant to "exercise a favorable influence" on the scientific community itself by demonstrating the "substantial sameness of scientific and every-day thought" as closely related manifestations of the same biological and evolutionary processes that governed all other aspects of organic life.[2] As the previous chapter argued, one of the central aims of these demonstrations was to purge thermodynamics, and physics more generally, of the commonsense notion of substance, which he characterized as a harmful intellectual vestige of the discipline's primitive past. But as the present chapter will argue, he also had more constructive goals in mind.

Specifically, Mach intended for his popular corpus to foster mutual understanding and cooperation among representatives of different forms and realms of expertise, ranging from university-affiliated professors who had spent their careers toiling away in obscure subfields to

[1] Ernst Mach, "Introduction," *Popular Scientific Lectures* 5th ed., trans. Thomas McCormack (Chicago: The Open Court Publishing Company, 1943), vi.
[2] Ibid.

industrial laborers who had little formal education but possessed broad bodies of practical experience. He was not the only scientist of the fin de siècle to suggest that exchange between intellectual milieus could be illuminating and productive. Indeed, Germanophone natural philosophers had already begun to harp on the importance of interdisciplinarity in the early nineteenth century, when it became glaringly apparent that the specialization of research would make it progressively more difficult for members of different academic communities to understand one another. But as the first part of this chapter will show, his understanding of precisely how communication across disciplinary borders contributed to specialist science was reflective of his peculiar philosophical worldview.

First, Mach maintained that ideas, like organisms, evolved over time, and that the primary catalyst of their evolutionary development was "the method variation," or the practice of exposing and adapting existing knowledge to new phenomena.[3] Interdisciplinarity was a powerful tool for producing variation and adaptation, he argued, because it subjected context- and field-specific knowledge to novel conceptual, empirical, and linguistic demands, akin to the way that transplanting a species into an alien ecosystem exposed it to novel environmental pressures. Second, he held that intellectual exchange between specialisms counteracted the baleful effects of disciplinary myopia, including ignorance of potentially useful ideas that had been formulated in other fields and the epistemic scourge of metaphysics, which tended to fester in research communities that were estranged from other branches of inquiry. And finally, he believed that interdisciplinary collaboration was scientifically essential because it enabled the construction of

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[3] Ernst Mach, "On Mental Adaptation," in *Popular Scientific Lectures*, 230.

theories that captured the "many-sidedness" of natural phenomena in a way that explanations based on a singular point of view could not.[4]

The second part of this chapter argues that Mach's primary strategy for facilitating the interdisciplinary collaborations and exchanges that he desired was to use his popular work to draw out the methodological, empirical, and ontological connections obtaining between distinct forms of intellectual activity. In his texts and lectures on the "psychology of enquiry," for example, he sought to show that the research methods adopted by different academic fields not only bore a family resemblance to one another but were part of a broader spectrum of reasoning practices that encompassed "vulgar" and even animal cognition. Similarly, in his work on music theory he attempted to bridge scientific and artistic perspectives on musical phenomena by drawing attention to the basic sensations and intuitions underlying all human reasoning about acoustics. And in many of his popular works on sensation and perception, he attempted to provide a link between the physical and mental sciences by showing that the working objects of the two domains were not fundamentally different, as many of his contemporaries assumed, but built up out of the same ontologically neutral "elements." Above all, his concern in the latter texts was to show the contingency and intellectual bankruptcy of the mind-matter distinction, which he blamed for artificially dividing the *Natur-* and *Geisteswissenschaften* into alien kingdoms and propagating "obnoxious pseudo-problems" that distracted researchers from more worthwhile pursuits.

Although the immediate goal of Mach's interdisciplinary endeavors was to reorient the intellectual landscape in more productive ways, the final part of this chapter will show that he also used them to pursue two longer-term projects. First, he hoped to lay the groundwork for the

[4] Ernst Mach, *History and Root of the Principle of the Conservation of Energy*, trans. Philip Jourdain (Chicago: The Open Court Publishing Company, 1911), 49.

unification of the different branches of knowledge into a single whole that was tied together by a common conceptual and methodological framework. Or as he remarked in *Analysis of Sensations*, he intended for his popular analyses to serve as stepping-stones towards the construction of a "complete science" that reflected the monistic unity of nature rather than the arbitrary distinctions imposed by disciplinary inquiry.[5] And second, he sought to advance the cause of social democracy by establishing a more dynamic and equitable relationship between scientific and working-class communities. Like his Austromarxist friends, he felt that granting the proletariat greater access to the scientific enterprise could thwart the formation of a predatory class of "intellectual capitalists" and assure that one of the fin de siècle's most important forms of capital—scientific knowledge—would not remain the exclusive property of bourgeois academics but "redound to the benefit…of the collective whole."[6]

In drawing attention to Mach's use of popularization as a tool for articulating the connections and similarities between different academic, artistic, and industrial fields, this chapter both supports and expands on several recent historiographical interpretations of the genre's role in nineteenth century science. In terms of support, it lends credence to a growing body of scholarship which suggests that popular media contributed to scientific discourse by providing synthetic pictures of the research landscape at a time when specialization was making it difficult for scientists to understand what was going on in neighboring disciplines, and in some cases their own fields of expertise.[7] Historians Jonathan Topham and Nicholas Rupke have pointed to geologist

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[5] Ernst Mach, *Analysis of Sensations and the Relation of the Physical to the Psychical,* trans. C.M. Williams (Chicago: The Open Court Publishing Company, 1914), 312, 341.
[6] Mach, *Popular Scientific Lectures*, vi.
[7] Jonathan Topham, "Rethinking the History of Science Popularization/Popular Science," in *Popularizing Science and Technology in the European Periphery, 1800-2000*, ed. Faidra Papanelopoulou, Agusti Nieto-Galan, and Enrique Perdiguero (Vermont: Ashgate, 2009), 1-20.

William Buckland's 1836 "Bridgewater Treatise" as a prominent example of the popular genre's growing importance as a way for scientists of the period to get a handle on general trends in their own and other fields of inquiry. In Topham's words, Buckland's text not only aimed at educating his lay readers but filling a "great blank" in the scientific literature by providing his colleagues with a general survey which gave "form and definition" to recent developments in geological research.[8]

Bernard Lightman recently made a similar point about popularization in his analysis of the Victorian-era texts of Mary Sommerville, Alice Bodington, and Agnes Clerke. Otherwise shut out of the male-dominated domain of specialist research, these women were able to carve out a special niche in the scientific community by producing popular work that supplied what Lightman describes as "a synoptic overview of knowledge to practitioners who could no longer keep up with the research outside their narrow areas of expertise."[9] One could easily apply Lightman's description to Germanophone writers like Wilhelm Bölsche, who was not a scientist himself but was able to exert a significant influence on fin de siècle scientific discourse by writing generalized accounts of recent findings and discoveries, and even to prominent researchers like Hermann von Helmholtz and Ernst Haeckel. As historians have frequently pointed out over the last several decades, it was not uncommon for scientists of Haeckel and Helmholtz's stature to use their popular work to generalize their work and to attempt to situate it within a broader philosophical framework or worldview.[10]

[8] Ibid, 18.
[9] Bernard Lightman, *Victorian Popularizers of Nature: Designing Nature for New Audiences* (Chicago: The University of Chicago Press, 2007), 487.
[10] Kurt Bayertz, "Spreading the Spirit of Science: Social Determinants of the Popularization of Science in Nineteenth-Century Germany," *Expository Science: Forms and Functions of Popularisation* ed. Terry Shinn and Richard Whitley (Dordrecht: Reidel, 1985), 209-227; Andreas Daum, *Wissenschaftspopularisierung im 19.*

This chapter also offers further evidence for historian Deborah Coen's claim that fin de siècle and interwar Austrian intellectuals valued the popular genre because they thought it exposed specialist reasoning to productive forms of intellectual and linguistic "resistance."[11] Although Coen finds traces of this idea in thinkers as diverse as the physicist Erwin Schrödinger and the journalist Karl Kraus, she locates its clearest formulation in Ludwik Fleck's *Genesis and Development of a Scientific Fact*, which cast popular exposition as an important corrective to specialist discourse because it subjected technical knowledge to creative reinterpretation and transformation.[12] Specifically: his argument in *Genesis* suggested that the process of preparing esoteric facts and theories for public consumption involved a series of communicative translations that imbued them with qualities they were previously lacking, including systematicity, simplicity, and "vividness."[13] As an explanation for why Fleck and his contemporaries were uniquely situated to grasp the transformative properties of popularization, Coen has indicated that the "everyday reality" of living and working in a culturally and linguistically heterogenous state; the Habsburg scientific community's peculiar emphasis on public engagement; and the unique culture of the Viennese press all played a role.[14]

This chapter expands on these historiographical assessments by arguing that Mach's belief in the scientific value of popularization's capacity generalize and translate was rooted in a factor that has often escaped notice in scholarly discussions of the genre: the rapid acceptance and spread

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Jahrhundert: Bürgerliche Kultur, naturwissenschaftliche Bildung, und die deutsche Öffentlichkeit, 1848-1914 (Munich: R. Oldenbourg Verlag, 1998).

[11] Deborah Coen, "Rise, Grubenhund: On Provincializing Kuhn," *Modern Intellectual History* Volume 9, Issue 01 (April 2012), 109-126.

[12] Ludwick Fleck, *Genesis and Development of a Scientific Fact* ed. Thaddeus J. Trenn and Robert K. Merton, trans. Fred Bradley and Thaddeus J. Trenn (Chicago: The University of Chicago Press, 1979).

[13] Ibid, 113.

[14] Coen, "Rise, Grubenhund," 122.

of evolutionary biology after 1860. For example: while Mach shared Clerke's and Bölsche's conviction that popularization was useful because it provided a synoptic perspective on contemporary knowledge at a time of aggressive specialization, he departed from them by framing the issue in terms of island species, natural selection, variation, and other evolutionary concepts. Similarly, his conception of popular media as tools for subjecting technical knowledge to productive forms of intellectual resistance and linguistic translation was less a reflection of his upbringing in the multilingual and multiethnic Habsburg state than of his youthful encounters with Lamarck and Darwin. This is not to say that his views were untouched by the peculiar socio-political and cultural contexts of the late-Imperial period, but that his career-long interest in evolutionary theory and commitment to naturalism were arguably more relevant to his understanding of how the popular genre helped produce and refine knowledge than anything else.[15]

Interdisciplinarity as Scientific Practice

Calls for interdisciplinary cooperation and exchange, particularly as foils to the epistemic problems raised by the professionalization and specialization of scientific inquiry, were already common in early nineteenth-century Germanophone intellectual circles. The botanist and *Naturphilosoph* Lorenz Oken was an especially vocal critic of his colleagues' growing tendency to cloister themselves in small disciplinary circles, and in 1821 he put forth plans for a yearly conference, the *Versammlung Deutscher Naturforscher und Ärtze* (VDNA), that would bring together representatives of the different branches of knowledge to meet, share information, and

[15] For some of Mach's reflections on the nature of language, see: Ernst Mach, *Principles of the Theory of Heat: Historically and Critically Elucidated*, ed. Brian McGuinness, trans. P.E.B. Jourdain and A.E. Heath, (Dordrecht: D. Reidel, 1986), 371-378; and Ernst Mach, *Knowledge and Error: Sketches on the Psychology of Enquiry* trans. Thomas J. McCormack (Dordrecht: D. Reidel, 1976). Mach was also an admirer of the Austrian philosopher of language Fritz Mauthner and corresponded with him periodically between 1889 and 1912. See: *Ernst Mach: Werk und Wirkung* ed. Rudolf Haller and Friedrich Stadler (Vienna: Hölder-Pichler-Tempsky, 1988), 231-242.

discuss issues of common interest. He thought that the advantages of this kind of interdisciplinary exchange were so clear to "every learned person" that they "required no discussion" or explanation.[16] Seven years later, famed naturalist and scientific statesman Alexander von Humboldt used his plenary address at Oken's *Versammlung* to make a similar point. Although he was fully convinced of the value and necessity of the new, specialized research communities, his speech urged German academics to combat the intellectual atomism those communities created by fostering contact and exchange between the domains of "organic and inorganic research" and organizing scientific life in a way that reflected the unity of nature.[17] He attempted to bridge and unify the various branches of science himself in his 1845 *Kosmos*; a semi-popular text which sought to live up to its lofty title by adopting a heroic number of analytic perspectives, ranging from anthropology and biogeography to cosmology and physics.

By the 1860's, Oken's philosophical views had long since fallen out of favor, but his *Versammlung* lived on as one of the important and anticipated events of the academic year, drawing scientists, physicians, engineers, and interested laypersons from across Central Europe to a rotating cast of "German" cities, including Vienna. Humboldt's *Kosmos* proved no less enduring or successful, having been adopted as a model by an entire generation of authors who were dedicated to furthering the original's synthesizing and unifying mission.[18] The tremendous influence of these pre-1848 creations among scientists of the post-1848 period was closely tied to the fact that interdisciplinary exchange, or at least the ideal of it, continued to be a pressing topic of scientific discussion and interest. Hermann von Helmholtz, who was not only one of the most

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[16] Lorenz Oken, "Versammlung der Deutschen Naturforscher," *Isis: Literarische Anzeiger* (1821), 196-198.

[17] Alexander von Humboldt, *Rede, gehalten bei der Eröffnung der Versammlung deutscher Naturforscher und Ärzte in Berlin, am 18. September 1828* (Berlin: Königl. Akad. d. Wissenschaften, 1828), 7-8.

[18] Daum, *Wissenschaftspopularisierung*, 280-286.

prominent physicists but public intellectuals of the late nineteenth century, echoed a common sentiment among his colleagues when he explained to a group of students that the intellectual health of science was tied to scientists' recognition that

> Each student works in his own department; he chooses for himself those tasks for which he is best fitted by abilities and his training. But each one must be convinced that it is only in connection with others that he can further the great work, and that therefore he is bound, not only to investigate, but to do his utmost to make the results of his investigation completely accessible…The annals of science abound in evidence (of) how such mutual services have been exchanged, even between departments of science apparently most remote.[19]

When he delivered these lines in 1862, he was experiencing the benefit of these "mutual services" firsthand in his pathbreaking physiological research, which applied insights and methods drawn from several different fields to the problem of human perception and cognition.

The importance of interdisciplinarity and dangers of scientific insularity were no less apparent to Central European biologists in the 1860's. The eminent pathologist Rudolf Virchow shared Helmholtz's conviction that the fate of German research depended on the cultivation of connections and mutual understanding between distant scientific disciplines and fields, and echoed Oken's fears that the centrifugal force of specialization was making those connections impossible. He outlined these concerns in detail at the *VDNA* in 1865, where he delivered an address that excoriated the growth of "small-state-ery" (*Kleinstaaterei*) and "intellectual feudalism" in the scientific community and condemned the *Versammlung* itself for focusing more on technical panels than plenary sessions, which ran contra to its original purpose.[20] He then suggested that future editions of the meeting emulate the British Association for the Advancement of Science by

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[19] Hermann von Helmholtz, "On the Relation of Natural Science to Science in General," in *Science and Culture: Popular and Philosophical Essays* ed. David Cahan (Chicago, The University of Chicago Press, 1995), 94.
[20] Rudolf Virchow, *Über die nationale Entwicklung und Bedeutung der Naturwissenschaften* (Berlin: August Hirschwald, 1865), 6.

enlisting prominent representatives of each discipline to give lectures which summarized the current state of research in their field. He concluded his address by arguing that the benefits of this kind of synthetic exposition would be twofold. First, the scientific community could discharge its political duty by guiding the general public towards "common views and ways of thinking," which were critical to the success of the nation-building project that was then underway under the watchful eye of Bismarck. And second, these synthetic expositions would enable scientists to look beyond "the small circle of their discipline" and engage with other scientific "goals and points of view," thereby assuring that specialists did not lose sight of the common ground underlying all knowledge and inquiry.[21]

One of Virchow's students, the zoologist Ernst Haeckel, took his message about the importance of interdisciplinary communication to heart, although from Haeckel's perspective it was far more critical to convince non-biologists of the truth of Darwinism than it was for biologists to engage with extramural ideas. He purposefully crafted his *Natural History of Creation* (1867) to be accessible to "laypersons and students of all faculties" for precisely this reason, writing that the text's lack of technical detail, factual omissions, and other faults were a small price to pay to "spread the beneficial light of evolutionary theory to wider circles."[22] Although Haeckel's aggressive propagandizing successfully won converts to the Darwinian cause, it also alienated him from his mentor, and the two became embroiled in a bitter controversy at the 1877 *VDNA* in Munich. Ironically, it was now Virchow who found himself seeking to limit the circulation of

[21] Ibid.
[22] Ernst Haeckel, *Natürliche Schöpfungs-Geschichte, Vol.1*, 8th ed. (Berlin: George Reimer, 1889), vii.

biological ideas beyond disciplinary circles based on fears that the uncritical adoption of evolutionary theory by non-specialists would have disastrous political effects.[23]

Ernst Mach shared Haeckel, Helmholtz, and Virchow's belief in the importance of interdisciplinarity but departed from them in his understanding of precisely why it was scientifically valuable. His deviation from the German perspective was in large part a reflection of the unique suite of philosophical beliefs and commitments that composed his philosophical worldview, the most important being his "biological and economic" epistemology, which held that ideas were heritable cognitive adaptations that the human organism used to organize and simplify the chaotic tapestry of phenomena that comprised the natural world.[24] He argued that the concept of cause did not pick out a feature of natural processes *an sich*, for example, but was a useful mental "notation" which the species acquired at some distant point in its evolutionary history and retained for its utility in rendering phenomenal relations intellectually tractable and predictable.[25] Similarly, he maintained that complicated intellectual constructs like the laws of optics were little more than "comprehensive and condensed report(s)" about the world that enabled the mind to understand and predict the behaviors of a class of phenomena with "the least expenditure of thought," similar to the way that a label enabled a merchant to quickly survey the contents of a shipping crate.[26]

Mach's naturalistic theory of knowledge also informed his belief that ideas were akin to complex organisms in their own right, insofar as "the trait of transformation and development"

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[23] For more on the Haeckel-Virchow controversy, see: Robert Richards, *The Tragic Sense of Life: Ernst Haeckel and the Struggle over Evolutionary Thought* (Chicago: The University of Chicago Press, 2009) 277-329.
[24] Ernst Mach, "Die Leitgedanken meiner Naturwissenschaftlichen Erkenntnislehre und ihre Aufnahme durch die Zeitgenosse," *Scientia* (Vol. 8, 1910), 226.
[25] Mach, *Analysis of Sensations*, 89.
[26] Ernst Mach, "The Economical Nature of Physical Enquiry," *Popular Scientific Lectures*, 194-201.

was "perceptible in them."[27] He first drew attention to the evolutionary properties of cognition and its products in an 1865 popular lecture, declaring that:

> thoughts do not spring up suddenly. Thoughts need their time to ripen, grow, and develop in, like every natural product…slowly, gradually, and laboriously one thought is transformed into a different thought, as in all likelihood one animal species in gradually transformed into new species. Many ideas fight the battle for existence not otherwise than the Ichthyosaurus, the Brahman, and the horse. A few remain to spread rapidly over all fields of knowledge, to be redeveloped, to be again split up, to begin again the struggle from the start…Whomever will look carefully into his own soul will acknowledge that thoughts battle as obstinately for existence as animals.[28]

At the time, he suggested that one of the most obvious places to find evidence of these evolutionary processes in action was the history of scientific instruments, which offered a palpable physical record of the ways in which contemporary technologies developed over time from more primitive ideas and tools. But in other lectures and texts he sought to show that the effects of descent with modification were at work in the more incorporeal realm of concepts and theories, including in sacrosanct domains of scientific reasoning like mechanics. In a polemical 1883 *Rektoratsrede*, he argued that Newton's laws of motion and gravitation were not only part of the same phylogenetic tree as the long-extinct theories of Aristotelian *physis* but that they continued to exhibit traits that they had inherited from their Greek ancestors. "After all," he asked his audience, was there not a trace of Aristotle's understanding of motion as the "search for place" in Newton's nebulous expressions about the motive power of attraction?[29]

Like any other good evolutionary theory, Mach's epistemology was not only descriptive but explanatory. In the same *Rektoratsrede* where he connected to Newton to Aristotle, he argued that the primary engine of intellectual progress was "the method of variation," or the practice of

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[27] Mach, *Principles of the Theory of Heat*, 351-352
[28] Ernst Mach, The Velocity of Light," *Popular Scientific Lectures*, 63.
[29] Mach, "On Mental Adaptation," 227.

exposing familiar ideas to new or unfamiliar states of affairs. He claimed that if one looked closely at the historical trajectory of optics, for example, they would find that the precipitating incident behind each advance in the theory of light was a researcher's encounter with some phenomenon that their "habitual ideas" could not explain, followed by a process of intellectual adaptation and transformation that yielded more appropriate ideas. Natural philosophers first adhered to the simple theory that light was a contiguous, homogenous ray. But as further investigation revealed new kinds of optical phenomena, they gradually transformed the "homogenous ray" conception into the explanatorily more powerful corpuscular theory, which maintained some structural aspects of its predecessor but also added new conceptual features derived from Newtonian mechanics. And when the corpuscular vision of light was unable to provide a satisfactory account for phenomena like periodicity, Fresnel and others crafted new wave-based theories which retained the useful elements of their precursors; jettisoned unhelpful notions like the rectilinear propagation of particles; and added new theoretical features that were able to adequately explain the recalcitrant facts.[30]

Mach maintained that the method of variation was no less critical in the historical development of mechanics and thermodynamics. With respect to the former, he argued that the field's tremendous progress between the sixteenth- and eighteenth centuries was not rooted in any one scientific feat or event but in physicists' efforts to gradually broaden their "field of experience" and to subsequently adapt their ideas to what was "new, uncommon, and not understood."[31] Newton did not grasp the laws articulated in the *Principia Mathematica* all at once, in other words, but by slightly modifying his "customary mode of thought" to accommodate the innumerable

[30] Ibid, 226.
[31] Ibid, 224.

empirical and conceptual disturbances that he came across in the course of his research.[32] With respect to thermodynamics, Mach claimed that J.R. Mayer's path to the principle of the conservation of energy began with a chance observation of the redness of venous blood in the tropics, which piqued his interest in the relationship between thermal and mechanical phenomena. Unable to reconcile what appeared to be the interconvertibility of heat and work with the prevailing ideas of the time, he argued that Mayer then set about transforming his theoretical framework to better accommodate his observations.[33] The end result of these adaptive efforts was the energy principle, which was not only better able to explain a particularly obscure set of facts and relations, including why blood would be redder in hotter environments and how the body was seemingly able to generate heat *ex nihilo*, but offered an immensely useful perspective on the interconnection of nature as whole.

Mach's theory that variation was a critical reagent in the evolution of scientific thought bore directly on how he understood the form and function of scientific practice in his own time in at least two critical ways. First, it informed his view that "all the varied methods of scientific inquiry and of purposive mental adaptation enumerated by John Stuart Mill, those of observation as well as those of experiment," were "ultimately recognizable as forms of one fundamental method, the method of change, or variation."[34] And second, it framed his belief that any activity or technique which facilitated the adaptation of extant knowledge, ranging from historical analysis and philological comparison to artisanal tinkering, was in some way scientific. This capacious conception of scientificity naturally included practices of interdisciplinary circulation and

[32] Ibid, 226.
[33] Mach, *Principles of the Theory of Heat*, 229-232.
[34] Mach, "On Mental Adaptation," 230.

exchange, which he praised for producing a potent form of variation by subjecting context-specific ideas to novel conceptual, empirical, and linguistic pressures.

The relationship between interdisciplinarity and intellectual evolution was apparent to Mach from the beginning of his career, although he did not yet use biological terms to describe their connection. One of his first monographs, *Introduction to Helmholtzian Music Theory: Represented Popularly for Musicians* (1866), began with an extended analysis of how “material and intellectual exchange” between scientific fields and the lay public contributed to the growth of knowledge by clarifying and refining the content of whatever was being communicated between the two domains.[35] To support this claim, he first drew attention to an experience that would have been familiar to any scientist of the period: that the process of translating one’s private thoughts into language accessible to a scientific public required the adaptation of one’s internal dialogue to external linguistic conventions, which served to transform fuzzy assumptions into intelligible propositions. He then suggested that a similarly useful transformation occurred when scientists decided to take the additional step of communicating their research to laypersons, because the process of couching technical ideas in everyday terms helped clarify the primitive experiences and ideas that were at their core.[36]

Mach would continue to expand on these initial thoughts about the relationship between interdisciplinary communication, translation, and knowledge production throughout the 1870’s and 1880’s. In *Science of Mechanic,* he claimed that the origin of science itself lay in some ancient individual’s attempts to render their experiences “into communicable form” and disseminate them “beyond the confines of class and craft.” As in his earlier analysis, he emphasized that this form

[35] Ernst Mach, *Einleitung in die Helmholtzsche Musiktheorie: Populär für Musiker dargestellt* (Graz: Leuschner and Lubensky, 1866), 1.
[36] Ibid, 2.

of translation was epistemically productive because it compelled the translator to frame the information they wanted to communicate in novel ways. In the case of the ancient proto-scientist, he hypothesized that their efforts to preserve and transmit their experiences to others forced them to bring a variety of "facts and their dependent rules" into "closer temporal and spatial proximity" in their mind, which revealed basic information about how those facts were connected with one another.[37] From this cognizance of the basic connection of facts, it was only a brief step to the formulation of the first natural laws.

Mach provided the clearest articulation of his theory that translating one's thoughts into other forms and idioms provided a valuable new perspective on them in *Principles of the Theory of Heat* (1896), where he argued that

> the very circumstance that language compels us to describe the new in terms of the known, and therefore to analyze the new by comparison with the old, is a gain, not only for the person addressed, but also for the speaker. A thought is frequently rendered much clearer by our imagining ourselves called upon to communicate it to others.[38]

The clarificatory potential of interpersonal communication was particularly evident, he continued, in situations where the interlocutors spoke foreign languages, insofar as the Sisyphean task of trying to establish exact correspondences between linguistic systems helped "place in relief slightly different aspects of the same thing."[39] But he also argued that it was manifest in situations where scientists tried to communicate technical knowledge to people who only spoke in the vernacular. To demonstrate this point, he once again returned to the work of Joseph Black, arguing that the English chemist's attempts to frame his "weighty ideas" in "plain, straightforward" language were part of what enabled him to grasp facets of latent and specific heat that colleagues

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[37] Mach, *The Science of Mechanics: A Critical and Historical Account of Its Development*, trans. Thomas McCormack (Chicago: The Open Court, 1942), 89.

[38] Mach, *Principles of the Theory of Heat*,

[39] Ernst Mach, "On Instructions in the Classics and the Sciences," *Popular Scientific Lectures*, 354.

who were “hidebound” in the linguistic conventions of “one narrow professional sphere” could not.[40]

In addition to arguing that interdisciplinary exchange facilitated a scientifically valuable form of linguistic variation, Mach held that it was a catalyst for empirical and conceptual variation because it circulated ideas “over a wider field than that for which they were originally formed.”[41] That is, he maintained that “complicated cross-relations” and exchanges between academic fields were vital to scientific progress because they uprooted discipline-specific concepts and theories from their point of origin and enabled their application in new settings and to novel problems. In *Analysis of Sensations*, he claimed that the one of most promising developments in contemporary thermodynamics was its adoption by non-physicists, because the latter group was able to make

> the most widely extended applications of the principle of energy and of other physical conceptions, with a freedom which the physicist would hardly venture to use in his own field…The success of this movement may be partly positive and party negative, but in any case the result of it will be a more precise determination of our conceptions, a more accurate delimitation of the sphere to which they apply, and a clearer idea of the different and the affinity between the methods of the departments in question.[42]

In the years after he wrote these lines, he would quickly discover that some applications of the energy principle were more promising than others. He admired efforts by biologists and social theorists to interpret complex ecological and social systems in terms of energy distribution, for example, and suggested that economists might arrive at a more just system of wages if they considered the issue through the lens of energy expenditure.[43] But he was far less sanguine about

[40] Mach, *Principles of the Theory of Heat*, 169-170.
[41] Mach, *Analysis of Sensations*, 83.
[42] Ibid, 84.
[43] Ernst Mach, “Der physische und der psychische Anblick des Lebens,” *Populär-Wissenschaftliche Vorlesungen*, 5th ed., ed. Elisabeth Nemeth and Friedrich Stadler (Berlin: Xenomoi, 2014), 359-369.

concepts like “psychic energy” and “mental work” because he felt that they were based on a misleading analogy between cognitive and mechanical processes.[44]

Mach often supplemented his analyses of the scientific value inherent in transplanting discipline-specific ideas into new contexts by emphasizing the problems that arose from intellectual insularity, which is to say from a disinterest in engaging with ideas that originated outside of one’s immediate intellectual circle. On the one hand, he recognized that insularity was a natural outcome of specialization, and that specialization was a “fundamental condition of a fruitful development of science” because “only by such…restriction of work can the economical instruments of thought requisite for the mastery of a special field be perfected.”[45] Indeed, in *Science of Mechanics* he remarked that the necessity of restricting inquirers to certain activities and domains was so self-evident that one could find primitive divisions of intellectual labor in the earliest societies, which did not create scientific castes *per se* but supported the creation of “special classes and professions” dedicated to the investigation and manipulation of certain classes of phenomena as a lifelong vocation.[46]

On the other hand, Mach held that the advantages of contemporary science’s immensely complex division of labor were often overshadowed by its downsides, including its propensity to exacerbate an already strong human predisposition to focus only on phenomena that were of immediate practical or social interest. He had experienced the destructive effects of this predisposition first-hand in the 1860’s and 1870’s, when his critiques of mechanics and

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[44] Mach’s suspicion of these concepts once again pitted him against the energeticist Wilhelm Ostwald and aligned him with Ludwig Boltzmann, who agreed that much of the current discourse concerning the possibility of “assign(ing) to mental phenomena a new form of physical energy” was fundamentally misguided. See: Ludwig Boltzmann, “Reply to a Lecture on Happiness Given by Professor Ostwald,” *Theoretical Physics and Philosophical Problems: Selected Writings* ed. Brian McGuiness (Dordrecht: Reidel, 1974), 174.
[45] Mach, *Science of Mechanics*, 610-611.
[46] Mach, *Science of Mechanics*, 4.

thermodynamics frequently met with indifference, if not outright hostility from physicists who could neither understand nor countenance his interest in epistemological questions. Exasperated after the rejection of one of his articles from the flagship physics journal of the period, he remarked that the discipline's gatekeepers would sooner publish pages of technical nonsense than countenance ideas that were not expressed in "physical language."[47] He would continue to maintain this view of his colleagues even after the tremendous success of his decidedly heterodox *Science of Mechanics* and *Analysis of Sensations* in the 1880's, complaining to Friedrich Adler in 1905 that the physics community would still rather concern itself with abstruse technical questions than engage in any "considerations that go out of the closest disciplinary circles."[48]

Aside from making his professional life more difficult, Mach argued that physicists' disinterest in extramural ideas blinded them to potentially useful ideas and findings originating in other fields, thereby closing off what had historically been one of the discipline's most important sources conceptual clarification and innovation. The examples he offered in support of this contention were legion. In *Knowledge and Error*, he noted that two of the great heroes of the scientific revolution, Copernicus and Kepler, had not made their paradigm-changing contributions to astronomy using natural-philosophical or physical notions alone but by utilizing "clarifying and simplifying" ideas drawn from commonsense, animist, and mystic strains of thinking.[49] And in *Principles of the Theory of Heat*, he explained that the progenitors of a different, but no less consequential revolution in scientific thought—J.P. Joule and J.R. Mayer—had profitably used

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[47] Mach, *History and Root*, 80.
[48] Ernst Mach to Friedrich Adler, Vienna, March 1st, 1905, in *Ernst Mach: Werk und Wirkung* ed. Rudolf Haller and Friedrich Stadler (Vienna: Hölder-Pichler-Tempsky, 1988), 265.
[49] Mach, *Knowledge and Error*, 214.

concepts and techniques that originated in chemistry, engineering, medicine, and even brewing to formulate the principle of the conservation of energy.[50]

Mach held that an even more damaging effect of physicists' myopia was that it allowed metaphysical ideas and speculations to fester in the disciplinary community. Put in a more biological idiom, he viewed intellectual isolation as a pathology which allowed unfit ideas to survive in much the same way that "a defenseless species might be spared on a remote island free from predators."[51] As a social democrat with a strong anti-clerical bent, he held up medieval scholasticism as a classic example of the constitutive relationship between epistemological insularity and the propagation of intellectual monstrosities, but he also pointed to contemporary physics as a case in point. Specifically, he argued that the discipline's lack of consistent interaction with other fields had led many of its practitioners to ascribe "a reality beyond and independent of thought" to its favored "intellectual implements," including the atom and space.[52] As a result of these illicit reifications, they tended to waste time pursuing "obnoxious and idle pseudoproblems" and to support the misguided reductionism of the mechanical worldview, which hindered their ability to grasp critical aspects of nature by directing their focus exclusively to the mechanical properties of phenomena.[53]

Mach's belief that physicists were uniquely predisposed to disciplinary myopia and disproportionately wracked by its effects led him to dedicate many of his popular texts to drawing their attention to ideas from "adjacent fields which they tend to neglect, but which can afford much clarification as to (their) own thinking."[54] He felt that the two most important of these neglected

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[50] Mach, *Principles of the Theory of Heat*, 120-125.
[51] Mach, *Knowledge and Error*, xxxiii.
[52] Mach, *Science of Mechanics*, 611.
[53] Mach, *Knowledge and Error*, xxxii.
[54] Mach, *Knowledge and Error*, xxxii.

fields were biology and physiology. Although he had more direct research experience in the latter than the former, he was also a voracious reader of biological literature and a great admirer of Lamarck and Darwin, proclaiming that the English naturalist was the Galileo of the nineteenth century.[55] He began to recognize that the findings emanating from both fields would have significant ramifications for physics during his years as a *Privatdozent* at the University of Vienna in the early 1860's, and by the end of the decade he had developed a "profound conviction that the foundations of science as a whole, and of physics in particular, await their next greatest elucidations from the side of biology, and especially from the analysis of sensations."[56] Many of his initial attempts to provide these biological and physiological elucidations went unheeded, but his persistence in using his popularizations to harp on the organic and evolutionary underpinnings of physical knowledge eventually succeeded in exposing many of his colleagues to ideas and perspectives they would have otherwise ignored. Perhaps the greatest indicator of his success in this regard was the aggressive counterreaction that his work provoked among more traditional physicists and philosophers like Max Planck, Edmund Husserl, and Friedrich Jodl, all of whom accused him of degrading the status of physical knowledge and thwarting scientific progress, among other high epistemic crimes.[57]

Although Mach felt that biology and physiology had the most to offer contemporary physics, he was also convinced that historical analysis offered a valuable perspective on the discipline. As he noted at the beginning of his *Popular Scientific Lectures*, many of the popular

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[55] Mach, "On Mental Adaptation," 215.
[56] Mach, *Analysis of Sensations*, vii-viii.
[57] Mach's most strident critic was arguably the physicist Max Planck, but he often provoked similar fury among philosophers, including Edmund Husserl and Friedrich Jodl. The latter angrily opposed his appointment to the philosophical faculty at the University of Vienna. See: Joseph Mayerhofer, "Ernst Machs Berufung an die Wiener Universität, 1895" *Clio Medica* 2 (1967), 47-55.

addresses and texts that he had produced over his career had been specifically geared towards articulating the historical connection between scientific, "every-day," and primitive thought as manifestations of the "developmental processes of life."[58] In this respect, he saw history as yet another tool for demonstrating the biological nature of knowledge, but he also assigned it a special role in tackling physicists' metaphysical illusions. He first explicitly suggested that historical analysis could serve as an antidote to metaphysical reasoning in his 1871 *History and Root of the Principles of the Conservation of Energy*, which began by claiming that thinkers who followed the "guiding hand of history" would not fall prey to illusions about their intellectual implements.[59] He expressed a similar view in *Science of Mechanics* twelve years later, claiming that the only way to "clear up ideas, expose the real significance of the matter, and get rid of metaphysical obscurities" in physics was to follow its historical development from ancient times to present.[60] And in *Principles of the Theory of Heat*, he posited that analysis of the "development, mutation, and decay of ideas" would not only help physicists understand the nature of physical knowledge but lead "directly to the discovery, scrutiny, and criticism" of their "unconsciously formed views," chief among them a belief in the substantial reality of thermodynamic processes.[61]

Lastly, Mach sought to revivify the stale conceptual framework of physics by integrating psychological considerations into disciplinary discussions of knowledge and method. His interest in the psychology of physical reasoning was a direct outgrowth of his interest in physiology, and as early as 1863 he declared that the three disciplines not only existed in an "unbreakable connection to one another" but that one could only understand each individual domain in light of

[58] Mach, "Introduction," in *Popular Scientific Lectures*, vi.
[59] Mach, *History and Root*, 15-18.
[60] Mach, "Preface to the First German Edition," in *Science of Mechanics*, xiii.
[61] Mach, *Principles of the Theory of Heat*, 5-6

its relationship with the other.[62] Although he offered brief articulations of this relationship in essays and book chapters throughout the 1880's and 1890's, he would not release a systematic treatment of the topic until his 1905 monograph *Knowledge and Error*. Based on lectures he had delivered upon assuming a chair in the "history and philosophy of the inductive sciences" at the University of Vienna, the text offered in-depth analyses of different topics that fell under the aegis of what he called the "psychology of enquiry," ranging from the nature of memory to the explanatory function of analogy, with a specific focus on their role in physics. As with his biologically and historically oriented texts, these analyses were ultimately intended to offer a naturalistic picture of the discipline, revealing that physical research was conditioned by the same basic psychological operations found in "animal and man in nature and society." Or as he explained in language presaging that of Thomas Kuhn in *Structure of Scientific Revolutions*, he wanted to show that physical reasoning was less a set of logical rules and procedures than a form of "ordinary puzzle-solving" which was not essentially different from an animal's attempts to navigate a disturbance in its environment.[63]

While Mach's theory of interdisciplinarity often focused its role as a method of variation and tool for combatting disciplinary myopia, he also conceived of it as a means of enabling the construction of synthetic, multi-perspectival representations of phenomena. His belief that multi-perspectival pictures were more accurate than accounts which reduced natural processes to one set of properties was closely tied to his neutral monist *Elementenlehre*, which posited that the world was a continuous fabric of ontologically neutral "elements" and elemental complexes that possessed a multitude of irreducible features. Whereas atomists held that physical objects were

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[62] Ernst Mach, "Vorträge über Psychophysik," *Österreichische Zeitschrift für Praktische Heilkunde* 9 (Vienna: Veit, 1863), 365.
[63] Mach, *Knowledge and Error*, 189.

agglomerations of material points held together by various forces, in other words, Mach held that objects were really stable systems of aspatial and insubstantial elemental relations that exhibited chemical, electrodynamic, mechanical, and thermodynamic aspects. And while he acknowledged that a phenomenon's mechanical properties would likely always attract the most attention, given their biological importance for the human mind, he was also adamant that they only provided one of the many different perspectives required to fully grasp the "many-sidedness" of nature.[64]

The advantages of bringing many different perspectives to bear on a singular phenomenon were particularly evident, according to Mach, in the field of sensory physiology, where decades of collaboration between physicists and physiologists had yielded synthetic theories that were far better able to account for human cognition than accounts rooted in one approach or the other.[65] His positive valuation of the field's progress was partly based on his investigations of auditory and spatial perception, which provided many of the insights that he would later incorporate into his epistemology, as well as Helmholtz's more famous findings on the physiological underpinnings of geometry and geometrical space. But he was also adamant that he and his fellow researchers would need to draw on even wider array of disciplines if they were going to fully understand psychic life. As he wrote in 1905, it was clear that mental representations not only had physical and physiological features but psychological, mathematical, and logical features as well, and that a complete theory of mind therefore required cooperation between "the physiologist, the psychologist, the physicist, the mathematician, the philosopher, and the logician alike."[66]

More controversially, Mach argued that the construction of adequate scientific theories not only involved cooperation between academics in different fields but the circulation of ideas and

[64] Ibid, 219.
[65] Ibid.
[66] Ibid, 299.

techniques between scientists, artisans, artists, industrial workers, and others involved in "practical life." He drew much of his evidence for this point from the history of physics, which revealed numerous instances of trained researchers benefitting from contact with studios, workshops, and even the "witch's kitchen."[67] Indeed, he noted that for much of human history artisanry and other forms of practical labor represented the species' primary, if not only engine of knowledge production, and that they provided the intellectual and practical foundations from which something recognizable as scientific inquiry could emerge. And while he ceded that this was by-and-large no longer the case, he was also adamant that contemporary science, for all its technical sophistication and alienation from the "every-day" practices and thoughts, could profit from closer relations with lay communities. He placed special emphasis on establishing a dialogue between physicists and industrial laborers, arguing that the latter's work "set up experiments of such vastness and precision that they cannot be carried out in another way," and thus had the potential to "supply science with new facts and abundantly repay science for its help."[68] In a lecture that he delivered to International Electrical Exhibition in Vienna in 1883, he noted that the electrical industry was already beginning to provide a solid return on physicists' initial investment, declaring that the "intellectual nourishment" they had provided workers in the 1870's in the form of metrical standards and laws was now being rewarded with "stupendous empirical results."[69]

Although Mach tended focus on industry as the most important extramural partner in any system of interdisciplinary exchange, he also drew attention to artists, artisans, and even crackpots as potential sources of empirical or conceptual insight. The point of his *Introduction to Helmholtzian Music Theory* was not only to familiarize professional musicians with recent

[67] Ernst Mach, "Why Has Man Two Eyes," in *Popular Scientific Lectures*, 87.
[68] Mach, *Principles of the Theory of Heat*, 407,
[69] Ernst Mach, "On the Fundamental Concepts of Electrostatics," in *Popular Scientific Lectures*, 108.

scientific discoveries in the field but to urge physicists and physiologists to take advantage of the former's immense base of experience and practical knowledge. It was only through "eager cooperation" between the two domains, he claimed, that researchers could ever hope to achieve a complete understanding of musical phenomena.[70] Similarly, he noted that scientists could garner useful information from interactions with artisans and amateur inventors because they tended to possess a form of practical "know-how" that yielded scientifically relevant observations.[71] He even held that scientists could clarify and refine their ideas by looking at how they were used by crackpots. Although he did not necessarily enjoy having variations of the same discussion with people who claimed to have invented perpetual motion machines and other "theoretical monstrosities," he remarked that their arguments and justifications often provided a "psychologically enlightening" view into the "embryology" of human thought.[72]

Arguments and Techniques

Mach's desire to foster cooperation and exchange among a wide and heterogenous array of academic and lay groups faced several daunting obstacles, chief among them the dramatic intellectual and practical differences between scientific fields, to say nothing of the vast gulf separating the worlds of "scientific and every-day thought." One of his favored strategies for overcoming the latter was to use his popular corpus to show that the scientific and lay worldviews possessed many underlying connections and commonalities. In *Knowledge and Error*, he focused primarily on demonstrating commonalities in method, which is to say on showing that practices and techniques that his colleagues often assumed were exclusive to scientific reasoning were manifest in other domains of human and even animal cognition, ranging from the formulation of

[70] Mach, *Einleitung in die Helmholtzsche Musiktheorie: Populär für Musiker dargestellt*, VII.
[71] Ernst Mach, "Allerlei Erfinder und Denker," in *Populär-Wissenschaftliche Vorlesungen*, 409-413.
[72] Ibid.

hypotheses to the execution of thought experiments. He of course readily acknowledged that there were many salient differences between a cat looking for its image behind a mirror; an artisan speculating about why a technique did not work; and a physicist looking for properties they assume will be latent in some phenomenon, but his fundamental point was that all three were manifestations of the same predisposition to "spin observation further and complete a fact as regards its parts, consequences and conditions."[73]

Mach also sought to show that the methodological similarities between scientific and everyday thought were apparent in their common reliance on cognitive forms and practices that scientists derided as anti-scientific, including fantasy and "a sense for the marvelous."[74] On the one hand, he ceded that scientists were right to be suspicious of any style of reasoning that eschewed sober analysis in favor of flights of fancy and the inducement of wonder, a la the contemporary spiritualist movement. On the other hand, he maintained that an inquirer's ability to imaginatively combine experience, memory, and existing ideas, and to feel a sense of wonder at the natural world, were prerequisites of scientific advance. There would be no science of electromagnetism, he claimed in *Principles of the Theory of Heat*, without the human capacity to feel awe at "extraordinary phenomena like the attraction of small particles of rubbed amber or the adherence of iron filings to certain ores," just as there would be no thermodynamics without man's amazement at the peculiar behaviors exhibited by heat.[75]

While in some popular works Mach sought to bridge the divide between scientific and everyday reasoning by highlighting their methodological similarities, in others he focused on linking distant or alienated forms of expertise by demonstrating that they dealt with fundamentally

[73] Mach, *Knowledge and Error*, 170
[74] Mach, *Knowledge and Error*, 111.
[75] Mach, *Principles of the Theory of Heat*, 338-341.

similar phenomena and objects. This was the approach he took in his efforts to affect a rapprochement between professional musicians and sensory physiologists. In a popular lecture on harmony, for example, he sought to show that the physiologists' mathematical descriptions of mechanical waves and frequencies were merely formal representations of things that were already intimately familiar to "any tolerably cultivated musical ear."[76] Similarly, in his popular lectures on electrostatics he attempted to further consolidate the already dynamic relationship between scientists and electrical workers by articulating the historical origin and empirical content of the metrical concepts and standards that had enabled the two communities to cooperate on massive techno-scientific projects like the laying of the first transatlantic cable.[77] Although in both of these cases he largely aimed at enabling non-scientists to better understand the connection between their work and that of the scientific community, he also intended to influence the latter. Specifically, he hoped that exposing scientists to other ways of thinking about phenomena like musical tone and electrical resistance would grant them a valuable and even corrective perspective on their "positive physiologico-psychological" content and other features which the "investigating and abstracting reason" was most likely to distort or obscure.[78]

Mach adopted a similarly object- and experience-oriented approach to linking fields within academia, focusing much of his time and energy on articulating the common ontological foundation of physics and psychology. His first lectures on the unity of the two fields began appearing in the early 1860's after he had fallen under the influence Gustav Fechner's psychophysics and Johann Herbart's mathematical psychology.[79] He particularly admired their

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[76] Ernst Mach, "On the Causes of Harmony," in *Popular Scientific Lectures*, 38
[77] Mach, "On the Fundamental Concepts of Electrostatics," 108-109.
[78] Ernst Mach, "On the History of Acoustics," in *Popular Scientific Lectures*, 385
[79] Mach, *Analysis of Sensations*, xii.

attempts to show how the mind constructed the seemingly incommensurable objects of physics and psychology out of a common base of primitive sensations and resolved to adopt a similar approach in his own work. His "point of departure" in writing *Analysis of Sensations*, for example, was to adopt the naïve perspective of "a child which has just begun to differentiate itself from its environment," and then to reconstruct how the child's mind would gradually build up conceptual distinctions like "subject" and "object" from primitive elemental relations.[80] The conclusion his readers were supposed to draw from these reconstructions was that "body" and "ego" were not unified objects that were immediately given to consciousness but "makeshifts" which the species used "for provisional orientation and for definite practical ends." More importantly, his readers were supposed to come away convinced that there was "no rift between the psychical and the physical, no inside and outside, no 'sensation' to which an external 'thing,' different from sensation corresponds. There is only one kind of elements (sic), out of which this supposed inside and outside are formed."[81]

Mach's primary aim in showing that there was no real ontological difference between the core objects physics and psychology was to resolve the "troublesome pseudoproblems" posed by the mind-matter distinction.[82] In the late nineteenth-century—as in the late seventeenth—the most vexing of these pseudo-problems concerned the possibility of explaining cognition in terms of atomic motion. When the first edition of *Analysis of Sensations* came off the presses in 1886, the issue had been subject to consistent and often vitriolic dispute in Germanophone physics for nearly thirty years, due in no small part to the materialism controversy of the 1850's and the German physiologist Emil du Bois-Reymond's repeated efforts to show that no amount of scientific

[80] Ernst Mach to N.N; o.D.;O.O, NL 174, no. 2043, Deutsches Museum.
[81] Mach, *Analysis of Sensations*, 13.
[82] Ibid, xii.

investigation would ever resolve the “world riddle” posed by the problem of consciousness.[83] But from Mach’s perspective the whole debate turned on a fundamental understanding of the nature of the objects involved, which did not betoken real natural distinctions but instinctive conventions adopted for the purposes of organizing different elemental relations. Du Bois-Reymond was therefore correct to set limits on mechanical explanation, he argued, but he also did not take “the further important step of seeing that recognition of a problem as insoluble principle, must depend on a mistaken way of stating the question. For he, too, like countless others, took the instruments of a special science to be the actual world.”[84]

Interdisciplinarity as Agent of Unity and Political Change

Aside from serving as a mechanism of variation, combatting disciplinary myopia, and producing synthetic representations of phenomena, Mach viewed interdisciplinary exchange as a means of achieving two longer-term goals. The first of these was the eventual dissolution of disciplinary divides altogether and the “union of the special sciences into a consolidated whole.”[85] Put another way, he not only understood interdisciplinarity as a means of facilitating mutual understanding and cooperation between specialists but as a practice that could lay the groundwork for the formation of a complete and perfectly economical science that was free of the artificial distinctions imposed by the contemporary division of intellectual labor. He likened the process of gradual scientific unification to bodily circulation, explaining that

> The different special departments are striving for closer union, and gradually the conviction is gaining ground that philosophy can consist only of mutual, complemental criticism, interpenetration…as the blood nourishing the body separates into countless capillaries,

[83] Emil du Bois-Reymond, *Über die Grenzen des Naturerkennens. Die Sieben Welträtsel.* (Leipzig: Veit und Co., 1891).
[84] Mach, *Analysis of Sensations*, 314.
[85] Ernst Mach, “The Part Played by Accident in Invention and Discovery,” in *Popular Scientific Lectures*, 261.

> only to be collected again and to meet in the heart, so in the sciences of the future all the rills of knowledge will gather more and more into a common and undivided stream.[86]

In keeping with his organic view of knowledge, he argued that the nearer the research community approached unification, the "the more capable it will be of controlling the disturbances of practical life, and thus of serving the purpose out of which its first germs were developed," akin to the way that a perfectly adapted organism could effortlessly navigate any environment.[87]

One way that Mach sought to further the cause of scientific unity was by providing general concepts and theories that researchers could use in all domains of intellectual activity. Or as he explained in *Analysis of Sensations*, there was no way to unify the disparate branches of human thought using the "limited conceptions" of a "narrow special department" like mechanics, as the proponents of the mechanical worldview suggested. Nor could one affect this unification using the metaphysical conceptions that Kant and his acolytes had formulated over the previous century. Rather, one had to create intellectual tools that were firmly rooted in empirical reality and that researchers could apply in all areas of inquiry without doing violence to the facts themselves.[88] He saw his theory of the elements as one such conception, insofar as it provided a metaphysics-free foundation upon which "a unified monistic conception" of all nature could be built.[89] Indeed, he claimed that it had already achieved the most difficult part of this unification by showing that the *Elementenlehre* could easily overcome the ostensible incommensurability of the physical and psychological sciences.

Another unifying conception that Mach sought to integrate into scientific discourse was the mathematical notion of "function." Traditionally used to describe relations between quantities

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[86] Ibid.
[87] Mach, *Principles of the Theory of Heat*, 336-337.
[88] Mach, *Analysis of Sensations*, 313.
[89] Ibid, 312.

or sets of quantities, he first began to speak of objects and processes as being "functionally related" or as "functions of" one another in 1863.[90] As he developed his thinking on the matter over the next two decades, he would repeatedly emphasize that the function concept had two distinct advantages over competing ways of accounting for the "connectedness" of natural events. First, a statement like "the temperature of an ideal gas in a closed system is a function of its pressure and volume" described the mutual dependence of the relevant phenomena on one another in a way that was stronger than mere Humean association but avoided the pitfalls of the "mysterious agency called causality," which carried undesirable hints of determinism and occluded the complexity of natural relations by simplifying the relationship between variables.[91] And second, he claimed that representation in terms of functions had the advantage of being able to explain the relative stability and permanency of objects without using the equally mysterious and unpalatable notion of substance. Rather than construing an iron bar as comprised of some quantity of empirically inaccessible matter, in other words, one could interpret it as an agglomeration of stable functional relations between what was "really subsistent" in any natural object, i.e., its mass, velocity, position, temperature, electromagnetic potential, chemical potential, etc.[92]

Although Mach largely understood interdisciplinary exchange as a means of achieving various scientific aims, he also understood it as a way of supporting his political goals, which were closely—albeit not completely—aligned with those of the Austrian Social Democratic Party.[93] He was deeply averse to clerical and conservative politics from a young age, in part because of his

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[90] Erik Banks, *Ernst Mach's World Elements: A Study in Natural Philosophy* (Dordrecht: Kluwer Academic Publishers, 2003), 38.
[91] Mach, "On Mental Adaptation," 221.
[92] Mach, *Principles of the Theory of Heat*, 388.
[93] As John Blackmore notes, Mach never officially joined the SDAPÖ. See: John Blackmore, *Ernst Mach: His Work, Life, and Influence* (Berkeley: University of California Press, 1972), 233-235.

disdain for the religious education he received in school and in part because his parents were "free-thinkers."[94] He once recalled that his father had taken the failure of the 1848 revolutions so hard that he made him apprentice with a local carpenter in the hopes that learning a trade would enable him to join the other "48ers" who had emigrated to the United States. And while he did not end up making it to America, his brief apprenticeship gave him a respect for the virtues of labor and the plight of "working people" that he would carry with him for the rest of his career.[95]

Mach became a relatively stable fixture in the world of socialist politics after he moved to Vienna in 1895 and began to foster relationships with SDAPÖ politicians and the intellectuals of the nascent Austromarxist movement. Most notably, in the early 1900's he became close friends with the physicist, social theorist, and eventual assassin Friedrich Adler, who often served as his guide to the latest happenings within the Second International and as a conduit for his ideas in socialist circles. By 1905, Adler had been so successful in embedding Mach's philosophy in socialist discourse that his ideas were almost as influential among Marxists as they were among scientists. Adler's efforts also attracted many prominent detractors, including Vladimir Lenin, who deemed "Machism" so threatening to dialectical materialism that he dedicated an entire monograph, *Materialism and Empirio-criticism: Critical Comments on a Reactionary Philosophy*, to explicitly refuting it in 1909.

Despite Mach's proximity to the intellectual and social heart of Central European socialism, he only occasionally involved himself in their political activities. This is not to say that he avoided political engagement altogether—at various points he spoke out in favor of social democratic projects at the Austrian parliament; publicly agitated for socialist policies; and wrote

[94] Ernst Mach, "Text of the 1910 Manuscript, in "Three Autobiographical Manuscripts" by John Blackmore, *Annales of Science* (July 1, 1978), 440.
[95] Ibid.

newspaper editorials railing against Christian Social malfeasance—but that most of his activism took place in arguments and asides in his popular works.[96] In most cases, his political arguments grew naturally out of his broader analyses of scientific knowledge and method. In *Principles of the Theory of Heat*, he used his critiques of intellectual superstitions like atomism and causality as springboards to attack the sociopolitical superstition of nationalism, arguing that both the former and the latter stemmed from the same psychological prejudices and predispositions.[97] And in *Analysis of Sensations*, he used his ruminations on the intrinsic connection between different kinds of intellectual work to suggest that the maintenance of artificial distinctions between artisanal and scientific inquiry perpetuated the social harm of classism.[98]

Even more than combatting classism and nationalism, Mach hoped that his attempts to highlight the scientific value of interdisciplinary cooperation and exchange would teach his readers that the results of research, like the results of human labor more generally, could not remain the exclusive property of a small group of experts "but must redound to the benefit… of the collective whole."[99] That is, from his perspective one of the most dangerous results of the professionalization and specialization of scientific research was the gradual accumulation of intellectual capital in small communities of specialists. On the one hand, he recognized that social elites had always tended to hoard ideas, usually to use them as mechanisms of social control. What else were the priestly castes and guilds of the middle ages, he asked in *Knowledge and Error*, than institutions which sought to leverage "learned secrecy-mongering" into social power?[100] On the other hand,

[96] Blackmore, *Ernst Mach: His Work, Life, and Influence*, 232-246.
[97] Mach, *Principles of the Theory of Heat*, 299,
[98] Mach, *Analysis of Sensations*, 23.
[99] Mach, "Introduction," in *Popular Scientific Lectures*, vii.
[100] Mach, *Knowledge and Error*, 165; and Ernst Mach to Paul Carus, Prague, 6/11/1894, in *Ernst Mach als Aussenseiter* ed. John Blackmore and Klaus Hentschel (Vienna: Braumüller, 1986),

he felt that the situation circa 1900 was more dire, insofar as the specialized research communities of the fin de siècle not only produced more and better knowledge than their medieval predecessors but possessed more effective institutional and legal instruments, including "patent laws and so forth," for rendering their findings into a form of private property.[101] He feared that the end result of this form of privatization would be the creation of "a regular caste of capitalists in thought…and these would certainly be the most dangerous capitalists of all" because they could withhold the working classes' most powerful tool for economic and social amelioration.[102]

Viewed in this light, Mach's attempts to foster the circulation of ideas between as many social groups as possible were also part of the eminently socialist political project of collectivizing resources and lessening working-class alienation. Put in the language of his friends in the Austrian Social Democratic Party, he understood his interdisciplinary work as a kind of anti-trust tool which aimed at breaking up bourgeois academics' monopoly on the means of knowledge production and creating a more equitable distribution of intellectual capital. As with his efforts to build a unified science, he recognized that familiarizing laborers with the rudiments of specialist inquiry was only the first step of a much longer process, but he was nevertheless adamant that this kind of scientific *Bildungspolitik* was even more important than direct action for the eventual triumph of socialism in Austria. As chapter four of this dissertation will show, the Austromarxists shared this belief as well.

Conclusion

Kultur und Mechanik, the last of Mach's popular works to appear before his death in 1916, was a fitting capstone to his long and heterogenous career. Written with the help of his son Ludwig,

[101] Ibid.
[102] Mach, *Principles of the Theory of Heat*, 226-227.

the text used recent anthropological findings and theories to shed light on the prehistoric origin of basic ideas about natural objects and forces. Like many of his other popularizations, he intended for *Kultur*'s arguments to provide more evidence for the empirical origins of physical laws that his colleagues often took to be a priori, and to draw more attention to the important role played by artisans and "simple workers" in establishing the field's conceptual foundations.

Mach also hoped that *Kultur* would bring yet another outside perspective to bear on physical reasoning, and to suggest to his readers that a full understanding of any given domain of nature was only possible by virtue of cooperation and exchange between different academic fields and forms of expertise.[103] As this chapter has shown, his belief that interdisciplinarity was integral to the intellectual health and growth of the scientific enterprise was not unique, but his arguments for how it contributed to scientific discourse were, insofar as they were rooted in his peculiar philosophical views. The most important of these views was his "biological and economic" epistemology, which provided the foundation for his conception of interdisciplinary exchange as a method of producing the kind of conceptual, empirical, or linguistic variation that drove human cognitive evolution. His naturalistic theory of knowledge also framed his belief that the circulation of ideas between different fields was an essential tool in the battle against disciplinary myopia, and thus in the battle against metaphysics. As he remarked in *Principles in the Theory of Heat*, allowing disciplines to seal themselves off from outside influence was akin to isolating a species on an island, because both actions tended to produce adaptations that were unfit, if not pathological, beyond a very narrow set of circumstances. And finally, his understanding of the scientific value of interdisciplinary collaboration reflected his ontological belief that reality was

[103] Ernst Mach, *Kultur und Mechanik* (Stuttgart: W. Spemann, 1915).

comprised of a unitary fabric of “many-sided” elements, which led him to renounce reductionism and champion multiperspectivalism as the only plausible way fully describe natural phenomena.

Mach adopted a variety of strategies to bring the interdisciplinary connections and relations he desired into being. In some cases, he used his popular texts to emphasize the methodological similarities between different kinds of inquiry. *Knowledge and Error* was in large part an attempt to convince physicists that iterations of the discipline’s methods could be found in other forms of human and even animal cognition, and that there was therefore no reason to assume that scientific reasoning was fundamentally different from other, ostensibly less developed forms of reasoning. In other texts, like his monographs and essays on acoustics and music theory, he focused less on unearthing commonalities in method than on showing that musicians and scientists based their seemingly disparate ideas about phenomena like tone and pitch on the same basic sensations and perceptions. And in some cases, he sought to demonstrate that disciplines that scientists took to be incommensurable, like physics and psychology, dealt with ontologically equivalent objects, and that the academic community’s obsession with the possibility of deriving consciousness from matter-in-motion was a “pseudoproblem” based on a fundamental category error.

Although Mach often had specific and relatively short-term scientific goals in mind when he called for greater contact between artisans, scientists, and workers, he also saw his interdisciplinary texts as preliminary steps towards two longer-terms goals. The first of these was the formation of a “complete science,” which is to say a science that eschewed disciplinary distinctions entirely in favor of a unified set of concepts, methods, terms, and theories. His second goal was to further cause of social democracy by putting scientific knowledge and research at the disposal of people whose lives revolved around technoscience but were nevertheless alienated from the means of its production. Aside from benefitting all the parties involved and discharging a moral duty, he maintained that increasing the public accessibility of expert science would have the hugely desirable effect of thwarting the formation of a “fearsome caste” of intellectual robber barons that hoarded scientific riches that were intended for the mutual benefit of all.

Unsurprisingly, Mach’s conception popularization as a mechanism of interdisciplinarity, and of interdisciplinarity as a mechanism of scientific and political progress, found a particularly receptive audience amongst the physicists and philosophers of the Vienna Circle. But as the next chapter will show, it found surprisingly few adherents or analogues amongst Austrian biologists, who shared his general belief that popular media contributed to scientific discourse but nevertheless used their popularizations to pursue a different set of epistemic goals.

Chapter 3:

From Volksbildung to Phylogenetics: Academic and Popular Biology, 1890-1914

On September 18th, 1912, the Austrian botanist Richard von Wettstein took to the pulpit at the 85th *Versammlung der Deutscher Naturforscher und Ärzte* (VDNA) to deliver one of the conference's plenary lectures. In keeping with the long-standing tradition of using the meeting to address issues of broad, interdisciplinary interest, Wettstein chose to speak on "biology and its meaning for contemporary culture." The bulk of his talk focused on what he saw as one of the discipline's major challenges in the age of mass media: moderating public discourse on biological knowledge and theory.[1] His specific concern was the dramatic and largely unpoliced expansion of popular and pedagogical literature on scientifically unsettled but conceptually rich and politically controversial topics like ecology and natural selection. He suggested that the hazards posed by this literature were twofold. First, bad popular works led unsuspecting lay readers and students to internalize falsehoods and misrepresentations of the biological enterprise, thereby damaging the sociopolitical status and long-term viability of the discipline. Although he did not offer any explicit examples of this sort of thing happening, virtually every member of his audience would have been familiar with the role of Ernst Haeckel's polemical popular lectures on Darwin in fanning the flames of a political struggle over evolution that resulted in biology being virtually stricken from German gymnasia in the late 1870's.[2] And second, he claimed that popular texts and textbooks

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[1] Richard von Wettstein, "Die Biologie in ihrer Bedeutung für die Kultur der Gegenwart," *Verhandlungen der Gesellschaft Deutscher Naturforscher und Ärzte: 84 Versammlungen zu Münster* ed. Alexander Witting (Leipzig: F.C.W. Voegel, 1913) 217-225.

[2] For Haeckel's conflict with Rudolf Virchow over the place of evolutionary theory in German gymnasia, see: Robert Richards, *The Tragic Sense of Life: Ernst Haeckel and the Struggle over Evolutionary Thought* (Chicago: University of Chicago Press, 2008), 312-329.

could exert a baleful influence on academic botanists and zoologists themselves by subtly informing the kinds of methods, theories, and projects that they adopted and directing students and younger researchers towards—or away from—certain areas of study.

Wettstein was not the first scientist to broach the topic of scientific communication and its discontents at the *Versammlung*, nor was he the first to suggest that biologists needed to take popular and pedagogical representations of biological knowledge and research more seriously.[3] Indeed, popularization and pedagogy had been recurrent topics of discussion at *VDNA* meetings since the 1860's, stemming in large part from the Germanophone scientific community's deep-seated anxieties about the epistemological and sociopolitical effects of professionalization and specialization, as well as biologists' specific concerns about the public reputation and institutional fate of their controversial discipline.[4] But as this chapter will seek to show, Wettstein's 1912 address was not just a reflection of general sentiments and trends within the life sciences in Central Europe but of his immersion in the peculiar intellectual environment of academic botany in Austria, where the relationship between popular exposition and scientific knowledge had been a topic of discussion since the 1860's.

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[3] Friedrich Ahlborn had made virtually the same argument as Wettstein at the VDNA in 1901, claiming that Darwinism's fusion with the "radical materialism" of Haeckel and Karl Vogt had done "irreparable harm to biology" by bringing about its exclusion from German gymnasia, although he did not mention that this fusion had largely occurred popular texts and lectures. See: Friedrich Ahlborn, "Die gegenwärtige Lage des biologischen Unterrichts in den höhern Schulen," *Verhandlungen der Gesellschaft Deutscher Naturforscher und Ärtze: 73. Versammlung zu Hamburg* ed. Albert Wangerin (Leipzig: FCW Vogel, 1902), 274-281.

[4] Rudolf Virchow, "Über die nationale Entwicklung und Bedeutung der Naturwissenschaften"; Emil Rossmässler, "Über naturgeschichtliche Volksbildung," *Amtlicher Bericht über die vierzigste Versammlung Deutscher Naturforscher und Ärzte zu Hannover* (Hannover; Hahn'sche Hofbuchhandlung, 1866), 56-64, 71-73. One could make a strong case that the German scientific community's interest in popularization as an antidote to overspecialization go back even further. For example, Lorenz Oken used his address at the first *VDNA* in 1822 to raise his concerns about the atomization of research into distinct specialisms, and to advocate for lectures and texts that served to generalize and synthesize field-specific results, although he did not call them "popularizations."

Wettstein and his Austrian colleagues' deep interest in popular media, as well as other non-technical forms of scientific communication like reference works and textbooks, had much to do with the odd admixture of research interests and institutions that dominated Austrian botany in the last decades of the nineteenth century. On the one hand, Austrian universities' early investment in novel, laboratory-based fields like plant physiology in the 1870's led many of the country's researchers to embrace experimental programs that focused on investigating the cellular and physico-chemical processes underlying plant life. Perhaps most famously, Julius Wiesner and his students at the Institute for Plant Physiology in Vienna—the first of its kind anywhere in the world—made critical contributions to the study of photosynthesis, plant tissues, and tropisms, and helped establish the foundations of plant ecology, in the 1880's.[5] Several alumni of the Institute would later go on to run the botanical section of the Viennese *Biologische Versuchsanstalt*, which was at the center of the avant-garde fields of developmental mechanics and experimental morphology in the early 1900's. On the other hand, the powerful institutional and intellectual influence of Wettstein's mentor Anton Marilaun von Kerner, director of the University of Vienna's Botanical Institute from 1878 to 1892 and a pioneering researcher on Habsburg flora, assured the local vitality of research programs focused on more traditional, which is to say less laboratory-oriented practices like observing, describing, and classifying the plants one encountered in the field.

As a result of Austrian botany's institutional and intellectual split between Wiesner and Kerner, or what amounted to a split between an older field-based and a newer laboratory-based

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[5] If Wiesner is familiar to historians of science at all, it is usually by virtue of his disagreement with Darwin on the causes of motion in plants. See: Soraya de Chadarevian, "Laboratory Science versus Country-House Experiments. The Controversy between Julius Sachs and Charles Darwin," *British Journal for the History of Science* Vol. 20, No. 1 (Mar. 1996), 17-41.

botany, practitioners often adopted ideas and positions that were at odds with those of the broader Central European botanical community. This was particularly true of Wettstein, who found himself simultaneously defending a novel form of neo-Lamarckism and an ostensibly outmoded approach to plant systematics around 1900. As the first part of this paper will show, a key aspect of his defense strategy was to argue that the marginalization of his theoretical and methodological commitments had less to do with honest scientific assessment of them than with his opponents using their popularizations to manipulate scientific opinion. With respect to evolutionary theory, he maintained that the lack of support for his conciliatory brand of neo-Lamarckism, which ceded a limited legitimacy to natural selection but put forth the inheritance of acquired characteristics as nature's primary mechanism of speciation, was closely tied to the outsize influence that a speculative and oftentimes myopic popular literature had exerted on discussions of the topic. Many of the popular works he had in mind were Darwinian in orientation, insofar as he thought that Darwin's popularizers were not only particularly effective in getting their message across but prone to making claims that went far beyond what the facts warranted, but he was also critical of any work in the genre that selectively read the evidence to support a given theory.

Wettstein and several of his followers, including his assistant K.C. Rothe and former student August Ginzberger, made much the same point with respect to the declining fortunes of their research program in plant systematics, arguing that the growing popular and pedagogical literature on ecology had adversely and unfairly affected the field in three distinct ways. First, they noted that this literature painted systematics and other primarily descriptive and classificatory branches of botany as boring and outmoded, which made it difficult to interest students in the field and threatened the status of careful observation and morphological comparison as the cornerstones of biological reasoning. Second, they accused popular ecologists of pushing highly speculative explanations of evolutionary phenomena. As staunch advocates of the principle that understanding an organism's evolutionary history was integral to its classification, Wettstein et. al. found it particularly galling that the ecologists often explained complex adaptations like mimicry and protective coloration as responses to contemporary environmental conditions rather than as artifacts of phylogeny. And third, they blamed the ecologists for diminishing the number of amateurs that were interested in observing and collecting local flora, which severely hampered their ability to carry out the kind of massive biogeographical surveys that were necessary to creating more accurate taxonomies.

Despite Wettstein, Rothe, and Ginzberger's occasionally aggressive denunciation of the popular and pedagogical literature on Darwinism and ecology, they also saw popularizations, reference works, and textbooks as a means of advocating for their intellectual commitments and research programs. The second part of this paper will focus on several such attempts, beginning with Wettstein's popular analyses of evolutionary theory between 1892 and 1914. The most basic function of these analyses was to articulate and spread his views on organic change and development, but he also used them to adjudicate a relatively technical dispute amongst his fellow neo-Lamarckians. Specifically, he hoped that his popularizations would help resolve a disagreement over the possibility that so-called adaptive and organizational traits, or traits that represented direct adaptive responses to environmental factors and those that were characteristic of the basic form of the organism and therefore insulated from the effects of transient "outer influences," could influence one another.[6] In arguing that there was no absolute barrier between the two, and that over time adaptive traits could become organizational traits, he once again found

[6] Carl Nägeli, *Mechanisch-physiologische Theorie der Abstammungslehre* (Munich and Leipzig: R. Oldenbourg, 1884).

himself on the margins of the disciplinary opinion and enmeshed in conflict with the other towering figure of fin de siècle Austrian botany, Julius Wiesner.

In addition to intervening in biological discussions on the topic of evolution, Wettstein, Ginzberger, and Rothe used their non-technical work to attempt to shore up support for their research programs in biogeography and systematics. One of their primary foci was reforming science instruction in the Monarchy's lower and middle schools so that these fields were made central to biological curricula. Above all, they wanted students to learn that all the different branches of biological inquiry, including ecology, reposed on a common empirical foundation established by careful observation, description, and classification. They also hoped that their suggested pedagogical reforms would strengthen the amateur botanical networks that they relied on for data and fieldwork by sparking greater interest in collecting specimens and making observations among students but especially among rural teachers, who were a historically important but diminishing source of botanical labor. And finally, they used their popular texts, as well as their connections to many of Austria's most powerful scientific and popular-scientific institutions, to train and deploy other labor pools, ranging from the urban working classes to bourgeois alpinists.

In his 2003 *Victorian Sensation*, historian James Secord characterized the Darwinian revolution not just as a moment in the history of ideas but as "an episode in the industrialization of communication and the transformation of reading audiences," meaning that the form and content of Victorian evolutionary theory was not determined by scientific discourse alone but by the production, circulation, and highly variegated interpretation of books by different publics.[7] In

[7] James Secord, *Victorian Sensation: The Extraordinary Publication, Reception, and Secret Authorship of Vestiges of the Natural History of Creation* (Chicago: University of Chicago Press, 2003), 4.

drawing attention to the material, circulatory, and communicative processes underlying the construction of evolutionary thought, Secord challenged historians to expand their gaze beyond elite scientific circles and sites to places, objects, and actors long considered irrelevant to the production of biological knowledge, ranging from working-class meeting halls to the mass-market products of the publishing houses on “Grub Street.”

In the decade following Secord’s pathbreaking work, historians of nineteenth-century Britain took up the gauntlet he had thrown down by examining the role of an even wider variety of non-elite actors, spaces, practices, and literary genres in the production and circulation of scientific knowledge.[8] In the process, they painted a historical picture of Victorian biology that was, as historian Denise Richards noted in 2010, far richer and more diverse than that of biology in nineteenth-century Germany, which remained resolutely focused on university-based scientists and institutes.[9] The disparity between the two literatures is still somewhat evident today, although a growing number of historians have shifted their focus towards more novel and oftentimes marginal loci of biological research in Central Europe. Among the most important of the new works in this vein is Lynn Nyhart’s *Modern Nature: Rise of the Biological Perspective in Germany*, which shed much-needed light on the role of taxidermists, museum curators, zookeepers, and other “practical naturalists” in establishing the conceptual outlines and methods of what would later become the academic field of animal ecology.[10] Another key text is Gerd

[8] To name only a few recent texts: Bernard Lightman, *Victorian Popularizers of Science: Designing Nature for New Audiences*, (Chicago: University of Chicago Press, 2007); *Geographies of Nineteenth-Century Science* ed. David Livingstone and Charles W.J. Withers (Chicago: The University of Chicago Press, 2011). *Science Periodicals in Nineteenth-Century Britain: Constructing Scientific Communities* ed. Gowan Dawson, Bernard Lightman, Sally Shuttleworth, and Jonathan Topham (Chicago, University og Chicago Press, 2020).

[9] Denise Phillips, “Reconsidering the *Sonderweg* of German Science: Biology and Culture in the Nineteenth Century,” *Historical Studies in the Natural Sciences*, Vol. 40, No. 1, (Winter, 2010), 136-147.

[10] Lynn Nyhart, *Modern Nature: The Rise of the Biological Perspective in Germany* (Chicago: The University of Chicago Press, 2009).

Müller's *Vivarium*: *Experimental, Quantitative, and Theoretical Biology at Vienna's Biologische Versuchsanstalt*, an edited collection which explored the relationship between the *Versuchsanstalt*'s experimental program and the practices of amateur aquarists, animal breeders, and other hobbyists.[11]

In addition to highlighting new actors and spaces of biological research, historians of Central European biology have increasingly drawn attention to the constructive role of non-technical and especially popular literature in nineteenth and early twentieth century biological discourse.[12] In Nyhart's *Modern Nature*, she not only demonstrated that animal ecology first took root in museums and zoological parks but that the field's practitioners first established and communicated its basic intellectual contours in popular and pedagogical texts.[13] Werner Michler and Nick Hopwood have maintained that popular texts were similarly central to the construction of evolutionary knowledge in Austria and Germany, and that they played a particularly important role in mediating between existing theories of descent and Darwin's.[14] And finally, Sander Gliboff and Manfred Laublichler have highlighted experimental zoologist Paul Kammerer's use of popularization as a tool to articulate his views on evolution and consolidate support for his controversial research on the inheritance of acquired characteristics.[15]

Although Nyhart, Gliboff, and others have provided a more detailed picture of the role of

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[11] *Vivarium: Experimental, Quantitative, and Theoretical Biology at Vienna's Biologische Versuchsanstalt* ed. Gerd Müller (Boston: MIT University Press, 2017).

[12] Andreas Daum, *Wissenschaftspopularisierung im 19. Jahrhundert: Bürgerliche Kultur, naturwissenschaftliche Bildung und die Deutsche Öffentlichkeit, 1848-1914* (Munich: Oldenbourg Verlag München, 1998). Daum's monograph remains the most comprehensive and important work on the topic.

[13] Nyhart, *Modern Nature*, 293-320.

[14] Werner Michler, *Darwinismus und Literatur: Naturwissenschaftliche und literarische Intelligenz in Österreich: 1859-1914* (Vienna: Böhlau, 1999).

[15] Sander Gliboff, "The Case of Paul Kammerer: Evolution and Experimentation in the Early Twentieth Century," *Journal of the History of Biology* 39 (2006), 525-563; Manfred Laubichler, "The Emergence of Theoretical and General Biology: The Broader Scientific Context for the Biologische Versuchsanstalt," in *Vivarium*, 95-115.

marginal figures, modes of communication, and spaces in Central European biology between 1800 and 1914, the scholarly literature on the topic still has several prominent lacunae. Most notably, historians have paid far greater attention to the German biological community than the Austrian, which remains something of an afterthought. Moreover, of the small number of texts that have focused on Austria, many have concentrated on the same cluster of intellectually and politically progressive but scientifically peripheral figures, including Kammerer, Hans Przibram, Julius Tandler, and Eugen Steinach.[16] Put another way, historians of Austrian biology have charted their own *Sonderweg* by passing over biologists who were central to Habsburg scientific life like Wettstein, who remains conspicuously absent from both the Anglophone and Germanophone historiographies. This chapter's arguments not only intended to address the historiographical gap on the eminent botanist but to suggest three revisions to the scholarly literature on Austrian neo-Lamarckism, plant ecology, and popular biology based on his practices and views.

While historians have long been familiar with Austria's role as a center of neo-Lamarckian thought and as a catalyst in the so-called "eclipse of Darwinism," they have had relatively little to say about variations within the Austrian neo-Lamarckian camp, or with respect to how its most important advocates arrived at and changed their views over time. One recent exception is Johannes Feichtinger's analysis of the various "*Darwinfeier*" that took place in Vienna to celebrate Darwin's hundredth birthday in 1909. Aside from demonstrating something resembling a consensus on the shortcomings of natural selection vis-à-vis the inheritance of acquired

[16] In addition to the work on the *Versuchsanstalt* noted above, see: *Wissenschaft, Politik, und Öffentlichkeit. Von der Wiener Moderne bis zur Gegenwart* ed. Christian Stifter and Mitchell Ash (Vienna: Wiener Universitätsverlag, 2002), and Veronika Hofer's contribution in particular, "Rudolph Goldschied, Paul Kammerer und die Biologen des Prater-Vivariums in der Liberalien Volksbildung der Wiener Moderne." See also: Cheryl A. Logan, *Hormones, Heredity, and Race: Spectacular Failure in Interwar Vienna* (New Brunswick: Rutgers University Press, 2013), and Klaus Taschwer, *Der fall Paul Kammerer: Das abenteurliche Leben des umstrittensten Biologen seiner Zeit* (Munich: Carl Hanser Verlag, 2016).

characteristics, Feichtinger argues that the celebrations occasioned an early split between proponents of what he has called "progressive and regressive milieu theories."[17] Advocates of the former were committed to the notion that continued improvement of environmental conditions would assure the progressive amelioration of the human species, while proponents of the latter deployed concepts like "biological inertia" to support a form of evolutionary declinism that was later adopted by anti-Semitic politicians and intellectuals.[18] Although this divide was certainly important, this chapter's analysis of Wettstein's dispute with Wiesner demonstrates that the Austrian neo-Lamarckians were also divided on issues that had more to do with divergent working objects, investigative methods, and career trajectories than political commitments.

This chapter also suggests that the lack of scholarly interest in Wettstein has led historians of biology to overlook his role in the advent of plant ecology. Specifically, Wettstein's work on Lamarckian mechanisms of speciation in the lowest taxonomic orders—i.e., species and genera—in the 1890's was closely tied to what he understood to be ecological considerations concerning the adaptive relationship between individual organisms and their abiotic and biotic environments. This research, in conjunction with his and his allies' attempts to mediate between systematics and popular ecology in the 1900's, indicates that the consolidation of plant ecology as a coherent field of academic inquiry was not exclusively connected to the work of lab-based physiologists like A.W.F. Schimper.[19] Rather, it was also part of an older biogeographical tradition that was focused less on uncovering the causal mechanisms connecting different elements of biomes than observing

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[17] Johannes Feichtinger, "Krisis des Darwinismus? Darwin und die Wissenschaften des Wiener Fin de Siècle," in *Darwin in Zentraleuropa: Die wissenschaftliche, weltanschauliche und populäre Rezeption im 19. Und frühen 20. Jahrhundert* ed. Herbert Matis and Wolfgang Reiter (Münster: LIT Verlag, 2018), 63-86.
[18] Ibid.
[19] Eugene Cittadino, *Nature as the Laboratory: Darwinian Plant Ecology in the German Empire, 1880-1900* (Cambridge: Cambridge University Press, 1990).

the adaptive dynamics of living plant communities *in situ*.[20]

Lastly, this chapter is intended to contribute to an ongoing reevaluation of the role of popular exposition in biological discourse.[21] As noted above, historians have made concerted strides in understanding how the popular genre conditioned the way that scientists thought about topics like animal ecology and Darwinism, but scholarly inquiry on the subject is still in its very early stages.[22] With respect to fin de siècle Austria, there are at least two open questions that historians have only just begun to answer: why did Austrian biologists display an interest in popularization to begin with, and how did their popular works inform the biology of the period?[23] By examining the popular output of Wettstein and his followers, this chapter offers a partial response to both inquiries. In terms of motive, it suggests that the Austrian botanical community's interest in popularization was at least partly a function of the marginality of its practitioners' theoretical and methodological commitments, which fostered a keen awareness of the intrinsic connection between the popular, pedagogical, and expert domains. In terms of epistemic effects, it indicates that their popular works exerted a significant influence on the way botanists thought about major topics like ecology and evolution as well as more technical issues related to the origin

[20] For more on the relationship between biogeography and ecology, see: William Coleman, "Evolution into Ecology? The Strategy of Warming's Ecological Plant Geography" *Journal of the History of Biology* Vol. 19, No. 2 (Summer, 1986), 181-196; and Malcolm Nicolson, "Humboldtian Plant Geography after Humboldt: The link to Ecology," *The British Journal for the History of Science* Vol. 29, No. 3 (Sep., 1996), 289-310.
[21] For a general overview of this nascent literature, see: Mitchell G. Ash, "Literaturübersicht: Wissenschaftspopularisierung und Bürgerliche Kultur im 19. Jahrhundert," *Geschichte und Gesellschaft* 28. Jahrg., H.2, (Apr.-Jun., 2002), 322-334; and Jonathan Topham, "Rethinking the History of Science Popularization/Popular Science," in *Popularizing Science and Technology in the European Periphery, 1800-2000*, ed. Faidra Papanelopoulou, Agusti Nieto-Galan, and Enrique Perdiguero (VT: Ashgate, 2009), 1-20.
[22] Andreas Daum, "Varieties of Popular Science in the Transformation of Public Knowledge: Some Historical Reflections," *Isis*, Vol. 100, No. 2 (June 2009), 320. Daum identifies several serious "imbalances" in the Anglophone literature on popularization, including an overemphasis on British the context.
[23] Deborah Coen, "Rise, Grubenhund: On Provincializing Kuhn," *Modern Intellectual History* Volume 9, Issue 01 (April 2012), 109-126. Although Coen does not address biology specifically, she offers a convincing argument for the relationship between Habsburg scientists' multilingual world and their interest in various forms of "translation," including translation into everyday language and experience.

and meaning of certain kinds of biological traits.

Popular Texts and the "Crisis of Darwinism"

Throughout the fall and winter semesters of the 1901/1902 academic year, the "Philosophical Society of the University of Vienna," one of the city's most important venues of intellectual discussion and exchange, hosted a series of lectures dedicated to addressing what it called "the crisis of Darwinism," or what it might have more accurately called "the crisis of the theory of natural selection."[24] To say that Darwinism was in crisis in Central Europe in 1901 was perhaps a slight exaggeration, as numerous biologists, including prominent researchers like Albert Kölliker, Wilhelm Roux, and August Weismann, were still avowed supporters of various aspects of the doctrine. Indeed, to many scientists it seemed as if Weismann's 1892 work on the impermeability of the barrier between an organism's somatic cells and its germ-plasm, where its hereditary material was stored, had definitively disproved the possibility of the inheritance of acquired characteristics.[25] But throughout the 1880's and 1890's, a growing number of voices in the discipline began to express doubts about the capacity of natural selection to fully explain the emergence of new species.

One especially strident voice among this crowd of doubters was Max von Kassowitz, who delivered the first lecture of the Philosophical Society's "Darwinism" series. A professor of pediatrics at the University of Vienna and author of several books on heredity, he was also representative of an extreme brand of neo-Lamarckism that refused to cede any explanatory ground to natural selection. His contribution to the debate was not necessarily notable for its aggressive

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[24] On the Philosophical Society, see: *Ernst Mach's Vienna, 1895-1930, Or Phenomenalism as Philosophy of Science* ed. John Blackmore et. al. (Dordrecht: Kluwer Academic Publishers, 2001), 227-315.

[25] For more on Weismann's reception in Austria, and critiques of his work by Austrian physiologists in particular, see: Cheryl Logan, *Hormones, Heredity, and Race: Spectacular Failure in Interwar Vienna* (New Jersey: Rutgers University Press, 2013).

tone or absolutism, which were par for the course in discussions of evolutionary theory, but for its psychosocial explanation of Darwinism's meteoric rise to unchallenged dogma. His argument revolved around two core points. First, he claimed that Darwin and his popularizers were so successful in spreading their message because they couched their arguments in "seductive" and easily digestible but misleading analogies and buzzwords like "survival of the fittest" and "the struggle for existence."[26] One could find a particularly egregious example of this kind of scientific demagoguery, he noted, in Ernst Haeckel's recently published *Die Welträtsel*, which anthropomorphized natural selection as a tool wielded by the "god of selection" in order to appeal to "the inclination of most people" to make sense of complicated natural processes in terms of personified forces.[27]

The second part of Kassowitz's argument posited that Darwin and his followers had been the lucky beneficiaries of new mechanisms for the mass publication and circulation of texts that began to appear after 1840, and which enabled the kind of rapid, trans-European spread of ideas that was far more difficult to achieve when Lamarck first released his *Philosophie Zoologique* in 1809. Put in Kassowitz's own words, most of Lamarck's contemporaries were "in no position to learn about the great results stemming from (his) work" because "the state of publishing with respect to scientific news was so poorly organized at the time," but when Darwin was ready to publish *Origin of Species* in 1859, "the periodical literature had already begun its uncanny upswing."[28] This explanation slightly exaggerated the role of print exposure in German biologists' rapid acceptance of Darwinism, which also strongly appealed to existing biological sensibilities,

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[26] Max von Kassowitz, "Die Krisis des Darwinismus," in *Wissenschaftliche Beilage zum fünfzehnten Jahresbericht der Philosophischen Gesellschaft an der Universität zu Wien: Die Krisis des Darwinismus*, ed. M. Kassowitz, R. v. Wettstein, B. Hatschek, C. Ehrenfels, and J. Breuer (Leipzig, J.A. Barth, 1902), 18.
[27] Ibid, 8.
[28] Ibid, 18.

but the difference in the reception of the two texts was nevertheless striking: *Philosophie Zoologique* was not even translated into German until 1873 while *Origin* had been translated, reviewed, and lectured on in both Germany and Austria within a year of its release.[29]

From the perspective of the botanist Richard von Wettstein, who delivered his own lecture for the "Darwinism" series several weeks later, Kassowitz was not entirely justified in his evaluation of the scientific merits (or lack thereof) of natural selection but correct in his claim that its unwarranted dominance in the marketplace of ideas had more to do with the rhetorical skill of its popularizers than the relevant empirical facts. His departure from Kassowitz on the issue of natural selection's scientific merits reflected his pluralistic understanding of the causes of evolution. He was above all a committed neo-Lamarckian, insofar as he was convinced that the inheritance of acquired characteristics was the primary mechanism by which nature produced new species. And as he explained to his audience at the Philosophical Society, he had acquired this conviction the hard way, which is to say through years of fieldwork and painstaking observation of the ways in which alpine flora adapted themselves to novel environmental conditions and passed those adaptative modifications along to their offspring.[30]

But Wettstein also acknowledged that natural selection, mutation, hybridization, and orthogenesis, or the theory that plants evolved according to an inner drive, all had minor roles to play in the evolutionary process. Once again, he emphasized that his views on the matter were not settled at his desk or in a museum cabinet but in the field, where he had undertaken extensive

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[29] For more on Darwin's immediate reception in Germany, see: Alfred Kelly, *The Descent of Darwin: the Popularization of Darwinism in Germany, 1860-1914* (North Carolina: University of North Carolina Press, 1981), and Sander Gliboff, *H.G. Bronn, Ernst Haeckel, and the Origins of German Darwinism: A Study in Translation and Transformation* (Boston: MIT University Press, 2008).

[30] Richard von Wettstein, "Die Stellung der modernen Botanik zum Darwinismus," in *Die Krisis des Darwinismus*, 26.

investigations of several of these alternative mechanisms of speciation.[31] Although he did not describe these investigations in any detail in his lecture, his acceptance of the limited validity of natural selection stemmed from his research on a phenomenon called "seasonal dimorphism," or the appearance of early-blooming and late-blooming varieties of the same species of alpine flower, in the late 1890's.[32] In essence, his fieldwork had revealed that human interference in the form of the seasonal mowing of alpine meadows was actively selecting for varieties of *Gentiana* and *Euphrasia* that bloomed at irregular intervals, enabling them to reproduce in contexts where non-dimorphic varieties could not. Granted, the mechanism at work was more akin to artificial selection than natural selection, but he nevertheless concluded that it was an active contributor to the production of what could eventually be entirely new sub-species.

After laying out his reasons for adopting an ecumenical, albeit heavily Lamarckian perspective on the mechanisms of evolution, Wettstein then sought to explain why so few of his colleagues had followed his lead. The first part of his explanation focused on the fracture of the Germanophone botanical community into several mutually opposing and seemingly irreconcilable theoretical camps. This fracture happened far later in botany than in zoology because botanists—unlike their zoologist colleagues—had almost immediately and universally accepted Darwin's arguments when they first appeared in German in 1860, and were therefore spared much of the commotion surrounding natural selection in the late 1860's and 1870's.[33] He did not mention why a consensus had formed so quickly, but by 1901 it was common knowledge that the plant physiologists Wilhelm Hofmeister and Franz Unger had already established the basic facts of

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[31] Ibid.
[32] Richard von Wettstein, *Descendenztheoretische Untersuchungen. I. Untersuchungen über den Saison-Dimorphismus in Pflanzenreich* (Vienna: K.K. Hof- und Staatsdrückerei, 1900).
[33] Wettstein, "Die Stellung der modernen Botanik zum Darwinismus," 21. There were of course exceptions like Albert Wigand, but Wettstein claimed that his anti-evolutionary views found very little traction in the discipline.

organic evolution and common descent in the plant kingdom in the mid-1850's, which made it relatively easy for other Germanophone researchers to synthesize *Origin*'s insights with their own.[34]

By the late 1870's, however, cracks and fissures began to appear in the botanical consensus as practitioners, including Wettstein's mentor Anton Kerner, started to challenge certain aspects of the theory of natural selection. These fissures became truly palpable, according to Wettstein, with the release of the Swiss botanist Carl Nägeli's *Mechanisch-physiologische Theorie der Abstammungslehre* in 1884. Alongside Hofmeister and Unger, Nägeli was a key figure in the transformation of botany from a "dilettantish game," as one of his colleagues put it, into a full-fledged science in the 1840's and 1850's.[35] Like other many other pioneering researchers of the period, his investigations focused on bringing new microscopic techniques and technologies to bear issues in plant anatomy and physiology, but he also focused on evolutionary questions concerning the phylogenetic relationship different divisions of the plant kingdom. It was in the process of trying to answer the latter set of problems that he gradually accrued the critiques and objections to natural selection that he would finally bring together in his 1884 text, including an argument in favor of the inheritance of acquired characteristics through a mechanism he called "direct effect."

Within a decade of *Abstammungslehre*'s publication, Wettstein claimed that the already tenuous theoretical unity of the 1880's was shattered. Whereas most pre-Nägeli botanists were "ruled by a conviction in the almighty power of the selection principle (*Selektionsprinzip*)," in other words, after 1884 the botanical community's theoretical allegiances not only splintered along

[34] Julius Sachs, *Geschichte der Botanik von 16 Jahrhundert bis 1860* (Munich: Oldenbourg, 1875).
[35] Ibid, 203.

Lamarckian and Darwinian lines but into numerous other warring sub-factions.[36] By 1900, there were no fewer than five groups in open conflict with one another. Botanists who followed neo-Darwinians like Weismann and Kerner—who had since fully renounced his previous interest in the inheritance of acquired characteristics—fought with neo-Lamarckians like Wettstein as well as an older generation of Darwinians who still insisted on a limited role for environmental factors in producing heritable variations. Representatives of the latter positions also faced heated opposition from botanists who had adopted Nägeli's theory that organisms possessed an "inner perfecting principle" that was responsible for their progressive complexity, and from a nascent group of "mutationists" who claimed that evolution happened via sudden saltations.[37]

Wettstein concluded his "Darwinism" lecture by arguing that the acrimony and confusion that characterized debate between these competing botanical cliques had been exacerbated by a popular literature that tended to abjure sober analysis of the relevant facts—all of which pointed to the validity of *multiple* mechanisms of speciation—in favor of speculative, selective, and often dogmatic arguments for the primacy of one theoretical point of view. Although he shared Kassowitz's distaste for Ernst Haeckel "propagandistic" popular works on Darwin, he directed most of his opprobrium towards other authors and texts, including the Russian botanist Sergei Korschinky.[38] While he claimed to admire the latter's work on mutations because it was based on solid empirical data, he reproached the Russian for using popular-scientific magazines like *Naturwissenschaftliche Wochenschrift* to circulate one-sided accounts of the new theory's advantages.[39] He had far harsher words for Anton Kerner, arguing that his *Das Pflanzenleben der*

[36] Richard von Wettstein, *Der Neo-Lamarckismus und Seine Beziehung zum Darwinismus* (Jena: Gustav Fischer, 1902), 9.
[37] Wettstein, "Die Stellung der modernen Botanik zum Darwinismus," 28.
[38] Ibid, 21.
[39] Ibid, 25-26.

Donauländer (1891), a hugely successful popular reference work that doubled as a vehicle for the author's unpublished thoughts on evolution, was representative of how popular expositions could actively harm the discipline's capacity to resolve its theoretical issues.

Wettstein had a long and unpleasant history with Kerner's *Pflanzenleben* that began long before he publicly excoriated it in 1901. When the first edition of the text came off the presses in 1891, he recalled that he was privately "horrified" by its empty Darwinian posturing but chose to keep his opinions to himself, perhaps out of respect for his former mentor or because he had not yet fully developed his own views on the matter.[40] When Kerner asked him for help in preparing the second edition for publication in 1896, however, he could no longer bite his tongue and aired his concerns directly. And while he managed to wrangle some minor concessions from the older botanist, the two were unable to find common ground on evolutionary theory before the revision project died with Kerner in 1898. When Wettstein revisited these events in his lecture to the Philosophical Society, he explained that his primary objection to the text was its provision of "theoretically seductive" but dogmatic, empirically anemic, and myopic arguments for hybridization and natural selection as the sole mechanisms of evolution.[41] His point was not that Kerner was entirely wrong about the importance of these mechanisms, which did in fact help produce new species, but that his popular representations of them eschewed objective analysis of all the available evidence in favor of a one-sided proselytizing that not only served to mislead the lay public but to hinder biologists' efforts to arrive at a legitimate consensus on evolutionary theory.[42]

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[40] Richard von Wettstein, "Selbstbiographie," *Mappe 4, Schachtel 236, NL Richard Wettstein, Archiv der Universität Wien*, 16.
[41] Wettstein, "Die Stellung der modernen Botanik zum Darwinismus," 25.
[42] Ibid.

In the years after his "Darwinism" lecture, Wettstein would continue to claim that the popular literature on evolution was exacerbating the crisis that Nägeli had unleashed in 1884 by muddying the discursive waters. In a review of the new evolutionary literature published in 1906, for example, he noted that the majority of recent work on the topic—nearly all of it popular or semi-popular in nature—was still far too dogmatic and devoid of the kind of observational and experimental evidence that was necessary to actually resolve scientific debate on the issue.[43] He now had little compunction about singling out Ernst Haeckel in particular, writing that he admired the German zoologist for his courage in standing up for Darwin in the face of fierce opposition in the 1860's but that his recent *Der Kampf um den Entwicklungsgedanken* (1905) was characteristic of the work of "an enthusiast" who had fallen "into the error he accused the clerics of: dogmatism."[44] In a 1908 biography of Kerner, he trained his sights on his mentor once again, praising the latter for his inestimable contributions to botany but critiquing late-career texts like *Pflanzenleben* for overstating the evolutionary importance of hybridization and natural selection and "tragically" omitting facts that would have led readers to embrace a Lamarckian point of view.[45] And in 1927, as the rise of Mendelian genetics seemed likely to assure the demise of Lamarckism once and for all, he sought to temper his Darwinian colleagues' excitement by reminding them that biologists' impressions about the unassailability of natural selection, both in 1870 and in the present day, were not only reflective of the facts but of the rhetorical skill of the theory's popularizers.[46]

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[43] Richard von Wettstein, "Neuer descendenztheoretischer Literatur," *Das Wissen für Alle* 6, Issue 21 (1906), 119-122, 134-138.
[44] Ibid.
[45] Richard von Wettstein, "Einleitende Worte der Errinerung an A. Kerner von Marilaun," in E.M Kronfeld, *Anton Kerner: Leben und Arbeit eines Deutschen Naturforschers* (Leipzig: Tauchnitz, 1908), XII-XVII.
[46] Richard von Wettstein, "Fünfundsiebzig Jahre Biologie," *Verhandlungen der Zoologisch-Botanischen Gesellschaft in Wien* 76 (1926), 22-24.

In short, by the time Wettstein delivered his VDNA lecture in 1912, he had been arguing that popular representations of evolutionary theory had thwarted biologists' ability to reach a scientific consensus and impeded acceptance of his "many mechanisms" theory for over a decade. Although the latter issue occupied most of his attention, he also occasionally addressed the role of popularization in the declining fortunes of his primary area of botanical expertise.[47] Specifically, he worried that the popular literature on ecology, or what many of his contemporaries called "biology in the narrower sense," was having a detrimental effect on the scientific and public reputation of systematics.[48]

One of the fundamental branches of botany and the province of many of its most esteemed practitioners, ranging from Aristotle to Linnaeus, the aims and methods of systematics had undergone several radical changes over the course of the nineteenth century. Although it remained a science of identification and classification, the advent of Darwinism altered the way that many systematists constructed their taxonomies by shifting their focus away from artificial taxonomic criteria like stamens and pistils and towards traits that seemed to offer insight into an organism's phylogeny. Botanists' embrace of phylogenetic markers as the key to finally establishing the "natural system" of classification that had eluded their predecessors also substantially altered the discipline's methods. Systematists outside Austria increasingly turned to the laboratory to study embryological development based on the belief that close analysis of a species' ontogeny would reveal its phylogeny, while Wettstein formulated a "geographic-morphological" approach that he felt was better suited to classifying the genetic relations obtaining amongst varieties, sub-species, species, and genera. But as the latter noted as late as 1914, these methodological changes were not

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[47] Richard von Wettstein, "Die gegenwärtigen Aufgaben der Systematischen Botanik", *Neue Freie Presse* (April 6, 1905), 20-21.
[48] Wettstein, "Die Biologie in ihrer Bedeutung für die Kultur der Gegenwart," 217-225.

universal, and many systematists continued to pursue the same kind of descriptive and "purely morphological systematics" that botanists had practiced a century earlier.[49]

One of the most unfortunate results of systematists' continued use of descriptive and "purely morphological" methods, according to Wettstein, was the field's gradual decline in prestige in the 1870's. Although this decline had much to do with botanists flocking to the new laboratory-based field of plant physiology, which purported to move beyond mere description by offering causal or "mechanical" explanations of biological phenomena, he noted that it was also a somewhat unintended result of internal critique. That is, many of the field's most vocal critics in the 1870's and 1880's were themselves systematists who were seeking to differentiate the new embryological, or "stain-and-slice" approach to classification from the older, descriptive-morphological systematics by "artificially exaggerating" the latter's obsolescence.[50] But the end result of these criticisms was the devaluation of systematics as a whole; its progressive absence from botanical curricula; and its condemnation by bench scientists who claimed that the only knowledge about plants worth having was produced in laboratories.[51] Wettstein included Wiesner among the ranks of systematics' naysayers, recalling an incident early in his career where the physiologist had told him that "it was all well and good if one wants to do systematics, but it is more important to pursue *scientific* botany."[52] He eventually found this sneering attitude towards

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[49] Richard von Wettstein, "Das System der Pflanzen," in *Abstammungslehre, Systematik, Paleontologie, Biogeographie* ed. R. Hertwig and R. v. Wettstein (Leipzig: B.G. Teubner, 1914), 165-175.
[50] Richard von Wettstein, "Die Entwicklung der Morphologie, Entwicklungsgeschichte, und Systematik der Phanerogame in Österreich von 1850 bis 1900," in *Botanik und Zoologie in Österreich in den Jahren 1850 bis 1900* ed. Anton Handlirsch, Richard Wettstein, and Karl Wilhelm Dalla Torre (Vienna: Alfred Hölder, 1901), 199-201. I have borrowed the fortuitous phrase "stain-and-slice" from Lynn Nyhart, who uses it in her description of the "revolt from morphology" that took place in German zoology in the 1890's. See: Lynn Nyhart, *Biology Takes Form: Animal Morphology and the German universities, 1900-1900* (Chicago: The University of Chicago Press, 1995), 203-204.
[51] Ibid.
[52] Richard von Wettstein, "Selbstbiographie," *Mappe 4, Schachtel 236, NL Richard Wettstein, Archiv der Universität Wien,* 10.

systematics so intolerable that he decided to prematurely end his sojourn at Wiesner's Institute for Plant Physiology in 1884, bringing about a rift in their personal relationship that they would never fully repair.[53]

The popular-ecological literature was therefore not solely responsible for the general decline of systematics, which had begun long before plant ecology was even a recognizable branch of biology, but Wettstein nevertheless saw it as an important contributor to the field's continued troubles around 1900. Indeed, because the plant ecologists drew many of their aims and methods from the plant physiologists, they often attacked systematics based on the same kinds of considerations that animated the latter.[54] The ecologists and physiologists both sought to offer causal, laboratory-based explanations of the relationship between a plant's "mode of life" and its surrounding biotic and abiotic environments, for example, which led them to look down on the "merely descriptive" work of classifying and taxonomizing.[55] Moreover, the ecologists and physiologists both engaged in a relatively ahistorical form of analysis that ascribed little importance to the historical and phylogenetic considerations that were central to how Wettstein and many of his fellow systematists understood and explained biological traits.

This is not to say that fin de siècle plant ecologists saw no value in the systematists' work or vice versa. Wettstein had in fact produced numerous articles and lectures on ecological topics over the years and used its concepts and perspectives to great effect in his analyses of alpine

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[53] Erwin Janchen, "Richard Wettstein: Sein Leben und Wirken," *Österreichischen Botanischen Zeitschrift*, Vol. 82, No. 1/2 (1933), 15.
[54] Julius Wiesner, "Einleitung," in *Elemente der Wissenschaftlichen Botanik: Biologie der Pflanzen*, Vol. 3, 2nd ed. (Vienna: A. Hölder, 1902), 1-2. According to Wiesner, one could not draw a "natural border" indicating where physiology ended, and ecology began.
[55] "Oekologie," in *Illustriertes Handwörterbuch der Botanik* ed. Camillo Schneider and Otto Porsch (Leipzig: Wilhelm Engelmann, 1905), 429-430.

biomes.[56] But he nevertheless maintained that the field's popularizers often misrepresented the nature of organic characters by explaining them in terms of an organism's place in a biological community rather than as functions of its historical development and position in a phylogenetic tree. He first raised these concerns to a small group of colleagues in 1906, explaining that it should be clear to any good biologist that "the structure of every organism is an expression of its evolution and of the adaptations which it has acquired," but that the "ecological method" which was then being articulated in a variety of popular and pedagogical texts "only considers one side: the adaptive."[57] In the process of emphasizing the adaptive side of things to the exclusion of all others, they cut out the invaluable perspective of the systematist, who could clarify whether a given trait was in fact an adaptation to some immediate environmental stimulus or part of the nexus of stable and long-since acquired traits that characterized an individual as part of some higher family, phyla, or domain.

Wettstein brought up the issue of popular ecology again in his 1912 lecture to the VDNA, this time focusing less on the way that this literature unjustly ignored phylogeny than on its superficiality and role in turning students and younger biologists away from systematics. His lecture began with a call to recognize that efforts to construct some sort of opposition between the experimental and descriptive branches of biology were misguided, insofar as every advance in "experimental biological research presupposes" and builds off of "a descriptive and observational" base.[58] Scientists would not be enjoying the illuminating results then being produced in laboratory-

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[56] Richard von Wettstein, "Die Biologie unserer Wiesenpflanzen," *Schriften des Vereins zur Verbreitung Naturwissenschaftlichen Kenntnisse* No. 44 (1904), 357-377. Wettstein communicated a great number of his findings and thoughts on ecological topics through this *Verein*.
[57] Richard von Wettstein, *Der naturwissenschaftliche Unterricht an den österreichischen Mittelschulen* ed. J. Brunnthaler, K. Fritsch, H. Lanner, P. Pfurtscheller, E. Witlaczil, and Richard Wettstein, (Vienna: Verlag F. Tempsky, 1908), 51.
[58] Wettstein, "Die Biologie in ihrer Bedeutung für die Kultur der Gegenwart," 223.

based fields like developmental mechanics, for example, were it not for the hard work of systematists and others in establishing a bedrock of basic empirical facts. He then addressed the topic of ecology, declaring that it was not his intention to deny the importance of the field in general but to shed light on the many problems that were inherent in its rapidly proliferating popular literature. The principal issue, he wrote, was that:

> when we follow the lectures of some of our popular ecologists, we often hear a plethora of ecological explanations, many of which appear very stimulating and educational, but when we ask on what investigative or observational ground these explanations rest, we receive no answer.[59]

One immediate consequence of the ecologists' inability or unwillingness to examine the empirical grounds of their explanations was the circulation of "incorrect and unproven" claims. An even graver consequence of this omission was that it gave the "impression that biology is a field in which interpretation plays a greater role than observation," and thereby convinced younger researchers that they could make valuable contributions to the discipline by speculating about ecological relations rather than doing painstaking fieldwork.[60] Put more succinctly, he feared that the popular ecologists' stimulating but superficial representations would not only reorient the field around theorizing but continue to hasten the demise of systematics by diminishing students' belief in the importance of its goals and methods.

Several of Wettstein's followers raised similar concerns in their own critical analyses of the popular and pedagogical literature on ecology. The naturalist K.C. Rothe, who worked closely with Wettstein at the University of Vienna's Botanical Institute and as part of the adult education movement that took root in Austria in the 1890's, complained that "since the rise of *Biologie*,"

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[59] Ibid.
[60] Ibid.

which he understood to be roughly synonymous with ecology:

> many teachers, individual popularizers, and a few experts have become accustomed to scorn the old methods, which were...intended to spread knowledge of names, forms, organs and their functions, the place of an organism in a system… as dull and incapable of having an edifying effect.[61]

Wettstein's student August Ginzberger made a similar assessment of the popular ecologists in 1908, writing that the latter had created "a low perception" of morphology and systematics among students and teachers, and therefore harmed the long-term prospects of these fields by dissuading people from doing them professionally. Like his mentor, he also fastened onto the idea that the ecologists' critiques of systematics were implicitly attacks on the practices of collection, observation, and comparison in general, and that the form of botany they envisioned would dispense with "systematic facts" altogether.[62]

Although Rothe and Ginzberger often spoke of "popular ecologists" in the abstract or of "the ecological direction" in recent popular literature in lieu of naming specific offenders, they made an exception for the German biologist Otto Schmeil. Their reason for singling Schmeil out stemmed from his decision to use his popular work to elucidate the phenomena of protective coloration and mimicry, or more accurately, to explain these traits as adaptive responses that granted an organism a greater chance of survival in whatever ecological niche it found itself. As systematists who studied the same phenomena from a historical and phylogenetic perspective, they felt that it was irresponsible to claim that an organism's traits would always reflect its present

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[61] K.C. Rothe, "Zur Reform des Naturgeschichtsunterricht," *Pädagogisches Jahrbuch* 29 ed. Theodor Steiskal (Vienna: Manzsche K.u.K. Hof-Verlags- und Univ.-Buchhandlung, 1907), 86.
[62] August Ginzberger, "Aus der Debatte," *Pädagogisches Jahrbuch* 29 ed. Theodor Steiskal (Vienna: Manzsche K.u.K. Hof-Verlags- und Univ.-Buchhandlung, 1907), 104-105; August Ginzberger, "Die Teilwissenschaft der Botanik und Zoologie," *Der Moderne Naturgeschichtsunterricht* ed. K.C. Rothe, (Vienna: Tempsky, 1908), 85-93.

circumstances.[63] Put in Rothe's words, it was dangerous to expose students and other non-specialists to Schmeil's work on topics like camouflage because it was clear that "variations of this type are not determined by ecological factors" at all, but rather by "evolutionary-historical conditions."[64] Ginzberger agreed, writing that it made little sense to introduce people to ecology by suggesting that biological traits represented adaptive responses to present-day states of affairs, as Schmeil did, when it was abundantly clear that an organism's evolutionary background and history were almost assuredly more determinative of biological form and function.[65]

In short, while many Austrian botanists were in agreement as to the "worth of the ecological method" in general, and even approved of the use of ecological heuristics like the notion of the "biological community" (*Lebensgemeinschaft*) in curricula, they were also concerned that the misleading representations of the field's popular advocates would lead to what Wettstein described as "the direct teaching of falsehoods, the cultivation of natural-scientific dilettantism, and the belief that intellectual constructions can replace observation."[66] Although much of this conversation revolved around the baleful effects of popular ecology on students, teachers, and researchers, Wettstein et. al. also worried about its effects on amateur botanists, whom they relied on for fieldwork, the collection and preparation of specimens, and other forms of botanical labor that were integral to the construction of accurate taxonomies but did not require a formal scientific education.[67]

According to Rothe, one of the systematists' most pressing problems was that the popular-

[63] Karl Grobben, "Die biologische Richtung im zoologischen und botanischen Unterricht," *Der naturwissenschaftliche Unterricht an den österreichischen Mittelschulen*, 36-42.
[64] Rothe, "Zur Reform des Naturgeschichtsunterricht," 90.
[65] Ginzberger, "Aus der Debatte," 85-93.
[66] Richard von Wettstein, *Der naturwissenschaftliche Unterricht an den Österreichischen Mittelschulen*, 50-51.
[67] Wettstein, "Das System der Pflanzen," 165-175.

ecological literature had dissuaded teachers at provincial schools from engaging in the kind of region-specific collecting and reporting that they could previously be relied on to do, and which had traditionally been a major source of "worthwhile knowledge of forms" and "facts of biological value."[68] That is, he argued that contemporary systematists could "no longer use friends of the natural sciences as volunteer workers in the same degree" as they had in the past because the volunteers had lost interest. And while he acknowledged that this lack of interest was partly connected to the increasing dominance of laboratory-based biology, he also blamed the popular ecologists' "highly superfluous fight against systematics" and their constant denigration of the activities of prior generations of amateur collectors.[69] The growing estrangement of amateurs from professional biologists was particularly devastating to Rothe because he believed that the former were not only more at home in the field than many specialists, whose knowledge of nature was increasingly derived from "books, cross-sections, preparations, and test tubes," but because they brought a different—i.e. untrained and "unalienated"—eye to the natural world.[70]

Securing Direct Adaptation and Systematics

For all their worries about the ramifications of popular discourse on biological knowledge and inquiry, Wettstein and his followers were not above using their own popular and pedagogical texts as a means of garnering scientific support for their theoretical views and buttressing their research programs. Wettstein's perspective on the scientific value of popular representation was not only forged in his battles with the neo-Darwinians but, as with so many other aspects of his worldview, by his relationship with Kerner. As he remarked in 1908, his mentor's inability to

[68] K.C. Rothe, "Der Lehrer auf dem Lande als Naturhistoriker," *Der Moderne Naturgeschichtsunterricht*, 217-228.
[69] Ibid.
[70] Ibid; K.C. Rothe, "Allgemeine Einleitung," in *Handbuch für Naturfreunde* Vol. 1 (Stuttgart: Kosmos, 1911), IX-X.

secure a scientific audience for his pathbreaking biogeographical work in the 1870's had somewhat inadvertently taught him that he could not rely on technical representation to carry his arguments alone, and that by clarifying his research to "wider circles" he could also influence his colleagues.[71] He put his belief in popularization's capacity to tip the scales of a scientific argument to the test throughout the 1900's in his attempts to sway specialist debate on an important technical issue amongst the neo-Lamarckians, namely: the status of adaptive and organizational traits.

Like the conflict over mechanisms of speciation, Wettstein felt that this dispute over traits was rooted in the work of Kerner and Nägeli; the former because he was among the first botanists to become heavily invested in understanding the nature and systematic meaning of different biological characters, and the latter because he first introduced the relevant distinction in his *Abstammungslehre*. Specifically, Nägeli argued that organisms possessed two distinct classes of trait: organizational and adaptive. He claimed that the former were more fundamental and of greater taxonomic importance because they were hereditarily stable and characteristic of higher biological orders like kingdoms and domains (e.g. an organizational trait of cryptogams was their capacity to reproduce by spores). In sharp contrast to the Darwinians, he tied the gradual, progressive development of organizational traits to an "inner perfecting principle" (*Vervollkommnungsprinzip*) rather than natural selection or the inheritance of acquired characteristics, meaning that their form and function was completely divorced from any environmental influence. Adaptive traits, by contrast, possessed none of the cellular insularity or long-term stability of their counterparts, and emerged and changed rapidly as individual organisms attempted to accommodate themselves to changing environmental conditions through a process he

[71] Wettstein, *Kerner*, XIII.

called "direct effect."[72] As transitory responses to outer influences, they therefore had no effect on the evolutionary trajectory of a given species.

Wettstein first began to investigate the relationship between adaptive and organizational traits himself in the late 1880's after his research on the systematics of the lower orders led him to inquire into the processes that led to the emergence of new species. In the mid-1890's, he started releasing a steady stream of both technical and popular texts that were intended to articulate his early results. On the one hand, his research had yielded further empirical support for Nägeli's claim that adaptive traits emerged from a process of "direct effect"—or what he had taken to calling "direct adaptation." On the other hand, it had shown that adaptive modifications could also gradually become organizational traits, which contradicted Nägeli's claim about the latter's insularity from environmental influence and evolutionary irrelevance.[73] Or as Wettstein declared in a 1902 lecture that encapsulated his findings:

> in direct adaptation we have come to know a process which, according to the demands of life, leads to a gradual transformation of organisms…Experience now strengthens the thought that continuous direct adaptation effects progressively higher levels of organization over immeasurable amounts of time, insofar as a good number of what we now consider organizational traits can be traced back to adaptations.[74]

This position not only sufficed to differentiate his views from Nägeli's but brought him into direct conflict with the other lodestar of Austrian botany at the time, Julius Wiesner.

A generation older than Wettstein, Wiesner played a critical role in establishing Austria as one of the world's most important centers of plant physiology. He was also an active teacher and mentor, and by the early 1900's he had managed to fill many of the Habsburg Empire's

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[72] Nägeli, *Abstammungslehre*, 102-183.
[73] Richard von Wettstein, "Die Gegenwärtige Stand unser kenntnisse betreffend die Neubildung von formen im Pflanzenreiche," *Berichte der Deutsche Botanischen Gesellschaft: General-Versammlungsheft* 18 (Berlin: Gebrüder Bornträger, 1901), 184-220.
[74] Wettstein, *Der Neo-Lamarckismus und Seine Beziehung zum Darwinismus*, 24-25.

professorships in botany with his own students.[75] The register of Wiesner-trained botanical chairs nominally included Wettstein, who did his dissertation under Wiesner but gravitated far more towards Kerner, and eventually took an assistantship with the latter after receiving his doctorate in 1884 rather than continuing on at the Physiological Institute for the reasons mentioned above. The strain created by his departure was no doubt made worse by his later insistence on the possibility that organizational traits were ultimately rooted in adaptive traits, which flew in the face of Wiesner's own orthogenetic views on evolution.

That Wiesner would have a strong opinion on evolutionary theory in the first place was somewhat out of character, given the disdain he sometimes heaped on biological questions that could not be explained through physiological mechanisms. But he also admitted that he had been interested in "vitalistic problems" throughout his career, including questions related to "ways of life, heredity, mutability, adaptation, and the natural distribution of organisms," and like Wettstein and many others, he often used his popular and pedagogical texts to articulate his perspective on these issues.[76] Of particular importance in this regard was his *Elements of Scientific Botany: The Biology of Plants* (1889), which not only contained one of the earliest programmatic expressions of the questions, methods, objects, and goals of plant ecology—predating Kerner's discussion of these topics in *Pflanzenleben* by two years and A.F.W. Schimper's *Pflanzengeographie* by nine—but outlined his basic beliefs about the nature of organic evolution. Although he recognized that each of the many different available theories had its own advantages, he ultimately threw his hat in with Nägeli, writing that it was "more probable" that the progressive evolution of organic life

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[75] Hans Molisch, "Vorwort," in *Wiesner und Seine Schule: Ein Beitrag zur Geschichte der Botanik* ed. Karl Linsbauer, Ludwig Linsbauer, and Leo von Portheim (Vienna: Hölder, 1903), VII.
[76] Julius Wiesner, "Einleitung," *Elemente der Wissenschaftlichen Botanik: Biologie der Pflanzen* Vol. 3 (Vienna: Hölder, 1889), 1-2.

was tied to a "principle of perfection" that "inheres in the organism" than to "accidental modifications and to outer conditions and influences."[77]

Wiesner would stick by his initial position on the high probability of the orthogenetic principle in the many subsequent editions of *Elements*. He also left much of his original analysis of the other evolutionary theories of the period unchanged, aside from one notable footnote in the 1902 edition which claimed that Wettstein's recent work indicated that he had

> very well taken a step back to Darwin in assuming a path between organizational and adaptive traits. According to his view the succulence of the leaves of the (family) *Crassulaceae* arose through adaptation and have become organizations traits. But from Nägeli's position one can well deny that a trait which arose through adaptation could become an organizational trait, which is to say, that such a trait could have ascended to the higher organizational level of *Crassulaceae*.[78]

The "step back to Darwin" remark was curious, given Wettstein's repeated critiques of natural selection, but it also pointed to a very real break among botanists who acknowledged the viability of Lamarckism but differed with respect to what the theory entailed and what it did not. For Nägeli and Wiesner, the inheritance of acquired characteristics accounted for the origin of more transitory traits but was not sufficient to explain progressive evolution. They also held that any attempt to explain increasing biological complexity without reference to processes "active within the organism" risked backsliding into a Darwinian world of chance modifications and accidental variations. Conversely, for Wettstein the essence of Lamarckism lay in explaining common descent and the transmutation of species in terms of a continual dialectic between organism and environment and refusing to cede that there were aspects of the evolutionary process that this dialectic did not touch.[79]

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77 Ibid, 227.
78 Julius Wiesner, "Einleitung," *Elemente der Wissenschaftlichen Botanik: Biologie der Pflanzen*, 2nd ed, 276.
79 Ibid, 258.

Over the next decade, Wettstein would continue to press his arguments for and against the possibility of adaptive traits becoming organizational traits in his popular and pedagogical works. He directed much of his attention to further refining what he meant by "direct adaptation" and continually resituating his ideas within the complicated and ever-shifting set of allegiances, camps, and movements that comprised early twentieth century evolutionary discourse. He was especially driven to reach some sort of accommodation between his views on the relationship between direct adaptation and organizational traits and the rapidly growing literature on mutation and Mendelian genetics, which suggested that changes at the organizational level were connected to random, multi-trait modifications to hereditary material that was sequestered within the germplasm.[80] As he quite rightly perceived, if the theory of genetic mutation were true the inheritance of acquired characteristics was most likely false. Wiesner, who had retired from his position at the Institute for Plant Physiology in 1903, pursued much the same path as his friend and fellow professor emeritus Ernst Mach by spending his twilight years writing philosophical feuilletons for popular magazines like the *Österreichische Rundschau*. Rather than continuing to pursue his feud with Wettstein, he used these works to further align himself with the vitalist movement that was then ascendant in the work of fellow experimentalists like Johannes Reinke and Hans Driesch.[81] He did, however, find time to make one last reference to his apostate student, writing in the last edition of his *Biology of Plants* (1913) that Wettstein had not so much "taken a step back to Darwin" in his work as recapitulated, "albeit in modified form, the older views" of Wallace, Darwin's colleague and the co-founder of the theory of natural selection.[82]

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[80] Wettstein, "Der Gegenwärtige Stand der Descendenzlehre," 563-564.
[81] Julius Wiesner, "Der Licht- und Schattenseite des Darwinismus," in *Natur-Geist-Technik: Ausgewählte Reden, Vorträge und Essays* (Leipzig: Verlag Wilhelm Engelmann, 1910), 358-385.
[82] Julius Wiesner, *Elemente der Wissenschaftlichen Botanik: Biologie der Pflanzen* Vol. 3 (Vienna: Hölder, 1913), 307.

In addition to using his popular and pedagogical works to help adjudicate the nature and status of biological traits, Wettstein used them to help garner support for his research projects in biogeography and systematics. Given his emphasis on the harm that popular ecology had wrought on these fields in classrooms across the country, it is unsurprising that one of his primary points of emphasis was reforming natural science instruction at the middle-school level. His most significant step towards achieving this goal came in 1908, when he put together a commission of prominent Austrian biologists to provide recommendations for an "enquete" that the Ministry of Education was going to hold on middle school education later that year. Although he modelled the commission's work on the so-called "Hamburg theses," a set of reforms that a group of German scientists had articulated at the 1901 VDNA calling for the reorientation of natural history instruction in the German lower and middle schools, he also made a point of noting that the Austrian group was going to work "in conscious opposition to the excesses" which the theses had called forth.[83]

The specific "excess" that Wettstein opposed was the aggressive centering of ecological concepts and theories in biological instruction.[84] This is not to say that he wanted to avoid teaching ecology altogether, but that he wanted the commission to create an "*Ausgleich* between descriptive and ecological methods" in its model curriculum.[85] As an example of what this *Ausgleich* might look like, he suggested that students would be trained to view biological phenomena from ecological, morphological, and systematic points of view. In practice, this meant that students

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[83] Wettstein, *Der naturwissenschaftliche Unterricht an den Österreichischen Mittelschulen*, 5.
[84] Friedrich Ahlborn, "Die gegenwärtige Lage des biologischen Unterrichts in den höhern Schulen," 278-279. The so-called "Hamburg Theses" put forth by the GDNA commission called for natural history instruction to focus on teaching "the relations of organisms to inorganic nature, to one another, and to man," among other things.
[85] Ibid, 51.

would not only be taught to understand adaptations as accommodations to contemporary living communities, as much of the contemporary ecological literature emphasized, but as historical phenomena with roots in the evolutionary history of the organism. The commission would eventually translate this suggestion into a formal "thesis" stating that

> the ecological method shall not supplant the descriptive analysis of morphological and systematic relations. Morphological and systematic knowledge of the most important organs of animals and plants represents an indispensable foundation of the study of zoology and botany. The student can best learn the practice of observation through the morphological description of natural objects, particularly when this description is connected with drawing.[86]

From the German perspective, the Austrian commission's recommendations likely seemed somewhat backward looking, but for Wettstein and his colleagues' that was a small price to pay to raise the quality of biological instruction in the Dual Monarchy and assure that students were at least familiar with systematics.

Rothe and Ginzberger supplemented Wettstein's efforts to revamp the Monarchy's pedagogical apparatus by taking part in their own reform project that same year, although their project not only concerned middle-school students and teachers but the thousands of adults who participated in Austria's exceptionally well-developed adult education network, or *Volksbildungsbewegung*. Like similar projects across Europe and North America, this network represented a continuation of earlier Enlightenment projects aimed at dispelling superstition from the body politic as well as an attempt to meet the educational needs of an industrializing society.[87] It also owed its origins to more local concerns, including the Austrian academic community's desire to create renumerated positions for the many unemployed or underemployed scientists it

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[86] Ibid, 41-42.
[87] Emil Reich, "Bericht des Vereins 'Volksheim," *Zentralblatt für Volksbildungswesen 11. Jhrg*. (15. Januar 1911 No. 1 / 2), 16.

had (over-)produced through years and drum up public support for academic research at a time when democratization increasingly tied research budgets to the ballot box.[88] Put in the physicist Anton Lampa's words, he and his colleagues embraced adult education because they recognized that it was

> not only a matter of the greatest cultural significance, but also of utmost and direct importance for the universities themselves. It means anchoring the universities and the principles of free research in broader social classes, contributing in outstanding ways to strengthening the universities' status and supplying unexpected allies in a potential fight for vital resources.[89]

The Viennese philosophy professor Friedrich Jodl offered a similar, albeit more humorous variation of this argument in 1907, writing that his interest in the *Volksbildungsbewegung* was a natural result of his belief that "an age of universal suffrage must also be an age of general education or else it will be an age of general misery."[90]

The confluence of scientists' overwhelming desire to educate the lay public and the lay public's overwhelming demand for scientific education helped the Austrian adult education network grow rapidly throughout the 1890's. By 1905, it had become something of a working-class "shadow university" that provided a wide range of free or low-cost educational services, ranging from individual popular lectures to multi-year courses of study; as well as access to scientific collections, laboratories, libraries, and reading rooms; to hundreds of thousands of people across Vienna and lower Austria.[91] As avid participants in this "shadow university" system,

[88] *Bericht über die Volksthümlichen Universitätsvorträge im Studienjahre 1895/1896* (Wien: Adolf Holzhausen, K. und K. Hof- und Universitäts-Buchdrucker, 1896), 1-4.
[89] Anton Lampa, "Über die von der Universität Wien veranstalteten auswärtigen Kurse," *Zentralblatt für Volksbildungswesen 8 jhrg*. (10 Juli 1908, No 5 / 6), 71.
[90] Friedrich Jodl, "Festrede zur Feier des 20 Jährigen Bestehens des Wiener Volksbildungs-Vereines," *Wiener Volksbildung-Verein. Bericht über die Vereins-Thatigkeit im Jahre 1906* (Verlag des Wiener Volksbildungs-Vereines, 1907), 10-11.
[91] Christian Stifter, "Knowledge, Authority and Power: The impact of university extension on popular education in Vienna 1890-1910," in *Masters, Missionaries and Militants. Studies of Social Movements and Popular Adult Education, 1890-1939* ed. Barry Hake, Tom Steele, Alejandro Tiana (Leeds: University of Leeds, 1996), 159-190.

Ginzberger and Rothe recognized its massive potential as tool for training gymnasia students, provincial educators, and other key mediators of biological knowledge to eschew ecological speculation and theorizing in favor of careful observation and description.

To assure that their fellow *Volksbildner* shared the same vision, Ginzberger and Rothe put together *Die Moderne Naturgeschichtsunterricht*, an edited volume that was intended to serve as a reference point for the adult education network's biological teaching corps. One lesson that ran through the text was that it was necessary to pay systematics its proper due. In one of Rothe's essays, for example, he noted that it was pedagogical malpractice for biology instructors to privilege the ecological perspective when morphology and systematics were the true heart of the discipline.[92] Like Wettstein, he insisted that he was not opposed to the inclusion of ecology tout court, but that he thought that instructors needed to supplement their presentation of ecological concepts and ideas with a careful consideration of morphological, phylogenetic, and taxonomic facts. Ginzberger mirrored Rothe's point in his own contribution to the volume, which urged instructors to introduce systematics as one of biology's foundational fields and as the cornerstone of any well-rounded education in the discipline.[93]

Another key objective that Wettstein, Rothe, and Ginzberger set for their popular and pedagogical work in the 1900's was to recruit amateur observers and collectors back into their research projects. Although they had traditionally had little trouble finding amateurs who were eager to contribute to their endeavors, lay participation in specialist botany had declined in general since the 1890's. This decline was due in part to the influence of popular ecology, as the Austrian

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See also: Wilhelm Filla, *Wissenschaft für alle- ein Widerspruch: Bevölkerungsnaher Wissenstransfer in der Wiener Moderne. Ein historisches Volkshochschulmodell* (Wien: Verband Österreichisches Volkshochschule, 2001).
[92] K.C. Rothe, "Methodische Bemerkungen," in *Der Moderne Naturgeschichtsunterricht*, 93-95.
[93] August Ginzberger, "Die Teilwissenschaften der Botanik und Zoologie," 85-93.

systematists had been at pains to argue, but it was also tied to the professionalization and specialization of botanical inquiry, which had considerably raised the level of training required to participate in even the most basic scientific activities and made it difficult for non-specialists to contribute to the discipline in a non-trivial way. The heightened barrier to entry circa 1900 was nowhere more evident than in the rules for botanical classification and nomenclature, which had become so complicated that access to specialized collections and libraries was virtually a prerequisite for staking a claim to having discovered a new variety or species. Moreover, in the 1880's the editors and publishers of botanical journals began to take steps to make their publications more specialized and less accessible to lay readers. The history of the *Österreichische Botanische Zeitschrift* is instructive in this regard: while the journal's editors characterized it as "a generally useful organ for botanists, gardeners, economists, foresters, doctors, pharmacists, and technicians" throughout the 1860's and 1870's, by 1880 they had altered its tagline to state that it was simply "an organ for botanists." Wettstein, who took control of the journal in 1889, continued his predecessors' efforts to enhance its scientific reputation by gradually transforming its content and editorial policies in ways that privileged specialist discourse and excluded amateurs.[94]

It is somewhat ironic, then, that Wettstein also made numerous efforts to lure amateur botanists and interested laypersons into his research projects. In the 1890's and early 1900's, he focused on courting the members of the various mountaineering clubs he was a part of because many of the scientific questions he hoped to answer at the time concerned the classification, biogeographical distribution, and ecological relations of mountain flora. This was the high point of his research on the systematics of alpine species and genera, for example, and on the role of

[94] Janchen, "Richard Wettstein: Sein Leben und Wirken," 94.

geographic distribution in the production of sub-species of *Gentiana* and *Euphrasia,* all of which required reams of data on where and under what conditions these organisms could be found. He began making overtures to the alpinist community to provide him with this data in 1894. In one article for the magazine of the *Deutschen und Österreichischen Alpenverein,* he explained that "Alpenfreunde" like the club's members had traditionally provided valuable assistance to botanists in the form of "small floristic works," and that they could continue to aid in the noble cause of science by taking note of the climactic conditions surrounding the flora they encountered in the mountains.[95] He wove the *Alpenverein* further into his research in 1899 by convincing its members to provide a subvention for an "experimental garden" in the Rax mountain range that would provide him with the evidence that he needed to prove the role of direct adaptation in the production of new species.[96] Over the next fifteen years, the Rax garden and its attendants would not only help him achieve the latter goal but serve as an important pedagogical tool and source of specimens for other alpinists who wished to further their botanical education.[97]

Wettstein made a similar plea for help to the members of the *Verein zum Schutz und zur Pflege der Alpenpflanze* in 1901, writing that there were "a great variety of questions in plant geography" that Alpine enthusiasts could contribute to without "deep schooling or intense labor."[98] Most importantly, they could make and communicate observations of various kinds. He was once again particularly interested in using the alpinists to gather data about the relationship between

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[95] Richard von Wettstein, "Botanik," *Zeitschrift des Deutschen und Österreichischen Alpenverein: Festschrift zur Feier des Funfundzwanzigjährigen Bestehens des Deutschen und Österreichischen Alpenvereines* ed. Johannes Emmer (Berlin: Verlag des Deutschen und Österreichischen Alpenvereins in Berlin, 1894), 49-56.
[96] Richard von Wettstein, "Die wissenschaftliche Aufgaben alpiner Versuchsgärten," *Zeitschrift Deutschen und Österreichischen Alpenverein,* No. 31 (1900), 8-14.
[97] Richard von Wettstein, "Bericht über den Alpenpflanzengarten auf der Raxalpe für das Jahr 1906," *Bericht des Vereines zum Schutze und zur Pflege der Alpenpflanzen* 6 (1907), 26-30.
[98] Richard von Wettsein, "Botanischer Abende der Wiener Botaniker," *Österreichische Botanische Zeitschrift,* Vol. 51, (June 1901), 220-221.

climactic factors, biogeographical distribution, and concrete barriers to distribution as manifest in tree lines and krummholz formations. To further incentivize the members of the *Verein* to contribute to this project, he worked with the Austrian Zoological-Botanical Society to provide notepads that would enable even the most untrained observer to collect and transmit data and promised that the names of successful contributors would be mentioned in whatever publications emerged from the project.[99]

Rothe also believed that amateurs still had much to contribute to academic botany, although his efforts to enlist them in botanical research were often less focused on specific projects than on familiarizing them with broad domains of inquiry to which they might make contributions in the future. He had first drawn attention to the fact that declining lay participation in biology had much to do with a general ignorance of open research questions in his 1908 text on natural history instruction, writing that amateurs could accomplish "innumerable rewarding biological tasks" if only they knew what these tasks were and how to pursue them.[100] One of the primary aims of his *Handbuch für Naturfreunde*, an edited volume that he helped write and produce three years later, was to provide precisely that information.

Written in conjunction with several prominent representatives of various branches of biology, including Wettstein's former student Otto Porsch and the physiologist Rudolf Karzel, the two-volume *Hanbuch* assumed from the very beginning that the alienation of amateurs and specialists was an unnecessary and undesirable artifact of the contemporary division of scientific labor. As Rothe wrote in his introduction, there were still many things that amateurs could do to become part of "the great union of scientists" and much that existing members of this union could

[99] Ibid.
[100] K.C Rothe, "Die Lehrer auf dem Lande als Naturhistoriker," 217-222.

do to make sure they felt "welcome as colleagues."[101] The different chapters of the text sought to explain exactly what these things were and provide some of the preparatory knowledge that was necessary to do them. Unsurprisingly, Karzel's chapter on "Pflanzekunde" offered advice on how lay readers could contribute to systematics, which he described as having a steep learning curve but also as providing many opportunities for non-specialist participation. He then noted that amateur contributions would be particularly welcome in the sub-field of floristics, which entailed describing local and regional floral "neighborhoods," and in the study of cryptogams, which was greatly underdeveloped in comparison to the study of phanerogams.[102] Like Wettstein, he sought to provide his readers with the material support they needed to start these projects by directing them to the Zoological-Botanical Society, where they could find notepads, maps, and other tools necessary for carrying out fieldwork.

Karzel also attempted to direct the *Handbuch*'s readers towards ecological plant geography, writing that "no other area of botany could be so recommended to the *Pflanzenfreund*."[103] Although it was ostensibly strange that a Rothe-edited text would advocate for a form of popular ecology, the kind of work that Karzel had in mind was more empirical than theoretical. That is, he was not suggesting that amateurs speculate on the relationship between complex adaptations and environmental conditions, a la Schmeil; and he was certainly not suggesting that they dismiss whatever they had learned about morphology and taxonomy as boring or irrelevant. Rather, he hoped that they would merely collect useful data on plant distribution and climactic conditions and then turn it over to specialists for further analysis. Otto Heinrich reiterated Karzel's emphasis on popular ecology as a vehicle for amateurs to provide empirical evidence to

[101] K.C. Rothe, "Allgemeinen Einleitung," in *Handbuch für Naturfreunde,* VII-XII.
[102] Rudolf Karzel, "Pflanzenkunde," in *Handbuch für Naturfreunde,* 200.
[103] Ibid, 214.

experts in a later chapter on flower pollination, where he suggested that technologies like the camera made it easier than ever for laypersons to make precise observations that experts could later use to build better theories.[104]

Rothe and his co-authors thus mirrored Wettstein's earnest but nevertheless rigidly hierarchical vision of botany as an enterprise in which amateurs could be welcomed as colleagues of a sort but could never aspire to be much more than contributors of data. Or as Rothe remarked near the beginning of his *Handbuch*, the aspiring amateur botanist needed to remember that their acceptance into the scientific community was not only contingent on their capacity to produce quality work but to remain humble about their abilities and capacities, particularly with respect to theoretical interpretation, which was the province of experts.[105] After all, the last thing popular botany needed was a suffusion of more baseless speculation.

Conclusion

In his 1914 contribution to Paul Hinneberg's mammoth *Die Kultur der Gegenwart* series, the Austrian paleobotanist Othenio Abel provided an account of historical development and aims of his field as well as extensive commentary on the state of contemporary scientific publishing. One of the most important conclusions of his analysis was that the two topics were intrinsically connected. That is, he claimed that the form and content of contemporary biological knowledge was not determined by technical discourse alone but by a vast and heterogenous array of representations. In addition to popular texts and lectures, he listed reference works, yearly reports, catalogues, handbooks, textbooks, and illustrations as significant "factors in the development of a

[104] Otto Heineck, "Blütenbiologie," in *Handbuch für Naturfreunde*, 281-282.
[105] Rothe, "Allgemeine Einleitung," XII.

science."[106] In drawing attention to this state of affairs he did not intend to suggest that scientists needed to dampen the influence of non-technical exposition on specialist discourse, which would have been impossible anyway, but that they needed to enhance the quality of the popular and pedagogical elements of the scientific literature. As the fortunes of his own field of paleobiology demonstrated, failure to do so not only effected the public reputation of a science but informed its research trajectory in negative ways.

This chapter has sought to show that Abel's ideas had been circulating in the Austrian botanical community for at least two decades prior to his 1914 article in the work of Richard von Wettstein and his acolytes. As an advocate of a marginal form of neo-Lamarckism and practitioner in the increasingly unfashionable field of systematics, Wettstein was highly attuned to the role of popular and pedagogical literature in shaping the reception his theoretical and methodological commitments. In the late 1890's, he began to argue that popular works on evolutionary theory, Darwinian or otherwise, consistently failed to offer an objective view of the relevant facts because their authors preferred to provide speculative and selective readings that supported their chosen position. As a result of these popular narratives, the botanical community was still divided into a series of mutually opposing theoretical camps rather than united in the view that speciation happened in a variety of ways. After 1900, Wettstein, K.C. Rothe, and August Ginzberger applied similar arguments to the growing body of popular texts and textbooks on ecology, claiming that writers like Otto Schmeil captured some valuable aspects of the new domain of research but also

[106] Othenio Abel, "Paleontologie und Paleozoologie," in *Die Kultur der Gegenwart, ihre Entwicklung und ihre Ziele: Abstammungslehre, Systematik, Paleontologie, Biogeographie*, 385. It is worth noting that Abel was a vicious anti-Semite, and that his belief that social factors played a role in how scientists produced and warranted scientific knowledge would inform his later attempts to construct an "Aryan biology." These efforts are discussed in some detail in this dissertation's conclusion.

put forth a conception of biology that was misinformed about, if not outright hostile to, the methods and aims of systematics.

Although Wettstein and his circle were highly critical the contemporary popular and pedagogical literature, they also sought to harness it for their own scientific ends. Wettstein's popular work on evolutionary theory was not only intended to articulate his vision of speciation as a multivalent process that took place through multiple mechanisms but to adjudicate a technical issue within neo-Lamarckism concerning the nature of adaptive and organizational traits. Similarly, he and his followers used the various pedagogical reform movements that emerged in the Habsburg Empire throughout the fin de siècle to secure systematics' role as a foundational element of biological instruction, thereby assuring that they would have access to a steady stream of potential students and competent amateurs. Indeed, in many cases their popular works were more oriented towards the latter than the former, insofar as they saw amateur collectors and observers as a particularly essential but highly precarious labor pool.

Although these enlistment projects were rooted in an earnest desire to establish a productive working relationship with the lay public, they also evinced a steadfast desire to maintain expert hegemony over the discipline. That is, while Wettstein, Rothe, and Karzel understood botany to be a mixed amateur-expert enterprise, they also believed that amateurs were mainly there to provide experts with the observations and specimens. Even Rothe, who was generally more sanguine than his colleagues about the potential for amateurs to make real and lasting contributions to scientific knowledge, was careful to distinguish between popular and specialist botany and advised practitioners of the former not to exceed the bounds of their ability and education. In this respect, the botanists' views on popular knowledge and experience were closely aligned with those of their countryman Ernst Mach, who maintained that "every-day thought" could act as a useful check on the excesses of expert reasoning but never acquire the same epistemic status. As this dissertation's next chapter will argue, many of the same concerns and questions about the relationship between popular and scientific knowledge animated discussions amongst the country's social democratic politicians and theorists. Unlike the botanists, this latter group was clear that their popularizations were not only intended to integrate laypersons—or more accurately, workers—into elite science but to eventually grant them hegemony over it.

Chapter 4

“Wissen, um zu Leben”: Popular Science in Austromarxist Theory and Practice, 1895-1916

The intellectual and practical agendas of fin de siècle Austrian social democracy were intimately connected to, and in some cases driven by, natural-scientific discourse. From the very inception of the Social Democratic Worker’s Party of Austria (“SDAPÖ”) at the Hainfeld Conference in 1889, party officials expressed a deep interest in establishing the empirical and methodological credentials of socialism vis-à-vis more prestigious scientific disciplines like biology and physics. They also made popular-scientific texts, public lectures, and related forms of science education into core features of their program to build a politically conscious proletariat in the Habsburg Monarchy based on a unique belief in the revolutionary potential of workers who were conversant in Darwinism, mechanics, and other cornerstones of the scientific worldview.

The SDAPÖ’s outward consensus on the political importance of science and science education nevertheless belied several internal disagreements on what, philosophically speaking, the scientific enterprise was, and on what a socialist science curriculum should entail. These intraparty divisions became especially pronounced after the German socialist Eduard Bernstein published his powerful “revisionist” critiques of Marxism in the late 1890’s. As one prominent Austrian theorist noted in 1904, Bernstein’s attacks on the accuracy and predictive power of historical materialism had not only caused widespread unease about the scientific validity of the SDAPÖ’s platform but led many younger members of the party to seek to create their own theories of what socialism “understand(s) science to be, what the specific nature of social science is, what scientific necessity means," and "how the 'must' of causal necessity relates to the 'should' of ethics and reason."[1]

The locus of the Austrian social democratic movement to forge a new, post-Bernsteinian philosophy of science was situated within a small but highly influential cadre of Vienna-based intellectuals, including the sociologist Max Adler, the physicist Friedrich Adler, and political scientists Otto Bauer and Karl Renner. Aside from studying under the same professors at the University of Vienna, participating in the same clubs, and publishing in the same journals, what initially tied these so-called "Austromarxists" together was a commitment to engaging with ideas and thinkers that were outside the confines of traditional socialist thought.[2] According to Max Adler, Bauer, and Renner, for example, there was no way to formulate a coherent socialist theory of science without first drawing on the powerful neo-Kantian philosophical movement that had taken shape in the German universities over the second half of the nineteenth-century. For Friedrich Adler, by contrast, the future of socialist thinking about topics like scientific knowledge and necessity lay in a synthesis of Marxian dialectic and Ernst Mach's biologically inflected neutral monism, which had become the default philosophical perspective of wide swathes of the Central European scientific community by the early 1900's.

Although the Austromarxists' divergent intellectual commitments very quickly split the group into distinct and occasionally antagonistic philosophical camps, the first part of this chapter will argue that they nevertheless agreed on three fundamental positions. First, the neo-Kantians

[1] Max Adler, "Kant und Sozialismus," in *Max Adler: Ausgewählte Schriften* ed. Alfred Pfabigan and Norbert Leser (Vienna: Österreichischer Bundersverlag, 1981), 409.
[2] Tom Bottomore, "Introduction," in *Austro-Marxism* ed. Tom Bottomore and Patrick Goode (Oxford: Clarendon Press, 1978), 9-10. As Bottomore notes, "almost all the most prominent Austro-Marxists were students of (Carl) Grüneberg." Adler and Renner were particularly beholden to Gruenberg for the "conception of Marxism as a social science which should be developed in a rigorous and systematic way through historical and sociological investigation."

and Machians rejected the reductive, “ontological” materialism of orthodox Marxists like Vladimir Lenin and Georgi Plechanow because they felt that its mechanistic view of reality was philosophically indefensible and promoted fatalism among the working-classes. Second, both Austromarxist factions maintained that scientific knowledge was socially constructed, insofar as the social structures and relations of the capitalist state conditioned the form and content of scientific reasoning in various ways. And third, they agreed that the scientific enterprise should be subservient to the political aims of socialism, and that the point of scientific education was therefore not to communicate neutral facts but to help create a proletariat that was capable of revolution. Or as Otto Bauer explained in 1906, the true goal of scientific inquiry was to “show us how in our society the necessary struggle by the working classes for socialism arises as necessarily as the rejection of this struggle by the propertied classes; it places before each of us the question, as to which camp he will join.”[3]

Like many other elements of prewar Austromarxist theory, the group’s philosophy of science was largely confined to the pages of cadre journals like *Der Kampf*. But as the second part of this chapter will argue, elements of it were nevertheless manifest in the day-to-day operations of *Die Naturfreunde*, a socialist hiking club that Renner and several friends founded in 1895 to interest workers in the traditionally bourgeois hobby of outdoor recreation and provide them with a basic scientific understanding of the natural world. The philosophical overlap between the Austromarxists and *Die Naturfreunde* was most evident in the club’s pedagogical program. Rather than adopt the ideologically neutral approach to science education that was characteristic of several other major SDAPÖ-affiliated organizations like the Ottakring *Volksheim, Die Naturfreunde*

[3] Otto Bauer, “Marxismus und Ethik,” in *Austromarxismus: Texte zu ‘Ideologie und Klassenkampf’* ed. Hans-Jörg Sandkühler und Rafael de la Vega, (Frankfurt: Europäische Verlaganstalt, 1970), 73.

taught workers to view scientific facts as political tools whose emancipatory function had been obscured by the capitalist contexts of their production. Through popular lectures and texts on natural history, for example, the club's instructors endeavored to show that bourgeois society's insistence on the naturalness of individual competition and private property was at odds with what they could easily observe in nearby meadows and mountains, where cooperation and communism reigned supreme. In keeping with the Austromarxists' desire to combat proletarian fatalism, they also urged workers to see the world as something that they could actively change rather than a deterministic system that they had to passively accept.

While *Die Naturfreunde*'s primary purpose was to help workers divine the socialist lessons that were implicit in the natural world and apply them to their own lives, the club's leadership also hoped to establish working-class footholds in fields like botany and geology, and ultimately to lay the groundwork for a socialist takeover of the scientific community. To a certain extent, Renner and his *Naturfreunde* colleagues' conception of popularization as a mechanism for intervening in professional science and directing its development in certain ways was closely aligned with the perspective of Mach, Richard von Wettstein, and many other Austrian scientists. Indeed, Renner, Mach, and Wettstein maintained that popular representation was scientifically valuable for many of the same reasons. But as the conclusion of this paper will suggest, the popular works that *Die Naturfreunde* produced were nevertheless different from Mach's *Popular Scientific Lectures* and Wettstein's *System of Plants* in several key respects. Most importantly, they were actually and exclusively intended for laypersons and took them seriously as full participants.

The historical literature on socialist theory and SDAPÖ *Bildungspolitik* is massive, although there is surprisingly little scholarly work on the Austromarxists' attempts to formulate a coherent philosophy of science prior to the First World War, or on efforts by organizations like

Die Naturfreunde to translate the core insights of that philosophy into practical action. This is not to say that historians have ignored these topics altogether, but that they have often played a peripheral role in their analyses. The marginality of natural science as an object of historical inquiry is particularly palpable in the monographs that emerged from the groundswell of academic interest in the SDAPÖ's cultural programs in the 1980's and 1990's. Anson Rabinbach's *Crisis of Austrian Socialism* and Helmut Gruber's *Red Vienna: Experiments in Working-Class Culture*, which are now widely considered classics in the genre, only briefly touched on the role of scientific reasoning in party theory and practice, and then predominantly with respect to the interwar period.[4] This analytical blind-spot is less prominent in more recent historical work on the cultural politics of the pre-war SDAPÖ, but these more contemporary texts still offer little detail regarding the precise nature of the party's conception of science. The collected essays of *Wissenschaft, Politik, und Öffentlichkeit* clearly establish that the social democrats based much of their policy on the belief that "establishing a 'scientific spirit' in society…would almost automatically means a victory for social democracy," for example, but do not explain what they understood this scientific *Geist* to be.[5]

Conversely, intellectual historians who have primarily focused on Austromarxist theory have been more attuned to the ways in which thinkers like Max Adler and Friedrich Adler critically engaged with scientific reasoning in their work, but they have also tended to neglect the concrete ways in which SDAPÖ operatives sought to translate the Adlers' insights into working-class

[4] Anson Rabinbach, *The Crisis of Austrian Socialism: From Red Vienna to Civil War, 1927-1934* (Chicago: The University of Chicago Press, 1983); Helmut Gruber, *Red Vienna: Experiment in Working-Class Culture, 1919-1934* (Oxford: Oxford University Press, 1991).

[5] Ulrike Felt, "Wissenschaft, Politik und Öffentlichkeit – Wechselwirkungen und Grenzverschiebungen," in *Wissenschaft, Politik, und Öffentlichkeit. Von der Wiener Moderne bis zur Gegenwart* (Vienna: WUV-Universitätsverlag, 2002), 53.

educational programs. Ernst Glaser's *Im Umfeld des Austromarxismus* offered a magisterial account of the intellectual contours of Austromarxism, including its intimate connection to contemporary science and philosophy of science, but little information with respect to how this connection was manifest in the lectures and essays that the SDAPÖ and its affiliates produced for working-class consumption.[6] Similarly, Wolfgang Maderthaner's work on Friedrich Adler has done much to clarify his intellectual relationship with Mach but leaves open the question as to how workers actually encountered Machian ideas in organizations like the *Naturfreunde*.[7]

Although the historiography has only provided a partial picture of the role of natural science in SDAPÖ theory and practice, historians have nevertheless tended to agree that the party's conception of scientific knowledge and method represented a continuation of the "rationalist" and "Enlightenment" tradition adopted by their liberal predecessors. Carl Schorske offered one version of this claim in his immensely influential *Fin-de-Siècle Vienna: Politics and Culture*, where he argued that "of all the filial *revoltes* aspiring to replace" the declining "father figure" of post-1848 Austrian liberalism, "none bore the paternal features more pronouncedly than the Social Democrats. Their rhetoric was rationalist, their secularism militant, their faith in education virtually unlimited."[8] Anson Rabinbach put forth a similar, albeit less Freudian version of this argument in his *Crisis of Austrian Socialism*, which indicated that Austrian socialists "largely validated" the intellectual values of the liberals they sought to supplant, particularly in their adherence to "a rationalist world view."[9] And in a recent essay by Maderthaner, he remarked that

[6] Ernst Glaser, *Im Umfeld des Austromarxismus: Ein Beitrag zur Geistesgeschichte des Österreichischen Sozialismus* (Vienna: Europaverlag, 1981).
[7] Wolfgang Maderthaner, "Friedrich Adler und Graf Stuerghk – zur Psychopathologie eines Attentats," in *Physik und Revolution. Friedrich Adler – Albert Einstein. Briefe, Documente, Stellungnahme* ed. Michaela Maier and Wolfgang Maderthaner (Vienna: Loecker, 2006), 19-55.
[8] Carl Schorske, *Fin-de-Siecle Vienna: Politics and Culture* (New York: Vintage Books, 1981), 119.
[9] Rabinbach, *The Crisis of Austrian Socialism*, 18.

the socialists' advocacy of "scientific modernism" positioned them as "successors to," rather than apostates from, the "failed bourgeois liberalism" of the mid-to-late nineteenth-century.[10]

The present chapter engages with these disparate historiographical trends and tendencies in two ways. First, it adds texture to existing historical accounts of the relationship between socialist theory and *Bildungspolitik* by analyzing the circulation of the Austromarxist conception of science and nature from its point of origin in theoretical journals to its final destination in SDAPÖ associational life, where workers assimilated its core features through popular lectures and essays about phenomena like ant colonies and migratory patterns. And second, it modifies a commonplace assumption about the SDAPÖ's intellectual commitments by suggesting that the Austromarxists' denial of the neutrality of scientific reasoning; insistence on the contextuality of truth; and belief that that knowledge production should ultimately serve practical ends are better understood as breaks from traditional "liberal rationality" than continuations of it.

1889 and the Origins of Austrian Socialism

While the roots of Austrian socialism lay in the revolutionary movements that swept over Central Europe in the spring of 1848, the Social Democratic Worker's Party of Austria, or SDAPÖ, first began to take shape as coherent political entity at the Hainfeld Conference of 1888-1899.[11]

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[10] Wolfgang Maderthaner, "Austro-Marxism: Mass Culture and Anticipatory Socialism," *Austrian Studies* 14 (2006), 21-36. Even historians who are otherwise critical of traditional historiographical assumptions about the intellectual underpinnings of liberalism have reiterated the older literature's claims about the intellectual lineage of Austrian social democracy. For example: Deborah Coen has suggested that Schorske's conception "liberal rationality," if it ever existed, was more pronounced in the SDAPÖ than anywhere else, insofar as the Austromarxists "intended to train students to recognize that natural and social laws were absolute." See: Deborah Coen, *Vienna in the Age of Uncertainty: Science, Liberalism, and Private Life* (Chicago: University of Chicago Press, 2007), 238, 338-340.

[11] As Wolfgang Maderthaner notes, the party was not able to truly centralize its operations and effectively orchestrate between its various neighborhood, regional, and national organizations until 1909. See: Wolfgang Maderthaner, "Die Entwicklung der Organisationsstruktur," in *Die Organisations der Österreichischen Sozialdemokratie 1889-1995*, ed. Wolfgang Maderthaner and Wolfgang Müller (Vienna: Loecker Verlag, 1996). On the origins of Social Democratic *Arbeiterkultur*, see: Hugo Pepper, "Die frühe Österreichische Sozialdemokratie und die Anfänge der Arbeiterkultur," in *Sozialdemokratie und Habsburgerstaat* ed. Wolfgang Maderthaner (Vienna: Loecker Verlag, 1988), 79-101.

Given the intense ideological conflicts and internecine struggles that had characterized socialist discourse in the Dual-Monarchy in the 1870's and 1880's, one of the most important outcomes of the conference was the creation of the "Hainfeld Program," which established a set of unifying principles that would govern the party's agenda and tactics going forward. In a move that would have immense consequences for the political development of Austria over the next forty years, the program signaled that SDAPÖ officials would ignore demands for aggressive and direct revolutionary action, or "propaganda of the deed," in favor of the organization of stronger unions, parliamentary agitation, and the provision of working-class educational programs.

Party officials' decision to opt for a moderate, "anticipatory" form of socialist politics was rooted in the widespread conviction that the Austrian proletariat was not only unprepared for revolution, but that it was not even prepared to undertake a successful general strike.[12] According to the SDAPÖ's first chairman Victor Adler, one prominent reason for this unreadiness was the sociopolitical and economic backwardness of the Habsburg state, which had only begun to produce the kind of industrial working-class that Marx saw as the vanguard of revolution. When Belgian workers launched their ultimately successful mass strike in 1893, Adler was quick to declare that such an action was infeasible in Austria, explaining to Friedrich Engels that "mass strikes are easier in a highly developed, industrialized country with a small and unreliable army than they are in an industrially backwards country with a predominately agrarian population." This was particularly true, he added, if the underdeveloped country happened to be "modern in no other way than in its militarism."[13]

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[12] Maderthaner, "Austro-Marxism: Mass Culture and Anticipatory Socialism," 21-36.
[13] Victor Adler, "Die Lage Österreich und der sozialdemokratische Parteitag," in *Victor Adler / Friedrich Engels: Briefwechsel* ed. Gerd Callesen and Wolfgang Maderthaner (Berlin: Akademie Verlag, 2011), 199.

An even more important consideration for Victor Adler and his comrades when they were drafting the Hainfeld Program was their belief that the Habsburg Monarchy's working-classes were *intellectually* unprepared for revolution because the state's lackluster educational system had failed to provide its citizens with the kind of rudimentary facts and cognitive tools that were required for the cultivation of political consciousness. Having agreed that they could not achieve effective and lasting structural change without first creating an appropriate conceptual superstructure, they decided that one of the principal tasks of the SDAPÖ going forward would be "to organize the Proletariat, to make it conscious of its situation and its task, and to make it intellectually and physically ready for battle" by provisioning it with the right ideas. [14]

With respect to the exact kind of intellectual preparation that the working-classes should receive, the Hainfeld-era Social Democrats not only emphasized instruction in economics and politics but the natural sciences. For the most part, they justified the inclusion of scientific topics in any potential socialist propaedeutic based on a quintessentially Enlightenment view of scientific reasoning as a neutral tool which revealed the laws that governed nature and society, and thereby exposed the contingent sources of human misery and oppression. For example: many within the party argued that familiarity with the scientific worldview could serve as a universal solvent for the reactionary religious views and superstitions, or at least act as a bulwark against their return. Put in the words of Joseph Holzhammer, who led initial deliberations on the party's educational platform and strategy, failure to provide workers with access to the scientific worldview and other

[14] "Beschlüsse des Parteitags der Socialdemokratischen Arbeiterpartei Österreichs zum Parteitag zu Hainfeld ergänzt am Parteitag zu Wien," in *Verhandlungen des dritten Österreichischen Sozialdemokratischen Parteitags* (Vienna: Verlag Ludwig Bretschneider, 1892), IV.

fruits of the Enlightenment risked returning society "to those grim ages" when clerics ran amok burning witches and holding auto-da-fes.[15]

Party officials also saw basic familiarity with scientific knowledge and method as a tool which workers could use to improve their material situation and to cultivate some much-needed self-discipline. As a physician, V. Adler was adamant that workers be given a basic education in medicine and hygiene, and argued throughout his career that widespread ignorance of the basic facts of bodily health and care—and of the destructive effects of alcohol in particular—was a major source of proletarian immiseration.[16] Holzhammer, for his part, focused more on the potential material benefits that came from science education, remarking that "a proletariat without scientific knowledge" was a proletariat that lacked a critical tool for navigating the modern world and was therefore "forever excluded from a better future."[17]

The relatively stable consensus within the SDAPÖ on the meaning and political importance of natural science unraveled towards the end of the 1890's, however, as younger intellectuals within the party began to reject the Enlightenment views of their elders and pose critical questions about the neutrality of facts, the necessity of scientific laws, and the political function of science education. This change in opinion was spurred by several local trends, including a generational shift in interests and priorities. As Wolfgang Maderthaner has pointed out, many of the people who joined the party in the decades after Hainfeld were more interested in socialist theory and doctrine than their older comrades, who had been understandably preoccupied with the practical task of

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[15] Joseph Holzhammer, "Schule," in *Verhandlungen des Parteitags der Österreichischen Sozialdemokratie in Hainfeld* (Vienna: Verlag Ludwig Bretschneider, 1889), 103-104.
[16] For Victor Adler's collected work on the topic of public health, see: *Victor Adlers Aufsätze, Reden und Briefe, Vol. 3: Adler als Sozialhygieniker* ed. Parteivorstand der Sozialdemokratischen Arbeiterpartei Deutschösterreichs (Vienna: Verlag der Wiener Volksbuchhandlung, 1924).
[17] Holzhammer, "Schule," 104.

ensuring the survival of their new political movement.[18] But the younger socialists' rising circumspection about the nature of science and its relationship to socialism was also connected to a specific event in the wider world of European socialism: Eduard Bernstein's revisionist critiques of Marxism.

Bernstein, as the Austrian politician and theorist Otto Bauer described him, came of age during the relatively peaceful and prosperous period after the Franco-Prussian War of 1871, and was thus part of a generation of Central European socialists whose "belief in the coming revolution waned" in the face of the increasing fortunes of the German proletariat. This generation also tended to be confident that

> Social Democracy could no longer set its hopes on a revolutionary coup, but that it would be possible, through the development of democracy and workers' organizations, to gradually "hollow out" capitalism, to alleviate exploitation, and to conquer more space for state, municipal, and cooperative socialism. [19]

Bernstein went a step further and called into question the validity of socialism as a science, pointing out that Marx's core prediction about the "necessary" impoverishment of the working classes and the inevitability of revolution had not panned out. "There was only one specific 'socialist' element in socialist theory," he noted, and that was "its all-pervasive ethics and its conception of justice," which could never be raised to the level of a *Wissenschaft*.[20] The leaders of the SDAPÖ were not particularly alarmed by the idea that they should not seek to foment a revolution, as they were already inclined to work within rather than against the status quo anyway,

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[18] Maderthaner, "Anticipatory Socialism," 26.
[19] Otto Bauer, "Einleitung," in *Victor Adlers Aufsätze, Reden und Briefe, Vol. 6. Victor Adler der Parteimann, Pt. 1. Der Aufbau der Sozialdemokratie* (Vienna: Verlag der Wiener Volksbuchhandlung, 1929), XVI.
[20] Eduard Bernstein, "The Core Issue of the Dispute: A Final Reply to the Question, 'How is Scientific Socialism Possible?" *Sozialistische Monatshefte* 7 (1901), quoted in Sheri Berman, "The Roots and Rationale of Social Democracy," in *After Socialism* ed. Ellen Paul, Fred Miller, Jr., and Jeffrey Paul (Cambridge: University of Cambridge Press, 2003), 124.

but they were greatly troubled by Bernstein's contention that socialism was at best an ethical system.

One of Victor Adler's first actions in response to Bernstein's claim was to call for a reexamination and revision of the SDAPÖ's program in 1901, although he downplayed the exigency and extent of this revision by noting that "the wish to revise our party program emerged less out of a fundamental or practical need than out of a formal, one could almost say aesthetic one."[21] But for several rising intellectuals within the party a mere aesthetic revision did not go far enough, insofar as they felt Bernstein's attacks had revealed that the existing program relied on conceptions of science and scientific knowledge that were fundamentally outdated and suspect.[22] Specifically, the so-called "Austromarxists" held that the resolution of the revisionism controversy called for the construction of a completely new philosophical framework that could provide a more satisfactory account of "what we are to understand under concepts like science and practice, lawful regularity generally and natural law specifically, historical and social laws, necessity, freedom and chance, mechanism and teleology, evolution, conscious goal-setting, value, etc."[23]

In terms of what this new philosophical framework should look like, the Austromarxists were adamant that it had to be rooted in the insights of great thinkers like Marx and Engels but also needed to incorporate newer perspectives, including ideas that were not directly related to socialist theory or politics. Karl Renner, one of the group's most important political theorists, echoed a common sentiment when he argued that no legitimate theoretical system could remain static, and that the future of socialist thought lay in bringing "Marxist thinking and method…in conscious connection with modern intellectual life, that is, with the content of the philosophical

[21] Victor Adler, "Zur Revision des Parteiprogramms," *Arbeiter-Zeitung* (September 22nd, 1901), 2.
[22] Max Adler, "Zur Revision des Parteiprogramms," in *Max Adler: Ausgewählte Schriften*, 23-24.
[23] Max Adler, "Kausalität und Teleologie im Streite um die Wissenschaft," *Marx-Studien* (Vol. 1, 1904), 206.

and social scientific works of our time."[24] Above all, Renner and his cohort sought to bring socialist theory into contact with the neo-Kantian and Machian epistemologies that had developed alongside Marxism over the latter half of the nineteenth-century.

Neo-Kantians and Machians

The origins of neo-Kantian philosophy lay in a late-*Vormärz* reaction against the excesses of Hegelianism, although the movement truly took off in the 1860's among a generation of German intellectuals who thought that by going "back to Kant" they could imbue the philosophical enterprise with new life and purpose.[25] These intellectuals were particularly interested in revamping Kant's original epistemological insights to better accommodate the natural sciences, which were then experiencing a period of unprecedented growth, progress, and controversy. Indeed, some of the most avid participants in the movement to construct a more scientifically relevant Kantianism were themselves scientists, and thus inclined to view concepts like mind, category, and perception through the lens of empirical research in fields like sensory physiology. The most famous of this latter group was undoubtedly Helmholtz, who helped establish the contingent empirical origins of Euclidean geometry. But many other neo-Kantians, including prominent professional philosophers like Hermann Cohen and Paul Natorp, disdained the naturalism and psychologism of Helmholtz's approach and preferred to retain the idealism of Kant's original system. Whatever their background or preferred method, both groups were committed to the common task of better understanding scientific inquiry and unearthing the conditions that enabled it to produce objective truth.

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[24] Karl Renner, "Vorwort," *Marx-Studien* 1 (1904), VII-VIII.
[25] Klaus Köhnke, *The Rise of Neo-Kantianism: German Academic Philosophy between Idealism and Positivism*, trans. R.J. Hollingdale (Cambridge: Cambridge University Press, 1991).

By the early twentieth-century, neo-Kantianism had become one of the dominant intellectual and institutional force in Germanophone philosophy. It had also begun to take root among some members of the socialist intelligentsia, who gravitated towards the movement as a potential tool in their own struggles to define and delimit scientific reasoning and knowledge. Among the Austromarxists, the influence of neo-Kantian thought was most pronounced in the work of Max Adler, who published several influential articles and monographs throughout the fin de siècle and interwar periods which attempted to apply the new epistemology to problems within socialist theory. He recognized that his decision to adopt a bourgeois philosophical framework was problematic, especially given the socialist community's antipathy towards epistemology in general, but felt that there were compelling reasons to do so.[26] Most obviously, he was simply convinced that neo-Kantian philosophy offered the most adequate perspective then available for understanding the conditions and limits of human thought, including its propensity for false consciousness and error, and that socialists ignored it at their own peril.[27]

More controversially, he argued that adopting a neo-Kantian approach could put socialism on a more scientific footing. This position was anathema for those who held that Marxism was an all-encompassing and self-sufficient worldview, but for Adler it was clear that socialism was merely one positive science among many, and that its advocates could therefore derive "immediate benefit" from the kind of "clarification of its concepts and… investigation of its significance and limitations" that a neo-Kantian style of analysis could provide.[28] In terms of what this analysis

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[26] Max Adler, "Kausalität und Teleologie im Streite um die Wissenschaft," 3. As Adler noted at the beginning of "Kausalität," it was possible "that the time was not far off" when he would not have to offer an extensive justification for his interest in the epistemological and methodological aspects of Marxism, but as of 1904, work on these topics "could count on "a certain disconcertment, if not dismissive prejudice" from many socialists.
[27] Ibid, 4.
[28] Max Adler, "Marxismus und Materialismus," in *Max Adler: Ausgewählte Schriften*, 436- 437.

would actually look like, he indicated that the objects, laws, and experiences of social life were constituted by certain "necessary forms of consciousness," and that any legitimate investigation of the social needed to be based on a clear understanding of what these necessary forms were and how they were manifest in particular phenomena.[29] In much the same way that Kant sought to understand scientific knowledge by reflecting on the a priori conditions of experience and thought, in other words, the neo-Kantian philosopher of social life sought to explain sociological facts and categories by analyzing the a priori conditions of social experience and thought.

Around the same time that Max Adler was articulating the basic outlines of his neo-Kantian socialism, his comrade Friedrich Adler was seeking to push Austromarxist theory in the direction of the physicist and philosopher Ernst Mach. Mach had been a controversial figure in Central European intellectual life for much of the second half of the nineteenth century, in no small part because of his consistent antagonism towards the assumptions of both traditional philosophy and "classical" physics, but by the 1890's his ideas had become immensely influential in both academic and lay circles. He found a particularly receptive readership among younger and more naturalistically inclined scientists, who saw his monism and Darwinian epistemology as superior frameworks for understanding the essence of scientific inquiry and resolving various research problems.

Although Adler—who completed his doctorate in physics at the ETH in 1902—was a natural audience for Mach's work, he was initially skeptical of it, fearing that the Mach's conception of ideas as continually evolving mental adaptations threatened to undermine the universal and mind-independent validity of Marx's system. By the time he finished reading *History*

[29] Ibid.

and Root of the Principle of the Conservation of Energy in 1904, however, he was not only persuaded by its arguments but convinced that socialist theory needed to be re-oriented along Machian lines.[30] In many respects, he faced a less daunting task in crafting a synthesis of Mach and Marx than his colleagues did in constructing a neo-Kantian socialism. For one thing, Mach was a lifelong supporter of the SDAPÖ and had already filled his work with apercus and remarks indicating that his epistemology supported certain aspects of the socialist project, like the redistribution of wealth and destruction of private (intellectual) property. Moreover, Adler and Mach ended up becoming close friends and correspondents, writing one another frequently about the relationship between science and socialism in the years leading up to the latter's death. But the older physicist was also unfamiliar with many of the core texts and tenets of socialist theory—at one point he admitted to having never read Engels or Marx—which left Adler to figure out the specifics of how to fit the two systems together.

In keeping with his overarching interest in the relationship between scientific and Marxist approaches to explanation, Adler focused on articulating the ways in which Mach's evolutionary account of human reasoning could supplement Marx's historical materialism. In particular, he argued that Mach's analyses of the biological origin and historical development of physical concepts offered a powerful naturalistic framework into which socialists could fit their extant understanding of social concepts like labor and class. Like Max Adler, he faced criticism from socialists who felt that his suggestion implied some sort of fundamental flaw in Marx's theory, but as he explained in 1909, Mach did not so much "rectify" historical materialism as provide a

[30] John Blackmore, *Ernst Mach: His Work, Life, and Influence* (California: University of California Press, 1972), 186.

complementary conception of nature, thereby strengthening socialism's claim to be a unitary worldview.[31]

While the neo-Kantian and Machian factions remained politically aligned, they were highly critical of one another's philosophical views. For the former group, the Machians' aggressive antimetaphysics and naturalism seemed to demand the rejection of concepts that they found analytically useful, if not necessary. As Max Adler noted in his extensive critique of Mach in 1911, it is was one thing to "reveal the fetish-like qualities of the 'I' and 'object' concepts," which Kant had already done in his analysis of the paralogisms of pure reason, but it was another to attempt to dispense with these notions altogether. Moreover, he argued that Mach's insistence on the fundamentally empirical and contingent nature of all human thought led to a thoroughgoing relativism about social categories and relations, and thus to a form of "social and ethical nihilism."[32] Friedrich Adler was no less critical of his neo-Kantian comrades, echoing Mach's grave condemnation of any philosophical perspective that continued to employ a priori categories and decrying the "significant misunderstandings" that Mach's critics, Kantian or otherwise, spread about his philosophy.[33]

Despite disagreeing on a number of fundamental conceptual issues, the Adlers and their respective allies managed to find common ground on three philosophical positions. First, both factions agreed that the ontological, or "matter-in-motion" materialism that was endemic in fin de siècle socialist circles was rife with error, and therefore inadequate as an explanatory framework

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[31] Friedrich Adler, "Der 'Machismus' und die Materialistische Geschichtsauffassung" *Neue Zeit* 28 (February 4, 1910), 676.
[32] Max Adler, "Mach und Marx. Ein Beitrag zur Kritik des modernen Positivismus," *Archiv für Sozialwissenschaft und Sozialpolitik* 33 (1911), 377-380.
[33] Friedrich Adler, *Ernst Machs Überwindung des mechanischen Materialismus* (Vienna: Ignaz Brand und Co., 1918), 10.

for social phenomena. From the neo-Kantian perspective, the materialists' misguided picture of reality had its origins in a fundamental misinterpretation of Marxian dialectic as ontological rather than methodological in nature.[34] Put in Max Adler's words, Marx's remarks about "standing Hegel on his head" did not imply that matter was "the father of all events," as Vladimir Lenin and Georgi Plechanow suggested, but that empirical phenomena were a more appropriate starting point for scientific inquiry than Hegelian *Geist*.[35]

In addition to being an egregious misreading of Marx, M. Adler rejected ontological materialism on philosophical terms, drawing on two arguments that had been circulating in neo-Kantian circles since the 1860's. First, he claimed that Lenin and Plechanow's theory relied on extremely tenuous assumptions about the possibility of deriving consciousness from physical processes, reiterating a point about the incommensurability of matter and mental life that philosophers like F.A. Lange, Hermann Cohen, and others had made in response to the *Materialismusstreit* decades earlier.[36] And second, he objected to the materialists' determinism based on a Kantian understanding of the individual as "neither purely effective nor purely determined" but "determined in its actions and active in determination."[37] Put in simpler terms, he recognized that environmental conditions structured and constrained an individual's ability to think and act but denied "the materialist teaching" that humanity was entirely "the product of external circumstances." [38] In place of the latter view, he maintained that the human intellect and will remained free to actively change external circumstances and therefore free to create new

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[34] Karl Marx, "The German Ideology: Part One," in *The Marx-Engels Reader, Second Edition* ed. Robert Tucker (New York: WW Norton, 1978), 154.
[35] Max Adler, "Dialektik oder Metaphysik," *Der Kampf Vol. 5* (Vienna: Verlag von Georg Emmerling, 1912), 80.
[36] Friedrich Albert Lange, *Geschichte des Materialismus und Kritik seiner Bedeutung in der Gegnwart* 2nd ed. (Iserlohn: J. Baedeker, 1873).
[37] Max Adler, "Marx und die Dialektik," *Der Kampf Vol. 1* (Vienna: Verlag von Georg Emerling, 1908), 265.
[38] Ibid.

systems of necessity and determination, thus mirroring Kant's claim that the faculty of practical reason was free to create the rules that bound it.

Finally, and somewhat surprisingly, Adler objected to ontological materialism based on his reading of Mach, who was among the earliest and most influential scientific critics of substantialism, or the belief that perceptible phenomena were manifestations of some sort of latent substantial entity that was not directly accessible to the senses. Mach's intentions in attacking this doctrine were twofold. First, he sought to show that atoms were not real objects lying "behind" appearances but conventions that helped the mind make sense of unfamiliar phenomena, akin to the way that physical models helped physicists picture complicated natural processes. And second, he hoped to persuade scientists to embrace phenomenological description, which represented events and objects without interpolating any non-perceptible objects or properties into them, as a superior alternative to mechanical explanation. He ultimately did not have much success in converting the scientific community to phenomenalism, but he did manage to provisionally convince many of his colleagues that the atom was an intellectual construct. Adler picked up on this shift in scientific opinion and used it as further evidence against Plechanow, claiming that it was irrational to continue to advocate for the reality of matter when the scientific community, traditionally one of materialism's the most formidable redoubts, had itself embraced the position that it was at best a "tool for thinking."[39]

Unsurprisingly, Friedrich Adler's rejection of ontological materialism was also greatly informed by Mach's critique of substance, including his controversial contention that the sciences could, and indeed should abandon the concept altogether. Specifically, F. Adler shared Mach's

[39] Adler, "Dialektik oder Metaphysik," 80, 83.

conviction that matter and its various permutations were conventions that no longer served any legitimate explanatory function, and that intellectuals' continued reliance on substances to make sense of and represent the natural world was serious threat to scientific progress.[40] From Mach's perspective, the epistemic dangers of substantialism were most evident in physical fields like thermodynamics, where the complexity and novelty of the processes under investigation compelled physicists to fit them within the familiar framework of atomism and ignore phenomena which were not explicable in mechanical terms. Adler agreed with this assessment but also maintained that substance was no less an impediment to the scientific development of socialism, insofar as materialism saddled its adherents with a static conceptual framework that was incapable of grasping the dynamic, evolutionary nature of social life.[41] Like Max Adler, he was particularly critical of the materialists' determinism, arguing that their belief that certain material conditions "automatically" produced certain social formations obscured the essential role of willful action in bringing about sociopolitical change.[42]

In taking a strong stance against ontological materialism, the Austromarxists not only hoped to save socialism from the clutches of a false and misleading doctrine but to support their own theories, which emphasized the role of socio-historical factors in structuring human thought and action. This is not to say that Max Adler, Friedrich Adler, and their comrades denied the constitutive importance of modes of production and other "material" factors in determining intellectual "superstructure," but that their analyses placed greater emphasis on how contingent social conventions, institutions, and ideologies determined a society's horizon of possibility,

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[40] Ernst Mach, *History and Root of the Principle of the Conservation of Energy* trans. Philip Jourdain (Chicago: The Open Court Publishing Company, 1910).
[41] Friedrich Adler, "Friedrich Engels und die Naturwissenschaften," *Die Neue Zeit* 25 (1906-1907), 631.
[42] Friedrich Adler, "Wozu brauchen wir Theorien?" *Der Kampf Vol. 2* (Vienna: Verlag von Georg Emmerling, 1909), 263.

including the kind of science it supported. Indeed, in many respects the Austromarxists were "social constructionists" *avant la lettre* by virtue of their insistence that one could not fully understand scientific facts and methods without first examining the contexts in which they were produced and used.[43]

Max Adler's variation of the "social construction" argument was based on his theory that membership in a social community was a condition of possibility for the existence of individual consciousness, and that historically specific social forms and relations were therefore constitutive of what individuals could think at any given time. He articulated this point most clearly in his 1904 essay "Kausalität und Teleologie," where he declared that:

> the existence, experience, and development of man already thoroughly presuppose human community… In this way character and intellect, action and thought, are from the very beginning a product of a great human community, and there is no fiber of the body, no breath of the intellect, that can exist self-sufficiently.[44]

Other neo-Kantian philosophers would have recognized this argument as an extension of Kant's transcendental aesthetic and transcendental deduction, which maintained that consciousness was only possible by virtue of certain a priori cognitive forms and categories that enabled that individual to distinguish the subjective from the objective, but Adler was careful to show that his claims also had precedents in Marx.[45]

One of the most obvious manifestations of the social a priori of capitalist society, according to Adler, was widespread belief in the reality of the "I-form" and a concomitant rejection of the idea that individual consciousness was fundamentally social in nature.[46] Specifically, he held that

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[43] The historical origins of social constructionism are complex, and it would be difficult to pin down any one starting point, but it seems fair to say that historians of science have not paid enough attention to the role of fin de siècle Marxists in establishing that scientific truth is a contingent product of social forces.
[44] Max Adler, "Kausalität und Teleologie," 371.
[45] Ibid, 372
[46] Ibid.

the capitalist form of life revolved around "the epistemological and sociological nonsense of Robinson Crusoe," or the notion that the individual was a fundamentally self-contained and self-sufficient unity rather than "an animal which can individuate itself only in the midst of society."[47] Although the effects of the Crusoe myth were most evident in the world of economics and politics, he indicated that they were also palpable in the natural sciences, particularly in the assumption that knowledge was the product of an individual mind grasping at nature rather than a "a social product" whose "content can only be brought forth in the thinking, participating, and interacting heads of men."[48] Put another way, the fetishization of the I-form explained how liberal scientists could claim to be neutral observers of nature and yet consistently produce knowledge that just so happened to reflect the economic and sociopolitical commitments of their social group.

Although less philosophically inclined than Max Adler, Otto Bauer came to similar conclusions regarding the social nature of consciousness, cognition, and knowledge in his own work. In his 1907 text on the "nationalities question" in the Habsburg Monarchy, for example, he argued that membership in a national community determined how individuals were able to think and even what they perceived, and that variations in national form could therefore create divergent perceptual realities and "systems of representation."[49] Some of the evidence he offered in support of this contention would have already been familiar to an Austrian readership that had been immersed in debates over national identity and culture for decades, like the fact that different ethnic groups tended to harbor and cultivate different aesthetic and ethical preferences. But he also made the more controversial claim that national context determined how scientists worked and the

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[47] Max Adler, *Kant und der Marxismus: gesammelte Aufsätze zur Erkenntniskritik und Theorie des Sozialen* **(**Aalen: Scientia Verlag), 174.
[48] Adler, "Kausalität und Teleologie," 379.
[49] Ephraim Nimni, "Introduction," in Otto Bauer, *The Question of Nationalities and Social Democracy* ed. Ephraim Nimni, trans. Joseph O'Donnell (Minneapolis: University of Minnesota Press, 2000), xxxv.

outcomes that they held to be legitimate, remarking that English and German scientists working on "the same object of investigation" nevertheless used different methods and oftentimes arrived at "quite different results" because of their divergent cultural upbringings.[50]

While Bauer's understanding of the constitutive role of social forms and relations in structuring mental life mirrored Max Adler's conception of the social a priori, he departed from the latter in his embrace of Darwinism as an explanation for what these forms ultimately were. As he noted in his nationalities monograph, his conception of national community mapped neatly onto Darwin's theory of evolution, insofar as he held that the traits that defined a given national character were shaped by ancestral forms and refined through the "struggle for existence," just like the traits that defined biological species. Further, he maintained that the precursor to any national community was a "natural community" which emerged in response to local physical conditions, thus suggesting that the cultural traits that defined and delimited national identity were in certain respects biological in origin. In adopting this Darwinian framework in his analyses, Bauer was in many respects less aligned with the neo-Kantians than with Friedrich Adler, who also embraced a thoroughgoing naturalism in his account of the relationship between social life and cognition.

Like many other aspects of his philosophical worldview, Friedrich Adler's claim that "all knowledge" was a manifestation of "dialectical-historical and evolutionary processes" was a relatively straightforward recapitulation of Mach's epistemological position, which held that ideas were heritable mental traits that *Homo sapiens* developed over time to better reflect the natural world and accommodate new facts.[51] Perhaps most famously, in *Science of Mechanics* Mach argued that the concepts, laws, and principles of that most important branch of physics had

[50] Bauer, *The Question of Nationalities and Social Democracy*, 99.
[51] Adler, "Friedrich Engels und die Naturwissenschaften," 629.

originated as unconscious cognitive adaptations to mechanical phenomena, and that their contemporary form was the result of gradual, cumulative modification by generations of artisans, philosophers, and scientists.[52] Newton did not pluck the laws of motion fully-formed from some Platonic realm of ideas, in other words, but extended and refined an existing body of knowledge—much of it instinctive—using the intellectual resources that were then available to him as a late seventeenth-century natural philosopher, which was why the otherwise brilliant *Principia* was also rife with the intellectual prejudices of its time.

When Adler set about articulating his own version of these Machian arguments in the mid-1900's, he retained many of his mentor's original insights concerning the contextual nature of scientific reasoning but tailored them to suit the interests and concerns of socialist audiences. Believing that most of his readers did not need to be convinced of the historical contingency of knowledge, he focused on showing that Mach's adaptationist perspective was explanatorily superior to historical materialism in two key respects. First, he claimed that Mach's conception of ideas as part of a complex dialectic between psychological, physiological, and physical processes offered a more scientific picture of cognition than the historical materialists' vague and metaphysical theories about material base causally "determining" intellectual superstructure.[53] And second, he argued that Mach's emphasis on the mind's active, albeit biologically and socio-historically constrained role in constructing empirical reality avoided the "one-sidedness" of the historical materialist's conception of ideas as mere "mirrors" of modes of production." It was not that economic conditions were epistemologically irrelevant, he cautioned, but that socialist

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[52] Ernst Mach, *The Science of Mechanics: A Critical and Historical Account of its Development*, 5th ed., trans. Thomas McCormack (La Salle: The Open Court, 1942).
[53] Adler, *Ernst Machs Überwindung des mechanischen Materialismus*, 175-176.

theories of knowledge "would be better preserved from misunderstanding" by viewing them as one among several variables which structured the form and content of thought.[54]

The third point of philosophical overlap between the two Austromarxist factions was their belief that the scientific enterprise ultimately served to motivate and guide political action. For Max Adler, the notion that science was a handmaiden to socialism was rooted in Kant's *Critique of Judgment*, which demonstrated that the faculty of practical reason had "priority" over the mind's other cognitive capacities, including those associated with the production of causal knowledge.[55] Specifically, Adler argued that Kant's argument for practical reason's primacy over theoretical reason was also relevant to fin de siècle debates over the relationship between the social and natural sciences because it explained why the normative, teleological perspective of the former gave meaning and direction to the otherwise empty abstractions of the latter. Given that socialism was the *Geisteswissensschaft* that best embodied Kant's categorical imperative, he concluded that it was obvious that science "could not be viewed as anything other than as a means for socialism's moral goals."[56] Or as he wrote near the end of *Causality and Teleology*, Kant had firmly established that the "one true maxim of science" was not "knowledge for its own sake" but "know, in order to live," and that social theorists had established that there was no better guide to producing the best possible life for the greatest number of people than what Marx and Engels had produced at over the latter half of the nineteenth-century.[57]

Friedrich Adler came to very similar conclusions about the subservience of science to politics his own work. As with his views on the social construction of knowledge, Adler's

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[54] Ibid.
[55] Max Adler, "Das österreichische Chaos und seine Entwirrung," *Die Neue Zeit* 20 (1902), 646-647.
[56] Max Adler, "Kausalität und Teleologie," 431.
[57] Ibid.

overarching conception of science as a political tool was premised on the Machian notion that ideas were mental adaptations to experience, and that the truth or "correctness" of a given idea was therefore a function of its capacity to help people orient themselves to phenomena, solve problems, manipulate their environment, etc. He went beyond Mach, however, by arguing that the truth-value of a concept or theory was not only tied to its practical value in nature, the laboratory, or the artisan's workshop but to its capacity to help humanity survive and navigate the peculiar socio-political and economic conditions of the capitalist state. In a 1908 article for *Der Kampf*, for example, he suggested that social theories which "led the oppressed to remain under the thumb of the class state" were as scientifically illegitimate as astronomical theories that maintained that the sun revolved around the Earth for the simple reason that both hindered the species' ability to adequately adapt itself to its surroundings. Conversely, Marxism and other ideas that served as "tools for class-conflict" were as scientifically valid as the new energy laws because they enabled their users to successfully understand and manipulate a vast number of events and processes in their environment.[58]

Given their belief in the intrinsically political nature of science, the Austromarxists were understandably adamant that the aim of working-class science education was to inculcate a socialist worldview and to give workers the intellectual tools they needed to hasten the decline of capitalism. Put in Max Adler's Kantian idiom, the point of teaching the proletariat about biology and mechanics was to provide it with the ability to "to set goals for itself with ever clearer consciousness, to intervene with ever-stronger will in the course of events, to allow for representations of what should be to decide more and more in favor what is."[59] In making this

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[58] Friedrich Adler, "Wozu brauchen wir Theorien?" 263.
[59] Max Adler, "Zur Revision des Parteiprogramms," 31.

demand of socialist pedagogy, the group set itself apart from mainstream social democratic opinion and SDAPÖ policy, which supported a number of liberal educational initiatives that were premised on providing a "neutral" education to workers. Indeed, Adler and his comrades were among the strongest critics of these bipartisan pedagogical projects and fought tirelessly to show that the ostensibly value-free education that they purported to provide was actually geared towards indoctrinating students in "the ideas and possibilities of bourgeois ideology."[60]

According to several peripheral Austromarxist thinkers, the biases implicit in liberal science instruction were rooted in biases that were embedded within the scientific community itself. They held that biologists were particularly guilty of ladening their research with ideological baggage. On the hundredth anniversary of Darwin's birth in 1909, *Der Kampf* published a series of articles that explored topics ranging from natural selection to the resurgence of Lamarckian thought, and while the authors were at odds on some issues, they were unanimous in their belief that evolutionary theorists were using their scientific authority to pass off biased interpretations of socialism as objective knowledge. From the perspective of the Austromarxist pedagogue Johann Polach, it was "not surprising" that recent efforts to apply Darwin's insights to historical and social phenomena had yielded spurious critiques of social democracy because liberal researchers like Ernst Haeckel had been attempting to show that Darwinism "disproved" socialism since the late 1870's.[61] Otto Weiss agreed, writing that the scientific opponents of social democracy were quick to construct shoddy arguments claiming that "socialism is unnatural, that it seeks to weed out the

[60] Max Adler, *Der Sozialismus und die Intellektuellen* (Vienna: Ignaz Brand und Co., 1910), 33-34.
[61] Johann Polach, "Das Gesetz der natürlichen und der gesellschaftlichen Auslese," *Der Kampf Vol. 2* (Vienna: Verlag von Georg Emmerling, 1909), 328.

struggle for survival by creating the same conditions for all men," but slow to provide facts showing how it did so.[62]

But for others, the problem lay more with the organizations that claimed to offer neutral science instruction than with the scientists themselves. One of the Austromarxists' most frequent institutional targets was the *Freie Schule* movement; a liberal reform project which sought to push back against Christian Social influence on Viennese schools by reorienting education along more secular lines. Robert Danneberg, a frequent contributor to *Der Kampf* as well as the director of the SDAPÖ's "Central Education Committee," articulated a common complaint when he claimed that the *Freie Schule*'s aggressive anti-clericalism did not necessarily translate into support for socialism, and in some cases actively led workers away from it. This was not to say that anti-clericalism was entirely irrelevant to the SDAPÖ's agenda, he explained in 1908, but that it was more important for socialist educators to "work with all (their) power to awaken and keep a living interest in socialism among the wide masses of the Viennese proletariat" than it was for them to solidify workers' already deep mistrust of the church.[63] Otto Bauer was more sanguine about the political value of the *Freie Schule*, but he nevertheless agreed with Danneberg that the movement placed far too much significance on the "clerical question," which was already more-or-less settled among the working classes, and that it would be better for SDAPÖ pedagogues to focus their teaching on topics that were closer to the heart of socialism.[64]

Other Austromarxists were more hostile to the *Freie Schule*, particularly after the so-called "Wahrmund Affair." The basic facts of the "Affair" were relatively straightforward: in 1908 the

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[62] Otto Weiss, "Die Abstammungslehre," *Der Kampf Vol. 2* (Vienna: Verlag von Georg Emmerling, 1909), 236.
[63] Robert Danneberg, "Sozialdemokratische Erziehungsarbeit," *Der Kampf Vol. 2* (Vienna: Verlag von Georg Emmerling, 1909), 457.
[64] Karl Mann, "Proletariat und Religion," *Der Kampf Vol. 1* (Vienna: Verlag von Georg Emmerling, 1908), 541. Bauer wrote several other articles under the pseudonym "Karl Mann."

Innsbruck professor and theologian Ludwig Wahrmund, whom John Boyer has aptly described as "a querulous person with a self-righteous vision of a new, revisionist theology," found himself at the center of a controversy surrounding his inflammatory remarks about the Catholic Church.[65] As a tenured employee of the University and therefore of the state, his remarks were technically protected speech, but their subject matter pushed the boundaries of what authorities and the general public were willing to countenance, and quicky yielded frenzied denunciations and passionate declarations of support from political parties and groups around Austria. Although Karl Renner viewed the controversy as a political distraction that primarily served to display the pitiful cultural defensiveness of the Austrian bourgeoisie, many of his comrades were ready to use Wahrmund's treatment as evidence that the Social Democrats needed to affect a complete break with liberals.[66]

Josef Strasser, a political theorist and frequent contributor to *Der Kampf*, was among those who advocated for a schism, arguing that the "Wahrmund Affair" had definitively demonstrated that liberal intellectuals were not truly committed to academic freedom, and that their outward support for the ideals of "free thought" and scientific neutrality cloaked an underlying commitment to reactionary ideas and values.[67] In addition to being manifest in the university context, he argued that this hidden agenda was at work in the *Freie Schule*, where the working-classes were fed a steady diet of "neutral" ideas that really served to support the ideology of "radical *kleinbürger* democracy," including a form of anti-clericalism that was "in effect a moderate clericalism."[68] Like Bauer, he concluded that the best course of action for the SDAPÖ was to continue building

[65] John Boyer, *Culture and Political Crisis in Vienna: Christian Socialism in Power: 1897-1918* (Chicago: University of Chicago Press, 1995), 191.
[66] Ibid, 191-205.
[67] Josef Strasser, "Was kann die 'Freie Schule' noch leisten," *Der Kampf Vol. 1* (Vienna: Verlag von Georg Emmerling, 1908), 493-495.
[68] Ibid.

up its own pedagogical apparatus, thereby assuring that workers would receive a proper socialist education.

Die Naturfreunde

One educational organization that the Austromarxists could rely on to instill an explicitly socialist conception of science and nature in the proletariat was *Die Naturfreunde*, a hiking club that one of their own, Karl Renner, helped establish in 1895, and which had become one of the most popular voluntary association in Austria by the late 1900's. *Die Naturfreunde*'s meteoric rise from a singular, Vienna-based group to an international network that commanded over 23,000 members in 1913 admittedly had less to do with its pedagogical component than with its recreational aspects, which drew a disproportionate amount of working-class attention and interest. But from the perspective of Renner and co-founder Georg Schmiedl, the club's efforts to teach workers about the natural world were just as integral to its political mission as its efforts to get them to spend their free time hiking instead of drinking. "The transformation of work-animals into work-men" was not just a matter of exposing them nature, Schmiedl explained, but teaching them "what the trees and flowers, what the beetles and butterflies, what the rugged bluff and stone on the creekbank have to say."[69]

Unlike the *Freie Schule* or the Ottakring *Volksheim*, an SDAPÖ-affiliated "people's university" which provided workers with access to classes and courses on topics ranging from German literature to physical chemistry, *Die Naturfreunde* did not try to avoid explicit socialist messaging in its popular-scientific texts and lectures.[70] When social democratic critics claimed

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[69] Georg Schmiedl, "Welche Gedanken haben mich bei der Gründung unseres Vereines geleitet," *Der Naturfreund Vol. 24* (Vienna: Alois Rohrauer, 1920), 69.
[70] Ludo Hartmann, "Das Volkshochschulwesen" in *Bildung, Freiheit, Fortschritt: Gedanken österreichischer Volksbildner* ed. Dr. Hans Altenhuber and Aladar Pfniss (Vienna: Verband Österreischischer Volkshochschulen, 1965), 116. Hartmann, a social democrat and one of the architects of the *Volksheim*, justified the organization's

that the club was distracting workers from more important political tasks, Renner countered that the point of *Die Naturfreunde* was not just to teach workers basic scientific facts but to provide them with an understanding of nature "that was of great agitational value."[71] Schmiedl mirrored Renner's claim in his own account of the club's purpose, writing that he saw its efforts to help workers unlock the "secrets of polymorphous nature" as part of the SDAPÖ's broader battle to create the kind of politically conscious proletariat that was a prerequisite for the triumph of socialism.[72]

Despite Renner's and Schmiedl's insistence that *Die Naturfreunde* existed to further Social Democratic *Bildungspolitik*, the club was slow to develop its educational wing, and much of the popular-scientific work that it initially produced was relatively apolitical. A prime example of this political toothlessness was the botanist Emil Haberlandt's "Notes on Floristics" series, which reported on "botanical novelties…less well-known plant forms," and similar topics for *Der Naturfreund* between in 1902 and 1905.[73] Far from providing workers with information of "agitational value," the dry, descriptive content of Haberlandt's articles was virtually indistinguishable from that of mainstream scientific magazines like the *Botanische Zentralblatt*. The naturalists F. Wachter and Adolf Hoffman produced similarly apolitical work for *Der Naturfreund* during this period, publishing articles that taught workers how to collect minerals and

aggressive political neutrality by claiming that it was better for people to arrive at a worldview organically than to have one foisted on them.

[71] Karl Renner, *An der Wende Zweier Zeiten*: *Lebenserinnerungen von Karl Renner*, Vol. 1 (Vienna: Danubia-Verlag, 1946), 285.

[72] Schmiedl, "Welche Gedanken haben mich bei der Gründung unseres Vereines geleitet," 69.

[73] E. Haberlandt, "Floristische Notizen," *Der Naturfreund* 6 (Vienna: Alois Rohrauer, 1902), 26.

identify insects but offering them very little indication as to how entomological or geological phenomena related to socialism.[74]

By the late 1900's, however, *Die Naturfreunde* had not only begun to invest more heavily in its popular-scientific output but to communicate a more aggressively political message to its members. The reasons for this shift were threefold. First, in 1906 Renner and several others began to push for the club to place greater emphasis on science education in general, and eventually changed its statutes to reflect an explicit commitment to "the spread of natural scientific knowledge and conservation."[75] Second, the club's leadership resolved to hew more closely to the SDAPÖ party line, declaring at the organization's general assembly in 1908 that it was "the most urgent duty of the central chapter and all local affiliates to emphasize the party's point of view in modest but unambiguous ways at every opportunity."[76] And third, in 1909 the Viennese branch of the club established a dedicated "Natural History Department" (*Sektion für Naturkunde*) under the leadership of Angelo Carraro, a teacher, pedagogue, and naturalist who considered it his mission to "build a bridge between the physical world of labor and the world of teaching and research" through his popular lectures.[77]

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[74] Adolf Hoffmann, "Einiges von den Insekten," *Der Naturfreund* 8 (1904), 85-87; F. Wachter, "Wo und wie sammeln wir Mineralien," *Der Naturfreund* 6 (1902), 57-60. As Leopold Happish, the editor of *Der Naturfreund*, noted at the end of Wachter's article, the club's members had been calling for a guide to collecting minerals for some time, which suggests that the push for more popular-scientific content came from below as well as above.
[75] "Rechenschaftsbericht," *Der Naturfreund* 11 (1907), 242.
[76] *Protokoll der Touristenverein die Naturfreunde Hauptversammlung 1908*, quoted in Dagmar Günther, *Wandern und Sozialismus: zur Geschichte des Touristenverine 'Die Nautrfreunde' im Kaiserreich und in der Weimarer Republik* (Hamburg: Verlag Dr. Kovac, 2003), 12. A year after the resolution Happisch declared that the primary value of the club was not recreation but scientific *Bildung*, which served to awaken a feeling "of duty towards ones comrades" and as an instrument in "the battle for truth and justice." See: Leopold Happisch, "Der Bildungswert der Touristik," *Bildungsarbeit. Blätter für sozialistisches Bildungswesen* Nr. 7 (1909), 7.
[77] Angelo Carraro, "Tourist und Naturkunde," *Der Naturfreund* 15 (1911), 16-17.

Like the Austromarxists, Carraro first began to evince an interest in the relationship between epistemology, science, and socialist politics in the mid-1900's.[78] And like Friedrich and Max Adler, his work on the topic led him to believe that scientific inquiry was conditioned by the social contexts in which it was produced and used, and that much of what passed as objective knowledge bore the distinct imprint of contemporary capitalism. "We live in an age of natural science, or more accurately, we live in an age of the *capitalist application* of scientific research," he wrote in *Der Naturfreund*, which meant that "a tremendous portion of academic research must pass under the yoke of capitalism in order to gain entrance into the domain of modern society."[79] He saw the Natural History Department as a way to provide workers with a version of the scientific worldview that had not been distorted by the economical imperatives of the bourgeoisie and to equip them with "an armory" of facts that they could use in their battle for social elevation and emancipation.[80]

Both Carraro and *Die Naturfreunde*'s leadership were especially interested in using the department to pique working-class interest in biogeography, ecology, and other fields that stressed the interconnectedness of natural phenomena, as they felt that there was a clear connection between a holistic conception of nature and the socialist worldview. Put in Carraro's words, the political value of natural history was tied to its capacity to show workers that "valley and mountain, bush and heath…are all part of a whole, and should be researched and understood as a whole."[81] Schmiedl agreed, declaring that his support for the new *Sektion* was premised on his belief that it could propagate the view "that everything is tied by a thousand threads to its environment; a

[78] Angelo Carraro, "Gedanken über Umfang und Tendenz der Naturbeobachtung," *Pädagogisches Jahrbuch* 30 ed. Theodor Steiskal (Vienna: Manzsche K.u.K. Hof-Verlags und Univ.-Buchhandlung, 1909), 65-66.
[79] Ibid.
[80] Angelo Carraro, "Tourist und Naturkunde," 17.
[81] Ibid.

thought which pushes with irresistible force the conviction that man can only exist, feel comfortable, develop his power, fulfill his tasks, with the help of his brethren."[82]

In terms of more specific arguments, one point that Carraro repeatedly stressed in his popular texts and lectures was that relations among Alpine biota were not exclusively governed by the kind of cutthroat struggle for survival that social Darwinists used as justification for capitalism but also by mutual cooperation and aid.[83] He was not the first *Naturfreunde* pedagogue to use ecological facts and perspectives to question the biological importance of competition. In a 1907 article on Alpine flora, the teacher and naturalist Anna Pehersdorfer noted that the life of mountain flowers was not only characterized by "a continual battle for existence" but by membership in "living communities (*Lebensgemeinschaften*)" that revolved around "mutual support with other plants and animals."[84] He was nevertheless more explicit than Pehersdorfer in drawing out the political relevance of the *Lebensgemeinschaft*, arguing that the intricate forms of cooperation and dependence that linked its constituent organisms together were "meaningful from an economic standpoint" because they revealed that there was little scientific justification for the bourgeoisie's exploitation and immiseration of labor.[85] He stressed this point even more urgently after war broke out in 1914, writing that recent ecological research had provided further proof that "cruel natural selection" was only one among many mechanisms of evolution, and that "peaceful

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[82] Georg Schmiedl, "Der moderne Mensch und die Natur," *Der Naturfreund* 13 (Vienna: Alois Rohrauer, 1909), 272-274.
[83] For more on social Darwinism in fin de siècle Central Europe, see: Alfred Kelly, *The Descent of Darwin: The Popularization of Darwinism in Germany, 1860-1914* (North Carolina; The university of North Carolina Press, 1981), 100-123.
[84] Anna Pehersdorfer, "Alpenvegetation," *Der Naturfreund* 11 (1907), 70.
[85] Angelo Carraro, "Was da kreucht und fleucht," *Der Naturfreund* Vol. 18 (1914), 135-136.

compromise…and mutual aid" were far more important to biological life than conflict and expropriation.[86]

Another lesson that Carraro hoped to impart through his work with the Natural History Department was that workers were themselves part of an ecological community, and that much of what bourgeois society had taught them about their obligations to the other organisms was false. Contrary to the "superstition" that one could classify and evaluate animals and plants solely in terms of use-value, he explained to his readers that every living thing "occupies an important place in the household of nature," and that the flora and fauna of the world, like the workers themselves, did not exist solely for the use of others.[87] In drawing attention to human membership in broader ecological networks, he also sought to break down the ontological distinction between natural and social, and thus to further instill the notion that human society was part of, and actively contributed to, the evolutionary development of the *Kosmos*.[88] Put another way, Carraro adhered to the Austromarxist view that it was essential to convince workers that they weren't pawns in the hands of external forces, and saw his popular work on ecology and evolution as a way of showing them that they could be "pioneers of a better future, but only if they want it!"[89]

In addition to attacking bourgeois assumptions about individual competition, natural selection, and humanity's relationship to the natural world, Carraro intended for his popular analyses to counteract the alienation and lack of class solidarity that inevitably emerged from

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[86] Angelo Carraro, Review of *Allgemeine Biologie* by Paul Kammerer, *Der Naturfreund* 20 (1916), 179; Angelo Carraro, "Von der Wohnungsfrage in der Pflanzenwelt," *Der Naturfreund* 19 (1915), 194-195.
[87] Carraro, "Was da kreucht und fleucht," 135.
[88] Like many other members of *Die Naturfreunde*, Carraro's understanding of ontological foundation of this cosmic unity was monistic but not materialistic, insofar as he spoke of all phenomena as being fundamentally interconnected without ever suggesting that they were reducible to matter-in-motion. Pehersdorfer's monism, which embraced the concept of "plant souls," was more idealist. See: Anna Pehersdorfer, "Naturfreunde," *Der Naturfreund* (1910), 73.
[89] Angelo Carraro, "Erfinderin Nature," *Der Naturfreund* 16 (1912), 280.

capitalist conceptions of ownership and sovereignty, whether in the form of private property or the nation. *Die Naturfreunde*'s leadership had hoped that the organization would contribute to the destruction of the latter two ideas from the very beginning. In a speech commemorating club's founding in 1895, Renner repeatedly emphasized the importance of bringing workers into contact with nature so that they could learn to "laugh at those who would divide up the world" into private parcels and to recognize their alienation from resources that were the common property of all.[90] He brought up this point again in 1907, declaring that outdoor activities enabled workers to recognize that the world was not the property of individuals but of humanity as a whole, and that they themselves were not members of singular nations but "world-citizens" of a supranational entity united by labor.[91]

Whereas Renner often assumed that the aesthetic experience of being in the mountains or forests sufficed to combat the alienation produced by capitalism, Carraro sought to bolster working-class solidarity and class-consciousness with concrete facts and observations drawn from the field of biogeography, which analyzed the distribution of animal and plant life. In a 1913 article on animal migration, for instance, he claimed that the migratory patterns of certain species were like the migratory patterns of human laborers, insofar as "questions of the stomach" compelled both the former and the latter to rove across local and national borders in a search of resources. More importantly, in both cases it was the causes and conditions surrounding migration—not place of birth—that bound individuals together as part of a common class.[92] This was not to say that proletarians were no better than animals or solely defined by mobility, but that underneath the

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[90] Karl Renner, "Der Arbeiter als Naturfreund und Tourist," *Der Naturfreund* 30 (1926), 3.
[91] Karl Renner, quoted in "Unser Ehrentag," *Der Naturfreund* 11 (1907), 174.
[92] Angelo Carraro, "Das Tierische Wandern, *Der Naturfreund* 17 (1913), 288-289.

façade of national difference the workers of the world were united by common exigencies and practices.

Although Carraro and Renner primarily viewed the Natural History Department as a way to provide the proletariat with a socialist perspective on the natural world, they also saw it as a means of reforming the scientific enterprise itself by gradually incorporating workers into the professional research community. From Renner's perspective, the inclusion of proletarian naturalists in specialist circles would be mutually beneficial to all involved: workers could participate in an important form of cultural production from which they had been almost totally excluded and scientists could take advantage of the "the untold talent that sleeps within the people and which today is buried under the relentless glacial stream of capitalism."[93] Like Mach and Rothe, he also maintained that the working-classes possessed a unique way of observing and understanding nature that was rooted in their intimate practical experience with natural forces and events, and that they were therefore a valuable resource for scientists in their quest to construct a complete picture of the world.[94]

In the longer term, however, Renner not only hoped that *Die Naturfreunde* would contribute to the creation of a more egalitarian research community but that it would help workers "conquer the natural sciences" and put them in the service of "the political work of the proletariat."[95] Carraro shared a similar vision of the club's ultimate purpose, writing it—and the Natural History Department in particular—should work to "free the sciences from the shackles of anti-social powers" by creating "an international scientific center of proletarian scholars" that fully

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[93] Karl Renner, "Vorbemerkung" to Hans Filzer, "Versunkene Wälder: Eine heimatliche Eiszeitstudie," *Der Naturfreund* 18 (1914), 209.
[94] Karl Renner, "Dr. Karl Renner über die Naturfreunde," *Der Naturfreund* 35 (Vienna: Leopold Happisch, 1931), 104-105.
[95] Ibid.

synthesized the domains of research and labor.[96] In this respect, *Die Naturefreunde* not only embodied the Austromarxists' desire to transform the natural sciences into instruments of socialism but the SDAPÖ's belief that focusing on *Bildungspolitik* in the present would yield revolutionary change in the future.

Conclusion

For all its broad similarities to socialist parties elsewhere in fin de siècle Europe, the SDAPÖ was characterized by a unique set of interests and preoccupations. Unsurprisingly, party intellectuals and officials spent much of their time trying to figure out how to construct a coherent and politically conscious proletariat in a state that was characterized by its economic backwardness and ethnic heterogeneity. Many of those same intellectuals and officials also evinced a deep philosophical interest in the natural sciences, particularly after Eduard Bernstein's attacks on the scientific status of Marxism in the late 1890's. For a small but influential group of Viennese intellectuals, the "Austromarxists," the SDAPÖ's ability to successfully respond to the cultural and socio-political challenges posed by life in the Dual-Monarchy was in fact contingent on its ability to come up with a new and more adequate theory of what the *Wissenschaften* were and how they were connected to the socialist project and worldview. Between 1904 and 1914, they set about providing this new theory themselves.

Like many other thinkers of the period, the Austromarxists were heavily influenced by the neo-Kantian and Machian philosophies that had come to dominate Central European epistemology in the last quarter of the nineteenth century. For Max Adler, Otto Bauer, and Karl Renner, the former school of thought was more important, while Friedrich Adler inclined towards to latter.

[96] Carraro, "Tourist und Naturkunde," 16-17.

Despite these philosophical differences, they found common ground on three core positions. First, they maintained that ontological materialism was philosophically misguided and prone to breed political apathy and fatalism among the working classes. Second, they argued that social forces and structures played a critical role in determining what counted as scientific knowledge. And third, they held that the true value of science education, pace the *Volksheim* and *Freie Schule* movements, was not to provide neutral facts but motivate and guide political action.

The Austromarxists found an outlet for their philosophical views in *Die Naturfreunde*, a hiking club that Renner, Georg Schmiedl, and Alois Rohrauer founded in 1895, and which had become one of Austria' most popular voluntary organizations by the mid-1900's. The relationship between *Die Naturfreunde* and socialist *Bildungspolitik*, Austromarxist or otherwise, was initially weak. After 1907, however, Renner and other members of the club's leadership began to emphasize its importance as a conduit for SDAPÖ politics and pedagogy. One of the most important results of this shift in priorities was the creation of the Natural History Department under the leadership of the naturalist Angelo Carraro, who shared the Austromarxists' vision of science education as way to provide workers with scientific facts that were untainted capitalism and which they could use in their quest for political consciousness and emancipation.

Although Carraro's work for the Department touched on a variety of natural-historical phenomena, he often focused on biogeographical or ecological topics. His emphasis on these fields was reflective of their importance within the domain of professional natural history as well as his conviction that they supported the socialist worldview and undermined several prevalent assumptions about the biological origins of capitalism. One point that he repeatedly emphasized was that analysis of the "living communities" of the Alps revealed that biological relations were not exclusively governed by competition and "survival of the fittest," but also by mutual aid and cooperation. Similarly, he held that observation of the migratory patterns of animals and first-hand experience of the borderless expanses of the Alps were perhaps the surest ways to highlight the absurdity of bourgeois ideas about private property and nationality.

In the longer term, Carraro and Renner hoped that *Die Naturfreunde*'s pedagogical efforts would incorporate proletarian naturalists into the scientific community as equal participants, and eventually help transform the scientific enterprise into something that was rooted in, and served the interests of, the working-classes. Their understanding of popular lectures and texts as tools for shaping scientific discourse and inquiry was not only aligned with the Austromarxist perspective on the genre but that of many of the Dual-Monarchy's practicing natural scientists. Carraro's work on natural history for *Der Naturfreund*, Ernst Mach's essays on thermodynamics for *The Monist*, and Richard von Wettstein's article on systematics for *Der Kultur der Gegenwart* were very similar kinds of texts, in other words, because their authors viewed them as part of the process of producing and refining expert knowledge. Moreover, Carraro, Mach, and Wettstein maintained that the popular genre was scientifically useful for roughly the same reason: it brought "everyday" thought to bear on specialist discourse in some way. When the socialists claimed that their popularizations were part of a broader effort to compel professional scientists to acknowledge the perspective of labor, they were articulating virtually the same point that Mach had made in his *Popular Scientific Lectures*, where he argued that specialists could counter disciplinary myopia by engaging with quotidian ideas and experiences.[97]

The Austromarxists nevertheless spoke to fundamentally different audiences than Mach and Wettstein. Whereas the latter primarily addressed their popularizations to other specialists,

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[97] Ernst Mach, "Introduction," *Popular Scientific Lectures* 5th ed., trans. Thomas McCormack (Chicago: The Open Court Publishing Company, 1943), vi.

scientists outside of their respective disciplines, and to a highly educated sub-section of the general public, which is to say to people who had attended *gymnasia* or other institutes of higher learning, Carraro and his comrades intended for their popularizations to reach the working and lower-middle classes.[98] Because the two groups wrote for different audiences, their work was markedly different in terms of content and tone. The articles that appeared in *Der Naturfreund* did not include footnotes, technical language, or any of the other scholarly flourishes that were present in many of Mach and Wettstein's popular texts, nor did they presuppose a significant familiarity with the topic at hand. Carraro's work was also far more didactic than Mach's because it was intended to guide workers to a very specific set of ideas and instruct them to think in highly delimited ways. Put another way, while the professional scientists' popular expositions tended to be similar to scholarly articles, and therefore took the form of an academic discussion among equals, the socialists' popularizations were argumentatively and stylistically similar to what one might find in a primer or elementary textbook.[99] As the concluding chapter of this dissertation will argue, both forms of popularization would continue to flourish in the interwar period as Austrian intellectuals applied them to the scientific challenges raised by Mendelism, quantum mechanics, and the advent of the First Austrian Republic.

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[98] Given the time and resources required to take a daytrip in the mountains, to say nothing of an extended expedition, it is likely that many of *Die Naturfreunde*'s members were part of what Engel's called the "aristocracy of labor," which is to say low-level white-collar workers, skilled artisans, etc.

[99] As previous chapters of this dissertation have shown, many popularizations were so indistinguishable from scholarly articles that they were published in academic journals like the *Annalen der Physik*.

Conclusion: Popularization in Interwar Austria and Beyond

Ernst Mach died in Vaterstetten, a small town near Munich, on February 19th, 1916. Although he was wracked by various ailments and illnesses throughout the last years of his life, he still managed to publish one final monograph, *Kultur und Mechanik* (1915), prior to his passing.[1] Like many other texts in his popular oeuvre, *Kultur* provided a general and relatively non-technical analysis of mechanics which focused on an aspect of the field that he had only cursorily examined in his previous work on the topic: the origin and development of mechanical concepts, instruments, and practices in prehistoric societies. His primary goal in articulating the protohistory of human reasoning about phenomena like force and mass was to provide new evidence in support of his longstanding claim that the concepts and laws described in Newton's *Principia Mathematica* were historically contingent, and that groups of people that academic physicists tended to look down on had played an important role in their construction. As in his 1883 *Science of Mechanics*, he placed particular emphasis on the epistemic contributions of artisans, laborers, and tradespersons, writing that "however highly one wants to estimate the work of academics, the work of simple workers and observers is the necessary foundation preceding and determining the former."[2]

In addition to shedding light on the ancient origins of mechanical reasoning, Mach hoped that *Kultur* would further demonstrate the tremendous epistemological benefit of bringing multiple perspectives to bear on scientific phenomena; a point he had repeatedly emphasized in *Analysis of*

[1] Mach's unfinished *Principles of Physical Optics* did not appear in print until 1927, but he had already written substantial portions of the latter text several decades prior to drafting *Kultur und Mechanik*.
[2] Ernst Mach, *Kultur und Mechanik* (Stuttgart: W. Spemann, 1915), 19.

Sensation and *Knowledge and Error*. Like these earlier popular texts, *Kultur* not only utilized approaches and findings drawn from biology, physics, and physiology but from humanities disciplines like history and experimental psychology. The latter occupied a particularly important place in his analysis because he thought that it offered a window into the kind of instinctive experiences and intuitions that were at the core of all knowledge. *Kultur* was also far more anthropological than his previous monographs, and its arguments simultaneously reflected his growing interest in the field as well as his burgeoning professional relationship with the Austro-American curator Robert Lowie, who had written a series of ethnologies that helped stoke his curiosity about where "practical mechanics" came from and how it influenced later theories.[3]

Although *Kultur und Mechanik* did not achieve the same level of notoriety as Mach's classic popular monographs of the 1880's and 1890's, it was a fitting capstone to his enormously influential career in several respects. For one thing, it neatly encapsulated the core elements of the "biological and economic epistemology" he had been developing since his days as a *Privatdozent* at the University of Vienna, including his claim that ideas were mental adaptations that emerged in response to immediate practical need, and that scientific research was a more evolved form of artisanal tinkering. The text also added further empirical depth and plausibility to his "phylogenetic" account of physical reasoning, which had not yet adequately accounted for the span of time between the formation of the first human societies and the initial formalization of the physical worldview in ancient Greece. Lastly, *Kultur* was characteristic of the approach to popularization he had refined over the preceding six decades. Indeed, the text was not just emblematic of how Mach used the popular genre but, as the preceding four chapters of this

[3] Ernst Mach to Robert Lowie, Vienna, February 8, 1913, in *Ernst Mach's Influence Spreads* ed. John Blackmore, Ryoichi Itagaki, and Setsuo Tanaka (New Hampshire: Sentinel Open Press, 2009), 192.

dissertation have shown, of how many other fin de siècle Austrian intellectuals did as well, including the botanists Richard von Wettstein and August Ginzberger; the naturalists Karl Rothe and Angelo Carraro; and the Austromarxists.

The first and perhaps most fundamental commonality of the popularizations produced by Mach, Wettstein, et. al. was that they were meant to do scientific work. That is, the Austrians not only conceived of popular representation as a means of disciplining and edifying the lay public but as a mode of scientific communication that was not altogether different from more technical formats like the scientific article. As *Kultur und Mechanik* amply demonstrates, one of the basic scientific aims of Mach's popular corpus was to convince physicists to adopt a new philosophical perspective on their discipline's intellectual tools and methods. Or as he noted in the introduction to his *Popular Scientific Lectures*, his career-long interest in the popular genre stemmed from his belief that it was uniquely suited to draw his colleagues' attention to a class of facts that were absent from specialist discourse but nevertheless essential to fully understanding the form and content of physical reasoning. And while much of his popular work was concerned with the field of mechanics, he also dedicated a significant number of lectures and texts to topics in thermodynamics, beginning with his 1871 *History and Root of the Principle of the Conservation of Energy* (1871) and peaking with the 1895 *Principles of the Theory of Heat: Historically and Critically Elucidated*.

As the first chapter of this dissertation argued, Mach intended for these popular texts on thermodynamics to clarify various disputes and technical matters within the field, including the "energetics controversy" of the early 1900's, which pitted supporters of the mechanical theory of heat against those who favored a theory based on the primacy energy. His argumentative strategy was twofold. First, he sought to demonstrate that both the mechanist and energeticist sides of the debate were misguided, insofar as their explanations relied on the intuitive but fundamentally flawed concept of substance. And second, he attempted to position his own phenomenological approach to physics, which dispensed with concepts like substance and cause altogether, as a more appropriate way of representing thermodynamic phenomena.

Kultur and Mechanik was also characteristic of Mach's tendency to use his popular work to draw attention to the scientific value of interdisciplinary analysis and cooperation. And while his foremost concern was fostering closer contact between experts in different academic fields, he also sought to integrate artists and laborers into the scientific enterprise based on the belief that they had valuable first-hand experience and knowledge of various phenomena. This dissertation's second chapter analyzed his strategy for facilitating these wide-ranging interdisciplinary exchanges, arguing that he used his popular lectures and texts to unearth the empirical, methodological, and ontological connections obtaining between different domains of intellectual activity. It then argued that his efforts to build bridges between different forms of expertise were part of a shorter-term project to rid scientific reasoning of the mind-matter distinction as well as a longer-term project to unify the different branches of knowledge into a "complete science" that reflected the monistic unity of nature rather than the arbitrary distinctions imposed by disciplinary inquiry.[4]

Mach's understanding of popularization as a vehicle for furthering his scientific aims was shared by the botanist Richard von Wettstein and many of his colleagues in the Austrian biological community, including his onetime assistant K.C. Rothe and former student August Ginzberger. In Wettstein's particular case, he hoped that his popular analyses of evolution and speciation would

[4] Ernst Mach, *Analysis of Sensations and the Relation of the Physical to the Psychical*, trans. C.M. Williams (Chicago: The Open Court Publishing Company, 1914), 312, 341.

help resolve the ongoing conflict between neo-Darwinian and neo-Lamarckian biologists by articulating a conciliatory evolutionary theory which favored the inheritance of acquired characteristics but acknowledged the limited legitimacy of natural selection. He also intended for his popular work to help secure public and scientific support for his research program in biogeography and to bolster the fortunes of the flagging subfield of plant systemics. Wettstein was joined in the latter endeavor by Ginzberger and Rothe, who used their own pedagogical and popular work to critique the nascent field of plant ecology, which was in no small part responsible for the decline of systematics, and to resurrect interest in taxonomic issues and practices.

Like Wettstein and his associates, Angelo Carraro also used his popular accounts of evolution and taxonomy as a means to scientific ends, but the ends he chose to pursue differed considerably from those of his fellow naturalists. This dissertation's fourth chapter argued that the difference between what Carraro and Wettstein hoped to accomplish with their popular work was not only a reflection of their divergent research interests but of Carraro's post-revisionist conception of socialism and the scientific worldview. Specifically, Austrian socialists had ascribed tremendous ideological and political importance to natural science since the inception of the SDAPÖ in 1888, but the revisionism controversy of the late 1890's spurred a small cadre of intellectuals within the party—the "Austromarxists"—to reevaluate its theoretical meaning and political function. By the mid-1900's, Max Adler, Friedrich Adler, Karl Renner, and several other members of this cadre had formulated a new philosophy of science and nature that not only denied the determinism and materialism of orthodox Marxism but rejected several notions that were prevalent among older SDAPÖ leaders like Victor Adler, including the belief that liberal scientists could be relied on to produce neutral knowledge. As debates within the journal *Der Kampf* demonstrate, the Adlers and their comrades were particularly worried that Darwinian biologists were using their research on ecology and evolution to propagate an anti-socialist agenda and called for the SDAPÖ to take charge of instructing the working classes about these topics itself.

One of the organizations that the Austromarxists felt they could rely on to communicate their politically charged vision of the natural world to the proletariat was *Die Naturfreunde*, a socialist hiking club that Renner and several friends founded in 1895. And one of *Die Naturfreunde*'s most important pedagogues and popularizers was the naturalist Carraro, who founded the club's "Natural History Department" as a means of helping its members read the political lessons that were implicit in biological phenomena. In his writing on ecology, for example, he stressed that biological relationships were not solely defined by cutthroat competition but by cooperation and mutual aid. He also shared the Austromarxists' hope that piquing working-class interest in scientific research would lay the groundwork for the emergence of a scientific counterculture that was rooted in, and served the interests of, the proletariat. Put in Carraro's own words, the point of *Die Naturfreunde* was not only to "free science from the shackles of anti-social forces" but to provide an institutional framework for the emergence of "a working-class scientific international."[5] In this respect, Carraro shared Mach and Wettstein's vision of popularization as an instrument of scientific reform, although the reforms he hoped to institute were far more radical than merely re-centering systematics.

The second fundamental commonality that linked the Austrian popularizers together was their shared belief that the scientific value of the popular genre was directly related to its capacity to mediate between the experiential and intellectual world of the "average man" and that of the professional researcher. That is, Mach, Wettstein, and the Austromarxists were all convinced that

[5] Angelo Carraro, "Tourist und Naturkunde," *Der Naturfreund* (1911), 16-17.

everyday experience, commonsense reasoning, and public opinion were potent epistemic resources and that popular representation was the best way to bring them to bear on scientific discourse. Mach began to express an epistemological interest in the quotidian in the early 1860's as a result of his research in sensory physiology, which helped convince him of the biological nature of cognitive phenomena, and because of his embrace of Darwinism. As he recalled in an autobiographical sketch, *Origin of Species* did not introduce him to the idea of descent with modification but it did help solidify his conviction that all organic phenomena, ranging from ideas to organs, could be explained in evolutionary terms, and that to fully understand scientific facts and theories one had to analyze their intellectual ancestors in popular and instinctive thought.

Mach first explicitly suggested that popular texts were an appropriate medium for engaging in conceptual phylogenetics in his 1866 *Introduction to Helmholtzian Music Theory*, where he declared that the practice of producing popular representations was "not indifferent to the development of science itself" because it clarified the evolutionary relationship between ideas that were sealed away in the "scientific literature" and those that were "generally widespread, or popular."[6] He raised this point again his *Popular Scientific Lectures*, writing that the fundamental purpose of his popular corpus was

> to exercise a favorable influence by showing the substantial sameness of scientific and every-day thought. The public, in this way, loses its shyness towards scientific questions, and acquires an interest in scientific work which is a great help to the inquirer. The latter, in his turn, is brought to understand that his work is a small part only of the universal processes of life (*allgemeinen Entwicklungsprozesses*).[7]

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[6] Ernst Mach, *Einleitung in die Helmholtzsche Musiktheorie: Populär für Musiker dargestellt* (Graz: Leuschner and Lubensky, 1866), V-VII, 2-4.

[7] Ernst Mach, "Introduction," *Popular Scientific Lectures* 5th ed., trans. Thomas McCormack (Chicago: The Open Court Publishing Company, 1943), vi.

Of course, by the time *Popular Scientific Lectures* went to press in 1895 he had constructed a far more nuanced theory of how scientific ideas evolved out of commonplace ones than he possessed in 1866. Perhaps most importantly, by the early 1890's he had clarified the role of everyday thought at different stages of the research process and identified the precise point at which it ceased to be scientifically useful.

Curiously, the evolutionary considerations that framed Mach's perspective on the scientific salience of the everyday played a far less important role in convincing Ginzberger, Rothe, and Wettstein that popular ideas and opinions were relevant to specialist science. Rather, the biologists' interest in public discourse stemmed from their recognition that the veritable avalanche of popular work on Darwinism that started appearing in the late 1860's had perceptibly influenced the form and content of research in the discipline, and that it would continue to do so for the foreseeable future. Or as Wettstein explained at the *Versammlung Deutscher Naturforscher und Ärtze* in 1912, it was imperative for the scientific community to recognize that the advent of mass media had fundamentally altered the way that scientists produced and communicated knowledge; and that the intellectual and institutional agendas of specialist science—and of biology in particular—were increasingly driven by the sensational and often speculative representations contained in popular-scientific texts.[8] Although the Austrians were particularly critical of this development in the scientific literature, they also used their own popular and pedagogical work to shift public and scientific opinion in their favor. They directed many of their popularization efforts towards amateur botanists, students, and provincial teachers, seeing interest from these three groups as essential for the continued health and vitality of their research.

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[8] Richard von Wettstein, "Die Biologie in ihrer Bedeutung für die Kultur der Gegenwart," *Verhandlungen der Gesellschaft Deutscher Naturforscher und Ärzte: 84 Versammlungen zu Münster* (Leipzig: F.C.W. Vögel, 1913), 217-225.

Given that several of the Austromarxists were followers of, or at least sympathetic to, Machian philosophy, it is unsurprising that they framed their epistemological interest in the everyday in Machian terms. The physicist and politician Friedrich Adler began to subscribe to Mach's biological and economic theory of knowledge, including his claim that popular thought represented a phase in the broader intellectual evolution of the species, around 1904. But other key members of the Austromarxist group, including the sociologist Max Adler, rejected Mach's naturalism in favor of a neo-Kantian perspective that placed far more emphasis on the institutional and social determinants of what passed as commonsense knowledge. Despite these philosophical differences, both factions agreed that there was a constitutive relationship between human cognition and mind-external factors, and that to transform the sociopolitical environment of the Habsburg state they not only had to alter its material conditions but to change the way that workers thought.

Although the Austrians' beliefs about the utility of popular representation and scientific relevance of popular thought were directly shaped by various large-scale events and transformations in European science, ranging from the Darwinian revolution to the advent of mass media, this dissertation also suggested that they were reflective of distinctly local—and in some cases individual—concerns and interests. The origin of Mach's conception of popularization was perhaps the most idiosyncratic. As he remarked in his 1871 *History and Root of the Principle of the Conservation of Energy*, one of the reasons that he initially decided to adopt popularization a mode of scientific communication was because he found it difficult to get his admittedly heterodox critiques of mass and space published in specialist venues. He directed much of his ire towards the *Annalen der Physik*, writing that physics' flagship Germanophone journal would sooner publish "pages of fallacies about Torricelli's theorem and the blush of dawn," provided they were written in “physical language,” than entertain a coherent article that was “not wholly written in that jargon,” which compelled him to seek out alternative venues for his non-technical essays, ranging from medical textbooks to general-interest magazines.[9]

Mach’s understanding of popularization was also conditioned by his experiences as a student and *Privatdozent* at the University of Vienna in the 1860’s, which were not always or even mostly positive. He found the professors in the physics department to be somewhat aloof, for example, and complained that the university’s research facilities were too poorly appointed to support the experimental program that he wanted to pursue. Even worse, he was too poor to move to the University of Königsberg, where he would have had access to the advising and apparatus that he needed. But these challenges would also prove critical for his intellectual development. The financial precarity of his situation compelled him to give popular lectures to make ends meet, which helped him to better understand the genre and hone the craft of translating specialist research into generally understandable terms. More importantly, to make up for the university’s lack of experimental resources he chose to divert some of his attention to the field of sensory physiology, which was not only less resource-intensive but an area of strength among the University’s faculty. This unintended shift in research focus would end up being one of the most fortuitous accidents of his career, insofar as his work in the field informed many of the intellectual positions at the core of his philosophical worldview, including his conception of how and why analysis of the relationship between everyday and scientific thought represented a valuable contribution to knowledge.

[9] Ernst Mach, *History and Root of the Principle of the Conservation of Energy*, trans. Philip Jourdain (Chicago: The Open Court, 1911), 71-80.

The Wettstein Circle's understanding of popularization was also conditioned by local factors and influences, the most important of these being the odd admixture of research programs that characterized Austrian botany in the last decades of the nineteenth century. On the one hand, Austrian universities' early investment in novel, laboratory-based fields like plant physiology in the 1870's continually placed the country's practitioners at the forefront of experimental research on the cellular and physico-chemical processes underlying plant life. On the other hand, the powerful influence of Anton Marilaun von Kerner, director of the University of Vienna's Botanical Institute from 1878 to 1892 and a pioneering researcher on Habsburg flora, assured the local vitality of more classic research programs focused on observing, describing, and taxonomically ordering plants in the field. As a result of this institutional and intellectual split, Austrian practitioners often adopted ideas and positions that were at odds with those of the broader biological community. This was particularly true of Wettstein and his students, who found themselves simultaneously defending a novel form of neo-Lamarckism *and* an ostensibly outmoded approach to plant systematics around 1900. But the marginality of their methodological and theoretical commitments also made them highly attuned to the fact that biological discourse was not only driven by scientific articles and experiments but by popular texts.

Lastly, Carraro and the Austromarxists' conception of popularization as a tool for scientific and political reform was not only reflective of their embrace of Machian and neo-Kantian epistemologies but of the political philosophy of the SDAPÖ, which had emphasized the revolutionary importance of working-class education since the 1880's. Indeed, if there was one thing that distinguished the SDAPÖ from its counterparts elsewhere in Europe, it was its members' preoccupation with the proletarian mind and their belief that changing the way that workers thought was the key to political progress. This is not to say that Victor Adler and other leading members of the party were unconcerned with seizing the modes of production or other forms of material intervention, but that they believed that the backwardness of the Austrian working classes made such actions unfeasible, and that it was necessary to change the way that their constituents reasoned about the world around them before creating a new political system. In this respect, Carraro, Mach, and Wettstein were not only linked by their common understanding of popularization as a means of doing scientific work or by their interest in everyday thought but by their intellectual and institutional marginality vis-à-vis the rest of Europe, which led them to embrace a heterodox form of communication.

Popular Science in Interwar Austria, 1919-1938

Although Mach did not live to see the end of the First World War, Carraro, Ginzberger, Wettstein, and many other Austrian scientists did. The world they inhabited after the ink dried on the treaties of Versailles and Saint-Germain in 1919 was far different from the one they knew in 1914. The most jarring change brought about by the post-war settlement was the dissolution of the Habsburg Monarchy into a number of independent successor states, including Czechoslovakia, Hungary, the Second Polish Republic, the Kingdom of Yugoslavia, and the First Austrian Republic, which elected Karl Renner as its first president. Unlike their former countrymen to the north and south, many Austrians across the political spectrum were decidedly pessimistic about the prospects of their new state, believing that it was both economically and politically "unviable" (*Lebensunfähig*).[10] Their pessimism was bolstered by the myriad crises that immediately beset Renner's young government, including: a growing international conflict over the fate of the former "northern provinces" of Bohemia and Moravia; unrest in the former crownlands to the west of

[10] Helmut Gruber, *Red Vienna: Experiment in Working-class Culture, 1919-1934* (Oxford: Oxford University Press 1991), 24-25.

Vienna; and a powerful movement among Social Democrats for *Anschluss* with Germany.[11] These already severe political problems were further compounded by widespread disease, hunger, and homelessness. According to historian Maureen Healy, the deprivations of the immediate postwar period were so severe that between 1919 and 1921 foreign aid missions transported one in three Austrian children outside of the country for the purposes of "revitalization."[12]

Given the dire state of everyday, political, and social life in Austria during the early years of the First Republic, it is unsurprising that Austrian scientists also found themselves suffering from various hardships. One of the most basic problems facing Germanophone researchers after the war was that there was very little funding available for the purchase of scientific instruments, rebuilding library collections, or launching research programs that did not promise immediate social benefit. Senior scientists also had to contend with a lack of qualified assistants, curators, junior researchers, students, technicians, and other critical sources of academic labor, which made it difficult to keep their facilities in order, to say nothing of taking on new projects. Wettstein noted as much in a melancholy report to the *Society for the Conservation of Alpine Plants* in 1920, writing that the experimental gardens he had carefully built in the 1890's and 1900's had steadily deteriorated as a result of their caretakers being called up for duty, and that their primary function in recent years had not been the cultivation of useful specimens but the provision of flowers for the graves of soldiers who were killed in action.[13] He and his colleagues also found that the armistice had done little to ameliorate their labor shortage because many of the young people that

[11] John Boyer, "Silent War, Bitter Peace: The Revolution of 1918 in Austria," *Austrian History Yearbook* (Vol. 34, January 2003), 38.
[12] Maureen Healy, *Vienna and the Fall of the Habsburg Empire: Total War and Everyday Life in World War I* (Cambridge: Cambridge University Press, 2004), 255.
[13] Richard Wettstein, "Bericht über das Alpengarten der Raxalpe," *Bericht des Vereins zum Schutz der Alpenpflanze* 14 (1920), 22-26.

flooded back to the universities from the front were far less interested in dedicating themselves to scientific research than they were in completing their studies as quickly as possible and finding gainful employment.[14]

An even greater problem than empty research institutes or lack of funding, according to Wettstein, was the crisis of confidence that the Central Powers' defeat had caused among Austrian and German intellectuals, which was manifest in a concerted lack of mental energy and interest in taking steps to preserve what remained of their previously formidable scientific apparatus.[15] Drawing on his nearly boundless energy and talent for scientific administration, Wettstein attempted to resolve this crisis by establishing an organization that would render emergency aid to scientists in need and prepare the community to once regain its place atop the scientific world. Although Germanophone scientists would never really shake the feeling that their intellectual culture was in crisis or attain the level of scientific preeminence they enjoyed in the latter half of the nineteenth century, a dramatic uptick in the production of German-language scientific articles and journals in the mid-1920's indicates that they attained something resembling their pre-war productivity.[16]

The growing scientific output of the mid-1920's not only included technical work but popularizations, which Austrian scientists continued to view as valuable tools for producing and refining expert knowledge. Indeed, although their world had changed dramatically between 1914 and 1919, the botanists Ginzberger, Frierich Vierhapper, and Wettstein used their popular lectures

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[14] Erwin Janchen, "Richard Wettstein: Sein Leben und Wirken," *Österreichischen Botanischen Zeitschrift*, Vol. 82, No. 1/2 (1933), 27.
[15] Richard Wettstein, "Die Notgemeinschaft deutscher Wissenschaft und Österreich," *Neue Freie Presse* (Nov. 7th, 1920), 2-3.
[16] Werner Hollmann, *Die Zeitschriften der Exakten Naturwissenschaften in Deutschland* (Birkeneck: Schloss Birkeneck, 1937), 65. As Hollmann notes, this uptick in production coincided with the end of hyperinflation in Germany.

and texts to further scientific agendas that they had first articulated during the fin de siècle period. In 1924, Wettstein once again turned to the popular genre as a means of mediating between neo-Darwinian and neo-Lamarckian theories of evolution. The contours of the debate had of course changed in several key respects since the University of Vienna invited him to speak about natural selection and its discontents 1901. Most importantly, Erich Tschermak, Hugo de Vries, and Carl Correns' simultaneous rediscovery of Gregor Mendel's work on plant hybrids in 1900 helped spark the creation of a new area of biological inquiry—genetics—that helped set the stage for the near-complete victory of neo-Darwinism in the years leading up the "modern synthesis" of the 1930's.

At least initially, however, Mendel's rediscoverers and early champions were not looking to bolster Darwinian theory but actively searching for alternatives to it. Wettstein noted as much in 1925, writing that it was only with the decline of the "dogma of Darwinism" in the early 1890's that researchers felt free to pursue the kind of alternative explanations of heredity that would eventually led several of them to independently confirm the arguments contained in Mendel's paper.[17] One of those researchers was de Vries, who came upon Mendel's work in the course of developing a novel theory of evolution which held that organic development and variation were not functions of the gradual accumulation of traits, as Darwin suggested in *Origin of Species*, but of sudden, heritable mutations. William Bateson, who first coined the term "genetics" in 1905, also expressed serious doubts about the explanatory utility of Darwin's theory and preferred a model that resembled de Vries' *Mutationslehre* in its emphasis on dramatic "saltations" in organic form.

[17] Richard Wettstein, "Johann Gregor Mendel," in *Neue Österreichische Biographie, 1815-1918* ed. Anton Bettelheim, Part 1, Volume 2 (Vienna: Amalthea-Verlag, 1925), 9-16.

Whatever the Mendelians' initial intentions, by 1916 it was becoming clear to many within the biological community that research in the nascent field of genetics seemed to eliminate the possibility of the inheritance of acquired characteristics. A key factor in this growing consensus was Thomas Hunt Morgan's famous experimental study of *Drosophila melonogastger,* or the fruit fly. Like Bateson and de Vries, Morgan's early career research was indelibly shaped by the anti-Darwinian backlash of the 1890's. In one of his first monographs, *Evolution and Adaptation* (1903), he counted himself among the growing number of biologists who felt that the "Darwinian school" had become too dogmatic.[18] He also maintained that many aspects of the theory of natural selection could be "profitably rejected," and that researchers should begin seeking alternative explanations of speciation using the experimental techniques that Hans Driesch, Jacques Loeb, and Wilhelm Roux had pioneered in order to analyze the "developmental mechanics" of embryological growth.[19] In the process of trying to hone this alternative explanation through his research on *Drosophila*, however, he noticed a series of sex-linked mutations that seemed to follow the ratios described in Mendel's laws, which diverted his attention away from explaining variation and toward the material, or more accurately chromosomal basis of inheritance.[20] By 1916, his fruit fly studies had not only led him to adopt the Weismannian idea that the physical units of heredity were located on chromosomal structures sequestered within the nucleus of the cell, and that organisms were therefore unable to acquire heritable traits over the course of a lifetime, but to

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[18] Thomas Hunt Morgan, *Evolution and Adaptation* (New York: The Macmillan Company, 1908), vii-ix.
[19] Ibid. Driesch's work on developmental mechanics led him even further away from Darwinism. Around the time Morgan began his fruit fly experiments Driesch began arguing for a for of neo-vitalism premised on the Aristotelian notion "entelechy."
[20] Diana E. Kenney and Gary G. Borisy, "Thomas Hunt Morgan at the Marine Biological Laboratory: Naturalist and Experimentalist," *Genetics* (Vol. 181, no. 3, March 2009), 841-846.

embrace the theory that natural selection working on random genetic mutation provided the best model for explaining evolution.[21]

When Wettstein took stock of these developments in 1926, he remarked that it was impossible to deny the tremendous significance of Mendel's laws for modern biology. He also congratulated Morgan for his "remarkable work" demonstrating that chromosomes played a critical material role in the transmission of hereditary traits.[22] But just as he had refused to be swept up in the scientific mania for Darwinism in the 1870's and 1880's, he now refused to be carried along with the rising tides of Mendelian genetics. His weapon of choice in this struggle was once again the popularization, which he used to provide synthetic pictures of the state of current research and shed light on problems that Morgan and his fellow travelers had overlooked. As with his earlier popular critiques of neo-Darwinism and ecology, he did not adopt the posture of the angry polemicist but the sage mediator, drawing out the advantages and disadvantages of both sides in a way that ultimately led his reader to acknowledge the plausibility of the inheritance of acquired characteristics.

Wettstein articulated one of his earliest popular critiques of contemporary genetics in a short biography of Mendel that he produced for the *Neue Österreichische Biographie* in 1925. His analysis first touched on the circumstances surrounding the long neglect of Mendel's work, which he not only attributed to the rapid and overwhelming acceptance of Darwinism in the 1860's but to the Moravian friar's status as a scientific outsider. He then moved on to Mendel's positive contributions to biologists' understanding of heredity. The most obvious virtue of the latter's now-famous 1866 paper, he claimed, was its provision of a set of experimentally established laws that

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[21] Peter Bowler, *Evolution: The History of an Idea* (California: University of California Press, 2009), 271-272.
[22] Richard Wettstein, "Fünfundsiebzig Jahre Biologie," *Verhandlungen der Zoologisch-Botanischen Gesellschaft in Wien* 76 (1926), 22-24.

clearly described certain regularities in the transmission of traits. "But even more revolutionary" than these laws, he continued, "were the ideas that one must arrive at on the basis of them."[23] Whereas Darwin and many other biologists had argued that the characteristics of a breeding pair became blended in their offspring, Mendel's research had shown that heredity involved the transmission of discrete and freely combinable traits. It had also revealed the "highly important fact" that in the process of reproduction these discrete traits entered into a form of competition wherein the "dominant" traits hindered the expression of the "recessive" ones, which meant that "direct observation of accessible complexes of traits" offered immediate insight into an organism's genetic constitution.[24]

Having established the important shift in perspective brought about by the Mendelian program, Wettstein then briefly turned to its problems. One pressing issue was that botanists' "innumerable investigations" of plant genetics over the previous two decades had revealed that the hereditary patterns of certain plant lineages were more complicated and less predictable than those of Mendel's model organisms. Most notably, some plants exhibited a phenomenon called "genetic coupling," wherein two distinct traits were consistently transmitted to offspring together, which ran contrary to Mendel's law of independent assortment. More broadly, Wettstein suggested that genetics research had progressed so far so fast that biologists had begun to take "too one-sided a view" of the results that they were producing, much as the neo-Darwinians had been unwilling to countenance alternative evolutionary mechanisms three decades earlier.[25] Although he did not clarify exactly what this "one-sidedness" consisted of, or how one could salvage the inheritance of acquired characteristics from the new facts that had been arrayed against it, he would delve into

[23] Wettstein, "Johann Gregor Mendel," 14.
[24] Ibid, 15.
[25] Ibid.

these issues in greater depth over the next two years, first in a ceremonial address he gave to commemorate the seventy-fifth anniversary of the Viennese *Zoological-Botanical Society* in 1926, and again in a popular lecture he delivered the Fifth International Genetics conference in 1927.[26]

Wettstein's analyses in his 1926 and 1927 talks revolved around three gaps and flaws within the Mendelian paradigm. First, he argued that argued that the Mendelians "only focused on one aspect of hereditary phenomena, namely, the inheritance of traits associated with sexual reproduction," and that their "one-sided" focus on the role of chromosomes had led them to neglect other aspects and influences on the transmission of heredity traits, most notably the cytoplasm and hormones.[27] Second, he claimed that many of the experimental results that the Mendelians had produced made it difficult to see how genetics could be synthesized with evolutionary theory in general, and with Darwinism in particular. Many of the genetic mutations that scientists had succeeded in generating up to that point had degenerative, for example, and the available experimental evidence continued to suggest that natural selection merely served to regulate genetic process by selecting against unfit variations. And third, he argued that the idea of immortal genetic lineages raised the philosophical issue of where the "urgenes" of a given branch of the tree of life came from.

Having established the difficulties and limits of Mendelian genetics as a means of explaining heredity, Wettstein then suggested that biologists needed to investigate potential alternatives more fully, including the possibility that environmental factors could directly or indirectly alter an organism's genotype. He ceded that this Lamarckian suggestion was far more

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[26] Although the 1927 lecture was delivered before a scientific audience, it was accessible and "popular" enough to be reprinted in abridged form in the *Neue Freie Presse*. See: Richard Wettstein, "Das Problem der Evolution und die modern Vererbungslehre," *Neue Freie Presse* (October 9th, 1927), 27-28.

[27] Wettstein, "Fünfundsiebzig Jahre Biologie," 22-23.

controversial in 1927 than it was in 1907, and that biologists' suspicion of the inheritance of acquired characteristics was to a certain extent justified, but he also noted that the idea's critics had yet to marshal the empirical evidence necessary to definitively rule it out. Indeed, he claimed that many of the day's foremost opponents of the inheritance of acquired characteristics had let slip various claims that seemed to suggest that they did not "preclude such forms of influence."[28] Outside of an oblique reference to the potential genetic influence of cytoplasm and hormones, he remained curiously silent about the ongoing research program of the Vienna-based *Biologische Versuchsanstalt* (*BVA*), which had done more to show that environmental factors could alter heredity than virtually any other laboratory in Europe.

Wettstein's silence about the *BVA*, which he had supported since its inception in 1902 and personally helped bring under the control of the Austrian Academy of Science in 1913, becomes less curious when one notes that allegations of fraud had been levelled against one of the institute's most high-profile researchers, Paul Kammerer, in 1926. Although Kammerer was an experimental zoologist and Wettstein a field botanist, they had crossed paths numerous times since the early 1900's. The former was a staunch supporter of the latter's efforts to overhaul scientific instruction in the Dual Monarchy's middle schools in 1908, for example, and both were active participants in the extension program at the University of Vienna. An avid aquarist, Kammerer had also tried to convince Wettstein to publish in his journal, the *Blätter für Aquarien und Terrarienkunde*, as part of an attempt to raise its scientific profile.[29] And perhaps most importantly, Kammerer and his

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[28] Richard Wettstein, "Das Problem der Evolution und die Moderne Vererbungslehre," *Verhandlungen des V. Internationalen Kongresses für Vererbungswissenschaft, Berlin, 1927* (Leipzig: Gebrüder Bornträger, 1928), 379.
[29] For more on how Kammerer's passion for aquaria bled into his work at the *BVA*, see: Klaus Taschwer, "From the Aquarium to the Zoo to the Lab: Preludes to the Biologische Versuchsanstalt in the Viennese Wurstelprater," in *Vivarium: Experimental, Quantitative, and Theoretical Biology at Vienna's Biologische Versuchsanstalt* ed. Gerd Müller (Cambridge: The MIT Press, 2017),

BVA colleagues had been among Wettstein's most important and public allies in the battle to demonstrate the inheritance of acquired characteristics.

As Gerd Müller has noted, *BVA* scientists were dedicated to the broadly Lamarckian aim of showing "the reactive plasticity of developmental and physiological processes to environmental stimuli in the generation of form and function" from very early on in the institute's history.[30] They began to produce highly promising results in this direction in the years leading up to the First World War. In 1912, the endocrinologist and *BVA* director Eugen Steinach gained international renown for his research demonstrating that biological sex was not determined by gametes but hormones, and that by manipulating the endocrine system one could alter an organism's potential to be male or female.[31] Around the same time that Steinach began to make waves with his hormone research, Kammerer's complicated breeding experiments began to yield compelling evidence that alterations in environmental conditions could induce heritable traits. In the early 1920's, he joined forces with Steinach to uncover the physiological mechanisms that were responsible for mediating between environment and genotype in general, and between climactic factors like heat and racial attributes in particular, thereby adding yet another set of data in favor of the inheritance of acquired characteristics.

Kammerer recognized that getting biologists to accept his findings was going to be an uphill battle because he, like Wettstein, had adduced that expert debate over evolutionary theory was never just a matter of weighing specific facts and explanations but of adjudicating between competing conceptions of biology and the scientific worldview. There was no way to explain the

[30] Gerd Mueller, "Biologische Versuchsanstalt: An Experiment in the Experimental Sciences," in *Vivarium: Experimental, Quantitative, and Theoretical Biology at Vienna's Biologische Versuchsanstalt*, 15.
[31] For more on Steinach's research, see: Cheryl Logan, *Hormones, Heredity, and Race: Spectacular Failure in Interwar Vienna* (New Jersey: Rutgers University Press, 2013).

"one-sidedness" and "rigid dogmatism" of his opponents, he explained, without also understanding "the development of our political situation."[32] Indeed, as a socialist of Jewish descent who had endured vitriolic public attacks for his critiques of biological essentialism, negative eugenics, and other elements of a nascent fascist biopolitics, he had extensive firsthand experience of the ways that politics could efface scientific discourse on the topic of heredity.

As a result of these sociopolitical impingements on science, Kammerer elected to continue his pre-war practice of using popular lecture and texts to plead his case, writing that the genre was not only an ideal tool for straddling philosophical, political, and scientific concerns but for reaching both academic and lay audiences. One of his most influential popular treatments of evolutionary theory was the 1924 *Inheritance of Acquired Characteristics,* which provided a neat precis of his findings over the previous two decades as well as an analysis of their "eugenical" and sociopolitical implications. He began the text by acknowledging that Weissmann, Mendel, and their acolytes had provided a valuable service to the scientific community, and that their discoveries had not only helped explain a wide array of organic phenomena but sparked "a very necessary and beneficial reaction" against the credulous beliefs of many early Lamarckians.[33] But he then claimed that "this reaction developed into a reaction within itself; that is, to a halt of progress" which was most evident in the widespread but unjustified assumption that the inheritance of acquired characteristics was impossible. The strength of this assumption was unfortunately bolstered, he continued, by the intensification of a form of "nationalistic and racial consciousness" that was opposed to neo-Lamarckism because it implied that race was not an immutable trait, and that environmental factors played a significant role in shaping the categories that defined the

[32] Paul Kammerer, *The Inheritance of Acquired Characteristics*, trans. A. Paul Maerker-Branden (New York: Boni and Liveright, 1924).
[33] Ibid, 15.

individual.[34] Put in simpler terms, many Germanophone scientists rejected the inheritance of acquired characteristics because it implied that Germanness was a contingent property.

Despite the scientific and political power of his opponents, Kammerer nevertheless expressed confidence that the scientific community was coming around to his point of view. He noted with satisfaction that support for Mendelism seemed to have peaked in the early 1920's as a result of the theory's failure to fully explain all the various "manifestations of the descent of races, species, and groups" that appeared in organic world.[35] Reiterating an objection that Wettstein also often raised, he claimed that the opponents of the inheritance of acquired characteristics had not managed to come up with satisfactory evidence showing that natural selection could be a productive rather than purely negative force that merely served to eliminate unfit traits. And finally, he noted that several different researchers had recently produced findings that seemed to support the possibility that the environment could directly induce heritable traits, although he remarked with disappointment that a hostile intellectual climate had compelled them to camouflage this obvious conclusion by couching their results in ambiguous terms like "cumulative after-effect" and "oscillating mutations."[36]

Unlike his colleagues, Kammerer was not shy about explicitly stating that his research vindicated the neo-Lamarckian view. His confidence was largely based on two experiments. First, in his work on midwife toads he found that he was able to use variations in the climate of their enclosures to induce heritable changes to their sexual characteristics. Most notably, by raising the temperature he compelled his specimens to spend more time in the water than they would in the wild, which led the males to gradually develop a set of traits that made mating under aquatic

[34] Ibid, 16.
[35] Ibid, 19.
[36] Ibid.

conditions easier, including a soon-to-be-infamous feature he called "nuptial pads." And second, in a set of experiments he conducted on fire salamanders he was able to demonstrate that by changing the color palates of the organisms' enclosures he could produce lasting changes in their coloration. Taken together, he claimed that these results and numerous other facts presented in *The Inheritance of Acquired Characteristics* offered direct and indirect evidence that it was time to reembrace the biological perspective of "Lamarck, Goethe, and Darwin," which is to say a perspective that accepted the inheritance of acquired characteristics. Just as important, he thought his results suggested that the state should embrace a form of "productive eugenics" which did not strive to eliminate the unfit through extreme measures like forced sterilization but to create an environment which fostered positive characteristics in its population, much like the municipal government of "Red Vienna" was then attempting to do through its massive social welfare programs.

Despite its scientific promise, Kammerer's research program was derailed in 1926 by what were likely spurious claims that he had fraudulently manipulated his midwife toad specimens.[37] Historian Cheryl Logan has persuasively argued that several different, and in certain respects non-scientific factors contributed to his swift downfall. First: his findings were extremely difficult to replicate, although this difficulty was not a result of malfeasance but of the tremendous amount of skill that was required to breed and maintain complex model organisms like salamanders and toads over the course of multiple years. And second: the controversy was intimately related to an ongoing dispute between histologists and physiologists that was ostensibly about scientific ideas and methods but implicitly about sociopolitical issues. Put in Logan's words, Kammerer's work "and

[37] For a classic treatment of the scandal (and defense of Kammerer), see: Arthur Koestler, *The Case of the Midwife Toad* (New York: Random House, 1971).

the evidence supporting it were lost in the socio-scientific storm surrounding the social implications" of biological plasticity and flexible heredity, which made his opponents and even some of his erstwhile allies more likely to accept the charges against him on purely ideological grounds.[38] Several of his enemies went much further than merely picking sides. According to Klaus Taschwer, there is evidence to suggest that a cabal of anti-Semitic professors headed by the paleontologist Othenio Abel (to be discussed shortly) amplified the scandal in the hopes of destroying his career and the *BVA* itself, which was founded by, and continued to employ, a number of Jewish researchers.[39] Although several high-profile scientists came to Kammerer's defense, he committed suicide several weeks after the scandal first went public, arguably taking the entire Lamarckian movement down with him.

Kammerer and Wettstein were not the only Austrian biologists of the interwar period to use their popularizations to do scientific work. While the latter were busy attempting to persuade their colleagues to accept the inheritance of acquired characteristics, Wettstein's former students August Ginzberger and Friedrich Vierhapper were hard at work producing popular lectures and texts that were intended to garner scientific support for their own projects. For Ginzberger, that project was conservationism. He had been deeply involved in various conservation-oriented schemes prior to and during the First World War, including efforts to populate wilderness areas with rare species and to create a dedicated *Naturschutz* committee within the Viennese *Zoological-*

[38] Logan, *Hormones, Heredity, and Race*, 88. See also: Sander Gliboff, "The Case of Paul Kammerer: Evolution and Experimentation in the Early 20th Century," *Journal of the History of Biology*, Vol. 39, No. 3 (Autumn, 2006), 545.
[39] Klaus Taschwer, "Othenio Abel. Palaeontologe, nationalsozialistischer Fakultäts- und Universitätspolitiker," in *650 Jahre Universität Wien- Aufbruch ins Neue Jahrhundert, Volume 2* ed. Friedrich Stadler (Vienna: Vienna University Press, 2015), 147-149.

Botanical Society, and he continued to pursue the issue with the same vigor after the conflict was over.[40]

As a field botanist and biogeographer, Ginzberger's desire to preserve wilderness areas not only stemmed from an aesthetic and quasi-romantic attachment to the idea of unspoiled nature but from a belief that continued human encroachment on wild spaces would do irreversible damage to important objects of scientific investigation. He used popular journals like the *Blätter für Naturkunde und Naturschutz* to draw attention to this danger and inspire his lay and scientific readers to agitate for the provision of more natural monuments and protected areas. He was especially interested in raising awareness of the necessity of conserving marshlands, as he felt that these fragile and scientifically valuable ecosystems were subject to wanton destruction by people who considered them useless.[41] He also used the *Blätter* to apprise amateur and professional botanists of imminent development projects so that they could mobilize to collect specimens and observations before some bit of forest or meadow disappeared. In 1924, for example, he wrote an article warning of the impending construction of a power station in the picturesque Stubachtal area near Salzburg and of the need to save a local variety of moss.[42]

While Friedrich Vierhapper shared Ginzberger's interest in conservation, he primarily used his popular work to help clarify the concepts and methods of the novel botanical sub-field of plant sociology.[43] In many respects, Vierhapper's project was a continuation of K.C. Rothe and

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[40] August Ginzberger, "Der Schutz der Pflanzenwelt in Niederösterrich," *Blätter für Naturkunde und Naturschutz* 1 (April 1st, 1914), 12.
[41] August Ginzberger, "Naturdenkmalpflege in Deutschland," *Blätter für Naturkunde und Naturschutz* 3 (May 1st, 1916), 4-5.
[42] August Ginzberger, "Beiträge zur kenntnis der Pflanzen- und Tierwelt des Alpen Naturschutzpark im Pinzgau," *Blätter für Naturkunde und Naturschutz* 11 (April 1st, 1924), 45-51.
[43] In a eulogy for Vierhapper, who committed suicide after suffering a knee injury that prevented him from doing fieldwork, Ginzberger noted that his decision to publish some of his findings in popular venues was occasionally detrimental to his career because it meant that his research sometimes went unnoticed. See: August Ginzberger, "Friedrich Vierhapper," *Verhandlungen der Zoologisch-Botanischen Gesellschaft in Wien* 82 (1932), 4-28.

Wettstein's pre-war efforts to use popular and pedagogical texts to shape ecological research in ways that conformed to, or at least integrated aspects of, their existing scientific assumptions and commitments. Specifically, Vierhapper shared Wettstein's belief that plant ecologists tended to overfocus on environmental factors that conditioned plant life in the present and to undervalue geological change, evolutionary succession, and other historical influences on biotic association and distribution. Understanding these historical influences was particularly important, he argued, when came to describing and classifying plant formations or "societies," which is to say the complex organic communities that "the public has long called by names like forest, prairie, steppe, and so on."[44]

Throughout the 1920's and 1930's, Vierhapper sought to convince his colleagues of the advantages of his historically oriented form of plant sociology in a variety of different popular lectures and texts. The most influential of these popularizations was his widely cited "A New Classification of Plant Societies" (1921), which first appeared in print in the *Naturwissenschaftliche Wochenschrift*, a popular-scientific journal that catered to an interdisciplinary scientific audience. The article began by defining the aim, history, and current state of the field before suggesting that the future of research lay in the synthesis of three disparate approaches to characterizing plant formations: the ecological, which focused on the organic and inorganic relationships that governed a given formation in the present; the physiognomic, which focused on its gross morphology; and the evolutionary, which framed it as part of a broader historical-developmental trajectory. He made this suggestion yet again in his introduction and extensive amendments to his 1929 reissue of Anton Kerner's classic *Das Pflanzenleben der*

[44] Friedrich Vierhapper, "Eine neue Einteiling der Pflanzengesellschaften," *Naturwissenschaftliche Wochenschrift* Vol. 20 ed. H. Miehe (Jena: Verlag Gustav Fischer, 1921), 269.

Donauländer, declaring that the widely read popular text not only laid the scientific foundations for plant sociology but established the basic outlines of the tripartite classification scheme that he had fleshed out in 1921.[45]

Austrian physicists Franz Exner, Erwin Schrödinger, and Philipp Frank were no less invested in using the popular genre to further their scientific goals than their compatriots in biology. Indeed, popular representations were arguably more central to physical discourse in the 1920's and 1930's than they were at any point during the fin de siècle period. According to historian Paul Forman, the importance of non-technical communication in Germanophone physics during the interwar years was a direct result of the discipline's loss of prestige after the defeat of the Central Powers and physicists' subsequent vulnerability to public influence and opinion.[46] While Forman's claim may hold true for Germans like Max Born and Werner Heisenberg, it is less applicable to their Austrian counterparts, who viewed the role of popularization in physics in much the same way that their predecessors had before the war. That is, when Schrödinger claimed that the practice of crafting a lecture for general audiences compelled scientists to critically examine and refine their own assumptions, and Exner argued that popular texts were an ideal medium for discussing philosophical issues within the discipline, they were not making novel claims about the genre but restating points that Boltzmann and Mach had made long before 1914. In fact, Exner et. al. often used their popular work to address the exact same topics as Boltzmann and Mach, including indeterminism, the limits of theoretical modelling, and the relationship between physics and other scientific disciplines.

[45] Friedrich Vierhapper, "Vorwort des Herausgebers" and "Ergänzungen des Herausgebers," in Anton Kerner, *Das Pflanzenleben der Donauländer* 2nd ed. (Innsbruck: Universitäts-Verlag Wagner, 1929), XV-XVI, 349-445.
[46] Paul Forman, "Weimar Culture, Causality, and Quantum Theory, 1918-1927: Adaptation by German Physicists and Mathematicians to a Hostile Intellectual Environment," *Historical Studies in the Physical Sciences* Vol. 3 (1971), 1-115.

As Deborah Coen and Michael Stöltzner have shown, indeterminism was a defining feature of Austrian physics long before quantum mechanics and the Copenhagen interpretation permanently inscribed it into the intellectual edifice of science.[47] Mach argued that the principle of causality was an artificial and unnecessary mental crutch that did not reflect empirical reality as early as 1871, and Boltzmann's work on the second law of thermodynamics led him to suggest that disorder was a fundamental feature of the natural world several years later. Both physicists would continue to expand and refine their arguments for indeterminism in popular and technical publications into the early twentieth century, when they were joined by Franz Exner, who used his inaugural *Rektoratsrede* in 1908 to argue that natural laws described statistical regularities, and that their apparent necessity was actually a function of their high probability. When Exner raised this claim about the statistical nature of natural laws again his 1921 *Lectures on the Physical Principles of Natural Science*, a collection of popular essays that were directed at his colleagues but "did not presuppose more background knowledge than what a middle-school offers," he was therefore continuing a conversation that stretched back decades while also responding to more recent issues.[48]

One issue that Exner felt particularly compelled to address in his *Lectures* was Ostwald Spengler's attack on natural science in his immensely successful *The Decline of the West* (1918). Like many other Germanophone intellectuals of the period, Exner found much to like in Spengler's analysis, but he nevertheless felt that *Decline* misrepresented the history of physics and

[47] Deborah Coen, *Vienna in the Age of Uncertainty: Science, Liberalism, and Private Life* (Chicago: University of Chicago Press, 2007); Michael Stöltzner, "Vienna Indeterminism: Mach, Boltzmann, Exner," *Synthese* 119 (1-2).
[48] Franz Exner, "Vorwort zur Ersten Auflage," *Vorlesungen über die Physikalischen Grundlagen der Naturwissenschaften* 2nd ed. (Leipzig and Vienna: Franz Deuticke, 1922), V. For more on the continuity of Exner's pre-war and post-war intellectual interests, see: Richard Staley, "The Fin de Siècle Thesis," *Berichte zur Wissenschaftsgeschichte* Vol. 31, Issue 4 (December 2008), 311-330.

contemporary research landscape in two ways. First, he argued that Spengler's claim that nineteenth century physicists invariably and uncritically accepted the principle of causality was incorrect, as evidenced by Boltzmann's sterling work on the second law of thermodynamics in the 1870's. And second, he denied that the discipline's recent turn toward indeterminism was a symptom of intellectual decline. Rather, he argued that physicists' embrace of acausality represented the beginning of a new period of growth and development that was based on exciting research in atomic physics. The purpose of the last section of the *Lectures* was to examine this research in a way that not only refuted Spengler's pessimism but contradicted physicists who continued to maintain a Kantian view of the causal principle as a condition of possibility for scientific knowledge. He directed some of his most pointed attacks towards his—and Mach's—old foe Max Planck, arguing that recent revelations about the atomic realm demonstrated that Planck's distinction between "causal and merely statistical laws" was untenable, and that recognition of the stochastic character of physical phenomena was not an admittance of epistemological defeat but a move from "fantasy to reality."[49]

In the years following the publication of the *Lectures*, Exner's former student Erwin Schrödinger would publish his own popular and semi-popular articles on the issue of indeterminism in physics. One of his most influential essays on the topic, "What is a Natural Law" (1929), did not add much to existing arguments.[50] Like Exner, he pointed to phenomena like Brownian motion and radioactive decay as evidence of the probabilistic nature of atomic reality. He also reiterated a version of Mach's claim that physicists' belief in the necessary connection between events was not a reflection of the world *an sich* but a product of psychological

[49] Exner, "94. Vorlesung," *Vorlesungen über die Physikalischen Grundlagen der Naturwissenschaften*, 710.
[50] Erwin Schrödinger, "Was ist ein Naturgesetz," in *Was ist ein Naturgesetz? Beiträge zum naturwissenschaftlichen Weltbild* (Munich and Vienna: R. Oldenbourg, 1962), 9-18.

compulsion. But in several other popular representations of the late 1920's and early 1930's, he offered more novel arguments for indeterminism based on recent findings in the new field of quantum mechanics, including Werner Heisenberg's uncertainty principle, which stated that it was impossible to simultaneously determine all the key variables of a quantum system, insofar as establishing the position of an electron meant that one would be unable to measure its momentum and vice versa, and that there were therefore absolute limits to what physicists could observe. In his "Transformation of the Physical World Picture," for example, Schrödinger explained that physicists' inability to grasp position-momentum and other sets of "complementary" variables also meant that they were unable to fully predict how quantum systems would evolve over time, and that they consequently had to abandon their desire for a world-picture that was "at least in principle deterministic" in favor of one that accepted that any given state were merely more-or-less probable.[51] Or as he declared in a different popular lecture several years later, he and the other quantum theorists had merely provided further proof for what Boltzmann and Exner knew but were unable to fully convince their colleagues of: that natural laws are statistical laws, and that no amount of calculation and precision observation could ever reveal them to be otherwise.[52]

In addition to showing that nature was fundamentally indeterministic, the transition from classical to quantum mechanics made it clear that atomic objects and processes bore very little resemblance to macroscopic ones, and that it was therefore mistaken to use familiar mechanical analogies and concepts to make sense of them. One of the first victims of this transition was Niels Bohr's 1913 model of the atom, which represented electrons as material points which orbited an

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[51] Erwin Schrödinger, "Die Wandlung des physikalischen Weltbegriffes," in *Was ist ein Naturgesetz? Beiträge zum naturwissenschaftlichen Weltbild,* 24.
[52] Erwin Schrödinger, "Über Indeterminismus in der Physik," in *Über Indeterminismus in der Physik. Ist die Naturwissenschaft Milieubedingt. Zwei Vorträge zur Kritik der naturwissenschaftlichen Erkenntnis* (Leipzig: J.A. Barth, 1932), 12-13.

atomic nucleus in defined trajectories like moons around a planet, but it also raised the specter of more troubling casualties. Given the uncertainty principle, some quantum theorists were unsure if they were even able to use a traditional space-time framework to describe the atomic realm at all, which sparked a prickly debate about what kind of model they *should* use. Schrödinger provided one option with his wave mechanics, which admitted intuitive and "visualizable" (*Anschaulich*) descriptors like "matter waves" to enhance the intelligibility of quantum phenomena.[53] The other option was Heisenberg and his allies' empirically accurate but mathematically abstruse matrix mechanics, which not only initially dispensed with physically familiar notions like waves but with spatiotemporal representation altogether.[54]

Like the discussion on indeterminism, the philosophical contours of the debate over wave and matrix mechanics closely resembled the fin de siècle dispute between Boltzmann and Mach over whether physicists should seek to create purely phenomenological descriptions (Mach) or whether it was permissible to construct theoretical pictures that included artificial explanatory components (Boltzmann), with Schrödinger representing the latter perspective and Heisenberg et. al. the former. And just like the interlocutors in this earlier dispute, the quantum theorists occasionally used popular representations to plead their case. Schrödinger articulated one of his strongest arguments in favor of the wave approach in a popular lecture entitled "Conceptual Models in Physics and their Philosophical Value."[55] By the time he delivered this lecture in 1928,

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[53] Erwin Schrödinger, "On the Relation between the Quantum Mechanics of Heisenberg, Born, and Jordan, and that of Schrödinger" in *Collected Papers on Wave Mechanics* by Erwin Schrödinger, trans. J.F. Shearer and W.M. Deans (London: Blackie and Son Limited, 1928), 58-59.

[54] As Forman notes, Heisenberg also used popularization to get his message across. He even occasionally published important arguments in popular venues before articulating them in technical papers, including a key formulation of the principles of quantum mechanics. See: Forman, "Weimar Culture, Causality, and Quantum Theory, 1918-1927," 105-106.

[55] Erwin Schrödinger, "Conceptual Models in Physics and their Philosophical Value," in *Science, Theory and Man* by Erwin Schrödinger, trans. James Murphy and W.H. Johnston (New York: Dover Publications, 1957).

wave mechanics had already found widespread support among physicists, including highly influential researchers like Arnold Sommerfeld, because they felt that it was much easier to use.[56] But the proponents of matrix mechanics had nevertheless convinced many within the discipline that it was desirable to continue to purge quantum mechanics of concepts that existed only for the purposes of visualization.[57] In his "Models" lecture, Schrödinger sought to counter this trend by ceding that many familiar physical concepts were simply inapplicable to the quantum realm but denying that the incongruity between the macro and micro domains implied that "no visualizable scheme of the physical universe whatever will prove feasible." Like Boltzmann pointing out that the mathematical descriptions favored by phenomenologists like Mach implicitly relied on a materialist ontology, he then argued that Heisenberg and his allies had themselves arrived at matrix mechanics based on a "very definite model of nature" that contained latent spatiotemporal elements. He concluded by remarking that the lesson of the uncertainty principle was not to "beware of forming models or pictures at all," as Paul Dirac had suggested, but simply to craft more apposite ones.[58]

Schrödinger's understanding of *Anschaulichkeit* would not only bring him into conflict with Dirac and Heisenberg but with another eminent Austrian physicist and philosopher: Philipp Frank. Like many other intellectuals involved in the logical positivist movement that emerged in Vienna in the 1920's, Frank's philosophical worldview was heavily influenced by Wittgenstein's logical atomism, Poincare's conventionalism, and above all by Mach's phenomenalism.[59] In the

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[56] Suman Seth, "Crisis and the Construction of Modern Theoretical Physics," *The British Journal for the History of Science* Vol. 40, No. 1 (March 2007), 25-51.
[57] Schrödinger, "Conceptual Models in Physics and their Philosophical Value," 160.
[58] Ibid, 160-164.
[59] Hans Hahn, Otto Neurath, and Rudolf Carnap, "Wissenschaftliche Weltauffassung: Der Wiener Kreis," in *Empiricism and Sociology* by Otto Neurath, ed. Marie Neurath and Robert Cohen (Boston: D. Reidel, 1973), 304.

mid-1930's, he produced several popular and general interest texts that were intended to apply this blend of formal logic, philosophy of language, and positivism to the debate over the role of intuition and visualizability in quantum mechanics. The main thrust of his critique was that Schrödinger's conception of what it meant for something to be *Anschaulich* overlooked the fact that standards of intelligibility were historically contingent and changed over time. Whereas most twentieth-century physicists considered Newtonian mechanics to be a model of intelligibility, it made far less sense to seventeenth-century critics like Leibniz, who found the idea of action-at-a-distance to be hopelessly obscure. Frank also claimed that physicists' inclination to believe that Newtonian explanations were more comprehensible than phenomenological descriptions was rooted in their adherence to a "metaphysical Weltanschauung" that revolved around the reification of cause, matter, space, and time, and that matrix mechanics avoided this kind of conceptual idolatry because it stuck to making statements about observable bodies and relations.[60]

In addition to downplaying Schrödinger's concerns about how unintuitive and divorced from everyday reality matrix mechanics was, Frank used his popular work to critique attempts to apply recent physical discoveries to academic fields and problems that lay far beyond the borders of atomic physics. He directed some of his most pointed critiques towards researchers who used the results of quantum mechanics to engage in wild flights of metaphysical fancy about the relationship between the exact and life sciences. In his *Interpretations and Misinterpretations of Modern Physics* (1938), he took aim at English physicist James Jeans' popular monograph *The Mysterious Universe* for using "slight modifications in terminology" to suggest that there was a connection between quantum complementarity and "the purely mystical complementarity of

[60] Philipp Frank, "Über die Anschaulichkeit physikalischer Theorien," in *Quantenmechanik und Weimarer Republik* ed. Karl von Meyenn (Braunschweig: Viewig und Sohn, 1994), 287-288.

individual and continuous stream of life."[61] He had much harsher words for Philip Lenard, Johannes Stark, and other advocates of the anti-Semitic "Aryan Physics" (*Deutsche Physik*) movement that had taken root in Germany, writing that their house journal, *Zeitschrift für die Gesamte Wissenschaft*, was a veritable clearing house for mystical and organicist nonsense that was based on a highly tenuous understanding of contemporary research.[62]

In short, interwar Austrian biologists and physicists remained as committed to using the popular genre to do scientific work as their fin de siècle predecessors. For the most part, the biologists also continued to endorse the idea that popularization made a unique contribution to science by bringing everyday experiences, ideas, and opinions to bear on specialist discourse. Kammerer was among the most vocal proponents of this point of view, declaring that "the laity's interest in a question" could act as

> an agent of fermentation, stimulating the sluggish metabolism of scientific reasoning. A certain pressure of the public's opinion, a certain contrast between the voice of the laity, and established academic-scientific conceptions—be they rightly established or not—has often enough beneficially enhanced the desire for truth."[63]

Wettstein and fellow botany professor Hans Molisch expressed similar opinions in their popular texts on gardening. In his 1927 "History of a Garden Plant," for example, Wettstein argued that the Mendelian turn in biology had demonstrated that intellectual exchange between botanists and gardeners was more important than ever because it had shown that the latter were capable of providing invaluable data on hereditary patterns in their plants.[64] Molisch agreed, albeit from a

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[61] Philipp Frank, *Interpretations and Misinterpretations of Modern Physics* trans. Olaf Helmer and Milton Singer, (Paris: Hermann and Cie, 1938), 16-20. Although Jeans' *Mysterious Universe* was overly speculative and sensationalistic, other texts that grappled with the relationship between recent advances in biology and physics made genuine and lasting contributions to scientific thought, e.g., Schrödinger's *What is Life*? (1945).
[62] Ibid, 29.
[63] Kammerer, *The Inheritance of Acquired Characteristics,* 22.
[64] Richard Wettstein, "Die Geschichte einer Gartenpflanze," in *Festschrift der Österreichischen Gartenbaugesellschaft, 1827-1927* ed. G. Klein and F. Kratochwjle (Vienna: Julius Springer, 1927), 137.

different perspective, declaring that the motto of his immensely successful *Plant Physiology as a Theory of Gardening* was that "physiological problems are embedded in the experience of gardeners. The physiologist should therefore attend the school of the gardener and the gardener should attend the school of the physiologist. Both can learn much from one another."[65] Kammerer, Molisch, and Wettstein's notions about the epistemic value of integrating laypersons into biology were also manifest on an institutional level in the book reviews that appeared in the *Österreichische Botanische Zeitschrift* and the *Verhandlungen der Zoologisch-Botanischen Gesellschaft*, which frequently lauded texts that taught amateurs to contribute to biological research by identifying and collecting specimens, making observations, and recording other useful kinds of data.[66]

The physics community, both in and outside Austria, was far more ambivalent about the scientific utility of everyday experience and commonsense ideas. Many in the discipline felt that the advent of general relativity and quantum mechanics had effectively severed any remaining conceptual connection between commonsense and scientific cognition because it had shown that most familiar physical concepts did not apply at the atomic level or to objects moving at very high speeds. Schrödinger went so far as to suggest that the intellectual chasm between the average person's understanding of nature and the picture presented by quantum mechanics and general relativity was so great that worthwhile popularization was scarcely possible at all. When he attempted to provide a popular account of Einstein's research in 1929, for example, he prefaced his analysis by remarking that attempts to make "understandable by mere everyday expressions and everyday reasoning a conception which in its genuine rigid mathematical form is thoroughly

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[65] Hans Molisch, *Pflanzenphysiologie als Theorie der Gärtnerei* 6th ed. (Jena: Verlag Gustav Fischer 1930), III.
[66] Otto Wettstein, review of Hans Rebel, *Die freilebenden Saugtiere Österreichs als Prodromus einer heimischen Mammalienfauna*, in *Verhandlungen der Zoologisch-Botanischen Gesellschaft in Wien* 83 (1933), 215-216.

understood by only a small fraction of trained physicists" were "rather hopeless."[67] But the ostensible futility of translating contemporary physics into ordinary language did not ultimately stop him from trying, nor did it stop him from using "everyday reasoning" as an argumentative resource. Indeed, his claim that visualizability was a precondition of intelligibility was implicitly a claim that scientific knowledge had to bear at least a passing resemblance to the commonsense conception of the world. Like Wettstein, he also recognized that scientific opinion was invariably conditioned by public opinion, and that success in the marketplace of ideas was not always a function of having better facts but of manipulating one's cultural milieu.[68]

Exner also denied that the average person had much to contribute to expert reasoning about electrons and spacetime but nevertheless subscribed to Mach's belief that analyzing the relationship between the scientific and everyday thought was critical to understanding and resolving several of the discipline's conceptual and philosophical problems. Specifically, Mach argued throughout his career that analyzing the genetic relationship between popular and scientific reasoning revealed that physicists were instinctively predisposed to think in terms of causes and substances, and that this predisposition explained their unwarranted confidence in the primacy of mechanical explanation. Exner made a very similar point in his arguments against determinism, writing that physicists' attachment to the causal principle was not a reflection of empirical facts but of their "*Denkgewohnheiten*," or inherited habits of thought. Conversely, and perhaps hypocritically, he used the concept of *Denkgewohnheiten* to argue *for* the scientific validity of contingency, noting that the idea of accident played so great a role in everyday life and language

[67] Erwin Schrödinger, "Einstein Explained," in *Erwin Schrodinger, Gesammelte Abhandlungen, Band 4: Allgemein wissenschaftliche und populäre Aufsätze* ed. Österreichischen Akademie der Wissenschaften (Vienna: Verlag der Österreichischen Akademie der Wissenschaften, 1984), 301.
[68] Erwin Schrodinger, "Ist die Naturwissenschaft milieubedingt?" in *Über Indeterminismus in der Physik. Ist die Naturwissenschaft Milieubedingt. Zwei Vorträge zur Kritik der naturwissenschaftlichen Erkenntnis*, 25-62.

that physicists would be foolish not to "assign it an importance and meaning grounded in nature itself."[69]

Perhaps the greatest advocates for the epistemic value of popular thought during the interwar period were those who saw their attempts to bridge the intellectual and institutional divide between scientists and laypersons as part of broader, politically motivated efforts to reform the scientific enterprise as a whole. One of the most famous of these popular-political projects was Otto Neurath's ISOTYPE system of visual education. A sociologist by training but also deeply interested in physics and the philosophy of science, Neurath laid out the rudiments of ISOTYPE in the mid-1920's as part of his work for the Social and Economic Museum in Vienna, which was tasked with transmitting basic economic and social facts to the city's citizens. What made his system different from other popular-pedagogical tools available at the time was that it was almost completely graphical. Or as he remarked in 1925, he designed ISOTYPE based on the belief that modern man got most of his information visually, and that "if one wanted to spread social knowledge, one should use means similar to modern advertisements," which were able to transmit a tremendous amount of easily digestible information in a limited amount of space.[70]

As a participant in the socialist municipal government of Red Vienna, Neurath's initial hope for ISOTYPE was that it would educate the city's citizens on the great things that he and other local officials were doing for them. But he also saw the system as part of the logical positivists' project to create a universal language that was firmly rooted in ordinary, natural language, and that would enable researchers in distant fields to communicate with one another and members of the lay public without loss of meaning or misinterpretation. Put in Frank's words,

[69] Exner, "89. Vorlesung," in *Vorlesungen*, 678.
[70] Otto Neurath, "The Social and Economic Museum in Vienna," in *Empiricism and Sociology* by Otto Neurath, ed. Marie Neurath and Robert Cohen (Boston: D. Reidel, 1973), 214.

Neurath and the philosopher Rudolf Carnap were pursuing Mach's dream of creating a language that could serve as the linguistic basis for a unified science, and just like the venerable physicist, they envisioned this universal mode of speech as being bound by "the language of everyday life."[71] ISOTYPE's specific role in this project was to provide a graphical supplement to Carnap's universal protocol language that helped individuals across different scientific and non-scientific domains better understand one another while "remain(ing) in the field of factual arguing."[72]

The Nazi paleontologist Othenio Abel had a far darker and more malevolent vision of how academics could use the popular-scientific genre to reform science. Abel was only one among many high-profile Austrian biologists to align himself with the National Socialist movement in the years leading up to the Second World War. Although Hans Molisch generally managed to keep his political views out of his scientific and popular-scientific work, he nevertheless used his position as rector of the University of Vienna to grant special favors to National Socialist student groups.[73] The zoologist Konrad Lorenz, who would become famous after the Second World War for his popular work on animal behavior, was even more involved with the National Socialists than Molisch, and routinely published articles in the *Zeitschrift für die Gesamte Wissenschaft* before eventually joining the NSAPD in 1938. But Abel's commitment to the Nazi cause went far beyond either of the latter. Aside from trying to destroy the careers of Jewish scientists like Paul

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[71] Frank, *Interpretations and Misinterpretations in Modern Physics*, 32.
[72] Otto Neurath, "A New Language," in *Empiricism and Sociology*, 232. See also: Otto Neurath, *International Picture Language: The First Rules of Isotype* (London: Kegan Paul, Trench, Trubner, and Co. Ltd., 1936), 110-111.
[73] According to the Social Democrats, Molisch rejected an application by a socialist student group to start a shooting club while readily granting the same privilege to a national socialist student group. See: "Eröffnung einer hakenkreuzlerschiessstätte in der Universität unter Assistenz des Herrn Rektors," *Arbeiter-Zeitung* no. 18 (January 14th, 1927), 6. Molisch did not mention this incident in his 1934 autobiography, perhaps because the Nazi party had been outlawed by the Austrofascist government and he feared that his former association with Nazi student groups would have repercussions. He died a year before the Anschluss.

Kammerer, he was also one of the leading intellectuals of the "Aryan biology" movement and used his popular works as outlets to spread its message and garner public support for its endeavors.

Like the better known "*Deutsche Physik*," the purpose of the Aryan biology movement was to formulate a biological science that was free of "Jewish influence" and reflective of what its advocates considered to be the intellectual sentiments and values of the German people. From the perspective of Abel, Edwin Hennig, and Karl Beurlen, that meant articulating a theory of evolution that rejected "mechanistic" natural selection in favor of orthogenetic and vitalistic accounts of species change, which fit more readily into the Nazi's organicist worldview. They also sought to use fields like geology and paleontology to help create a *Völkisch* self-understanding among the German people.[74] Abel had already begun to tailor his popular-scientific work to further this agenda in the early 1920's. He prefaced *Wein: Sein Boden und Sein Geschichte*, a collection of essays that he edited which included a contribution from Richard Wettstein, by noting that his analysis of Vienna's geology and geography was intended to convince the people of Vienna that their city was "a German cultural site" that could not be allowed to become "Levantine."[75] He began agitating for Aryan biology even more aggressively after he was pushed out of the University of Vienna and emigrated to Germany in 1934, using popular-biological monographs to explore the paleontological roots of common Nazi symbols like the sun wheel and to show the biological necessity of the German fight for *Lebensraum*.[76]

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[74] Olivier Rieppel, "Karl Beurlen (1901-1985), Nature Mysticism, and Aryan Paleontology," *Journal of the History of Biology* Vol. 45, No.2 (Summer, 2012), 253-299. It is worth emphasizing that there is no intrinsic or necessary connection between orthogenesis, vitalism, and national socialism. For more on the Nazi's oftentimes tenuous grasp of evolutionary biology, see: Robert Richards, *Was Hitler a Darwinian? Disputed Questions in the History of Evolutionary Theory* (Chicago: University of Chicago Press, 2013).
[75] Othenio Abel, "Vorwort," in *Wien: Sein Boden und Sein Geschichte* ed. Othenio Abel (Vienna: Wolfrum Verlag 1924), i-ii.
[76] Othenio Abel, *Vorzeitliche Tierreste im Deutschen Mythus, Brauchtum, und Volksglauben* (Jena: Gustav Fischer, 1939), VII-IX; and Othenio Abel, *Das Reich der Tiere: Tiere der Vorzeit in ihrem Lebensraum* (Berlin: Deutscher Verlag, 1939), 4.

Popularization in the Twenty-First Century: Problems and Questions

In his 1935 masterpiece *Genesis and Development of a Scientific Fact*, the Polish immunologist Ludwik Fleck provided an account of the role of popular representation in specialist science that has recently garnered a significant amount of attention from historians. His essential argument was that the popular genre did not merely transmit facts to laypersons but actively conditioned scientific research and thought in various ways. One of its most basic epistemic functions, according to Fleck, was to enable communication between scientific "collectives" by translating "specialized esoteric knowledge" into "simplified, lucid, and apodictic" terms that were accessible to non-experts.[77] He then claimed that these translations served as a kind intellectual common ground which provided "the major portion of every person's knowledge. Even the most specialized expert owes to it many concepts, many comparisons, and even his general viewpoint."[78] Scientists did not derive their worldviews from technical articles, in other words, but from popularizations, meaning that the genre played a constitutive role in cognition in general.

As this dissertation has shown, Fleck's understanding of how popularization shaped scientific reasoning and contributed to the production of scientific knowledge was not particularly novel. Rather, it rearticulated and synthesized a set of claims that Austrian scientists across a variety of disciplines had been making since the late nineteenth-century. Indeed, it seems plausible to say that Fleck not only based his analysis on facts that would have been plain to any Central European scientist who cared to look at the communicative conventions of the 1930's but on his own experiences attending a Habsburg university prior to the First World War. But in the decades following the publication of *Genesis and Development*, what was evident to Fleck, other scientists

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[77] Ludwick Fleck, *Genesis and Development of a Scientific Fact* ed. Thaddeus Trenn and Robert Merton, trans. Fred Bradley and Thaddeus Trenn (Chicago: University of Chicago Press, 1981), 112.
[78] Ibid, 113.

of period, and academics interested in the topic of popularization became less evident. By the 1990's, historians and sociologists of science had apparently become so ignorant of the scientific importance of popular representation that manifestos and calls to action became necessary to remind them.[79] This strange turn of historiographical events raises a question that it will be incumbent on future scholars to answer, namely: how do we explain the dramatic transformation of academic conceptions of popularization between 1935 and 1990?

Deborah Coen has persuasively argued that the post-Fleck shift had much to do with the overwhelming influence of Thomas Kuhn's *The Structure of Scientific Revolutions* and his concept of the paradigm in particular, which propagated a picture of science that ascribed very little epistemic importance to popularization.[80] It also possible that the rise of "laboratory studies" in the 1970's served to further marginalize the popular genre by shifting scholarly attention to communicative and technical practices that were more directly relevant to experimental research. Alternately, it is plausible that historians' claims about the historiographical invisibility of popularization in the latter half of the twentieth-century were overblown, and that the incessant critiques of the "diffusion" model that characterize scholarly discourse on popularization to this day are attacking a strawman.[81] Whatever hypothesis one decides to entertain, it is clear that further investigation of the matter should reveal much about the epistemological assumptions and practice of late twentieth-century scholars of science.

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[79] See, for example: Roger Cooter and Stephen Pumfrey, "Separate Spheres and Public Places: Reflections on the History of Science Popularisation and Science in Popular Culture," *History of Science* 32:3 (Sept. 1994), 237-242.

[80] Deborah Coen, "Rise, Grubenhund: On Provincializing Kuhn," *Modern Intellectual History* Vol. 9, Iss. 1 (April 2012), 109-126.

[81] Andreas Daum suggested a similar point recently in *Isis*, writing that "criticism of the (diffusion) model has been endlessly varied, almost becoming a mantra; but in itself offers no useful alternatives. This is rather ironic, since hardly any historians---if any at all---in the last thirty years or so have actually subscribed to the two-stage model." See: Andreas Daum, "Varieties of Popular Science in the Transformation of Public Knowledge: Some Historical Reflections," *Isis*, Vol. 100, No. 2 (June 2009), 320.

This dissertation's arguments also raise important questions about the normative value of the fin de siècle Austrian approach to popularization for contemporary science. On the one hand, there are many surface similarities between the status of the popular genre in 1900 and 2021. The mammoth success of Richard Dawkins, Neil DeGrasse Tyson, Siddhartha Mukherjee, and Michio Kaku, among many others, suggests that contemporary biologists and physicists not only continue to take popularization seriously but that the public continues to have an immense appetite for it. Further, the measurable scientific impact of some of the ideas set forth in texts like *The Selfish Gene* indicates that the genre continues to structure the way that specialists think about their own fields and do their research. And finally, scientists' continued interest in fostering what is now called "citizen science," whether in the novel form of distributed computing or in the guise of more traditional practices like birdwatching, is evidence that they are no less willing than their nineteenth-century predecessors to integrate non-specialists into the scientific enterprise. Indeed, the sprawling laboratories, billion-dollar research projects, and massive staffs of the "big science" era have ostensibly involved more laypersons in the research process than ever before.

On the other hand, Mach, Wettstein, and their colleagues' understanding of popularization was in many respects unique to its time and place. Their conviction in the epistemic value of everyday experience, opinion, and thought was not only relatively rare in the broader fin de siècle scientific community but is mostly absent from contemporary scientific discourse as well. As the interwar debate over the role of intuition and visualizability in quantum mechanics demonstrates, an important factor in physicists' growing disinterest in the everyday was their recognition that the atomic realm looked so little like the macroscopic world that they would have to invent entirely new ways of describing it. Another potential factor in contemporary scientists' general indifference, if not distrust, towards the average person's notions about the natural world is the high social prestige that is now generally accorded to scientific research, which ostensibly lessens scientists' need to make concessions or overtures to public opinion. Whereas Mach and his colleagues had to fight for decades to convince the public that they were as essential to the university's mission as their colleagues in humanities, that is certainly no longer the case. If anything, the situation has reversed, with Latinists and philosophers struggling to preserve their departments from extinction while money is funneled into programs that fall under the STEM umbrella.

Given these similarities and differences, would contemporary scientists have anything to gain from adopting an Austrian perspective on the relevance of everyday thought for their work? That is, would it be broadly beneficial for scientists to recognize that hyper-specialization creates its own intellectual pathologies and that engagement with non-specialists could be epistemically productive? One argument in favor of the latter approach is that it could potentially stunt the growth of sub-cultures that have rejected scientific expertise on the grounds that it is authoritarian, elitist, and untrustworthy. But as Mach well knew, the "man on the street" was more likely to have unshakeable convictions about perpetual motion machines and the spirit realm than worthwhile insights about the natural world, and that by engaging the latter in honest discussion he was liable to give further credence to their fantasies. One can see echoes of this pattern of engagement in the contemporary back-and-forth between experts and laypersons on COVID-19 pandemic and climate change, which suggests that finding a solution to the seemingly intractable problem of public engagement is not just a matter of practical but world-historical importance.

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