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To my parents, Shaila and Ashok,

whose tireless efforts allowed the lives that Preetam and I have lived possible

and uplifted our family for generations to come.

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ABSTRACT

Work requirements remain a popular yet controversial policy in welfare programs around the world. This dissertation provides a comprehensive evaluation of welfare-to-work reforms by estimating their long-run effects on multiple outcomes for a generation of birth cohorts. I apply recent advances in difference-in-differences methods and large-scale administrative data to study the introduction of mandatory, publicly-managed work programs in Denmark's social assistance program. Effects are highly heterogeneous across cohorts based on the time the reforms were introduced in the life-cycle. Adults already eligible for welfare at the time of the reforms incur null or modest negative effects on income and substitute toward crime and alternative welfare programs. Meanwhile, not-yet-eligible children experience significant gains in schooling and income. This heterogeneity is consistent with a model where younger cohorts invest in their human capital in anticipation of future work requirements while older cohorts adjust along margins with high social costs. Evidence suggests that heterogeneity over birth cohorts can persist for decades over the life-cycle and spill over to the next generation. Cost-benefit analyses reveal that welfare-to-work is cost-effective in the long-run, but is likely driven by anticipatory behavioral responses of younger cohorts aging into the population rather than the effect of participating in work requirements. This sheds light on the interpretation of aggregate effects of welfare-to-work over time and alternative, more efficient policy designs.

CHAPTER 1

INTRODUCTION

In the last few decades, means-tested social assistance (SA) or "welfare" programs targeting disadvantaged, able-bodied adults have been radically redesigned across the developed world. Precipitous increases in welfare rolls in the U.S. and Europe during the 1980s generated fears that providing SA benefits with "no strings attached" disincentivized recipients from gaining self-sufficiency and would lead to unsustainable welfare states. To address these concerns, many countries enacted welfare-to-work reforms during the 1990s that required SA recipients to participate in work programs, such as remedial education, vocational training, job counseling, and public and private apprenticeships.

However, such work requirements remain a contentious policy debate to this day.[1] Advocates claim that attaching such "strings" to SA would screen out individuals who possess capacity to participate in the labor market while equipping recipients with the skills necessary to gain self-sufficiency, improving their income and lowering public costs in the long-run. Meanwhile, critics argue that these requirements provide a low return relative to the costs borne by recipients to fulfill them.[2] This could incentivize recipients to exit SA in favor of other options like crime or alternative welfare programs, which would hurt families and raise public costs in the long-run. Although debates surrounding welfare-to-work are framed by arguments on their long-run effects on disadvantaged families and the government budget, the empirical evidence is scarce. The bulk of studies focus on short-run impacts on SA take-up and labor market outcomes (Ziliak 2016) and are often confounded by other policy

1. The COVID-19 pandemic has reignited debates over the role of work requirements in U.S. welfare programs at the state and federal levels. For examples of recent news coverage, see Kim (2021), Michelmore (2022), Adolphsen (2022), Matthews (2022), and Kuang (2022).

2. See Card et al. (2010) and Card et al. (2018) for reviews of job training programs in the U.S. and Europe. Edin and Lein (1997) provide ethnographic evidence on the costs of participating in work requirements or the labor market among single mothers on U.S. welfare, such as time, childcare, transportation, and administrative burdens.

changes, hampering the scope of policy evaluation.[3]

This dissertation studies the case of welfare-to-work in Denmark. I use large-scale administrative data to provide new evidence on the long-run effects of welfare-to-work reforms across a generation of birth cohorts and their children. The multitude of outcomes—income, education, crime, hospitalizations, and social program participation—and the breadth of cohorts I study allow me to conduct a more comprehensive evaluation of welfare-to-work reforms relative to much of the existing literature.

To do this, I estimate the effects of welfare-to-work reforms along two distinct dimensions. The first is the *starting age* dimension: What are the effects on birth cohorts who first face the reforms at different stages of the life-cycle? Economic theory predicts that adolescents not yet eligible for SA may respond differently to the reforms than already-eligible adults since they possess different margins of adjustment, implying heterogeneous effects over cohorts.[4] The second is the *outcome age* dimension: What are the reforms' effects on later-life outcomes and spillovers on children? Work requirements may directly affect the human capital of SA participants with returns that only manifest later in life, or incentivize behavioral responses like schooling and crime that affect intra- and intergenerational trajectories. This motivates me to estimate a family of "starting age-outcome age effects" of welfare-to-work reforms that capture both dimensions.

Identifying starting age-outcome age effects is useful for two key reasons. First, they reveal the specific cohorts and ages where work requirements are socially costly, pointing to how alternative policies that target specific starting ages or outcome ages can improve efficiency. Second, the family of starting age-outcome age effects can be aggregated to

3. In the case of U.S. welfare reform, the introduction of work requirements were accompanied with changes to time limits on benefit receipt; federal allocation of state welfare funds and state-level benefit rates; and expansions of the Earned Income Tax Credit, federal child care funds, and Medicaid and child health insurance. See Fang and Keane (2004) for an effort to decompose the overall effects by these factors.

4. For example, adolescents may be diverted from schooling to SA's work requirements. Young adults may leave SA to engage in crime. Older adults may opt for disability benefits.

assess the overall effects of welfare-to-work reforms in the population over time. Since the composition of the population evolves to include younger starting age cohorts over time, understanding heterogeneity across the starting age and outcome age dimensions can shed light on the underlying forces that shape aggregate effects and on more efficient policy designs that target specific starting or outcome ages.

To estimate the family of starting age-outcome age effects, I leverage variation generated by a series of Danish welfare-to-work reforms during the 1990s. By 1998, the reforms mandated that SA recipients participate in publicly-organized work programs for at least 30 hours per week. The scale and scope of Denmark's administrative data provide a unique opportunity to estimate a family of starting age-outcome age treatment effects on a multitude of outcomes. I track outcome ages up to 30 years after the reforms for 19 birth cohorts with starting ages ranging from 8 to 30. Anonymized personal identifiers enables me to link individuals across tax, municipal, school, police, hospital records, providing an unusually rich set of outcomes. In addition, birth records permit me to observe outcomes of children of the cohorts in my sample, affording a rare opportunity to analyze intergenerational spillover effects.

The first part of my dissertation presents my main results on long-run intragenerational effects. I estimate starting age-outcome age effects of welfare-to-work reforms by exploiting variation in exposure to the reforms through a triple difference (TD) strategy that adapts Callaway and Sant'Anna (2021) and Sun and Abraham (2021). Specifically, I compare differences in outcomes over the life-cycles of birth cohorts who fell within the reform's targeted age group (the "treatment" group) against differences in outcomes over the life-cycles of birth cohorts who were not yet exposed to the work requirements (the comparison group). To remove confounding business cycle effects that arise from cohort-age comparisons, these double-differenced outcomes are further differenced between disadvantaged sub-groups who are at high- and low-risk of participating in SA in the future. Under a suitable parallel trends condition, this strategy identifies the starting age-outcome age effects of interest.[5]

Estimates reveal significant heterogeneity along the starting age dimension. Cohorts who were between ages 8–15 at the time of the reforms—before they were eligible to participate in SA or leave school—experience significant *positive* effects in later-life labor market outcomes and schooling, null effects on SA take-up and incarceration, and negative effects on inpatient hospitalizations and the take-up of other social programs. These effects last throughout young adulthood, though largely fade out after age 30. For cohorts with starting ages between 16–18—when individuals could exit school and gain eligibility for SA—effects on SA and social program take-up follow similar patterns while effects on other outcomes are largely statistically insignificant. In contrast, cohorts with starting ages between 19–24—who were already eligible for SA and suddenly faced the reforms—experience modest *negative* effects on labor market outcomes that grow in magnitude beyond age 30, null effects on education and hospitalizations, and significant positive effects on incarceration and take-up of other welfare programs like disability insurance. Meanwhile, for older cohorts between starting ages 25–29, estimated effects are largely null over the life-cycle. This heterogeneity is validated through a battery of robustness and falsification checks that test for diverging trends, confounding policy changes, or sensitivity to measurement choices and samples.

The heterogeneous patterns across starting ages are consistent with a model where agents face distinct margins of adjustment at different stages of the life-cycle. Children and adolescents who are not yet eligible for SA at the time of the reforms anticipate significant reductions in leisure due to SA work requirements, adjusting along margins that build their human capital. This finding aligns with theories on the "deterrent effect" of SA work requirements (Besley and Coate 1992) and the role of information in influencing human capital investments (Becker 1962). Meanwhile, already-eligible adults who experience the reforms

5. This identifies effects up to outcome age 29, as identification is limited by the largest starting age cohort in my data. To provide insight into longer-run effects, I also estimate effects relative to the counterfactual of being assigned to starting age 30.

as a surprise lack the education margin, resorting to crime or alternative welfare programs. The results highlight that introducing work requirements can generate perverse incentives for eligible adults, which are often ignored in prior work but can meaningfully affect cost-benefit calculations.[6]

The second part of the dissertation extends my main analysis in two ways. First, I examine the role of anticipation effects in driving effects among younger starting ages. I show how the effects of welfare-to-work reforms can be decomposed into anticipation effects, the direct intent-to-treat (ITT) effects of work requirements, and spillovers from their own parents' ITT from facing work requirements. Under stronger parallel trends assumptions, I demonstrate how unique institutional features of the Danish setting allow me to disentangle these factors from one another. The results suggest that anticipation effects play an outsize role in explaining effects among younger starting ages. These results align well with empirical evidence that shows how immediate SA take-up at age 18 changes among cohorts who could anticipate work requirements in the future.

Next, I provide suggestive evidence on intergenerational spillover effects of the welfare-to-work reforms using a subsample of my data. Remarkably, starting age heterogeneity appears to persist to the next generation. Children born to young mothers of starting ages 8–15 have higher income, higher schooling, and lower welfare dependence while children born to mothers of later starting ages face opposite-signed effects and significantly higher crime.

With the family of starting age-outcome age estimates at hand, the final part of the dissertation assesses the overall effects of welfare-to-work over time. To do this, I aggregate the starting age-outcome age effects on key outcomes over cross-sections of the population by calendar year. Over time, the positive effects of younger starting age cohorts aging into the population outweigh the negative or null effects among starting ages who were already eligible for SA. Cost-benefit calculations that weigh the fiscal impacts of the reforms across outcomes

6. For a recent example, see Deshpande and Mueller-Smith (2022), who illustrate how accounting for the social costs of crime dramatically affects the value of the U.S. Supplemental Security Income program.

against increased expenditure from financing public work requirements reveal meaningful heterogeneity over the starting age dimension: Younger starting age cohorts provide a *net positive* return on the government budget while older starting age cohorts impose a *net negative or null* return. Accounting for intergenerational spillover effects amplifies this result.

Policy implications. Overall, this dissertation provides three key takeaways for economists and policymakers. First, welfare-to-work reforms appear to be an effective long-run investment for the government solely because of its positive effects on younger cohorts. Meanwhile, they can have harmful fiscal impacts among cohorts *already eligible* for SA who typically comprise the immediate targeted population. Work requirements have been promoted to combat perverse incentives of traditional SA programs. Yet they themselves can create powerful perverse incentives with high social costs among older cohorts, making the cure worse than the disease. This suggests that redesigning work requirements to take effect for cohorts aging into SA eligibility while grandfathering already-eligible cohorts to the previous SA regime could improve efficiency.

Second, the positive return among younger starting age cohorts appears to be driven by anticipation effects rather than the direct intent-to-treat effects of work requirements themselves. This suggests that the reforms may only be effective as a tool that incentivizes behavioral responses among these cohorts before they age into eligibility rather than as a direct skill investment that shapes their later-life outcomes.

These points lend to the third take-away: Aggregate estimates on cross-sections of the population over time should be viewed with caution. These estimates mask important heterogeneity as the composition of the "treated" and comparison groups evolve over time with the entrance of younger starting-age cohorts who could anticipate work requirements. Aggregate estimates viewed in isolation could lead to misleading interpretations of the impacts of welfare-to-work over time.

Contributions to the literature. This dissertation contributes to an active literature on the long-run impacts of social safety net programs. A burgeoning literature has estimated the long-run intra- and intergenerational effects of *expansions* of cash and in-kind transfer programs along the outcome age dimension.[7] However, evidence on long-run impacts of *reforms* to existing programs, particularly those involving work requirements, is far more limited. Hoynes and Schanzenbach (2018) point out in a recent review that "the long-term impact of welfare reform is limited" (p. 106). Moreover, a report by the National Academies of Sciences, Engineering, and Medicine (2019) concludes that "[t]here is insufficient evidence to identify mandatory work policies that would reliably reduce child poverty. . . The dearth of evidence also reflects underinvestment over the past two decades in methodologically strong evaluations of the impacts of alternative work programs" (p. 210). This dissertation helps fill these gaps and contributes to several strands of related literature.

First, to the best of my knowledge, this dissertation is one of the only studies to estimate effects of SA reforms along the starting age dimension.[8] Much of the literature estimates effects of reforms on the adult population, though there are some notable exceptions.[9] Hernæs et al. (2017) and Bratsberg et al. (2019) study the effects of the introduction of SA work requirements on older adolescents' short-run outcomes, finding positive effects on schooling and negative effects on crime. A set of papers examine effects among adolescents at the time of U.S. welfare reform, which rely on small-scale survey data and find mixed results on effects

7. Recent work has examined the long-run effects of cash welfare (Aizer et al. 2016, Aizer, Eli, and Lleras-Muney 2021), food stamps (Hoynes et al. 2016, Bailey et al. 2020), Medicaid (Brown et al. 2020, Goodman-Bacon 2021b), Earned Income Tax Credit (Bastian and Michelmore 2018, McInnis et al. 2022), negative income tax (Price and Song 2018), and disability and Social Security Income (Deshpande 2016, Deshpande and Mueller-Smith 2022).

8. Morris et al. (2001), Gennetian et al. (2002), and Bloom and Michalopoulos (2001) synthesize effects from the Manpower Demonstration Research Corporation's U.S. welfare demonstration projects on young children, adolescents, and adults, respectively. However, (i) very few projects solely involved work requirement programs and (ii) all interventions only aimed at adults (i.e., adolescents were not treated later).

9. See Gray et al. (2021) for a recent study on the impact of work requirements among older food stamp recipients.

on schooling.[10] In contrast, my dissertation uncovers robust heterogeneous effects across 19 birth cohorts on a multitude of outcomes. Uncovering heterogeneous effects across a wide range of starting ages is essential to determine how disincentives among certain cohorts weigh against the benefits reaped by others (Aizer et al. 2022).[11]

In this vein, my dissertation also contributes to work examining anticipatory responses to safety net programs. A sizeable literature considers the impacts of time limits on SA use and child outcomes.[12] In the context of disability insurance benefits, Deshpande and Dizon-Ross (2022) finds no evidence of parental investments in child human capital while Dahl and Gielen (2021) finds evidence of anticipation effects among adolescents whose parents were affected by a reform. My findings suggest anticipation effects may play a crucial role in long-term outcomes. This broadly relates to a literature on the "threat effects" of work requirements in unemployment insurance.[13]

Second, this dissertation is among a few studies that estimates the long-run effects of welfare-to-work reforms over the outcome age dimension. There is an expansive literature on SA reforms in European countries (e.g., Dahlberg et al. 2009, Mogstad and Pronzato 2012) and in the U.S. (Ziliak 2016), yet the vast majority of studies focus on SA participation and labor market outcomes after 5 years. One exception is Løken et al. (2018), who find that work-encouraging SA reforms that targeted single mothers in Norway did not raise their disposable incomes yet caused modest declines in child test scores. Vaughn (2017, 2021) focus on the effects of early childhood exposure to U.S. welfare reform on outcomes measured in young adulthood and finds it improved schooling, household structure, and health outcomes. The results of these studies are consistent with my findings for older

10. See Kaestner et al. (2003), Dave et al. (2012); Dave et al. (2021), and Bastian et al. (2021a, 2021b).

11. Bitler et al. (2006) and Kline and Tartari (2016) emphasize how mean effects mask heterogeneity for a given cohort. I show that, in spite of this, theory predicts heterogeneity in mean effects across starting age cohorts.

12. See Grogger and Michalopoulos (2003), Grogger (2004), Chan (2018), and Low et al. (2018).

13. See Black et al. (2003), Geerdsen (2006), and Rosholm and Svarer (2008).

children and older adults. My dissertation also provides estimates of welfare-to-work's long-run effects on crime, inpatient hospital care, and participation in other social programs—outcomes which are rarely studied in the existing literature.

Third, this dissertation adds to a longstanding literature evaluating the efficacy of public work programs (also known as active labor market programs or job training). Much of this literature focuses on short- or medium-run effects among the broader unemployed population; some studies focus on effects of training welfare recipients.[14] More recently, Schochet (2018) and Aizer, Eli, Lleras-Muney, et al. (2021) find that the effects of youth work programs persist after several decades. An important distinction between this literature and the present study is that the former is concerned with the direct treatment effects of work programs on later-life outcomes. In contrast, this dissertation focuses on the effects of introducing mandatory work programs in SA, thereby capturing the overall effects from program participation as well as other choice margins.

Fourth, my methodological approach can help reconcile existing evidence in the welfare reform literature. Traditional studies of the early 2000s (e.g., Meyer and Sullivan 2004) focused on short-run aggregate effects of welfare reform and found small or null impacts on disposable income and consumption. More recent studies (e.g., Han et al. 2021) employ similar approaches over longer time horizons and find substantially larger effects. My starting age-outcome age estimates that underlie these aggregate estimates demonstrate that longer-run effects are driven by younger, anticipating cohorts aging into the new policy regime. More broadly speaking, my main methodology is related to "fuzzy" difference-in-differences settings studied in Fricke (2017) and de Chaisemartin and D'Haultfoeuille (2018). Additionally, my decomposition exercise involving anticipation, ITT, and parent ITT effects is related to recent work on multiple treatment effects in DD settings (de Chaisemartin and D'Haultfoeuille 2022).

14. Examples include Dyke et al. (2006), Hotz et al. (2006), and Lechner et al. (2011).

Finally, it is worth emphasizing the benefits of this dissertation's policy context. While U.S. studies make up the bulk of the literature, it is not possible to cleanly isolate the impacts of work requirements from concurrent policy changes (see Footnote 3). Yet work requirements remain central in U.S. policy debates. In contrast, the Danish SA reforms provide a better setting since their major focus was on the work requirement policy lever and other policy changes targeted different populations. Moreover, the Danish work requirements were similar to those introduced in other countries in the 1990s, so these results may be informative beyond the Danish setting.

Organization. The remainder of the dissertation unfolds as follows. Chapter 2 describes the Danish institutional setting and the predicted impacts of the reforms, the data, and an overview of the empirical methods used to estimate starting age-outcome age effects. Chapter 3 reports my main estimates on the intragenerational effects of the reforms over a multitude of outcomes. Chapter 4 extends these results by probing the role of anticipation effects that underlie effects among younger starting ages and providing suggestive evidence on intergenerational spillover effects. Chapter 5 aggregates effects over calendar time, evaluates the reforms through a cost-benefit analysis, and concludes.

CHAPTER 2

INSTITUTIONAL CONTEXT, DATA, & RESEARCH DESIGN

2.1 Institutional Context and Theory

2.1.1 Overview of the Danish Social Assistance Program

Social assistance (SA; *kontanthjælp*) was formally established in Denmark in 1976 to provide financial assistance to disadvantaged adults. SA primarily targets able-bodied adults who have capacity to participate in the labor market but do not qualify for unemployment insurance (UI).[1]

Individuals at least 18 years old may apply, though exceptions were made under special circumstances.[2] Eligibility is based on three criteria: an individual (1) experienced a social event (e.g., lost a job), (2) is unable to provide for herself (e.g., lack UI), and (3) exhausted their opportunities for work (e.g., unable to find a new job). "Social events" were broadly defined; for example, an individual who exited high school without a job could qualify. Individuals were expected to exhaust their resources before relying on SA, so the second criterion included a liquid asset limit of $550.[3]

Benefit rates comprised of three components: a basic rate, a housing supplement, and individual assistance. Basic rates depended on the individual's age and household composition. Individuals qualified for a "youth basic rate" or a higher "adult basic rate" based on an age threshold, which was age 23 before 1995 and age 25 after 1995. Young recipients cohabiting with parents received a lower youth rate while those with children, irrespective of their age, received an adult rate that increased in the number of children. Meanwhile, housing

1. Unlike many countries, the Danish UI system is based on voluntary membership. Disability insurance targeted individuals who lacked labor market capacity, though some could qualify for SA.

2. E.g., individuals below age 18 but living independently without parental support or facing domestic abuse.

3. Liquid assets exclude savings earmarked for education, home improvements, and child care.

supplements covered living expenses like rent and individual assistance covered idiosyncratic expenses (e.g., dental treatment, transportation).[4]

The sum of the three components was assessed against a cap, which was generous relative to international standards. For individuals with no recent earnings, the maximum "adult" benefit rate was $1,038/month (2010 U.S. dollars) in 1993, while the maximum "youth" benefit rate was $610.[5] For reference, at that time an American mother with two children received a maximum $609/month from Aid to Families with Dependent Children (AFDC).[6] Benefits had a 100% marginal tax rate.[7] Conditional on maintained eligibility, there was no time limit on receiving benefits.

The SA program was locally governed by Denmark's 273 municipal councils. Each municipality had flexibility in how they administered the program so long as they followed national guidelines. Caseworkers also had discretion in handling SA applicants.

SA was a well-known and popular program, especially among young adults. In 1989, approximately 7.5% of the adult population (ages 18–60) received SA benefits; among 18–24 year olds, this rate exceeded 16.7%. In fact, among the birth cohorts I study in this dissertation, nearly 45% participated in SA at some point between ages 18–29.

2.1.2 The Welfare-to-Work Reforms

The welfare-to-work reforms were enacted in response to strained economic conditions. Unemployment in Denmark remained persistently high during the 1980s, swelling above 10% by

4. Housing supplements were formally separated from SA in 1994, but almost all SA-eligible individuals qualified for housing supplements and they continued to count toward the total benefit cap (described in next paragraph).

5. For individuals who earned income prior to participating in SA, benefit levels were capped by the maximum between this level and 90% of the individual's previous net-of-tax income. Caps were benchmarked by maximum UI benefits and state education grants.

6. This amount averages the maximum AFDC benefits across U.S. states reported in Crouse (1995). See Appendix Table A.2 for a comparison between the Danish SA and U.S. AFDC programs.

7. Spousal income counted toward deductions, but rules evolved over time; see Hansen and Schultz-Nielsen (2017).

the end of the decade. Policymakers diagnosed that the generous SA program disincentivized able-bodied individuals, particularly young adults, from achieving self-sufficiency (Haahr et al. 1997). Reports of "undeserving" individuals exploiting the SA program—like middle-class teenagers enrolling in SA the summer before college—accelerated political momentum for reforms (Rosdahl and Weise 2001).

The Danish government enacted a series of 7 reforms between 1990 and 1998 aimed to encourage work among SA recipients. The most prominent feature of the reforms were publicly-organized work requirements (also known as "activation"), which included job search, vocational training, short-term educational programs, apprenticeships, community service, or subsidized apprenticeships in the public or private sectors. While SA recipients could voluntarily participate in these activities in the 1980s, these reforms marked the first time they became mandatory. The reforms also came with relatively smaller changes to benefit levels, sanction policies, and treatment of parents lacking daycare; see Appendix Table A.1 for more details.

Work requirements were enforced after individuals exhausted a "passive period" of receiving SA benefits, which lasted between 1/2–3 months. Work activities were extremely heterogeneous, though were frequently public work projects or apprenticeships involving unskilled labor.[8] Qualitative studies document several cases where activities amounted to "busy work" with onerous schedules that frequently frustrated participants.[9] Failure to complete work requirements led to reductions or discontinuation of SA benefits; some municipalities required repayment of benefits.

Table 2.1 provides a timeline of the national reforms. The first reform enacted in July 1,

8. Documented examples include janitorial work, public park beautification, snow shoveling, daycare assistants, food-packing in factories, carpentry, and bricklaying. Specific work requirements could be determined by the individual's preferences. Individuals had the option to participate in an 8-week long guidance period that involved meeting with caseworkers to develop professional goals, which could inform their assigned work activity.

9. See Mik-Meyer (1999), Larsen (2005), Katznelson (2008), Andersen (2020), and Hansen and Nielsen (2021).

Table 2.1: Summary of welfare-to-work reforms, 1990–1998

Date enforced	Ages	Minimum work requirements
Jul. 1, 1990	18–19	5 months, 20 hr./wk.
Oct. 1, 1991	18–20	5 months, 20 hr./wk.
Jul. 1, 1992	18–24	5 months, 20 hr./wk., with optional renewals
Jan. 1, 1994	18–24	6 months, 20 hr./wk., with required renewals
	≥ 25	Mandatory, but duration up to municipality's discretion
Jul. 1, 1995	18–24	6 months, 30 hr./wk.
	≥ 25	Mandatory, but duration up to municipality's discretion
Apr. 1, 1996	18–24	18 months, 30 hr./wk. if education does not qualify for UI membership
	≥ 25	Mandatory, but duration up to municipality's discretion
Jul. 1, 1998	18–29	18 months, 30 hr./wk. if education does not qualify for UI membership
	≥ 30	Mandatory, but duration up to municipality's discretion

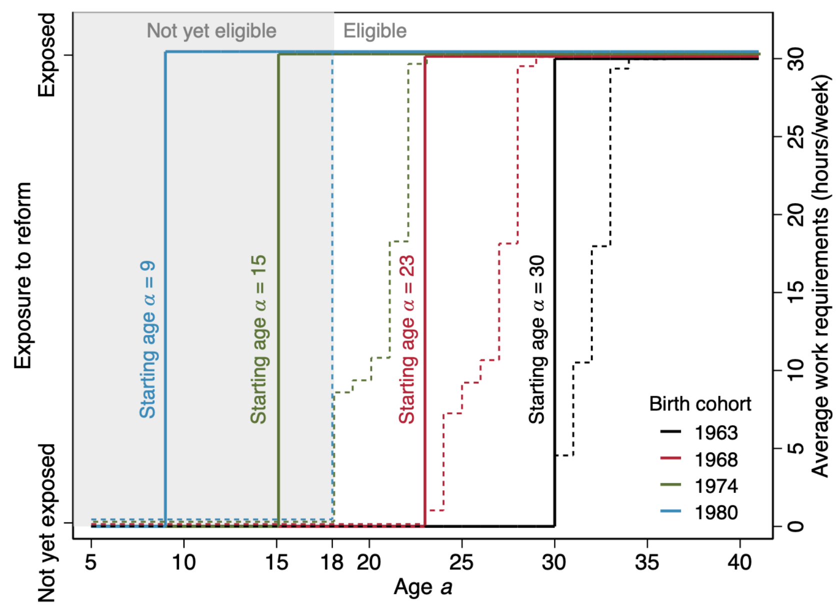
Notes: All information based on national laws. Municipalities had discretion to implement stricter policies.

1990 specifically targeted SA recipients between ages 18–19 to participate in work requirements for 20 hours per week for at least 5 months. By 1992, the targeted age group expanded to recipients between ages 18–24. The 1994 reform marked a significant expansion: *All* SA recipients aged 25 and older were required to fulfill work requirements. In addition, recipients who failed to exit SA after completing 6 months of work requirements were assigned to a new round of work requirements within 3 months. The last three reforms further intensified work requirements. By 1998, recipients below age 30 who lacked vocational credentials were required to work at least 30 hours per week in 18 month work requirement spells with mandatory renewals.

Municipal councils had some discretion over local policies. The national reforms explicitly called municipalities to decide the intensity and duration of work requirements for older-aged recipients, but in most cases they applied uniform policies across all ages.[10] Also, in practice,

10. According to a survey of municipal councils prior to the 1996 reform, over 85% of municipalities enforced work requirements for adults above age 25 for at least 30 hours per week for 6 months. See Appendix Figure A.2 for the distribution of work requirements across municipalities.

Figure 2.1: Exposure to reforms and work requirements over outcome ages, by birth cohort



Notes: Solid lines indicate exposure to any SA reform that affects a birth cohort immediately or in the future (left axis). Dashed lines plot the average work requirements measured in hours per week according to the national reforms' rules on the duration of work requirements, intensity of work requirements, mandatory renewals, and passive period before work requirements and between renewals (right axis); see Appendix Table A.1 for more details. The gray region represents ages below 18 before individuals are formally eligible for SA.

municipalities enacted work requirements at different times, but in most cases implemented work requirements within 6 months of the national reform.[11]

Figure 2.1 illustrates how different birth cohorts were exposed to the welfare-to-work reforms over the life-cycle. Each solid line corresponds to a different birth cohort and indicates their "starting age"—that is, the age when the cohort first faced a reform that would affect them immediately or in the future. For example, the solid black line indicates that the 1963 cohort was first affected by welfare-to-work at age 30 (due to the 1994 reform). Each dashed line corresponds to the average intensity of work requirements faced by each birth cohort over the life-cycle. For example, the dashed black line shows that work requirements immediately began at age 30 for the 1963 cohort (since this cohort fell within targeted age

11. Nevertheless, there are some extreme outliers. For example, Farum municipality implemented work requirements for all SA recipients starting in 1987 (Fallesen et al. 2018).

group of the 1994 reform) and quickly intensified due to subsequent reforms.[12] In contrast, for younger birth cohorts with starting ages below age 18, exposure to the reforms occurred *before* they were eligible for SA. For example, the solid green line shows how the 1974 cohort was exposed to the reforms starting at age 15 (due to the 1990 reform). While this cohort would not face work requirements until they gained SA eligibility at age 18—as indicated by the dashed green line—they could *anticipate* facing work requirements at age 18. Appendix Figure D.1 verifies that these patterns align with actual SA work requirement participation rates by birth cohorts over the life-cycle using the available data.

This figure offers two key insights. First, it suggests that the welfare-to-work reforms may have heterogeneous effects along the starting age dimension. Cohorts who could anticipate the reforms could potentially respond differently compared to others who lacked anticipation. Second, the figure motivates my research design by suggesting how older cohorts who faced the reforms later in the life-cycle could act as a comparison group to identify effects for younger starting ages. The following two sections expand on these ideas in turn.

2.1.3 Predicted Effects of Welfare-to-Work Reforms

What are the expected long-run effects of the welfare-to-work reforms? To answer this question, it is useful to model each starting age cohort's behavioral responses to the reforms by examining how the reforms impact their budget constraints over the life-cycle. This step provides a range of starting age-outcome age effects, which can then be mapped to predict aggregate effects of the population over calendar time.

I begin with a simplified model of human capital development that predicts these effects on income, education, and SA participation. I then discuss plausible effects on a broader set of outcomes.

12. Individuals who were already enrolled in SA when the reform was enforced typically faced some exemption period to transition into new requirements; see the notes of Appendix Table A.1 for details.

Predicted effects on income, education, and SA participation.

Model primitives.—Consider a dynamic labor supply model where forward-looking agents aim to maximize the lifetime value of consumption and leisure.[13] Suppose there are four outcome ages of the life-cycle indexed by a: childhood C when individuals are not yet eligible (ages ≤17), young adulthood YA when individuals become eligible for the youth benefit rate (ages 18–24), adulthood A when individuals become eligible for adult benefit rate (ages 25–29), and older adulthood OA (ages ≥30). Correspondingly, consider four cohorts with different starting ages of exposure to the welfare-to-work reforms, indexed by $\alpha \in \{C, YA, A, OA\}$.

In addition to choosing consumption and leisure, individuals possess four mutually exclusive discrete choices. The first is school, which is available up to young adulthood, $a \in \{C, YA\}$. The second and third are work and SA participation, which are available starting in young adulthood, $a \in \{YA, A, OA\}$. The fourth choice is an outside option.[14]

Human capital is the key state variable that evolves over the life-cycle. It appreciates if the individual chooses school or work. If SA possesses (lacks) work requirements, SA participation may appreciate (depreciate) human capital.

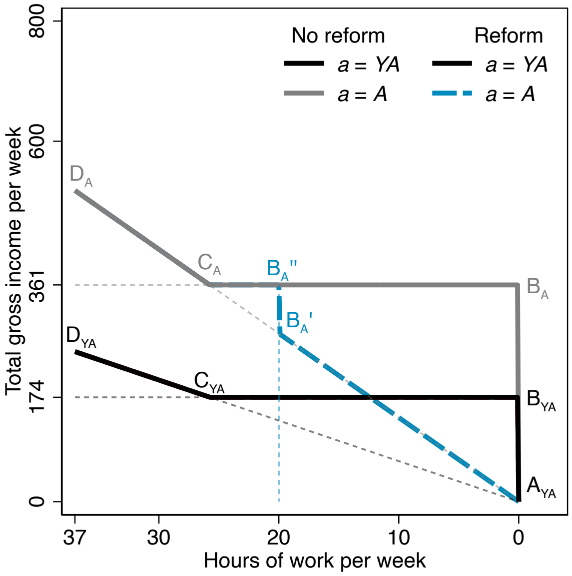
Budget constraints and predicted effects by starting age.—How does the reform affect the education and income of different starting age cohorts? Figure 2.2, Panels (a) and (b) model the budget constraints for starting ages $\alpha \in \{C, A\}$ in outcome ages $a \in \{YA, A\}$ before and after the reform. In both panels, the black and gray lines are the budget constraints under no reform. The black line $\mathrm{A}_{YA}\mathrm{B}_{YA}\mathrm{C}_{YA}\mathrm{D}_{YA}$ is the budget constraint in young adulthood $a = YA$. The point B_{YA} is the maximum weekly SA youth benefit rate, and the flat segment $\mathrm{B}_{YA}\mathrm{C}_{YA}$ reflects the 100% marginal tax rate on SA benefits. The slope of segment $\mathrm{C}_{YA}\mathrm{D}_{YA}$

13. See, e.g., Moffitt (2002). Key ways that my model departs from the canonical one are: (i) the life-cycle includes a period *prior* to SA eligibility, (ii) the choice sets include school in the initial periods.

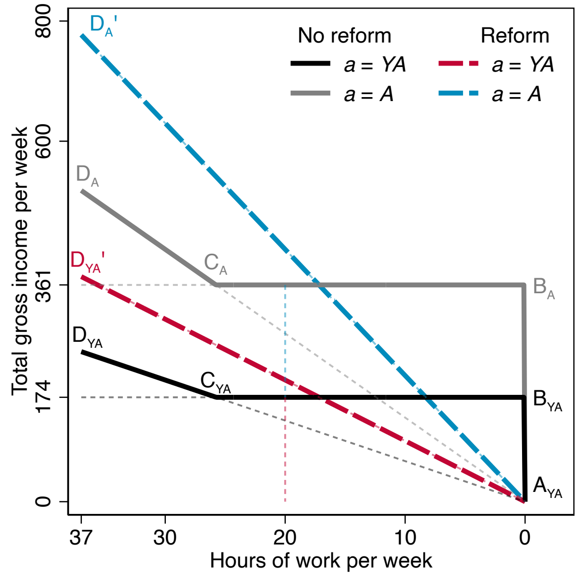
14. Recall that SA eligibility is determined by a lack self-sufficiency. This means individuals may participate in SA if and only if they are not working or in an education program through support from state educational grants. Mutual exclusivity between work and school is assumed for simplicity.

Figure 2.2: Effects of reforms on budget constraints and aggregate income over time

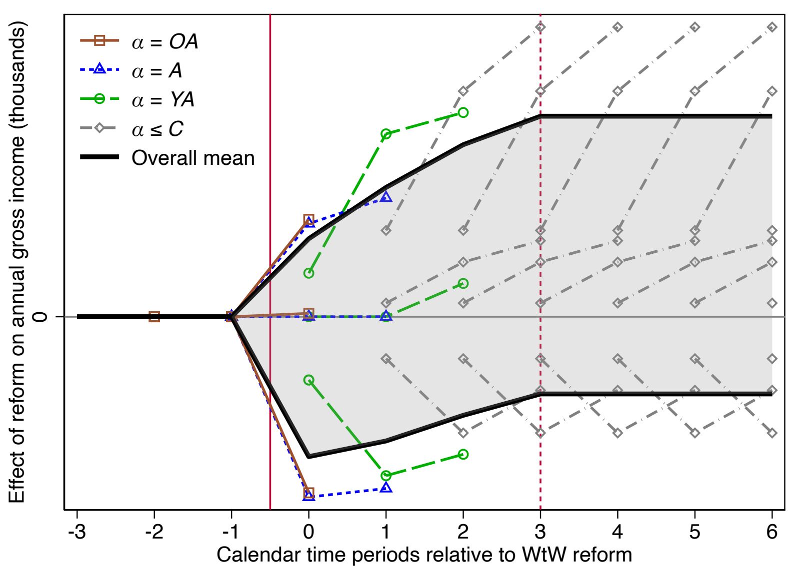
(a) Starting age $\alpha = A$ (no anticipation)



(b) Starting age $\alpha = C$ (anticipation)



(c) Range of aggregate effects over calendar time



Notes: Panels (a) and (b) plot hypothetical weekly budget constraints, with and without work requirements, for starting ages $\alpha = A, C$ at outcome ages $a = YA, A$ based on 1995 SA policy. Panel (c) plots a hypothetical range of effects on average gross income among the eligible SA population over calendar time. See main text for more details.

is the market wage rate earned from choosing work, where C_{YA} is the labor supply level where income from work breaks-even with SA benefits. Similarly, the gray line $\mathrm{A}_{YA}\mathrm{B}_A\mathrm{C}_A\mathrm{D}_A$ is the budget constraint in adulthood $a = A$, which is shifted higher due to the adult benefit rate and (possibly) higher market wages available to adults.

Now consider the budget constraints under the reform. For starting age cohort $\alpha = A$ (Panel (a)), the reform first comes into effect at outcome age $a = A$. The dashed blue line $\mathrm{A}_{YA}\mathrm{B}'_A\mathrm{B}''_A\mathrm{C}_A\mathrm{D}_A$ is the new budget constraint, which shifts inwards to reflect the SA work requirement.

Individuals of $\alpha = A$ who would have chosen SA under no reform (point B_A) may respond in ways that have three possible effects on their gross income at outcome age $a = A$, depending on their preferences for consumption and leisure. Individuals with stronger preferences for leisure would face a negative effect on gross income (moving to line segment $\mathrm{A}_{YA}\mathrm{B}'_A$); those with stronger preferences for consumption would experience increased income (moving to segment $\mathrm{A}_{YA}\mathrm{B}'_A$); and some may opt to comply with SA work requirements, resulting in no change in their gross income (moving to point B''_A). These heterogeneous effects on gross income result in ambiguous mean effects for the $\alpha = A$ cohort (Bitler et al. 2006). However, all three responses could result in higher human capital and, consequently, higher market wages in older adulthood $a = OA$. This implies that the overall effects on income in outcome age $a = OA$ may be larger than in $a = A$.

In the case of starting age cohort $\alpha = C$ (Panel (b)), the budget constraints under the reform may look very different. The reform came into effect before they were eligible for SA, so they could *anticipate* facing work requirements in the future. Moreover, unlike $\alpha = A$, this cohort possesses school as a choice in outcome ages $a \in \{C, YA\}$. In response to anticipating SA work requirements from $a = YA$ onward, individuals who would have chosen SA if there was no reform may be more incentivized to invest in their own human capital through school (Becker 1962, Besley and Coate 1992). Intuitively, if the market wage return to school is sufficiently high to compensate for the utility cost of choosing school, then individuals expecting to supply a certain level of labor under SA in return for the same SA benefit rates may prefer to supply labor in formal work in return for higher income.[15] If the returns to school are sufficiently high, then this could result in fanned-out budget constraints that exceed the SA benefit kinks; this special case is illustrated by the red and blue dashed lines in Panel (b). Alternatively, individuals may opt to participate in SA work requirements to build their human capital and reap higher market wages from work in later ages.

Just like the case of $\alpha = A$, the behavioral responses of the $\alpha = C$ cohort can generate negative, positive, or null effects on gross income. However, there is a higher probability that the mean effect among $\alpha = C$ is higher than the mean effect among $\alpha = A$ since increased incentives for human capital accumulation expand their budget constraints. In particular, the reforms are predicted to increase educational attainment and decrease SA participation of $\alpha = C$ relative to $\alpha = A$.

Similar insights extend to starting age cohorts $\alpha = YA$ and $\alpha = OA$. In general, younger cohorts possess more margins to build their human capital than older cohorts. Conditional on outcome age a, mean gross income is expected to decrease in starting age; conditional on starting age α, it is expected to increase in outcome age.

Predicted long-run effects.—How does this heterogeneity map onto the aggregate effects on gross income over time? Panel (c) illustrates how different possible effects by starting age map to a range of mean effects by calendar time period. Each shaped marker is a possible starting age-outcome age effect, and connected lines and colors distinguish the possible outcome age effect trajectories by starting age α. For each starting age $\alpha \in \{C, YA, A, OA\}$, I plot three possible trajectories given the possible positive, negative, or null effects discussed earlier. The gray region illustrates the range of aggregate effects by averaging all possible starting age-outcome ages by calendar time.

While the sign of the overall effects over calendar time remain ambiguous, the simple model predicts the overall effect increases over time as more starting age cohorts (the gray

15. Appendix E derives this result with a formal version of the model and provides comparative statics.

dashed lines) age into eligibility. Steady state is attained when only $\alpha \leq C$ cohorts exist in the adult population, and is expected to be higher than the short-run effects immediately following the reforms.

Predicted effects on other outcomes. Enriching the model with more choice margins and preferences that capture additional outcomes gives rise to complex behavioral responses. I briefly discuss possible effects on other outcomes that are critical for policy evaluation and how they may interact with income and education.

Health.—The SA reforms may enhance the health of younger starting age cohorts given the well-documented relationship between education and health outcomes (Cutler and Lleras-Muney 2008). In contrast, the health of older starting ages may be diminished due to increased stress or accidents from fulfilling work requirements or participating in the labor market. Thus, I expect health to decrease over starting age cohorts and, hence, increase over time.

Effects on health may affect predictions on income. If the health decreases among older starting age cohorts, this could negatively impact their capacity to supply labor and, thus, later-life income.

Crime.—Individuals may be incentivized to leave SA to build "criminal capital" instead of human capital. However, participating in SA work requirements could incapacitate individuals from crimes that they would have committed if they participated in SA without work requirements. This leads to ambiguous effects over starting ages and over time. However, they are likely magnified for starting ages between late adolescents and early adults when crime peaks during the life-cycle.

Critically, behavioral responses in crime would be expected to also affect predicted effects on income. For example, if the reform causes increased crime among young adults, then this could have scarring effects on future labor market prospects, leading to decreased later-life income.

Other social program participation.—Older starting age cohorts who lack the education margin would be more incentivized to substitute SA for a different social program like disability insurance or unemployment insurance. Meanwhile, younger starting ages who gain human capital may be less prone to take-up these social programs in the future. Thus, I expect participation in these programs to increase over starting age cohorts and, hence, decrease over time.

Intergenerational spillovers.—The reforms' effects on choice behavior can affect the development of the next generation via investments, time, and role model effects. Given the previous predictions, I expect children of younger starting ages will tend to fare better than those of older starting ages.

2.2 Data

The previous model predicts heterogeneous effects of welfare-to-work over the starting age and outcome age dimensions, but shows that aggregate effects are ambiguous over time. It motivates estimating effects along both dimensions to assess this heterogeneity and aggregate effects. This section describes the data used for estimation.

I leverage administrative register data from Statistics Denmark. The registers cover the entire Danish population and usually span between 1980 and 2019. Anonymized identifiers allow individuals and households to be linked across registers and calendar years. This permits me to track a multitude of outcomes and covariates over panels spanning several decades of the life-cycle. Birth records also allow individuals to be linked with their biological or adoptive parents and their own children. See Appendix B for details on the register data.

SA participation data are available through a patchwork of different registers. Recordings of SA take-up significantly vary within and between registers to adapt to amendments made in four national laws that governed the SA program during the period of study.[16] I track all amendments to the national laws between 1980 and 2000 to harmonize SA data across registers over time.[17]

My base sample consists of native Danes born between 1963 and 1981.[18] These cohorts correspond to starting ages $8 \leq \alpha \leq 30$ excluding 20, 21, and 25, which do not exist due to differences in timing and targeted age groups across the reforms. I restrict my attention to these cohorts due to data restrictions and confounding policies. The 1963 cohort is the earliest one I can observe predetermined covariates before age 18. These covariates are critical for constructing the risk groups leveraged in my research design. Institutionally, the 1963 cohort is also the first one to experience formal SA eligibility rules at age 18.[19] Meanwhile, the 1981 cohort is the final one unaffected by a new SA reform that significantly departed from the work requirements studied in this dissertation.[20]

Background characteristics $\boldsymbol{X}_i$ include household income, household assets, mother years of schooling, number of siblings, marital status, and mean income in the local parish of residence. Most are defined before the individual turns age 18 and before 1994 to ensure that the individual's households are unaffected by the SA reforms; see Appendix C.1 for

16. The laws are the Social Assistance Act, Active Labor Market Policy Act, Municipal Activation Act, and Active Social Policy Act. See Appendix B.5 for details on the register data.

17. Unfortunately, participation of SA *work requirements* is censored in certain years for a subset of cohorts.

18. I exclude first- and second-generation immigrants to ameliorate compositional changes in my sample driven by increased immigration during the study period. This removes individuals who interact with the refugee SA program, which was governed by a different set of rules.

19. The SA program lacked formal eligibility criteria and benefit ceilings until the "Hunger Circular" (*Sultecirkulære*) of 1981 that institutionalized guidance for municipal councils (Hansen and Schultz-Nielsen 2017).

20. In 2006, another SA reform mandated recipients below age 25 to enroll in a formal vocational education program.

more details.[21]

In general, individuals who are ineligible for SA are far more socioeconomically advantaged than those who are eligible. Hence, pooling over all ineligible individuals would not provide a suitable control group for my triple difference identification strategy (explained below) since their outcomes evolve over the life-cycle in ways that cannot be captured by $\boldsymbol{X}_i$ (e.g., resilience to business cycles). To avoid this issue, my main sample is a socioeconomically-disadvantaged subset of the base sample defined by two background characteristics in $\boldsymbol{X}_i$: (i) household income between ages 12–17 is below the national median, and (ii) the individual's mother has at most 12 years of schooling.[22]

2.3 Research Design

This section describes the research design used to identify the starting age-outcome age effects on intragenerational outcomes, which is the primary focus of my dissertation. Due to data limitations, I use a different strategy to identify intergenerational spillover effects; I return to this in Section 4.2.

2.3.1 Ideal Target Parameters

Let $i \in \mathcal{I}$ index individuals, $b \in \mathcal{B}$ index birth quarters, and $a \in \mathcal{A}$ index integer-valued outcome ages. Let $D_i \in \mathcal{A} \cup \{\infty\}$ indicate the starting age of exposure α to the reforms, which is an absorbing "treatment" state.[23] The state $\alpha = \infty$ represents the "never treated"

21. In theory, an individual's parents may respond to the 1990–1992 reforms because they are aware that *their child* will be eventually affected by work requirements. However, Appendix Figures C.7–C.13 demonstrate that household characteristics included in $\boldsymbol{X}_i$ were unaffected by the 1990–1992 reforms. This aligns with Deshpande and Dizon-Ross (2022), who find that anticipated changes in child's welfare benefits do not change parents' human capital investments in their children. Nevertheless, I verify that my results are robust to measuring $\boldsymbol{X}_i$ before 1990.

22. Specifically, household income is averaged over the three highest ages before age 18 and before the 1994 reform.

23. This is largely true in the Danish reforms, but there are some minor exceptions. Due to the multiplicity of reforms that targeted different age groups, some cohorts face interim periods where they do not face work

(or never exposed) state. Let $Y_{ia}(\alpha)$ be the potential outcomes of individual i at age a under treatment assignment $\alpha \in \mathcal{A} \cup \{\infty\}$. Finally, let $G_i^\star \in \{0,1\}$ be a latent variable indicating i's (future) eligibility for SA. Since eligibility for SA could change as a result of being exposed to the reforms—particularly among individuals who are below age 18 at the time of exposure—$G_i^\star$ is an endogenous state.[24]

The ideal target parameters is the family of starting age-outcome age effects for starting ages $\alpha \in \mathcal{A}$ on outcome ages $a \geq \alpha$ among the SA-eligible population $G_i^\star = 1$:

$$\beta^\star_{\alpha,a} \equiv \mathbb{E}[Y_{ia}(\alpha) - Y_{ia}(\infty) \mid D_i = \alpha, G_i^\star = 1]. \tag{1*}$$

In settings with staggered treatment and heterogeneous effects, this parameter resembles the underlying primitive treatment effect of popular two-way fixed effect estimators (Goodman-Bacon 2021a, Sun and Abraham 2021) and provides a building block for policy-relevant parameters like aggregate effects over calendar time (Callaway and Sant'Anna 2021).[25]

In practice, the data I observe are realized outcomes where starting ages are determined by birth quarters $b \in \mathcal{B}$, so if b_i denotes i's birth cohort, then $D_i = D(b_i)$. Realized outcomes are thus $Y_{ia} = \sum_{\alpha \in \mathcal{A}} Y_{ia}(\alpha) \times \mathbb{1}(D(b_i) = \alpha) \times G_i^\star \;+\; Y_{ia}(\infty) \times (1 - G_i^\star)$.

There are two key challenges of identification. First, if $G_i^\star$ is observable, what is a valid research design to identify $\beta^\star_{\alpha,a}$? Second, given that $G_i^\star$ cannot be observed, what alternative parameters can be identified? The remainder of this section addresses each challenge in turn.

requirements. There are two cases where this occurs: (i) cohorts born between July 1–December 31, 1967 were first exposed to work requirements at age 24 (due to the 1992 reform), not at age 25, but subsequently at age 26 (due to the 1994 reform), and (ii) cohorts born between July 1–September 30, 1970 were first exposed to work requirements at age 19 (due to the 1990 reform), not at age 20, but subsequently at age 21 (due to the 1992 reform). If anything, these small exceptions would attenuate my estimated effects for these starting age groups.

24. Realistically, eligibility status may change dynamically. Thus, $G_i^\star$ can be defined at a particular age, such as $a = 18$ or $a = \alpha$. This choice does not affect any of the identification results that follow.

25. These studies refer to this primitive parameter as the "group-time" or "cohort-time" parameter.

2.3.2 *Identification Strategy*

Triple difference design. My main analysis employs a triple difference (TD) design that applies insights from the recent literature on difference-in-differences designs in staggered treatment settings.[26] Assuming $G_i^\star$ is observable, this design exploits variation between birth cohorts of different starting ages, their outcome ages, and eligibility groups. Identification of $\beta^\star_{\alpha,a}$ rests on the following assumption:

Assumption 1* (Conditional parallel trends among eligibility groups)**.** *For any $\alpha' > a$,*

$$\begin{aligned}
&\underbrace{\left(\mathbb{E}[Y_{ia}(\infty) \mid D(b_i)=\alpha, G_i^\star=1, \boldsymbol{X}_i] - \mathbb{E}[Y_{i\alpha-1}(\infty) \mid D(b_i)=\alpha, G_i^\star=1, \boldsymbol{X}_i]\right)}_{\textit{life-cycle trend of eligible treated}} \\
&\quad - \underbrace{\left(\mathbb{E}[Y_{ia}(\infty) \mid D(b_i)=\alpha, G_i^\star=0, \boldsymbol{X}_i] - \mathbb{E}[Y_{i\alpha-1}(\infty) \mid D(b_i)=\alpha, G_i^\star=0, \boldsymbol{X}_i]\right)}_{\textit{life-cycle trend of ineligible treated}} \\
&= \underbrace{\Big(\mathbb{E}[Y_{ia}(\infty) \mid D(b_i)=\alpha', G_i^\star=1, \boldsymbol{X}_i] - \mathbb{E}[Y_{i\alpha-1}(\infty) \mid D(b_i)=\alpha', G_i^\star=1, \boldsymbol{X}_i]\Big)}_{\textit{life-cycle trend of eligible not-yet-treated}} \\
&\quad - \underbrace{\Big(\mathbb{E}[Y_{ia}(\infty) \mid D(b_i)=\alpha', G_i^\star=0, \boldsymbol{X}_i] - \mathbb{E}[Y_{i\alpha-1}(\infty) \mid D(b_i)=\alpha', G_i^\star=0, \boldsymbol{X}_i]\Big)}_{\textit{life-cycle trend of ineligible not-yet-treated}},
\end{aligned}$$

Assumption 1* thus states that, conditional on $\boldsymbol{X}_i$, differences between the eligible and ineligible group's potential outcomes by starting age cohort move in parallel over outcome ages. In other words, calendar time shocks (e.g., business cycles) that affect different cohorts do not vary between the eligible and ineligible groups. Conditioning on $\boldsymbol{X}_i$ accounts for the possibility that potential outcomes of the eligibility groups may differentially evolve since the groups possess different distributions of $\boldsymbol{X}_i$. Following standard arguments, Assumption 1* implies the ideal target parameter $\beta^\star_{\alpha,a}$ can be nonparametrically identified with factual

26. See Borusyak et al. (2021), Callaway and Sant'Anna (2021), Goodman-Bacon (2021a), and Sun and Abraham (2021), who invoke a "group-time effect" concept that is analogous to my "starting age-outcome age effect."

quantities.[27]

Limits of difference-in-differences. To motivate why a TD design is necessary, consider following a difference-in-differences (DD) approach. For a given starting age and outcome age pair $(\alpha, a) \in \mathcal{A} \times \mathcal{A}$, restrict the population to the subset $G_i^\star = 1$ and birth cohorts $b \in \mathcal{B}$ such that $D(b) = \alpha$ or $D(b) > a$. Callaway and Sant'Anna (2021) use a parallel trends assumption between the untreated potential outcomes of treatment group $D(b) = \alpha$ and not-yet-treated group $D(b) > a$ to identify the average treatment on the treated effect at (α, a). In my context, this assumption means that, absent any reforms, the outcomes of starting age group $D(b) = \alpha$ would have evolved from ages α to a in parallel to the outcomes of starting age group $D(b) > a$.

Nevertheless, it is unlikely that this condition will be satisfied due to confounding calendar time shocks, such as business cycles, that affect the potential outcomes of cohorts at specific ages.[28] For example, comparing outcomes at age $a = 28$ between starting ages $\alpha = 8$ (the 1981 cohort) and $\alpha = 29$ (the 1964 cohort) would be confounded by the 2009 recession, which affects $\alpha = 8$ at $a = 28$ but not $\alpha = 29$ at that age. The TD method purges confounding cohort-by-age effects by further differencing the DDs between eligible and ineligible groups.

27. That is, if $\mathbb{E}_{\boldsymbol{X}}$ is the expectation operator with respect to $\boldsymbol{X}_i$,

$$\begin{aligned}\beta^\star_{\alpha,a} = \mathbb{E}_{\boldsymbol{X}}\big[&(\mathbb{E}[Y_{ia} \mid D(b_i) = \alpha, G_i^\star = 1, \boldsymbol{X}_i] - \mathbb{E}[Y_{i\alpha-1} \mid D(b_i) = \alpha, G_i^\star = 1, \boldsymbol{X}_i]) \\ &- (\mathbb{E}[Y_{ia} \mid D(b_i) = \alpha, G_i^\star = 0, \boldsymbol{X}_i] - \mathbb{E}[Y_{i\alpha-1} \mid D(b_i) = \alpha, G_i^\star = 0]) \\ &- (\mathbb{E}[Y_{ia} \mid D(b_i) > a, G_i^\star = 1, \boldsymbol{X}_i] - \mathbb{E}[Y_{ia\alpha-1} \mid D(b_i) > a, G_i^\star = 1, \boldsymbol{X}_i]) \\ &+ (\mathbb{E}[Y_{ia} \mid D(b_i) > a, G_i^\star = 0, \boldsymbol{X}_i] - \mathbb{E}[Y_{i\alpha-1} \mid D(b_i) > a, G_i^\star = 0, \boldsymbol{X}_i])\big].\end{aligned}$$

28. To see this point formally, without loss of generality, express expected untreated potential outcome for $D(b) = \alpha$ as $\mathbb{E}[Y_{ia}(\infty) \mid D(b_i) = \alpha, G_i^\star = 1] = \delta_b + \delta_a + \tau_{ba}$. Note that the typical parallel trends assumption, $\mathbb{E}[Y_{ia}(\infty) \mid D(b_i) = \alpha, G_i^\star = 1] - \mathbb{E}[Y_{i\alpha-1}(\infty) \mid D(b_i) = \alpha, G_i^\star = 1] = \mathbb{E}[Y_{ia}(\infty) \mid D(b_i) > a, G_i^\star = 1] - \mathbb{E}[Y_{i\alpha-1}(0) \mid D(b_i) > a, G_i^\star = 1]$, implies that $\tau_{ba} - \tau_{b\alpha-1} = \sum_{b_0 \in \mathcal{B}, D(b_i) < a} \Pr(b_i = b_0 \mid G_i^\star = 1) \cdot (\tau_{b_0 a} - \tau_{b_0 \alpha-1})$. Consider the simple case where cohort-by-age effects are due to calendar time effects that are homogeneous over birth cohorts, so that $\tau_{ba} = \tau_{b+a}$ for all $(b, a) \in \mathcal{B} \times \mathcal{A}$. Even in this case, it is implausible that differences in calendar time effects equal weighted averages of those across different years for all possible (α, a) pairs.

2.3.3 Feasible Target Parameters

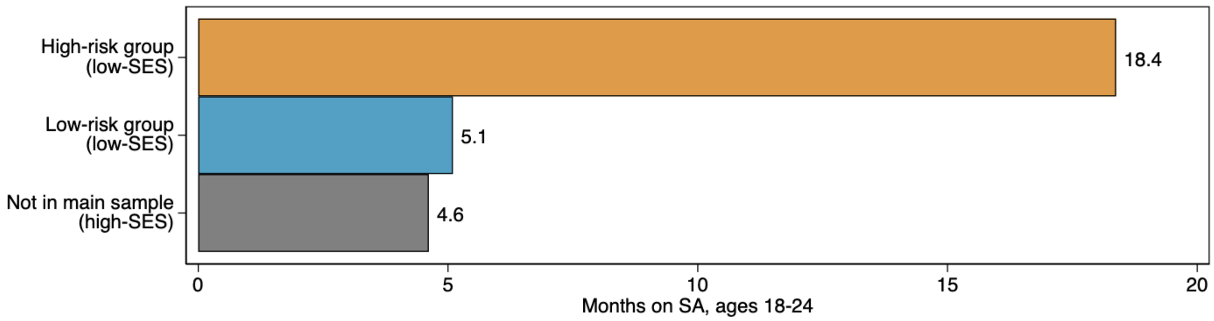
Unfortunately in practice, $G_i^\star$ cannot be perfectly observed. First, it is impossible to observe eligibility under the untreated state among starting ages $D(b) < 18$, the minimum SA eligibility age. Second, the Danish data only provides noisy proxies of eligibility criteria on liquid assets and social events. Third, non-targeted groups often participated in SA.[29] Instead, I identify alternative starting age-outcome age parameters using pre-determined *risk groups* that predict large differences in later-life SA participation.

Risk groups. The "high-" and "low-risk" groups, indicated by $G_i \in \{0, 1\}$, are defined by two predetermined characteristics measured before age 18: (i) the individual's household participated in SA, and (ii) the individual's neighborhood block of residence has a high SA take-up rate. Both characteristics are theoretically and empirically relevant. First, household participation in SA during adolescence is motivated by a large literature documenting high intergenerational persistence of welfare participation, with recent studies demonstrating a significant causal channel (Dahl et al. 2014, Hartley et al. 2022). I measure whether any of the individual's older household members ever participated in SA participation over the three highest ages before age 18 and before 1994.[30] Second, using neighborhood-level SA participation rates is motivated by a similar logic that accounts for spillover effects through local social networks (Bertrand et al. 2000, Åslund and Fredriksson 2009, Shang 2014). I classify individuals to high- or low-SA neighborhoods by measuring whether their local neighborhood block (consisting of roughly 150 households) between ages 12–17 have a SA participation rate above the median within neighborhood income and urbanicity bins to

29. Over 40% of individuals from high-socioeconomic backgrounds (mothers more than 12 years of schooling and household income during childhood above the national median) participated in SA at some point between ages 18–29.

30. This alone predicts 10 more months of SA take-up between ages 18–24; see Appendix Figure D.2(b).

Figure 2.3: Risk groups are highly predictive of later-life SA participation



Notes: This figure compares SA participation between risk groups and a high-socioeconomic status (SES) group outside of the main sample, comprised of individuals whose household income is above the national median or mothers with more than 12 years of schooling. Data constrained to individuals born before July 1, 1967 who did not face the SA reforms between ages 18–24 (i.e., $D(b_i) \geq 25$).

ensure risk groups represent similar geographic areas.[31]

The high-risk group is comprised of individuals who come from households that participated in SA *and* from a high-SA neighborhood, while the low-risk group is comprised by those from households that did *not* participate in SA *and* resided in a low-SA neighborhood. Figure 2.3 shows that risk groups are highly predictive of SA participation later in life. On average, high-risk individuals participate in SA for 18.4 months between ages 18–24; this is 13.3 months higher than the low-risk group. In addition, the low-risk group takes up just 0.5 months more months of SA than the high-socioeconomic status individuals excluded in my main sample, suggesting that they face very similar exposure to the reforms as the broader population.[32] To ensure my results are not sensitive to this definition of risk groups, I try alternate measures of risk groups in my robustness analysis.

Appendix Table D.1 reports summary statistics among the high-risk group and their

31. I group neighborhood blocks based on $1,000 bins of mean household income between 1980–1998 and whether they belong to a municipality with a population less than 10,000 (rural), between 10,000 and 50,000 (suburban), or greater than 50,000 (urban). See Damm and Schultz-Nielsen (2008) for details on the neighborhood blocks. High-SA neighborhoods alone predicts 3 more months of SA take-up between ages 18–24; see Appendix Figure D.2(c).

32. Appendix Figure D.3 shows that risk groups predict SA participation extremely similarly to a noisy proxy of SA eligibility groups, increasing confidence that risk groups capture policy-relevant population strata.

differences from the low-risk group. Relative to the low-risk group, high-risk individuals earn \$8,000 lower market income, spend 0.1 more days in inpatient hospital care, and sentenced 1.11 incarcerated prison per annum between ages 30–37. High-risk starting age cohorts ($8 \leq \alpha \leq 15$) tend to come from more disadvantaged families than their older counterparts ($25 \leq \alpha \leq 30$), revealing there are negative trends in socioeconomic background over birth cohorts. Against this context, it is striking that the long-run outcomes of high-risk group with starting ages $8 \leq \alpha \leq 15$ are similar or more favorable than $25 \leq \alpha \leq 30$. These summary statistics highlight the importance of conditioning on $\boldsymbol{X}_i$ to account for differences between risk groups and cohorts.

Identified parameters. I modify the TD design by using risk groups instead of eligibility groups. This relies on the following assumption:

Assumption 1 (Conditional parallel trends among risk groups)**.** *Assumption 1***, replacing all instances of* $G_i^\star$ *with* G_i*.*

If this parallel trends assumption is satisfied, the TD identifies

$$\beta_{\alpha,a} \equiv \mathbb{E}[Y_{ia}(\alpha) - Y_{ia}(\infty) \mid D_i = \alpha, G_i = 1] \tag{2.1}$$

for certain (α, a) pairs. This parameter is the effect of the high-risk group $G_i = 1$ being exposed to the welfare-to-work reform starting at age α at outcome age a. Note that $\beta_{\alpha,a}$ differs from the original parameter of interest, $\beta^\star_{\alpha,a}$, though it arguably holds greater policy relevance since it reflects the effect among those likely most affected by the reforms. In order to focus on starting age heterogeneity, I also consider its average over outcome ages,

$$\beta_\alpha \equiv \frac{1}{A_\alpha - \alpha + 1} \sum_{a \geq \alpha} \beta_{\alpha,a}, \tag{2.2}$$

where A_α is the highest possible age for α.

What is the interpretation of $\beta_{\alpha,a}$? Note that the TD strategy compares risk groups with different likelihoods and intensities of being affected by the reforms, so it follows a similar spirit as fuzzy DD designs (de Chaisemartin and D'Haultfoeuille 2018). With this in mind, the parameter $\beta_{\alpha,a}$ can be interpreted as a lower bound of $\beta^{\star}_{\alpha,a}$ if, on average, the effect of the reforms on the high-risk group is larger than that of the low-risk group (Fricke 2017).

Many papers rely on variation in intensity of treatments in DD designs (e.g., Duflo 2001, Havnes and Mogstad 2011). Most studies examining the impact of welfare reform on adolescents rely on DD methods between risk groups defined by higher- versus lower-income or -educated households (Kaestner et al. 2003; Hao and Cherlin 2004; Offner 2005; Bastian et al. 2021a, 2021b). Nevertheless, risk groups defined by socioeconomic background may be prone to diverging trends due to business cycles, structural labor market changes, or increasing returns to schooling over time. My analysis alleviates this concern by using a disadvantaged sub-sample. Indeed, Appendix Figure D.2 shows that the gap in later-life SA participation between my risk groups persists even *conditional on* socioeconomic background.[33]

The family of starting age-outcome age effects that can be identified is limited by the oldest birth cohort in my sample, which has starting age $\alpha = 30$. This is because the 1994 reform introduces work requirements to *all* adults, causing all individuals in the high-risk group to be assigned to a starting age group ($D_{ib} < \infty$). This means the family of identified effects is $\{\beta_{\alpha,a} \,|\, \alpha \leq 29, \alpha \leq a \leq 29\}$. In other words, effects can be identified only among starting ages below the largest starting age ($\alpha \leq 29$) and among outcome ages before the largest starting age faces the reforms (so $A_{\alpha} = 29$).

To give a sense of longer-run impacts over the life-cycle, I also identify starting age-

33. The figure shows lower household income predicts higher later-life SA participation. However, my analysis employs a conditional parallel trends assumption that directly controls for socioeconomic background.

outcome age effects *relative to* starting age $\alpha = 30$,

$$\beta^{30}_{\alpha,a} \equiv \mathbb{E}[Y_{ia}(\alpha) - Y_{ia}(30) \,|\, D_i = \alpha, G_i = 1], \tag{2.3}$$

and define β^{30}_{α} analogously. This parameter can be identified by using only the $\alpha = 30$ cohort as a comparison group instead of all cohorts $\alpha > a$. The advantage of this parameter is that it can be identified for outcome ages beyond 29. Specifically, the family of identified effects is $\{\beta^{30}_{\alpha,a} \,|\, \alpha \leq 29, \alpha \leq a \leq A_\alpha\}$, where A_α is the highest age available in the data. Checking if $\beta_{\alpha,a} = \beta^{30}_{\alpha,a}$ for ages $a \leq 29$ tests whether my identification strategy is sensitive to using different starting age cohorts as comparison groups.

2.3.4 Empirical Implementation

I use two types of TD regression specifications to estimate the effects of the welfare-to-work reforms on intragenerational outcomes.

First, I estimate an event-study specification to allow the reforms to have different effects over the life-cycle. This captures sensitivity of facing work requirements at certain outcome ages, any snowball or fade-out effects, and dynamics from the increasing intensity of requirements over time.

Specifically, I run a separate event study for each starting age $\alpha \in [8, 29]$ to estimate $\beta_{\alpha,a}$ for all outcome ages $a \geq \alpha$. This involves finding appropriate subsets of panels. For a given α, I restrict all panels to the range of outcome ages available for starting age α up to age $A_\alpha = 29$, denoted $\mathcal{A}_\alpha$. I further restrict panels of outcomes such that, for each outcome age $a \in \mathcal{A}_\alpha$, individuals belong to either the treated or not-yet treated starting age cohorts:

$D(b_i) = \alpha$ or $D(b_i) > a$. On this subsample, I run my preferred regression specification,[34]

$$
\begin{aligned}
Y_{ia} = & \sum_{k \in \mathcal{A}_\alpha \setminus \{\alpha-1\}} \beta_{\alpha,k} \Big(G_i \times \mathbb{1}(D(b_i) = \alpha) \times \mathbb{1}(a = k) \Big) \\
& + \delta_1 G_i + \delta_{b(i)} + \delta_a + \pi_{12} \Big(G_i \times \mathbb{1}(D(b_i) = \alpha) \Big) + \pi_{1,a} G_i + \pi_{2,a} \mathbb{1}(D(b_i) = \alpha) \\
& + \boldsymbol{\gamma} \boldsymbol{X}_i + \boldsymbol{\eta}_1 \boldsymbol{X}_i \times a + \boldsymbol{\eta}_2 \boldsymbol{X}_i \times b_i + \theta_{m_{\alpha-1}(i)} + \varepsilon_{ia}.
\end{aligned} \tag{2.4}
$$

To estimate $\beta^{30}_{\alpha,a}$, I instead define $\mathcal{A}_\alpha$ to include all outcome ages available for starting age α but subsample the data to individuals with $D(b_i) \in \{\alpha, 30\}$ only.

The parameters $\{\beta_{\alpha,k} \,|\, k \leq \alpha - 2\}$ serve as the "pre-trends" test to validate Assumption 1. They represent any differences in trends between the high-risk $D(b) = \alpha$ cohort and comparison $D(b) > a$ cohorts before either face the reforms; accordingly, I should expect $\beta_{\alpha,k} = 0$ for $k \leq \alpha - 2$. However, for several outcome variables Y_{ia}, the earliest age available in the data exceeds the starting age for the early starting age groups. For example, income is available starting at age 16, so there are no pre-trends available for starting ages $\alpha \leq 16$. In these cases, the event-study parameters are normalized around outcome age 17 instead of $\alpha - 1$. This choice of scale is innocuous for outcomes that occur at very low frequencies before age 18 (e.g., income, SA participation). For outcomes that frequently occur before age 18 (e.g., crime), I rescale estimates of $(\beta_{\alpha,a})_{a \in \mathcal{A}_\alpha}$ to the analogous DD event-study estimate on outcomes measured at age 17 (i.e., differences between risk groups, differenced over birth quarters) for $\alpha \leq 17$.[35] Given their lack of pre-trends, I conduct a series of falsification tests to check if estimates for early starting ages are driven by confounding factors.

34. Hence, all parameters in Equation (2.4) vary by α. I do not index parameters as such to reduce notational burden.

35. Specifically, I run the following DD regression:

$$
Y_{i17} = \sum_{\alpha \leq 16, \ \alpha \geq 18} \beta_{\alpha,17} \left(G_i \times \mathbb{1}(D(b) = \alpha) \right) + \delta_1 G_i + \delta_b + \boldsymbol{\gamma} \boldsymbol{X}_i + \boldsymbol{\eta} \boldsymbol{X}_i \times b_i + \theta_{m_{16}(i)} + \varepsilon_i.
$$

I checked that DD estimates on outcomes with low frequencies at age 17 are null, validating normalization at $a = 17$.

In addition to directly controlling for pre-determined covariates $\boldsymbol{X}_i$, Equation (2.4) also controls for linear trends of $\boldsymbol{X}_i$ over birth cohorts b and over the life-cycle a. These serve as a parametric means of satisfying Assumption 1. They ensure that the $\beta_{\alpha,a}$ estimates are not confounded by differential effects of background characteristics along either dimension.[36]

Equation (2.4) has a similar spirit as the interacted event-study model proposed by Sun and Abraham (2021), though there are some notable differences. First, my model uses ages over the life-cycle instead of "event time" (i.e., years relative to starting age of treatment). Second, my model adjusts the comparison cohorts to those who are not-yet-treated at each outcome age (by virtue of my subsampling scheme) instead of a single comparison cohort used over all outcome ages.[37] Finally, my model explicitly controls for trends in $\boldsymbol{X}_i$ to accommodate conditional parallel trends.

The second specification I use is a traditional TD regression that estimates β_α (β_α^{30}), the average effect between ages α and $A_\alpha = 29$ ($A_\alpha =$ highest available age) for each starting age α[38]:

$$\begin{aligned} Y_{ia} = \beta_\alpha\Big(G_i \times \mathbb{1}(D(b_i) = \alpha) \times \mathbb{1}(a \geq \alpha)\Big) \\ + \delta_1 G_i + \delta_b + \delta_a + \pi_{12}\Big(G_i \times \mathbb{1}(D_{ib} = \alpha)\Big) + \pi_{1,a} G_i + \pi_{2,a}\mathbb{1}(D_{ib} = \alpha) \\ + \boldsymbol{\gamma}\boldsymbol{X}_i + \boldsymbol{\eta}_1 \boldsymbol{X}_i \times a + \boldsymbol{\eta}_2 \boldsymbol{X}_i \times b + \theta_{m_\alpha(i)} + \varepsilon_{iba}. \end{aligned} \tag{2.5}$$

Estimates of β_α are more precise than the event-study estimates and provide a simple way to capture heterogeneity across starting ages.[39] As I show in the next section, starting age heterogeneity (rather than outcome age heterogeneity) is of primary importance. For the

36. Nevertheless, my results are robust to excluding these trends.

37. Callaway and Sant'Anna (2021) suggests using comparisons with the not-yet-treated group.

38. For outcomes with frequent, I adapt the DD regression described in Footnote 35 by replacing the outcome variable with the mean outcome over all ages observed up to $a = 17$.

39. Appendix Figure D.18 verifies that the event-study estimates of $\beta_{\alpha,a}$ from Equation (2.4) averaged over outcome ages $a \geq \alpha$ are closely aligned with the triple difference estimates of β_α from Equation (2.5).

sake of readability, all tables of regression results report estimates of β_α and β_α^{30} from (2.5); only figures use $\beta_{\alpha,a}$ and $\beta_{\alpha,a}^{30}$ from (2.4).

Standard errors are two-way clustered by the municipality of residence at age $\alpha-1$ and by birth quarter (Cameron et al. 2011). Municipalities had some discretion over implementing local SA work requirements. Birth quarters demarcate birth cutoffs of the starting age cohorts and capture cyclical patterns, such as variation in school starting age.[40]

40. In Denmark, birth years determine when children enter 1st grade, so children born in the 1st quarter tend to be younger than those born in the 4th quarter. Landersø et al. (2017, 2020) show this has significant effects later in life.

CHAPTER 3

LONG-RUN EFFECTS OF WELFARE-TO-WORK

3.1 Main Results: Long-Run Intragenerational Effects

This chapter reports my dissertation's main results: the intragenerational effects of welfare-to-work on a multitude of outcomes. I organize the discussion in two parts. First, I summarize effects on key outcomes on income, education, health, crime, and participation in SA and other social programs over the starting age dimension. I juxtapose patterns of starting age heterogeneity across different outcomes and interpret them together through the lens of my model. Next, I take a closer look at each category of outcomes, with an emphasis on labor market outcomes. I examine how estimates evolve over the outcome age dimension and provide more comprehensive sets of results. The section concludes with a battery of robustness checks to help increase confidence in my results.

3.1.1 Summary of Starting Age Heterogeneity across Outcomes

Table 3.1 summarizes estimated effects of the welfare-to-work reforms on eight later-life outcomes. Each row reports TD estimates of β_α and β_α^{30} from Equation (2.5) aggregated by four key starting age groups: $8 \leq \alpha \leq 15$, $16 \leq \alpha \leq 18$, $19 \leq \alpha \leq 24$. As a visual aid, Figure 3.1 plots these aggregate estimates (as horizontal black bars) as well as the individual β_α estimates (as gray dots) for three of the main outcomes. The estimates from the table and figure showcase striking heterogeneity in effects across starting ages, and that the nature of this heterogeneity differs across outcomes.

To begin, Table 3.1, Rows 1 and 2 report estimated aggregated effects of the SA reforms on participation in SA, in total (including passive benefit receipt and work requirements) and specifically in work requirements. I find that the youngest starting age group $8 \leq \alpha \leq 15$ has lower overall take-up of the SA program. While this aggregate estimate is

Table 3.1: Summary of welfare-to-work's effects on main outcomes, by starting age group

	Effect	Starting age group				High-risk mean
		$8 \leq \alpha \leq 15$	$16 \leq \alpha \leq 18$	$19 \leq \alpha \leq 24$	$25 \leq \alpha \leq 29$	[S.D.]
		(1)	(2)	(3)	(4)	(5)
1. SA participation, total (months)	β_α	−0.104 (0.073)	0.142* (0.071)	0.034 (0.070)	0.014 (0.055)	2.37 [3.05]
	β_α^{30}	−0.123 (0.092)	−0.049 (0.084)	−0.130 (0.107)	−0.002 (0.055)	1.98 [2.74]
2. SA work req's (months)	β_α	0.439*** (0.024)	0.362*** (0.026)	0.173*** (0.020)	0.117*** (0.016)	0.562 [1.09]
	β_α^{30}	0.320** (0.030)	0.136** (0.031)	0.060** (0.025)	0.095** (0.018)	[0.52] [0.98]
3. Market income (thousands $)	β_α	1.083*** (0.181)	0.225 (0.215)	−0.049 (0.123)	0.153 (0.238)	16.3 [10.8]
	β_α^{30}	1.123** (0.360)	0.246 (0.430)	−0.614* (0.260)	−0.235 (0.227)	19.9 [12.7]
4. Employed at least part-time	β_α	0.012 (0.007)	−0.009 (0.007)	0.000 (0.005)	0.002 (0.006)	0.606 [0.319]
	β_α^{30}	0.041** (0.013)	0.029* (0.012)	0.016* (0.008)	0.014** (0.005)	0.632 [0.312]
5. Years of schooling	β_α	0.074*** (0.022)	0.013 (0.029)	−0.011 (0.022)	−0.012 (0.016)	10.5 [1.54]
	β_α^{30}	0.061* (0.035)	0.013 (0.040)	−0.047 (0.046)	−0.059 (0.040)	10.79 [1.74]
6. Inpatient hospital care (days)	β_α	−0.072** (0.028)	−0.018 (0.036)	−0.043 (0.026)	0.083* (0.041)	0.81 [1.78]
	β_α^{30}	−0.048 (0.061)	−0.017 (0.062)	−0.060 (0.051)	−0.014 (0.037)	0.72 [1.46]
7. Incarceration sentence (days)	β_α	0.071 (0.151)	0.218 (0.198)	0.440** (0.181)	0.149 (0.192)	1.94 [14.28]
	β_α^{30}	0.263 (0.224)	0.333 (0.240)	0.276 (0.180)	−0.146 (0.176)	1.80 [12.75]
8. DI benefits ($)	β_α	−15.4 (30.7)	−67.5* (36.3)	90.8** (40.3)	5.0 (44.3)	390 [2,296]
	β_α^{30}	7.5 (88.1)	28.27 (107.2)	145.6 (100.4)	−32.9 (100.2)	746 [3,062]

Notes: Columns (1)–(4) report estimates of β_α and β_α^{30} from Equation (2.5), aggregated by starting age group. Column (5) reports the pooled mean outcome among the high-risk group averaged between ages 18–29 (for β_α) or ages 18–37 (for β_α^{30}); standard deviations of outcomes are in brackets. Row 5, β_α^{30}, reports estimates relative to starting age $\alpha = 29$ (not $\alpha = 30$) due to data constraints. All regressions estimates are based on main sample of approximately 170,000 low-risk and 60,000 high-risk individuals. Standard errors are two-way clustered over birth quarters and municipality of residence and reported in parentheses.

Stars indicate statistical significance: $^{\star}p < 0.10$, $^{\star\star}p < 0.05$, $^{\star\star\star}p < 0.01$.

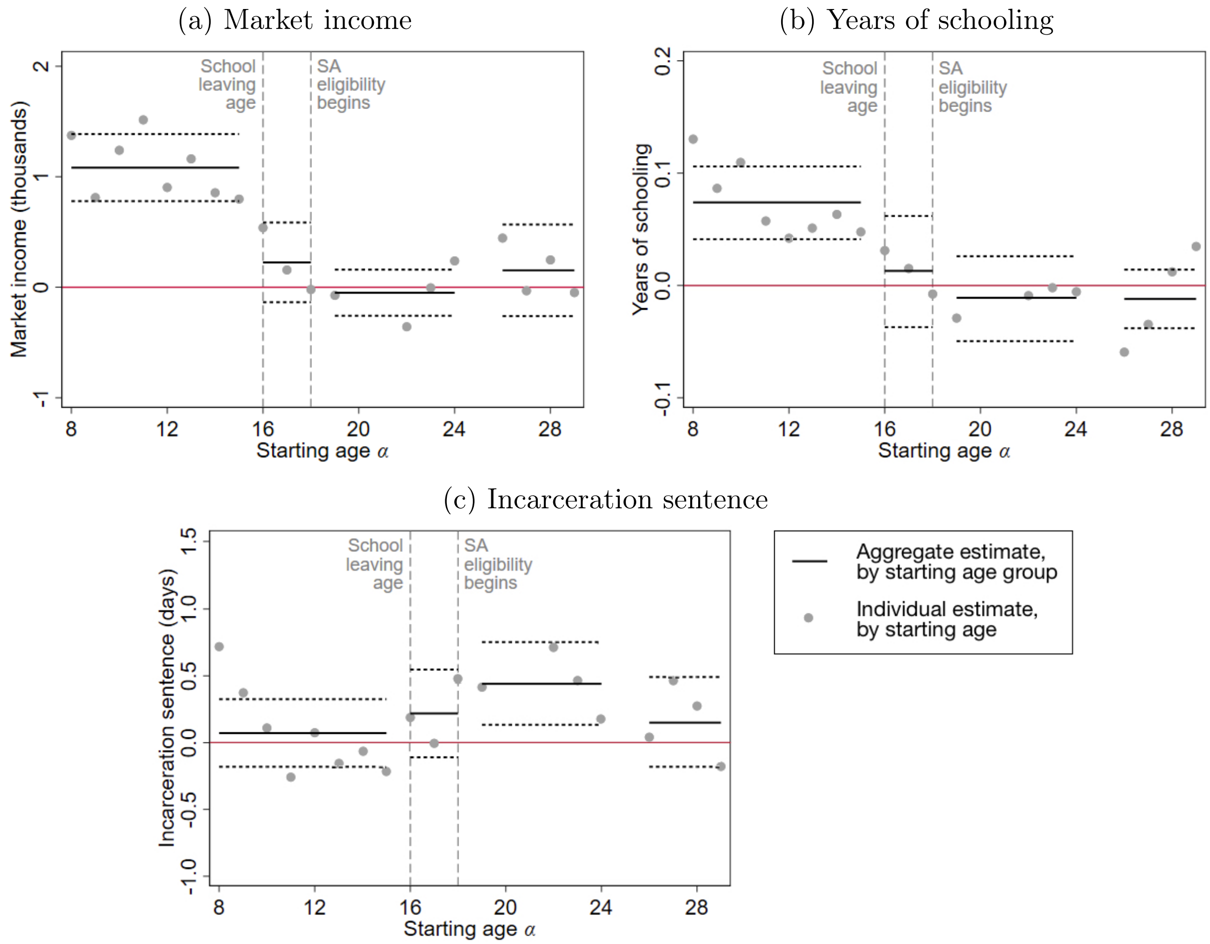
statistically insignificant, Appendix Figure D.4, Panel (a) reveals this masks a strong age gradient where the youngest starting age cohorts have statistically significant declines in overall SA participation by up to -0.35 months per annum. Meanwhile, starting ages $16 \leq \alpha \leq 18$ have modestly positive uptake of SA, though this does not appear to persist beyond age 29 based on its β_{α}^{30} estimate. The older starting age groups have no significant changes in their levels of SA participation. However, as expected, all starting age groups experience robust, statistically significant increases in their participation in SA work requirements after the reform. The take-up of SA work requirements decreases in starting ages, reflecting the increased intensity of SA work requirements from the reforms over time and that SA take-up monotonically declines over the life-cycle; see Appendix Figure D.6.

Rows 3 and 4 report estimated effects on two key labor market outcomes: market income (before taxes and transfers) and part-time employment (measured by market income above \$7,000).[1] Estimates on market income are statistically significantly positive among younger cohorts who were in school at the time of the reforms, ranging around \$1,000–\$1,100 per year. But as soon as α crosses 16—corresponding to the most common minimum school leaving age—estimates drop to statistically insignificant levels. Figure 3.1, Panel (a) reveals a few of the point estimates for young starting ages between $19 \leq \alpha \leq 24$ dip below 0; in fact, the corresponding aggregate estimate of β_{α}^{30} finds that this decline is modestly statistically significant. Meanwhile, the aggregate β_{α} estimates for part-time employment are statistically insignificant for all starting age groups, but all are uniformly significant at modest levels relative to the $\alpha = 30$ cohort. These findings suggest that the SA reforms cause individuals to earn more and eventually attain higher employment status.

Effects on years of schooling, reported in Row 5, have qualitatively similar heterogeneity as effects on market income: the earliest starting age group reaps 0.074 additional years of

1. The \$7,000 threshold corresponds to the Danish government's "basic amount" level used to classify self-sufficiency. Appendix Figure D.5 reports estimates for $8 \leq \alpha \leq 15$ and $16 \leq \alpha \leq 18$ for alternative thresholds.

Figure 3.1: Triple difference estimates of welfare-to-work's effects on select outcomes, by starting age



Notes: This plots estimates of β_α from (2.5) for different outcomes. Horizontal solid lines are aggregate estimates of β_α by starting age group; dashed lines are 90% confidence intervals based on robust standard errors two-way clustered over birth quarters and municipalities. Gray dots are the point estimates of β_α for each starting age α. Appendix Figure D.4 provides a version of this figure with confidence intervals around each estimate of β_α.

schooling, while starting ages $\alpha \geq 16$ who face the reforms after the school leaving age remain largely unaffected. The gray dots of Figure 3.1, Panel (b) illustrates evidence of a gradient where earlier starting age cohorts tend to attain more education. These results corroborate my basic life-cycle model's prediction: the reforms have higher impacts on later-life income of among younger starting ages than their older counterparts since they substitute away from SA to education. Increased schooling, coupled with high extensive margin labor supply response, explains the reforms' large effects on market income.[2]

Rows 6–8 report heterogeneous effects on inpatient hospital care, incarceration sentence, and DI participation—outcomes that are traditionally ignored in the welfare-to-work literature but are critical ingredients for cost-benefit analysis. Row 6 shows the reforms reduce the duration of inpatient hospital care among the youngest starting age group by 0.07 days per year. There is again a break for $\alpha \geq 16$, where effects on hospitalizations are insignificant and, if anything, modestly increase for the oldest group $25 \leq \alpha \leq 19$. Starting age heterogeneity for incarceration, however, possesses a starkly different pattern. Figure 3.1, Panel (c) reveals statistically and economically significant effects on incarceration sentences for $\alpha \in [19, 24]$, but null effects for the youngest and oldest starting ages. Finally, Row 8 finds that evidence that the reforms decreased disability insurance (DI) benefit receipt for $\alpha \leq 18$ but led the $19 \leq \alpha \leq 24$ group to substitute toward it.

To make better sense of these heterogeneous patterns, Table 3.2 illustrates (through different colors) how these groups possessed distinct choice margins at the time of the reforms; Appendix Figures D.6–D.10 empirically validate this by plotting life-cycle dynamics of choices conditional on the age they are chosen. Two margins differ in their availability across groups. First, the education margin was fully available to the first group, who were exposed to the reforms *before* they could leave school, and either partially available or unavailable to older groups who were beyond the school leaving age.[3] Second, the crime margin was available for all but the oldest group, who faced the reform after they passed the peak of the crime-age profile (see Appendix Figure D.8). Meanwhile, all starting age groups possessed

2. Appendix Figure D.5 finds that the $8 \leq \alpha \leq 15$ group experienced a 6 percentage point increase in extensive labor supply (measured by market income lying above \$500) between ages 18–29; given a mean market income of \$16,300, this translates to about \$970 of the \$1,083 increase in market income. The remaining effect on market income implies a 0.09 internal rate of return to schooling, which is above the Danish population estimate of 0.06 reported in Harmon et al. (2003) but within the range of estimates of other countries. Indeed, it is plausible that the returns to schooling are higher for completing schooling above 9th grade, especially among a highly-disadvantaged subpopulation.

3. In Denmark, adolescents are required to complete 9th grade, which most often occurs at age 16.

Table 3.2: Availability and change in utilization of choice margins, by starting age group

Choice margin	***Starting age group*** $8 \leq \alpha \leq 15$	$16 \leq \alpha \leq 18$	$19 \leq \alpha \leq 24$	$25 \leq \alpha \leq 29$
Education	↑	—	—	—
Crime	—	—	↑	—
Employment⋆	↑	↑	↑	↑
SA (total)	↓	↑	—	—
SA work requirements	↑	↑	↑	↑
Other social programs (DI, UI)	↓	↓	↑	—

Notes: This table illustrates the availability of choice margins for members of different starting age groups at the time of facing the SA reforms. Green cells indicate choice margins that are available to each starting age group. Yellow cells indicate partial availability; red cells very little availability. The ↑, —, and ↓ symbols indicate if estimates of the reforms on outcomes associated with the choice margins are positive, largely null, or negative for each starting age group.

⋆Based on estimates of effects on part-time employment relative to $\alpha = 30$.

margins over employment and SA. DI was only partially available to all groups depending on whether individuals met their eligibility criteria.

Differences in availability of choice margins help rationalize the heterogeneous patterns across starting age groups across outcomes, which are indicated via symbols in Table 3.2. Starting ages $8 \leq \alpha \leq 15$ who were already in school opted to attain more education and employment in anticipation of facing work requirements. Starting age groups $16 \leq \alpha \leq 18$ and $19 \leq \alpha \leq 24$ lacked this margin, and instead responded to SA work requirements by moving toward crime or increasing their dependence on social programs like SA or DI. Finally, starting age group $25 \leq \alpha \leq 29$ showcase null effects in education and crime, with modest increases in long-term employment beyond age 29.

Appendix Tables D.2 and D.3 validate that starting age heterogeneity is robust to alternative specifications of the TD regression that exclude additional controls. Adding controls and fixed effects of municipality of residence (prior to the reform) improve estimates' precision, while adding trends in background characteristics increases the magnitude of results since socioeconomic background tends to decline over birth cohorts.

3.1.2 Estimated Effects over Outcome Ages

With a high-level understanding of heterogeneity across starting age cohorts, I now take a closer look at the impact of reforms on long-run outcomes. In particular, I study the extent that starting age heterogeneity persists over the outcome age dimension. For the sake of brevity, this section focuses on labor market outcomes and briefly summarizes effects on other categories of outcomes.

Labor market outcomes. Table 3.3 reports event-study estimates of the starting age-outcome age effects $\beta_{\alpha,a}$ from Equation (2.4) for a variety of labor market outcomes. To gain precision and improve readability of results, I average TD event-study estimates over ranges of outcome ages, by the four starting age groups used earlier. Event-study estimates of individual outcome ages are provided in Appendix Figures D.11–D.14.

Panel A of Table 3.3 reports aggregated event-study estimates for market income (before taxes and transfers). There are two key takeaways. First, among older starting age groups $19 \leq \alpha \leq 24$ and $25 \leq \alpha \leq 29$, there are precisely estimated null pre-trends prior to the reform (indicated by the shaded cells), giving greater confidence in the triple difference identification strategy.[4] Second, heterogeneity across starting ages tend to persist over the life-cycle. Starting ages $8 \leq \alpha \leq 15$ experiences a growing *positive* effect on market income that peaks by their mid-20s, with more attenuated and insignificant effects relative to $\alpha = 30$ after age 30. Meanwhile, $19 \leq \alpha \leq 24$ incurs statistically significant *negative* effects throughout adulthood that persists at modestly significant levels even beyond age 40 (relative to $\alpha = 30$). Negative effects on market income may be driven by increased incarceration and disability insurance receipt among this starting age group.

The reforms' effects on disposable income (after taxes and transfers), reported in Panel B,

4. In addition, Appendix Figures D.11–D.14 illustrate that estimates of $\beta_{\alpha,a}$ and $\beta^{30}_{\alpha,a}$ generally track each other quite closely over all common outcome ages, providing confidence that my results are not spuriously driven by certain not-yet-treated cohorts with diverging trends.

Table 3.3: Event-study estimates on labor market outcomes, by starting age group

Starting age group	Main estimates ($\beta_{\alpha,a}$) $16 \leq a \leq 18$ (1)	$19 \leq a \leq 24$ (2)	$25 \leq a \leq 29$ (3)	Relative to $\alpha = 30$ ($\beta^{30}_{\alpha,a}$) $30 \leq a \leq 39$ (4)	$40 \leq a \leq 54$ (5)	High-risk mean [S.D.] (6)
A. Market income (thousands $)						
$8 \leq \alpha \leq 15$	0.092 (0.067)	0.935*** (0.185)	1.506*** (0.221)	0.764 (0.523)	0.232 (0.778)	21.1 [14.0]
$16 \leq \alpha \leq 18$	0.076 (0.076)	0.199 (0.236)	0.374 (0.309)	0.377 (0.571)	−0.558 (0.764)	22.3 [15.0]
$19 \leq \alpha \leq 24$	−0.038 (0.057)	−0.217* (0.127)	−0.409** (0.180)	−0.343 (0.307)	−1.159* (0.502)	22.9 [15.5]
$25 \leq \alpha \leq 29$	−0.052 (0.154)	−0.030 (0.093)	0.042 (0.210)	0.126 (0.309)	−0.427 (0.431)	24.8 [15.5]
B. Disposable income (thousands $)						
$8 \leq \alpha \leq 15$	−0.046 (0.059)	0.024 (0.124)	0.432*** (0.146)	0.424 (0.292)	0.202 (0.475)	21.9 [6.92]
$16 \leq \alpha \leq 18$	0.018 (0.051)	−0.041 (0.136)	0.133 (0.165)	0.466 (0.340)	−0.106 (0.459)	22.7 [7.22]
$19 \leq \alpha \leq 24$	−0.056 (0.068)	0.026 (0.124)	−0.122 (0.150)	0.114 (0.283)	−0.376 (0.404)	23.1 [7.36]
$25 \leq \alpha \leq 29$	−0.020 (0.110)	0.062 (0.048)	0.003 (0.097)	0.089 (0.123)	−0.214 (0.183)	23.8 [7.49]
C. Net taxes paid (thousands $)						
$8 \leq \alpha \leq 15$	0.138** (0.057)	0.911*** (0.106)	1.074*** (0.128)	0.340* (0.160)	0.031 (0.286)	−0.756 [8.51]
$16 \leq \alpha \leq 18$	0.058 (0.050)	0.240** (0.101)	0.240 (0.146)	−0.089 (0.228)	−0.453 (0.265)	−0.482 [9.09]
$19 \leq \alpha \leq 24$	−0.061 (0.079)	−0.243** (0.114)	−0.287 (0.181)	−0.457 (0.306)	−0.783* (0.335)	−0.220 [9.35]
$25 \leq \alpha \leq 29$	−0.032 (0.098)	−0.092 (0.067)	0.039 (0.058)	0.037 (0.142)	−0.214 (0.218)	1.053 [9.72]
D. Employed at least part-time						
$8 \leq \alpha \leq 15$	0.002 (0.004)	0.003 (0.009)	0.030*** (0.007)	0.037* (0.016)	0.048** (0.014)	0.620 [0.322]
$16 \leq \alpha \leq 18$	−0.001 (0.005)	−0.010 (0.011)	−0.002 (0.010)	0.034* (0.017)	0.030* (0.015)	0.623 [0.322]
$19 \leq \alpha \leq 24$	0.002 (0.004)	−0.010 (0.006)	−0.007 (0.008)	0.019 (0.014)	0.020 (0.013)	0.633 [0.320]
$25 \leq \alpha \leq 29$	−0.006 (0.007)	−0.002 (0.004)	−0.001 (0.004)	0.010 (0.009)	0.015 (0.008)	0.666 [0.310]

Notes: Columns (1)–(3) report estimates of $\beta_{\alpha,a}$ from Equation (2.4) aggregated by groups of starting ages and outcome ages; Columns (4)–(5) report estimates of $\beta^{30}_{\alpha,a}$. Column (6) reports mean outcomes among the high-risk group; standard deviations are in brackets. Shaded cells indicate pre-reform outcome ages. Standard errors are two-way clustered over birth quarters and municipality of residence and reported in parentheses.

Stars indicate statistical significance: $^{\star}p < 0.10$, $^{\star\star}p < 0.05$, $^{\star\star\star}p < 0.01$.

are far more attenuated than market income. Declines in disposable income among $19 \leq \alpha \leq 24$ are not statistically significant, indicating that this group is receiving more transfers that counteracts their declines in market income. Panel C reports estimates on net taxes, defined as the difference between gross income tax paid and public transfers received. Relative to $\alpha = 30$, the government raises about \$200–1,000 per year from $8 \leq \alpha \leq 15$ when they are between ages 19–39 but incurs costs of around \$200–500 per year from $19 \leq \alpha \leq 24$. This indicates that welfare-to-work's effects on different starting age groups can lead to uneven impacts on the government's budget. Finally, Panel D reports that, relative to $\alpha = 30$, part-time employment after age 30 increases across all groups by 0.01–0.05 percentage points, with weaker effects among older starting ages.

Other outcomes.

Education.—Appendix Table D.4 reports starting age group $8 \leq \alpha \leq 15$ experienced statistically-significant gains in educational attainment, including a 1.8 percentage point (4.2% relative to mean) increase in completing high school and 2.3 percentage point (6.3%) increase in attending post-secondary school.[5] Meanwhile, starting age group $16 \leq \alpha \leq 18$—who faced the reforms just after the school leaving age—do not experience such gains, highlighting how welfare-to-work only incentivized younger cohorts still in school at the time of the reforms ($8 \leq \alpha \leq 15$) to continue their education.[6] The event-study estimates illustrated in Appendix Figure D.15 reveal the older starting age groups $19 \leq \alpha \leq 24$ and $25 \leq \alpha \leq 29$ have flat pre-trends and that gains in schooling among $8 \leq \alpha \leq 15$ are concentrated between outcome ages 16–24 before gradually fading out.[7]

5. Following Karlson and Landersø (2021), high school completion and post-secondary school attendance are based on the threshold of completing 12 years of schooling.

6. Landersø and Heckman (2017) find descriptive evidence that the SA reforms had contemporaneous effects on college attendance among the full population born between 1969–1974.

7. This highlights how older cohorts slowly upgrade their educational credentials over time, as highlighted in Riddell and Riddell (2014) and Appendix Figure D.7. Fade-out of effects on educational attainment can help rationalize the fade-out of effects on income among this group.

Health.—Appendix Table D.5 validates that reductions of inpatient hospital care among $8 \leq \alpha \leq 15$ is robust to excluding hospitalizations related to birth.[8] Event-study estimates plotted in Appendix Figure D.17 reveals these declines are concentrated during young adulthood. Unlike other outcomes, inpatient hospital records are available throughout childhood, permitting me to validate there are no pre-trends for all starting age groups. Declines in inpatient hospital care among $8 \leq \alpha \leq 15$ may be interpreted as improvements in health since (i) there were no systemic institutional barriers preventing individuals from receiving hospital care and (ii) inpatient hospital care signals relatively serious health events since they involve overnight hospital stays.[9]

Crime.—In contrast, starting age group $19 \leq \alpha \leq 24$ experience increased incarceration. Appendix Table D.6 finds incarceration among this group increased by 0.2 percentage points (16.7% relative to mean) but overall convictions decreased, suggesting that increases in crime is driven by intensive margin responses among those already criminally active. However, this should be interpreted with caution since convictions tend to lump multiple charges that occurred over a span of time and focus on the most serious charged offense.[10] Appendix Figure D.16 illustrates that incarceration sentences for $19 \leq \alpha \leq 24$ have flat pre-trends, gradually grow to statistically significant levels by age 30, and fade-out thereafter, consistent with life-cycle profiles of crime.[11]

8. The table finds this decline is driven by hospitalizations related to internal diseases, such as bacterial infections, respiratory infections, and blood diseases.

9. Throughout my study's period, Danish healthcare was universal and free with automatic enrollment at birth.

10. Unfortunately, due to a break in the charge register data in the 1990s, I am unable to analyze the underlying charges or the exact time that the charged crimes occurred. For this reason, incarceration sentences will tend to be concentrated in more extreme offenses; effects on the types of offenses associated with the incarceration sentence should thus be interpreted with great caution. I can, however, observe the number of charges among children of the main sample. Section 4.2 finds evidence of *increased* number of charges among children born to these cohorts.

11. The gradual increase in incarceration sentencing is partly mechanical, as court sentencing tend to lag

Other social program participation.—Appendix Table D.7 reveals heterogeneity in take-up of social welfare programs across starting ages. Younger starting ages $\alpha \leq 18$ decreased their receipt of DI and UI by about 0.4 and 1.9 percentage points (17.7% and 9.1%). Already-eligible cohorts $19 \leq \alpha \leq 24$ receive substantially higher DI benefits; meanwhile, there appears to be no change in UI take-up.[12] These results reveal the SA reforms reduced overall welfare dependence among the youngest starting ages but generated incentives for already-eligible starting ages to increase usage of other programs.

3.1.3 Robustness Analysis

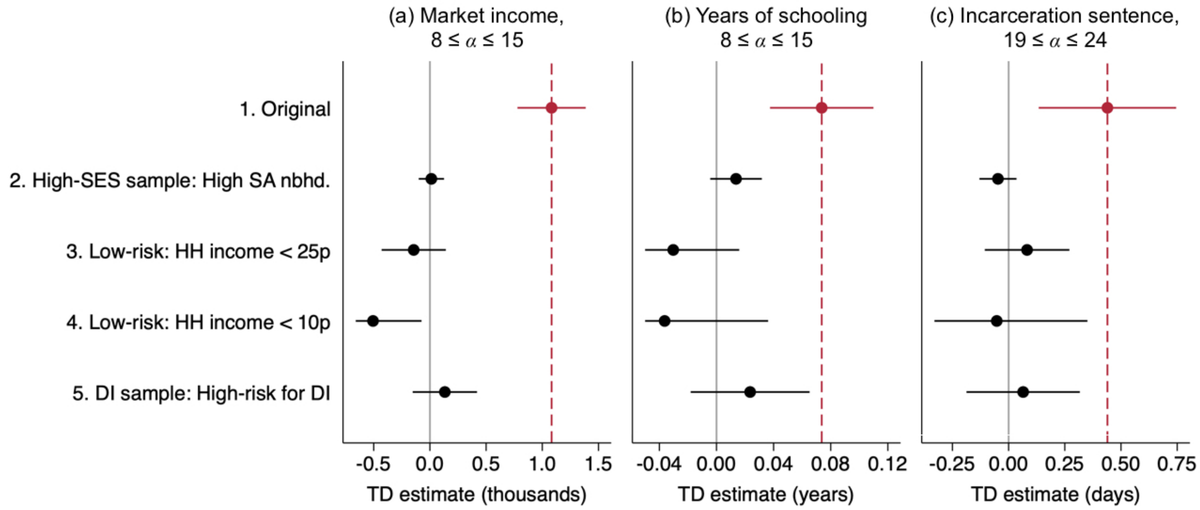
The previous results suggest there is significant heterogeneity in long-run treatment effects by starting age groups. This section examines possible threats to the internal validity of my research design and whether my results are robust to alternative measurement choices and model specifications.

The internal validity of my analysis rests on the parallel trends condition (Assumption 1). While my event-study estimates exhibit flat pre-trends for older starting age cohorts, many outcomes have left-censored panels that begin after the starting age of younger cohorts. Irrespective of the data availability, pre-trend tests do not guard against confounding factors that violate parallel trends, such as other policy changes or economic shocks that coincide with timing of the SA reforms. I focus on key falsification exercises that aim to uncover such confounding factors, followed by a brief summary of other robustness checks; details and results are provided in the appendices. Overall, I find my main results are highly robust to a battery of tests and alternative specifications.

the actual date of offenses by about 200 days and sometimes beyond one year. Kyvsgaard (2004) verifies there were no major changes to Danish criminal justice policy in the 1990s and early 2000s, giving greater confidence that the estimated effects reflect genuine responses to the welfare-to-work reforms.

12. This null result may be explained by a reform to the UI program, explained below and in Appendix C.2.3.

Figure 3.2: Falsification tests for select outcomes and starting age groups



Notes: This figure plots estimates of β_α from variations of Equation (2.5), aggregated by starting age group. Row 1 plots the original estimates (in red); vertical dashed lines signify their magnitude. Rows 2–5 are falsification tests (in black). Row 2 uses a high-socioeconomic status (SES) sample (defined by household income above median or mothers with more than 12 years of schooling) and defines treatment and control groups as individuals from high- and low-SA neighborhood blocks, respectively (see main text for more details). Row 3 uses low-risk sample and defines treatment and control groups as individuals whose household income (age 12–17) are in the first and second quartiles, respectively. Row 4 repeats the Row 3 except uses the first decile to define treatment and control groups. Rows 3–4 exclude household income and household assets as controls in the regression model to avoid overcontrolling for treatment status. Row 5 uses a subset of the base sample in which the treatment group is households that participated in disability insurance (DI) and not SA (ages 12–17) and the control group is households that participated in neither DI nor SA. Horizontal lines are 90% confidence intervals based on standard errors two-way clustered by birth quarter and municipality of residence in the age prior to the reform.

Main falsification tests. Figure 3.2 plots estimates on market income, years of schooling, and incarceration sentence from various specifications to test the presence of confounders. The first row reproduces my main estimates from Table 3.3 for reference. The remaining rows plot estimates on samples that should be unaffected by the SA reforms. This figure focuses on effects on starting age group $8 \leq \alpha \leq 15$ for income and schooling and $19 \leq \alpha \leq 24$ for incarceration, as these groups showcase the largest estimated effects for these outcomes.[13]

Row 2 finds precise null estimated effects among a high socioeconomic status (SES)

13. Appendix Figures D.19–D.22 plot estimates for all starting age groups and additional outcomes like inpatient hospital care. In short, these figures demonstrate that, like my main estimates, the falsification tests for other starting age groups have statistically insignificant effects. This demonstrates that my falsification tests are not systematically negatively biased relative to my main estimates.

sample who are unlikely to be affected by the SA reforms. In this specification, the risk group is replaced by an indicator of whether an individual grew up in a high-SA neighborhood block. In this way, this result demonstrates that differences in neighborhood blocks over time, such as school quality or local labor markets, do not lead to diverging later-life outcomes.

Another threat to my research design is the presence of other confounding policy or economic changes that predominantly affect the low-SES population, such as education reforms or structural changes in labor market.[14] Row 3 tests the presence of such confounders *within* the low-risk group $G_i = 0$ by comparing individuals between the first and second quartiles of the household income distribution. I find no evidence that such confounders affecting the low-income population drive my results. Row 4 repeats this exercise except focuses on effects among the bottom decile of the distribution, demonstrating that these results are not sensitive to my choice of income cutoff.[15]

One may be concerned that low-SES individuals who belong to the low-risk group are not subject to policy or economic changes that specifically affect welfare recipients. To check this, Row 5 plots estimates from a sample of low-SES individuals at risk of participating in DI, which targeted disadvantaged individuals with limited capacity for work. Here, the "high-risk for DI" group includes individuals from households on DI but *not* on SA, while the "low-risk for DI" group includes those whose households participated in neither program.Work requirements in DI were not introduced until 1998, only applied to DI recipients with partial work capacity, and were less intensive and more flexible than the SA program. Thus, estimates for individuals at high-risk for DI should reflect confounders that would also affect individuals at high-risk for SA. Reassuringly, the fifth row's estimates are statistically indistinguishable from zero, suggesting that my main estimates genuinely reflect effects of

14. One example is a 1988 student aid reform. Appendix C.2.1 details why this reform is not of particular concern.

15. These specifications exclude controls for household income and assets to avoid overcontrolling for the low-income group indicators.

the SA reforms.

Other robustness exercises. I also perform a variety of other checks that support my results. See Appendix C.2 for more details.

Pre-treatment outcomes.—I estimate DD event-study specifications to check if differences between risk groups vary across starting ages in early life outcomes, including inpatient hospital care in early adolescence and birthweight.[16] Appendix Figure D.23 shows estimated effects on adolescent hospital care and birthweight are pointwise insignificant for all starting ages. In fact, specifications that control for cohort trends in $\boldsymbol{X}_i$ fail to reject their joint equality.

Difference-in-differences.—I also apply the same DD event-study specifications on my main outcomes measured between outcome ages $16 \leq \alpha \leq 28$. Focusing on these early adulthood outcomes affords pre-trends for the oldest starting ages $\alpha \geq a$. Appendix Figures D.24–D.26 corroborate the patterns found in my main results and showcase either flat pre-trends or noticeable breaks in trends.

Validation exercises.—Appendix Figures D.19–D.22 report several validation checks. Some test whether the presence of a 1994 unemployment insurance (UI) reform that introduced work requirements for long-term UI recipients is responsible for my main results. Recall that, in most cases, UI members do not participate in SA, so the SA and UI reforms targeted different populations. I re-estimate effects for a subsample of high-risk individuals whose households lacked UI membership and find estimates that are similar or larger in magnitude

16. Note that my main TD specification uses life-cycle variation to difference out any fixed cohort-risk effects. However, one may be worried that early-life cohort-risk differences interact with exposure to SA reforms, leading to violations in parallel trends in later-life outcomes.

than my main estimates.[17]

Variation in municipal adoption.—The starting age cohorts used in my main results are defined by the timing of the *national* SA reforms. In reality, however, municipalities adopted work requirements at different times.[18] I exploit this variation to validate the heterogeneity in estimates across starting ages.[19] Appendix Figure D.27 shows that estimates for members of cohorts who lived in municipalities that adopted work requirements *before* the national reform resemble those of members of later cohorts who lived in other municipalities.

Alternative measurement choices and samples.—Finally, I apply a battery of robustness checks to ensure my main results are unaffected by particular choices made in my main model specification; see Appendix Figures D.29–D.32. Checks include alternative risk groups (e.g., defining high-risk with only household participation in SA), different subsamples of my main data (e.g., excluding the four most populous cities), and shortening the window of not-yet-treated cohorts used as a comparison group. All results survive these alternative measurement choices and sampling schemes.

17. I also estimate the effects of the *1994 UI reforms* on a separate, non-overlapping sample of individuals at high-risk of receiving UI benefits (defined in Appendix C.2.3). These estimates showcase qualitatively different starting age heterogeneity, providing reassurance that the UI reform does not explain my main results and that my TD design appears to identify effects of distinct policy reforms.

18. For example, the municipality Farum instituted work requirements in the late 1980s; see Fallesen et al. (2018).

19. To find municipal adoption dates, I estimate structural breaks in the available time series of SA work requirement take-up rates through a procedure similar to Card et al. (2008); I use these adoption dates to categorize municipalities into these two groups. See Appendix C.2.3 for details on this procedure. To the best of my knowledge, there are no institutional records of the exact timing that work requirements were implemented in each municipality.

CHAPTER 4

ANTICIPATION & INTERGENERATIONAL EFFECTS

4.1 Identifying Anticipation Effects among Younger Starting Ages

A key pattern uncovered in my main results is that younger cohorts exposed to the reform *before* reaching SA eligibility tend to fare better than their older counterparts through increased schooling and income. The simple model developed in Section 2.1.3 suggests this phenomenon is driven by *anticipation effects*—the effect of knowing the existence of SA work requirements before becoming eligible of actually facing them.

This section examines anticipation effects in two ways. First, I provide motivating evidence on SA take-up patterns at age 18 sharply changes across different starting age cohorts that is consistent with anticipatory behavior. Second, I show how my identified starting age-outcome age effects can be decomposed into anticipation effects and other component factors. I harness unique institutional details of the SA program and additional assumptions to quantify how much anticipation effects contribute toward the overall effects of the reforms for a subset of younger cohorts.

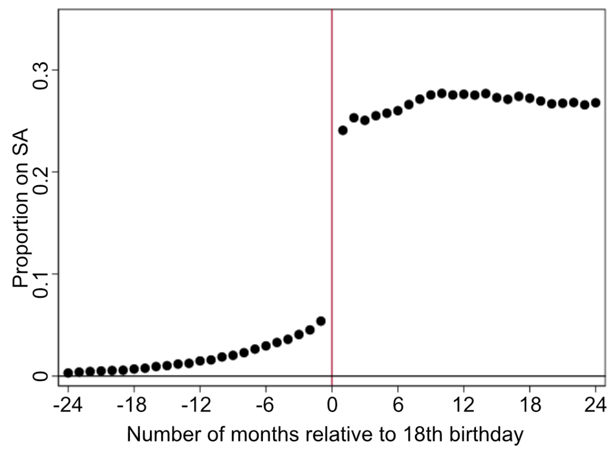
4.1.1 Take-up of Social Assistance around Outcome Age $a = 18$

I begin by providing empirical evidence that is consistent with younger starting age cohorts anticipating work requirements in the future. Figure 4.1, Panel (a) plots monthly high-risk SA take-up rates around the 18th birthday—when the vast majority of individuals become eligible for the program. SA take-up spikes *immediately* at the month of their 18th birthday, establishing that individuals claim SA benefits as soon as possible. Such a sharp increase suggests that SA rules are salient among individuals aging into eligibility and take-up at age $a = 18$ may be planned in advance.

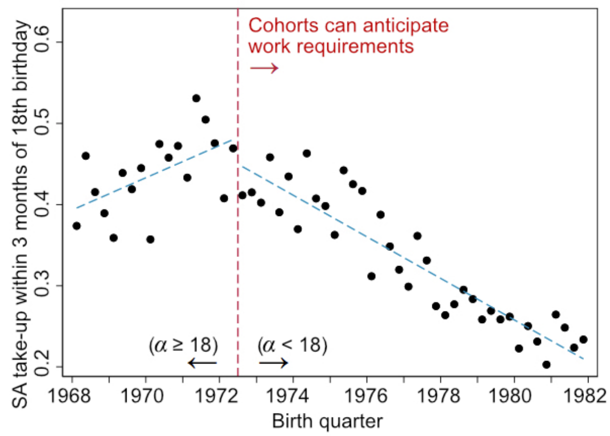
Were $\alpha = 17$ individuals just months or weeks away from gaining SA eligibility aware of

Figure 4.1: Monthly SA participation rates around the 18th birthday, by starting age group

(a) SA take-up spikes at age $a = 18$



(b) Spike in take-up changes around $\alpha = 18$



Notes: Panel (a) plots monthly take-up of SA around the 18th birthday, which is defined as ever receiving SA benefits by calendar month. Take-up at the 18th birthday (month 0) is excluded. Panel (b) plots the proportion who ever used SA up to the 3 months after the 18th birthday by birth quarter. In both panels, samples are restricted to the high-risk group $G_i = 1$.

SA work requirements? Panel (b) provides evidence consistent with this idea by plotting how SA take-up rate immediately following the 18th birthday evolves over birth cohorts. There is a kink in the SA take-up rate around July 1st, 1972—the date of birth cutoff corresponding to the July 1st, 1990 reform that separates starting ages $\alpha \geq 18$ and $\alpha < 18$. Individuals of starting ages $\alpha \geq 18$ did not face the reforms when they turned age 18; indeed, there is an upward trend in immediate SA take-up rate among these cohorts. Meanwhile, starting ages $\alpha \leq 17$ were aware of the reforms *before* reaching the formal SA eligibility age $a = 18$ and their immediate SA take-up rate declines. This kinked pattern is consistent with individuals becoming increasingly reluctant to participate in SA on their 18th birthday since they could anticipate facing work requirements upon entering SA.[1]

1. The binned data may suggest that the kink begins slightly before July 1st, 1972. This could arise since some municipalities implemented work requirements in the months leading up to the national reform, which would reduce the sharpness of the kink. Regardless of the exact cutoff date, there is a qualitative change in take-up rate patterns for cohorts born around 1972.

4.1.2 Identification of Anticipation Effects

Is it possible to quantify how much anticipation effects play a role in my previously-identified effects? The remainder of this section shows how, under stronger assumptions, anticipation effects can be identified for a subset of starting age cohorts. This requires developing a framework that decomposes starting age-outcome age effects into anticipation effects and other constituent factors.

All starting age-outcome age effects are aggregates of three distinct components. The first, of course, is the anticipation effect. The second component is the *intention-to-treat* (ITT) effect, defined as the effect of potentially facing SA work requirements after becoming eligible for SA.[2] The third is the *parent ITT* effect. Even though younger starting ages are ineligible before age 18, their parents could be affected by SA work requirements in ways that spillover to their children.[3]

To proceed with the decomposition, it is useful to formalize ideas by expanding the potential outcomes framework. Let D_i^{Antic} denote the starting age of when individuals face the anticipation effects, D_i^{ITT} the starting age of ITT effects from facing work requirements, and D_i^{Par} denote the starting age of their parent ITT effects. With this notation, all realized potential outcomes may be expressed as $Y_{ia}(D_i) = Y_{ia}(D_i^{Antic}, D_i^{ITT}, D_i^{Par})$.[4] Denote values of starting age of anticipation, ITT, and parent ITT as η, τ, and π; let their realized values given starting age α be η_α, τ_α, and π_α.

With this notation, I formally define the anticipation effect at starting age-outcome age

2. Note this captures the effect among "compliers" who actually participate in SA work requirements as well as those who opt to adjust along other choice margins.

3. Section 4.2 provides suggestive evidence of such spillovers on the subsequent generation. Note that anticipation and ITT effects may include parental responses to the *individual's* exposure to the reforms; in contrast, parent ITTs focus on the parents' *own* exposure to the SA reforms. Recall that the high-risk group's households participated in SA during their childhood, so their parents were also likely affected by the reforms.

4. There is a bijective mapping between the original starting age D_i and $(D_i^{Antic}, D_i^{ITT}, D_i^{Par})$; in other words, no information is lost from using $(D_i^{Antic}, D_i^{ITT}, D_i^{Par})$ instead of D_i and vice-versa.

(α, a) for fixed ITT and parent ITT starting age values (τ, π) as

$$\beta^{Antic}_{\alpha,a}(\tau, \pi) \equiv \mathbb{E}\left[Y_{ia}(\eta_\alpha, \tau, \pi) - Y_{ia}(\infty, \tau, \pi) \,|\, D_i = \alpha, G_i = 1\right]. \tag{4.1}$$

This represents the causal effect of anticipating work requirements starting at age η_α relative to facing work requirements as a surprise at age 18, holding all else fixed. I discuss which (τ, π) pairs I can identify the anticipation effect below. One can analogously define the ITT and parent ITT effects, $\beta^{ITT}_{\alpha,a}$ and $\beta^{Par}_{\alpha,a}$, as differences in generalized potential outcomes.[5]

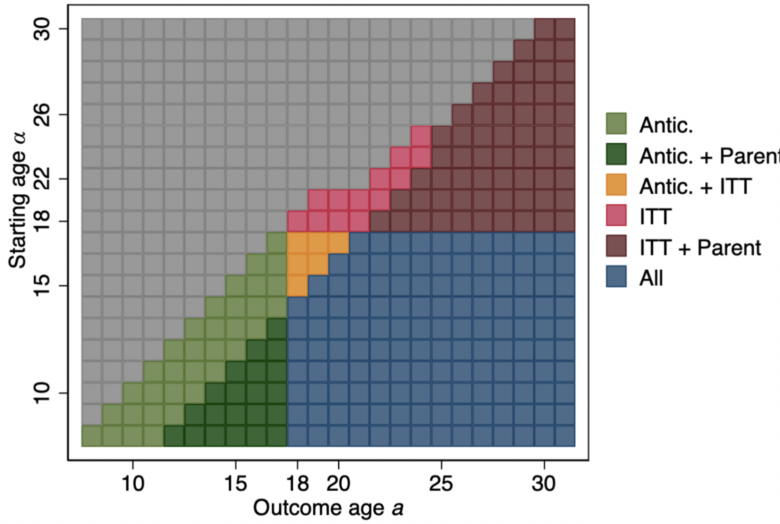
What conditions are required to disentangle the anticipation effect $\beta^{Antic}_{\alpha,a}$ from the other components? I utilize two ingredients. The first ingredient involves exploiting two unique features of the Danish institutional setting. First, as leveraged in Figure 4.1, the minimum eligibility age of 18 shuts down anticipation effects for $\alpha \geq 18$. Second, the Danish experience involved multiple welfare-to-work reforms that targeted different cohorts. The first reform in 1990 targeted only ages 18–19; it was not until January 1st, 1994 that work requirements broadened its reach to all recipients aged 18 and above. This feature shuts down the parent ITT effect for certain (α, a) pairs.[6]

Figure 4.2 illustrates how these features map the starting age-outcome age effects into different combinations of the three components. To begin, focus on starting ages $\alpha < 18$. For outcome ages $a < 18$, the identified effects are either only anticipation effects (light green cells) or anticipation and parent ITT effects (dark green cells). Note that anticipation starts at age $\eta_\alpha = \alpha$ while the parent ITT lags by four years. Once these starting age cohorts become eligible for SA at outcome age $a = 18$, the ITT effect comes into play (orange and blue cells). Meanwhile, for starting ages $\alpha \geq 18$, there is no anticipation effect: $\eta_\alpha = \infty$.

5. Specifically, the ITT for fixed (η, π) is $\beta^{ITT}_{\alpha,a}(\eta, \pi) \equiv \mathbb{E}\left[Y_{ia}(\eta, \tau_\alpha, \pi) - Y_{ia}(\eta, \infty, \pi) \,|\, D_i = \alpha, G_i = 1\right]$ while the parent ITT effects for fixed (η, τ) is $\beta^{Par}_{\alpha,a}(\eta, \tau) \equiv \mathbb{E}\left[Y_{ia}(\eta, \tau, \pi_\alpha) - Y_{ia}(\eta, \tau, \infty) \,|\, D_i = \alpha, G_i = 1\right]$.

6. Note that if the SA reforms affected the entire cross-section of the population at once, it would be impossible to separately identify the anticipation effect from the parent ITT effect since they arise simultaneously.

Figure 4.2: Underlying components of identified starting age-outcome age effects



Notes: This figure expresses identified starting age-outcome age effects as combinations of anticipation effects, ITT effects, and parent ITT effects defined in the main text. Gray cells indicate pre-reform starting age-outcome age combinations (i.e., whenever $a < \alpha$).

Thus, all starting age-outcome age effects are combinations of only ITTs and parent ITTs. In particular, cohorts closer to $\alpha = 18$ face only ITT effects (light red cells) for a number of years until the 1994 reform affects their parents (dark red cells). Critically, this mapping allows me to isolate the ITT effect and anticipation plus ITT effects in certain starting age-outcome age regions.

The second ingredient is the following assumption:

Assumption 2 (Conditional parallel trends with only ITT)**.** *Implicitly condition on* $\boldsymbol{X}_i$ *to reduce notational burden. For starting ages* $\alpha < 18$ *and outcome ages* $a < \pi_\alpha$*,*

$$\begin{aligned}
&(\mathbb{E}\left[Y_{ia}(\infty, 18, \infty) \mid D_i = \alpha, G_i = 1\right] - \mathbb{E}\left[Y_{i17}(\infty, 18, \infty) \mid D_i = \alpha, G_i = 1\right]) \qquad (4.2)\\
&\quad - (\mathbb{E}\left[Y_{ia}(\infty) \mid D_i = \alpha, G_i = 0\right] - \mathbb{E}\left[Y_{i17}(\infty) \mid D_i = \alpha, G_i = 0\right])\\
&= (\mathbb{E}\left[Y_{ia}(\infty, 18, \infty) \mid D_i = 18, G_i = 1\right] - \mathbb{E}\left[Y_{i17}(\infty, 18, \infty) \mid D_i = 18, G_i = 1\right])\\
&\quad - (\mathbb{E}\left[Y_{ia}(\infty) \mid D_i = 18, G_i = 0\right] - \mathbb{E}\left[Y_{i17}(\infty) \mid D_i = 18, G_i = 0\right]).
\end{aligned}$$

This assumption is similar to Assumption 1 but involves potential outcomes of *the ITT starting* at age $\tau_\alpha = 18$ without anticipation or parent ITT ($Y_{ia}(\infty, 18, \infty)$) instead of the pure *untreated* state ($Y_{ia}(\infty)$). In other words, Assumption 2 states that differences in potential outcomes of the ITT without anticipation between high- and low-risk $\alpha < 18$ cohorts evolve over the life-cycle in the same way as the corresponding differences for the $\alpha = 18$ cohort. Admittedly, this is a strong assumption since it involves potential outcomes of "partly treated" states.[7] A sufficient condition of Assumption 2 is a homogeneity condition: ITT effects do not depend on birth cohorts, but rather their assigned starting age determined by the timing of the reforms.[8]

Pairing this assumption with the original parallel trends assumption enables identification of $\beta^{Antic}_{\alpha,a}$ at certain (α, a) pairs by taking differences of previously identified parameters:

Proposition 1. *Under Assumptions 1 and 2, for* (α, a) *pairs with* $15 \leq \alpha < 18 \leq a$ *and* $\pi_\alpha = \infty$,

$$\beta^{Antic}_{\alpha,a}(18, \infty) = \beta_{\alpha,a} - \beta_{18,a}.$$

Proof. See Appendix F. □

From the vantage point of Figure 4.2, Proposition 1 states that differences between the light red cells corresponding to $\alpha = 18$ and the orange cells corresponding to $15 \leq \alpha \leq 17$ at a given outcome age identifies the anticipation effect contributing to the orange cells. Since these orange cell starting age-outcome age effects can be decomposed as $\beta_{\alpha,a} = \beta^{Antic}_{\alpha,a}(18, \infty) + \beta^{ITT}_{\alpha,a}(\infty, \infty)$, Proposition 1 implies the residual ITT parameter is simply $\beta^{ITT}_{\alpha,a}(\infty, \infty) = \beta_{18,a}$.[9]

7. Unfortunately, it cannot be tested using typical pre-trend checks, since $Y_{ia}(\infty, 18, \infty)$ is only realized for $D_i = 18$ for $a \geq 18$ and no other starting age cohorts.

8. This can be justified if the timing of the reforms was quasi-random. This would imply that starting age assignments are independent of birth cohorts $b_i \in \mathcal{B}$ (conditional on $\boldsymbol{X}_i$).

9. Notice that I identify $\beta^{Antic}_{\alpha,a}(18, \infty)$, the anticipation effect *given* ITT starting age $\tau_\alpha = 18$ at select ages $a > 18$, and not $\beta^{Antic}_{\alpha,a}(\infty, \infty)$, the pure anticipation effect under *no* ITT effect. While the latter

A stronger assumption about parent ITT effects provides a larger range of outcome ages that can be utilized in this decomposition:

Assumption 3 (No parent ITT starting after age 18)**.** *For any starting age α such that $\pi_\alpha \geq 19$ and outcome age $a \geq 19$,*

$$\mathbb{E}[Y_{ia}(\eta_\alpha, \tau_\alpha, \pi_\alpha) \mid D_i = \alpha, G_i = 1] = \mathbb{E}[Y_{ia}(\eta_\alpha, \tau_\alpha, \infty) \mid D_i = \alpha, G_i = 1].$$

This assumption states that if the parent ITT starts at age $\pi_\alpha \geq 19$, then it does not affect average outcomes. Empirical evidence supports this view. Appendix Table D.8 reports these cohorts' mother's household income are unaffected by the 1994 reform (consistent with findings among older starting ages $25 \leq \alpha \leq 29$), suggesting financial spillovers onto their children are unlikely. Moreover, individuals' cohabitation with parents drastically declines after age 18; see Appendix Figure D.33. Under Assumption 3, all starting age-outcome age effects after $a \geq 18$ for starting age $\alpha = 18$ are only ITT effects, while those for starting ages $15 \leq \alpha \leq 17$ are anticipation and ITT effects. Thus, I can use the summary parameters β_α to decompose effects among $15 \leq \alpha \leq 17$.[10]

Table 4.1 reports the decomposition results on key outcomes for starting ages $15 \leq \alpha \leq 17$ that follow from Proposition 1 and Assumption 3. Comparing Columns (1) and (2) reveals that anticipation effects and ITT effects tend to be opposing forces. In particular, the anticipation effect generates statistically significant increases in market income while modest statistically significant declines in incarceration, while the ITT effects are opposite signed and statistically insignificant. Relative to the total effects reported in Column (3), anticipation effects appear to play an outsize role in shaping the effects of the reforms among these cohorts.

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is theoretically interesting, this parameter is neither possible to identify without strong assumptions nor policy-relevant given that SA eligibility begins at age 18.

10. Decomposing effects without Assumption 3 leads to imprecise results due to the limited number of appropriate starting age-outcome age pairs.

Table 4.1: Decomposing welfare-to-work's total effects by anticipation and ITT components for starting ages $15 \leq \alpha \leq 17$

	Anticipation (1)	ITT (2)	Total (3)
Market income (thousands)	0.819**	−0.020	0.799***
	(0.396)	(0.396)	(0.256)
Employment	0.023	−0.017	0.007
	(0.015)	(0.015)	(0.010)
Years of schooling	0.039	−0.008	0.031
	(0.041)	(0.036)	(0.027)
Incarceration sentence (days)	−0.690*	0.472	−0.218
	(0.366)	(0.472)	(0.403)
SA participation	−0.096	0.161	0.065
	(0.113)	(0.109)	(0.088)

Notes: This table decomposes the total estimated effect on starting age cohorts $15 \leq \alpha \leq 17$ into anticipation and ITT effects using estimates of β_α from Equation (2.5). See main text for more details. Standard errors are two-way clustered over birth quarters and municipality of residence and reported in parentheses.

Stars indicate statistical significance: $^{\star}p < 0.10$, $^{\star\star}p < 0.05$, , $^{\star\star\star}p < 0.001$.

While this decomposition only holds under strong assumptions, my findings suggests that anticipation effects, coupled with additional choice margins like schooling, may play a critical role in generating the long-run effects on socioeconomic outcomes among younger starting age cohorts. This affects our interpretation of the efficacy of welfare-to-work. It suggests that reforms are effective as a tool for incentivizing behavioral responses among anticipating cohorts before they reach eligibility, rather than as a direct skill investment that shapes their later-life outcomes.

4.2 Intergenerational Spillover Effects

My main results establish substantial heterogeneity in intragenerational effects of the SA reforms across starting ages, which sometimes remain for decades over the life-cycle. Does this heterogeneity persist to the next generation? This section provides suggestive evidence supporting this view. Individual identifiers in birth records enable me to link my main sample to their own children, offering a rare window into intergenerational outcomes in social program participation, labor market outcomes, education, crime, and hospitalizations.

Due to data limitations, my analysis of intergenerational spillover effects departs from my previous analysis in two key ways. First, I use a difference-in-differences (DD) strategy that examines how an individual's *child's* outcomes are affected by the individual's *own* starting age cohort. I am unable to pursue my usual TD strategy here since the data provides only limited snapshots of child life-cycle outcomes; as a result, I can only exploit variation between different cohorts and risk groups. Note that by the time child outcomes are measured, all cohorts from my main sample already faced the SA reforms, implying that all child outcomes could have been affected by their parent's exposure to the reforms. Consequently, my DD analysis can at best identify the effect of an individual's starting age cohort on their child's outcomes *relative to* another starting age cohort. In practice, I choose to identify effects relative to starting ages $25 \leq \alpha \leq 29$; this is an attractive choice since this cohort group had largely null intragenerational effects.

Second, my analysis focuses on subsamples of children whose mothers were aged 25 or below at the time of birth.[11] Most child outcomes are first observed in late adolescence, causing observations from younger cohorts to come from disproportionately young mothers. I thus focus on this subsample of young mothers and limit the starting age cohorts in my estimation sample (depending on the age of child outcomes and data availability) to ameliorate compositional changes.[12]

I assign children based on their mother's starting age rather than their father's since, despite changes in household structure, nearly all children cohabit with their mothers through-

11. Among the high-risk group, this subsample of young mothers constitutes 62% of my original sample of women.

12. Appendix Table D.9, Column (1), reports the youngest starting age used in the sample for each outcome. For example, when estimating spillover effects on child SA participation between ages 18–20, I constrain my sample to mothers with starting ages $\alpha \geq 14$.

out their childhood. This decision, however, should not to discount the role that father's starting age can influence child outcomes. Indeed, it is noteworthy that birth fathers are 3.8 years older on average than birth mothers—implying that fathers tend to come from older starting age groups than mothers. This will be critical for interpreting the results that follow.

The DD method identifies effects *relative to* starting ages 25–30 by comparing snapshots of child outcomes between high- and low-risk groups and between groups of their *mother's* starting ages. To gain greater precision in my subsample, I partition children into the four starting age groups used earlier in my analyses. Let $\tilde{\mathcal{A}} \equiv \{[8,15],[16,18],[19,24]\}$. The DD regression is

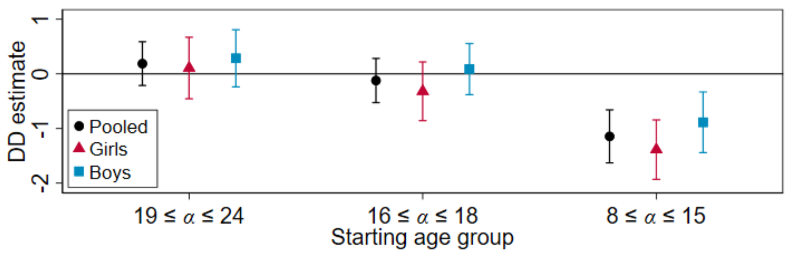
$$Y_i = \sum_{\tilde{\alpha}\in\tilde{\mathcal{A}}} \beta_{\tilde{\alpha}} G_i \times \mathbb{1}(D(b_i) \in \tilde{\alpha}) + \delta_1 G_i + \delta_{b(i)} + \boldsymbol{\gamma}\boldsymbol{X}_i + \boldsymbol{\eta}\boldsymbol{X}_i \times b_i + \theta_{m_{D(b_i)-1}(i)} + \varepsilon_i \quad (4.3)$$

where $\boldsymbol{X}_i$ are as before, $\theta_{m_{D(b_i)-1}(i)}$ is the municipality of residence prior to starting age $D(b_i)$, and standard errors are two-way clustered as before. The parameters of interest, $\beta_{\tilde{\alpha}}$, capture the effect of being born to mothers of starting age group $\tilde{\alpha}$ relative to $25 \leq \alpha \leq 29$.

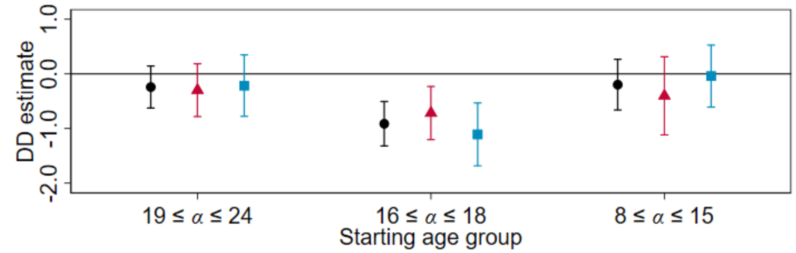
Figure 4.3 summarizes estimates of these intergenerational effects for a key set of child outcomes. Fascinatingly, my effect estimates suggest that starting age heterogeneity persists to the next generation. Focusing on the black circles representing pooled estimates of $\beta_{\tilde{\alpha}}$ from Equation (4.3), I find that children born to mothers of the youngest starting age group $8 \leq \alpha \leq 15$ use 1.2 fewer months on SA participation between ages 18–20 relative to those born to the oldest starting age group (Panel (a)). Meanwhile, there are no effects for children born to starting age groups $16 \leq \alpha \leq 18$ and $19 \leq \alpha \leq 24$. These findings suggest that parents facing SA reforms earlier in childhood not only reduces their own dependence on SA, but also diminishes their own children's SA usage in the future. Meanwhile, for children born to starting age group $16 \leq \alpha \leq 18$, I find statistically significant decreases in market

Figure 4.3: Intergenerational spillover effects of welfare-to-work, by mother's starting age group

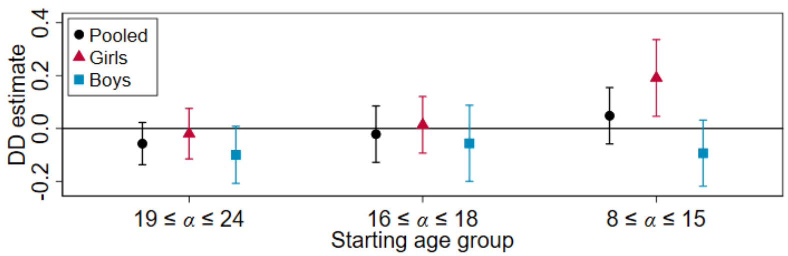
(a) Duration on SA, age 18–20 (months)



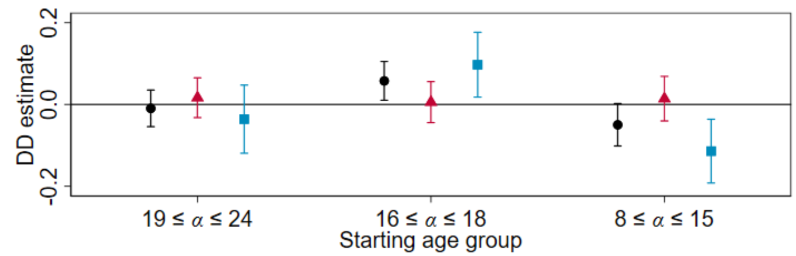
(b) Market income, age 18–20 (thousands



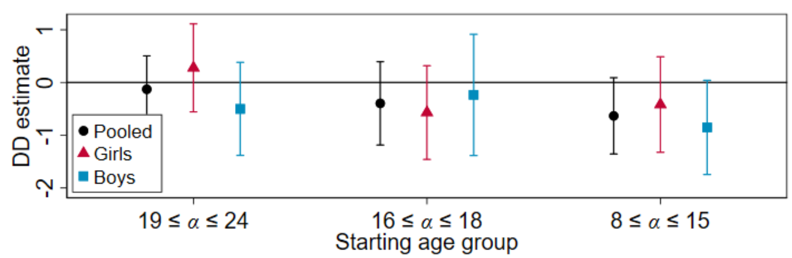
(c) Years of schooling, age 20



(d) Number of serious crimes, age 15–17



(e) Inpatient hospital care (days), age 0–6



Notes: This figure plots DD estimates of the effect of mother's starting age group on child outcomes relative to starting age group $25 \leq \alpha \leq 29$. Black markers are estimates of $\beta_{\tilde{\alpha}}$ from Equation (4.3), which pools over child gender. Red and blue markers are estimates for female and male children from a triple difference analog of Equation (4.3) that interacts effects by a child gender indicator. Vertical capped bars are 90% confidence intervals based on two-way clustered standard errors. All estimates conducted on subsamples of young mothers described in the main text; see Appendix Table D.9, Column (1), for the youngest starting age cohort used for each outcome.

income between ages 18–20 (Panel (b)) and increases in criminal activity between ages 15–17 (Panel (c)). To understand these patterns, note that most of these children's fathers belong to starting ages $19 \leq \alpha \leq 24$, who experienced modest declines in market income and increase in crime from the reform. This suggests that fathers' responses to the SA reforms has spillover effects on this group of children.

The red triangles and blue squares of Figure 4.3 reveal interesting heterogeneity by child gender.[13] I find that among children of starting ages $8 \leq \alpha \leq 15$, girls have statistically significant increases in educational attainment while boys have significant decreases in criminal activity. Meanwhile, boys of $16 \leq \alpha \leq 18$ explain all of the increased criminal activity.

Appendix Table D.9 reports effects on a broader set of child outcomes. The same qualitative patterns hold. Overall, these findings suggest that starting age heterogeneity is not only pervasive in intragenerational outcomes, but also persists in intergenerational outcomes. This highlights how welfare-to-work reforms can have long-run consequences that span across multiple generations.

13. To retrieve these estimates, I estimate a triple difference analogue of Equation (4.3) by allowing the parameters ($\beta_{\tilde{\alpha}}$, δ_1, $\delta_{b(i)}$) to interact with indicators of child gender.

CHAPTER 5

EVALUATION & CONCLUSION

5.1 Evaluating Welfare-to-Work over Time

5.1.1 Aggregate Effects over Calendar Time

Having examined the heterogeneity of starting age-outcome age effects, what are the overall effects of the SA reforms in the population over calendar time? To answer this question, I aggregate the starting age-outcome age estimates $\{\beta_{\alpha,a}\}$ over cross-sections of starting age cohorts by calendar year (Callaway and Sant'Anna 2021). The resultant estimates of overall effects are often sought after by policymakers and economists for judging whether welfare-to-work is a worthwhile reform in the long-run. Critically, however, the fact that $\beta_{\alpha,a}$ serves as a building block helps provide a more nuanced understanding of the specific cohorts that drive the overall effects over time. Given the composition of the population evolves to include younger and younger starting age cohorts over time who have different choice sets and anticipation effects, this would affect our interpretation of the reform's long-run impacts.

Let $\bar{t}_\alpha$ denote the calendar year that the SA reform affects starting age α.[1] Thus, the outcome age of starting age cohort α in calendar year t is $a = t - \bar{t}_\alpha + \alpha$. Recall that $\beta_{\alpha,a}$ is identified only up to outcome age $a = 29$. In order to map how longer-run outcome ages shape aggregate outcomes over time, I use $\beta^{30}_{\alpha,a}$ whenever $t - \bar{t}_\alpha + \alpha \geq 30$. Let $\mathcal{A}_t$ denote the set of starting age cohorts with identified starting age-outcome age effects (whether they are $\beta_{\alpha,a}$ or $\beta^{30}_{\alpha,a}$) in year t, which captures how the composition of cohorts change over time. To extrapolate effects over longer-run horizons which include cohorts too young to appear in my main sample, I assume that starting ages $\alpha < 8$ possess the same starting age-outcome age effects as $\alpha = 8$ and include these cohorts in $\mathcal{A}_t$. The aggregate cross-sectional effect of

1. Specifically, $\bar{t}_\alpha = 1990$ for $\alpha \leq 19$, $\bar{t}_\alpha = 1992$ for $22 \leq \alpha \leq 24$, and $\bar{t}_\alpha = 1994$ for $26 \leq \alpha \leq 29$.

the SA reforms in calendar year t is thus defined as

$$\bar{\beta}_t \equiv \sum_{\alpha \in \mathcal{A}_t} \frac{1}{|\mathcal{A}_t|} \cdot \Big(\beta_{\alpha, t-\bar{t}_\alpha+\alpha} \cdot \mathbb{1}\{t - \bar{t}_\alpha + \alpha \leq 29\} + \beta^{30}_{\alpha, t-\bar{t}_\alpha+\alpha} \cdot \mathbb{1}\{t - \bar{t}_\alpha + \alpha \geq 30\} \Big). \quad (5.1)$$

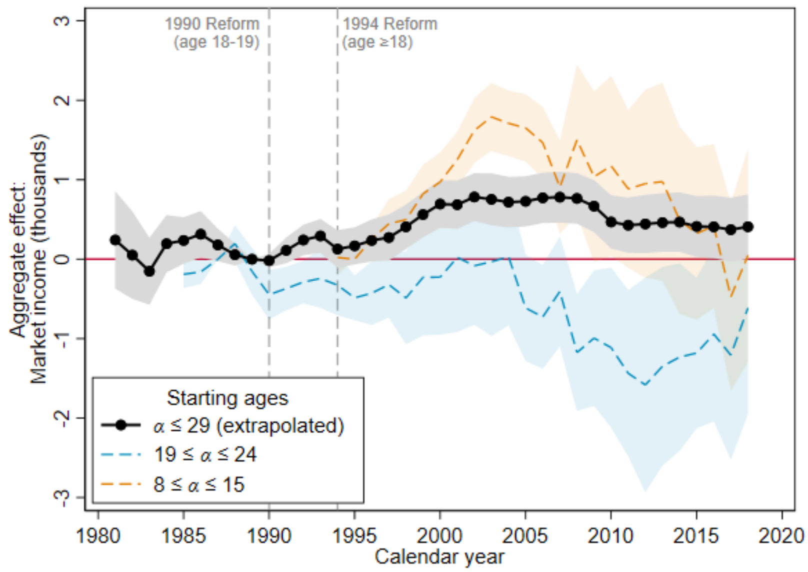
Figure 5.1 plots estimates of $\bar{\beta}_t$ over calendar time for market income. The black series shows that aggregate effects on market income grows at a somewhat slow pace, reaching statistically significant positive levels eight years after the 1990 SA reform; thereafter, estimates largely stabilize at positive and statistically significant levels. The dashed blue and orange lines overlay the grouped starting age-outcome age event-study estimates for starting ages $8 \leq \alpha \leq 15$ and $19 \leq \alpha \leq 24$. The different dynamics of cohort groups showcases how aggregate effects mask important heterogeneity within cross-sections of the population.

Appendix Figure D.34 illustrate aggregate calendar time effects for other outcomes. Like market income, net taxes increases in magnitude over time, though the negative effects among $19 \leq \alpha \leq 24$ gives rise to a more uneven effect trajectory over time. Meanwhile, the overall duration of inpatient hospital care steadily declines over time since there are largely null effects among starting age cohorts $\alpha \geq 18$. These results illustrate that welfare-to-work has beneficial effects on aggregate over time driven by younger cohorts aging into the population.

5.1.2 Cost-Benefit Analysis

Were the welfare-to-work reforms worth it from the government's perspective? To assess this, I conduct a cost-benefit analysis of the fiscal impact of the reforms on each main outcome borne on the government's budget. I focus on three key components contribute to the total fiscal effects of the SA reforms: (1) net taxes, defined as tax revenue accrued from market income minus any public transfers allocated for SA, DI, other social programs, and post-secondary education stipends (which I directly observe in the register data); (2) expenses of crime based on policing, court expenses during convictions, and expenses of maintaining and

Figure 5.1: Aggregate effects of welfare-to-work on market income over calendar time



Notes: This figure plots aggregate estimates of the effects on market income for different groups of cohorts. The black series plots estimates of $\bar{\beta}_t$ defined in Equation (5.1). It extrapolates estimates of $\alpha < 8$ cohorts with $\alpha = 8$ estimates. The dashed lines plot event-study estimates of $\beta_{\alpha,a}$ from Equation (2.4) aggregated between $19 \leq \alpha \leq 24$ and $8 \leq \alpha \leq 15$; these are overlaid over calendar time using the outcome age of the median cohort in each group. Shaded regions are 90% confidence intervals based on two-way clustered standard errors.

staffing incarceration (based on calculations detailed in The Rockwool Foundation Research Unit (2006)); (3) expenses of inpatient hospital care based on bed days and medical staff (provided in Czernichow et al. (2021) drawn from the Danish Health Data Agency).[2] These total effects are assessed against the direct expenditure that the government (both national and municipal) spends in financing the work requirement programs, which are documented in Bach (2002) and Christensen (2002).[3] Due to limited evidence, I ignore fiscal impacts generated from SA work programs' productive activities and the equilibrium responses of

2. I estimate costs from crime and convictions by (i) regressing measures of crime and convictions on a quadratic of incarceration sentence length fully interacted by indicators of ever incarcerated on data after 1992 (after the data break described in Footnote 10) with all controls and (ii) scaling coefficients on all incarceration variables by their respective triple difference estimates. Standard errors are recovered via the delta method.

3. Due to data limitations, SA work requirements are censored for certain birth cohorts and ages. I use all available data to estimate β_α via Equation (2.5).

the reforms on employer behavior.[4]

Figure 5.2 illustrates the cost-benefit analysis by starting age α. As shown in Panel (a), net taxes is by far the most consequential component within total effects. For cohorts with $\alpha < 16$, the net taxes accrued from increased market income generates large, statistically significant benefits that contribute to the government's budget. Meanwhile, costs borne from increased crime among older adolescents and young adults tends to have a negative effect on the budget. Panel (b) demonstrates that, after accounting for expenses from increased take-up of SA work requirements from the reforms, the government enjoys a net fiscal gain from the younger starting age cohorts but suffers statistically significant losses from young adults. Among older starting ages $\alpha \geq 25$, there appears to be no significant fiscal gain or loss.

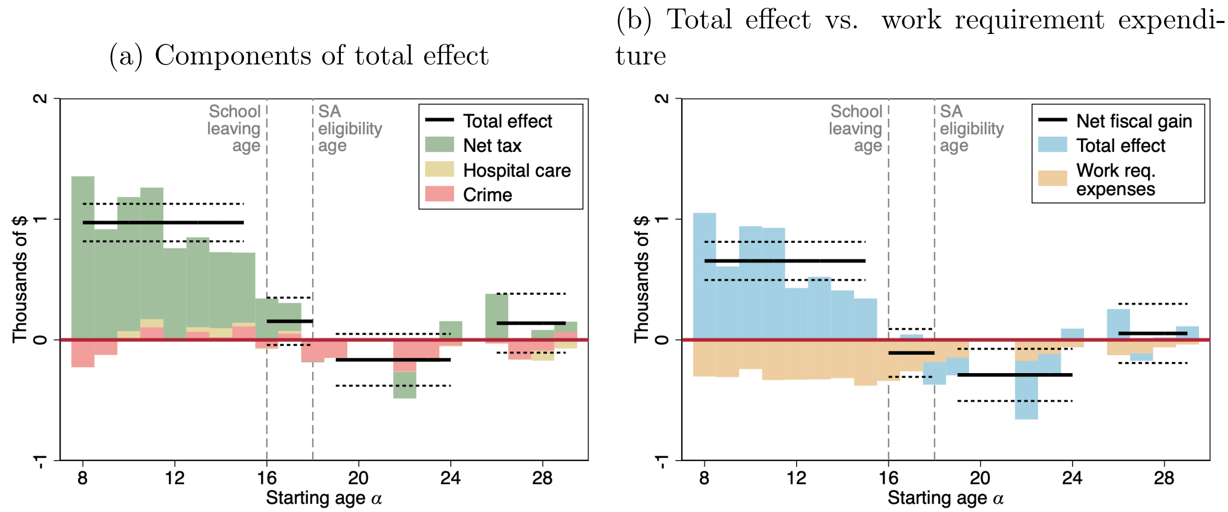
In light of the dynamics of aggregate effects over time, this cost-benefit analysis implies that the government stands to enjoy net fiscal gains in the long-run. All gains are driven by younger starting age cohorts who possessed the education choice margin and anticipation effects. The cost-benefit analysis by starting ages also highlights that alternative policy designs could enhance efficiency. Targeting welfare-to-work only to cohorts entering SA eligibility and grandfathering already-eligible adults under the previous SA regime would generate even larger effects by preventing older adults from committing more crime or participating in other social programs.

5.2 Conclusion

This dissertation provides new evidence on the long-run effects of welfare-to-work reforms by estimating the impacts of Danish SA reforms over the life-cycles of a generation of cohorts. These starting age-outcome age effects enable a more nuanced understanding of the under-

4. To the best of my knowledge, no study has estimated the productive gains of the mandated Danish work programs. Hussain and Rasmussen (2007) find some evidence of a negative association between firms' formal employment and temporary employment of SA recipients from municipal wage subsidy schemes.

Figure 5.2: Cost-benefit analysis of welfare-to-work reforms, by starting age



Notes: Panel (a) decomposes the total effect of welfare-to-work over net taxes, hospital care, and crime over starting ages α, following the main text. Panel (b) compares the total effect against government expenses for implementing work requirements and plots the net fiscal gain over α. Horizontal solid lines are aggregate estimates of the total effect and net fiscal gain; dashed lines are 90% confidence intervals. Colored bars represent estimates of components for each starting age α.

lying drivers of aggregate effects found in cross-sections of the population over time. Using rich administrative data, I estimate effects on a multitude of outcomes that help provide a more comprehensive cost-benefit analysis than much of the existing literature.

I document striking heterogeneous effects across cohorts that depends on the age they start facing the reforms. Younger starting age cohorts gain greater income, employment, and education compared to their older counterparts, consistent with a model where cohorts invest in their human capital during childhood (Becker 1962, Besley and Coate 1992). Meanwhile, the reforms tend to generate perverse incentives for already-eligible young adults, who tend to commit more crime or substitute toward disability insurance programs. These patterns are consistent with the fact that different cohorts possess different choice sets depending on when they face the reforms during the life-cycle. Under stronger assumptions, I demonstrate that anticipation effects—rather than the intent-to-treat of work requirements themselves—appear to play a major role in the long-run effects for younger starting ages. I also provide suggestive evidence that the disparate effects across cohorts spills over to their own children.

In sum, I find that welfare-to-work reforms are cost-effective for the government in the long-run, but this is entirely driven by younger cohorts who could anticipate and behaviorally respond to the reforms during childhood. The net taxes reaped from these cohorts translates to a growing positive effect on the government's budget. On the other hand, perverse incentives among already-eligible young adults generate negative fiscal impacts. Therefore, the body of evidence suggests that welfare-to-work does, indeed, "work" over time, but aggregate effects mask important heterogeneity in the population.

I conclude with two notes relevant for policymakers. First, as usual, one must be cautious with generalizing the findings of this study to countries with different policy contexts. An important determinant of the effects of welfare-to-work reforms that mandate public work programs is the quality of the programs themselves. Qualitative evidence suggests that, in many cases, the Danish work programs were not attractive options to build lasting labor market skills.

Second, heterogeneity in net fiscal impacts across cohorts suggests that targeting only younger cohorts would improve efficiency. However, it may be the case that allocating the same public funds in alternative investments in youth at high-risk of SA participation could be a more attractive option. It is critical to compare the estimated impacts of welfare-to-work against the potential gains for a portfolio of other policy reforms or interventions that target this disadvantaged population (Hendren and Sprung-Keyser 2020).

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APPENDIX A

ADDITIONAL INSTITUTIONAL DETAILS

Section A.1 provides additional details of the SA reforms, including the nature of work requirements and other changes to the SA program. Section A.2 compares the SA program with the traditional U.S. cash welfare program.

A.1 Overview of Changes to the Social Assistance Program

Table A.1 details the timing of legislation, passive benefit duration, severity of sanctions, and exemptions for certain parents.

Nature of work requirements. Work requirements were extremely heterogeneous in nature, depending on the individual needs of the SA recipient and the resources available at the municipality that governed the local program. Activities include job search, vocational training, short-term educational programs, apprenticeships, community service, or subsidized apprenticeships in the public or private sectors. Individual work programs could include combinations of these activities.

Figure A.1 illustrates the availability of activities provided by municipalities. The most popular programs offered included educational programs and job training through private or public apprenticeships. For young adults who have left high school, classroom instruction aimed to teach basic skills and provide direction in formulating future plans for a career; see Mik-Meyer (1999) for illuminating case studies. Job training included a wide spectrum of options. Documented examples include janitorial work, public park beautification, snow shoveling, daycare assistants, food-packing in factories, carpentry, and bricklaying. See Mik-Meyer (1999), Larsen (2005), Katznelson (2008), Andersen (2020), and Hansen and Nielsen (2021).

Despite this large heterogeneity, the intensity of work requirements is remarkably consistent across municipalities and between different age groups—despite the national law's provision that allows municipal discretion of work requirements of older adults. See Figure A.2.

Other changes. Youth and adult benefit levels were generally earmarked to state education grant and unemployment insurance benefits, which modestly grew over time with inflation. Note that the 1994 benefit reform restructured benefits for both youth and adults. Housing benefits were formally separated from SA benefits (but still counted toward the total benefit limit). Benefits became taxable, and benefit formulas were inflated to account for the average family's expected tax incidence. There were also some changes in the disbursement of child supplements for families with multiple children. However, Hansen and Schultz-Nielsen (2017) demonstrate that these changes actually resulted in very few changes in the overall net benefits received by the vast majority of SA-eligible households.

Two other changes affected eligibility for adult benefit rates over time. First, in 1995, the age threshold for the adult benefit rate was adjusted from age 23 to 25. Second, youth eligibility for the adult basic benefit rate gradually grew stricter over time. For example, cohabitation with a spouse was no longer a sufficient condition for an individual below the age threshold to be eligible for adult benefit rates. See Hansen and Schultz-Nielsen (2017) for more details.

The SA reforms also adapted guidelines on sanctions and exemptions for parents of young children who lacked daycare from work requirements over time. In general, these guidelines loosened over time; see the last two columns of Table A.1. In practice, however, many municipalities did not follow these guidelines and enacted stricter sanctions and some enforced work requirements to parents lacking daycare. Note that the extent that sanctions were loosened and exemptions were adopted would tend to attenuate the estimated effects of facing work requirements.

Table A.1: Details of Social Assistance reform work requirements, 1990–1998

Dates			Age	Work req's		Passive period (months)		Guidance	Renewal	Diff. for	Sanctions
Year	Announced	Enforced	group	Hr./wk.	Months	Initial	Pre-renewal	period	policy	parents	
1990	6/13/1990	7/1/1990[a]	18-19	20	5	≤0.5	N/A	≤1.8	None	—"—	DoM⋆
1991	6/6/1991	10/1/1991[b]	18-20	20	5	≤0.5	N/A	≤1.8	None	—"—	—"—
1992	6/24/1992	7/1/1992	18-24	20	5	≤0.5	≤3	≤1.8	Optional	—"—	—"—
1993	12/23/1992	1/1/1993[c]	18-20	20	5	≤0.5	≤3	≤1.8	Optional	—"—	—"—
			21-24	20	5	≤3	≤3	≤1.8	Optional	—"—	—"—
1994	6/30/1993	1/1/1994[d]	18-24	20	6	≤3	≤3	≤1.8	Required	Passive period extended to max. 12 months if lack childcare	If reject activ. offer, repay benefits; if accept activ. offer, benefits reduced by % absence[e]
			≥25	DoM⋆	DoM⋆	≤12	N/A	0	None	None	—"—
1995	12/21/1994	7/1/1995[c]	18-24	≥30	6	≤3	≤3	≤1.8	Required	Passive period extended to max. 12 months if lack childcare	If reject activ. offer, repay benefits; if accept activ. offer, benefits reduced by % absence up to max. 20%[e]
			≥25	DoM⋆	DoM⋆	≤12	N/A	0	None	None	—"—
1996	12/20/1995	4/1/1996[c]	18-24	≥30	6 or 18[f]	≤3	≤3	≤1.8	Required	Passive period extended to max. 12 months if lack childcare	—"—
			≥25	DoM⋆	DoM⋆	≤12	N/A	0	None	None	—"—

Notes: (see below)

Table A.1: (*continued*)

Dates			Age	Work req's		Passive period (months)		Guidance	Renewal	Diff. for	Sanctions
Year	Announced	Enforced	group	Hr./wk.	Months	Initial	Pre-renewal	period	policy	parents	
1998	6/10/1997	7/1/1998[c]	18-29	≥30	6 or 18[f]	≤3	≤3	0	Required	Activation not required if lack childcare	If reject activ. offer, benefits ceased; if accept activ. offer, benefits reduced by % absence up to max. 20%[e]
			≥30	DoM⋆	DoM⋆	≤12	N/A	0	None	—"—	—"—

Notes: This table summarizes baselines rules from national reforms to the Social Assistance Act, Municipal Activation Act, and Active Social Policy Act (see retsinformation.dk for the original legislation). The table indicates whenever the national legislation explicitly states that a particular policy rules are left up to the discretion of municipalities (DoM). However, municipalities were granted discretion to implement more extreme rules in activation requirements, passive periods, renewal policies, and sanctions at potentially earlier dates. All dates expressed in MM/DD/YYYY format.

⋆ DoM = policy left up to the discretion of municipalities.

[a] Those already on assistance must be activated before they turn 20 or before 1/1/1991.

[b] Those already on assistance must be activated before they turn 21 or before 1/4/1992.

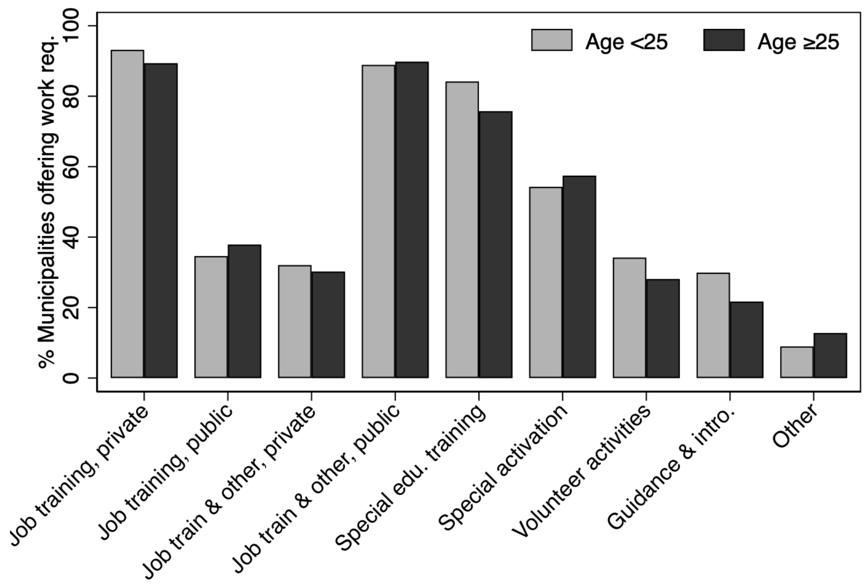
[c] Only applies to those who apply for assistance after start of the law.

[d] Those already on assistance and over age 25 must be activated by 1/1/1995.

[e] Recipients cannot be denied benefits for refusing an activation offer to manufacture munitions.

[f] Recipients whose education level entitles them to membership of an unemployment fund (i.e., vocational credentials) have a 6 month activation period; others 18 months.

Figure A.1: Types of work requirements offered by municipalities

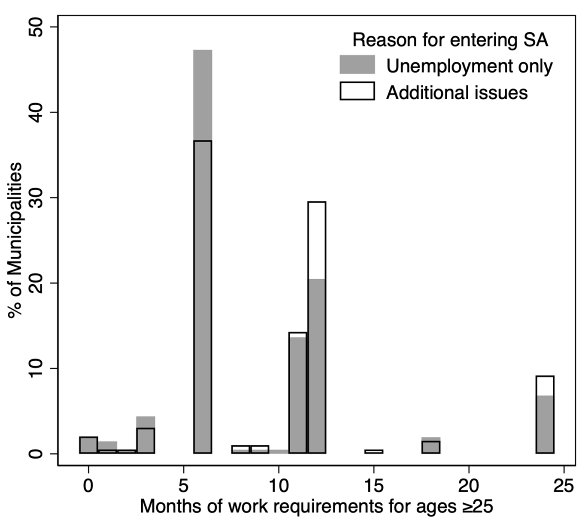


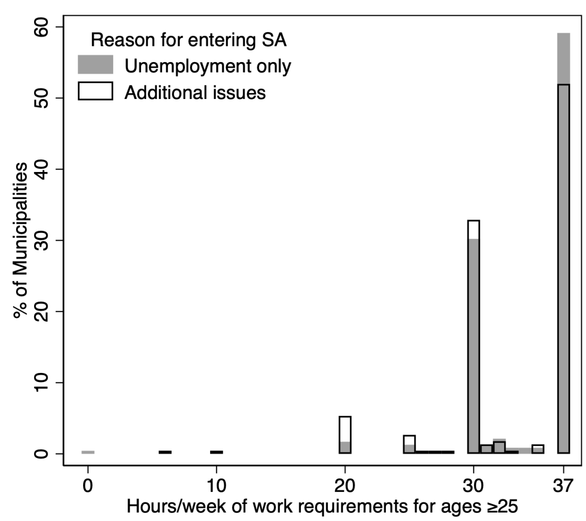
Notes: Data from Questionnaire to Municipalities, *Municipal Activation Scheme (1996–1998)*, conducted by Det Nationale Forskningscenter for Velfaerd and provided by the Danish National Archives.

Figure A.2: Distribution of work requirements for ages 25 or above across municipalities

(a) Months

(b) Hours per week





Notes: Data from Questionnaire to Municipalities, *Municipal Activation Scheme (1996–1998)*, conducted by Det Nationale Forskningscenter for Velfaerd and provided by the Danish National Archives.

A.2 Comparison with U.S.'s Aid to Families with Dependent Children Program

Table A.2: Comparison of Denmark's Social Assistance (SA) program and U.S.'s Aid to Families with Dependent Children (AFDC) program

Attribute	Denmark SA	U.S. AFDC
Target population	Disadvantaged adults (age $\geq$18) (childless and parents)	Disadvantaged single parents (AFDC-UP: 2-parent households)
Eligibility criteria	1. Experienced "social event" 2. Incapable of self-sufficiency 3. Exhausted work opportunities	1. Parent of child age $<$18 2. Earnings $<$ state threshold
Max. benefit	\$1,038/month (age $\geq$ 25 or parent, married, or sufficient prev. income) \$610/month (age $<$ 25 and childless) + housing benefit, individual benefit	\$609/month (single, 2 children)
Asset limit	\$550 (excl. savings for edu, home)	\$1,500 (excl. value of home)
Marginal tax rate	100%	67% (first 4 months) 100% (after 4 months)
Time limits	None	None

Notes: Based on 1993–4 data; monetary values expressed in 2010 U.S. dollars. U.S. benefit levels are averages across states reported in Crouse (1995).

APPENDIX B

DATA

B.1 Overview

The administrative register data is provided by Statistics Denmark. Most registers are available from 1980 to 2018, though some begin and end at different years. Section B.2 describes how the main sample is constructed. Section B.3 defines baseline characteristics used to define the main sample and as control variables in my empirical analysis. Sections B.5 and B.4 describe how SA participation and geographic identifiers are defined over time.

B.2 Sampling Scheme

I first select individuals born between 1963 and 1981. I exclude first- or second-generation immigrants from my sample to avoid compositional changes across birth cohorts. This population is comprised of 1.35 million individuals.

Next, I measure the individual's mean household income measured between ages 12–17 and mother's education level by age 17; see Section B.3 for precise definitions. I keep observations whose mean household income falls below the national median (weighted across birth cohorts) and whose mothers have no more than 12 years of schooling (equivalent to a high school education). I also exclude observations with any missing background characteristics. This leaves me with a *base sample* of approximately 429,000 individuals.

Finally, I define risk groups based on two characteristics (precisely defined in Section B.3, with additional details in Section C.1): (1) growing up in a household that participated in SA and (2) living in a neighborhood with above-median SA participation. The high-risk group ($G_i = 1$) satisfies both criteria; the low-risk group ($G_i = 0$) satisfies neither. My main sample is comprised of only high-risk and low-risk individuals; individuals that satisfy only one criterion are excluded from my main sample. This amounts to a *main sample* of about 230,000 individuals, of which 60,000 belong to the high-risk group.

B.3 Background Characteristics

Individual household identifiers are provided by the Population (*Befolkningen*; BEF) registers. (I specifically use the variable "CNR" for identifiers.) These identifiers allow me to obtain data among all who cohabit with each individual in my main sample at each available year. The timing of when these characteristics are measured in the life-cycle is motivated by analyses detailed in Section C.1.

- *Female* — dummy indicating whether an individual's gender at birth is female. Available in the Households and Families (*Husstande og Familier*, FAIN) registers.

- *Household market income* — the mean of household's total gross market income (before transfers or taxes) of up to three most recent years available between ages 12–17 of the individual and between 1980–1993. Available in the Income (*Indkomst*; IND) registers.

- *Household net assets* — the mean of household's total net assets of up to three most recent years available between ages 12–17 of the individual and between 1980–1993. Available in IND.

- *Household SA participation* — an indicator of whether any of the individual's older household members (age 18 or above) who received SA benefits between ages 12–17 of the individual and between 1980–1993. I exclude individuals in the main sample from consideration to ensure that this baseline measure is not influenced by young adults affected by the 1990–1992 reforms. Available in IND.

- *Mother's education, age 17* — highest years of completed schooling of birth mothers by the time individuals turn age 17. Since the registers are only available beginning in

1981, I use the mother's years of schooling when the 1963 turns age 18.[1] Available in the Education (*Uddannelseregister*, UDDANY) registers.

- *Number of children, age 17* — the number of children below the age of 18 that cohabit with individuals at age 17. Available in FAIN.

- *Single–parent household* — an indicator of whether an individual lives in a household with only one parent (who could be a birth, step, or adopted parent), measured at the highest age available between ages 12–17 of the individual and between 1980–1993. Available in FAIN.

- *Married household* — an indicator of whether an individual lives in a household where the parent (who could be a birth, step, or adopted parent) is married, measured at the highest age available between ages 12–17 of the individual and between 1980–1993. In nearly all cases, the spouse also cohabits with the individual. Available in FAIN.

- *Mean parish income, age 15–17* — I compute within-parish mean household-level total gross income excluding transfers by parish and year from 1980 to 1998 using the IND registers. Households are defined as individuals living in the same household, including married couples, cohabiting couples, registered partnerships, and children under the age of 18 who are not parents or in a cohabiting relationship.[2] Averages ignore cases missing parishes (which are more common in 1980 and 1981 due to missing parish assignments, as described in Section B.4 below). Since data is only available beginning in 1980, averages are limited to between age 17 and ages 16–17 for the the 1963 and 1964 birth cohorts, respectively. The share of 1963–1968 birth cohorts who move to different parishes between ages 15–17 is extremely small.

1. Since nearly all mothers are between 30–60 years old by the time the main sample turns 17, there are little differences in measuring mother's education when the sample turns 18.

2. This is the variable "C_FAMILIE_ID" in the FAIN register dataset.

- *High-SA neighborhood block* — an indicator of whether an individual's modal residential neighborhood block (comprising of approximately 150 households) had a historical SA participation rate among adults aged 18–60 (averaged measured between 1980–1993) above the median within mean income and urbanicity bins. Neighborhood block identifiers are constructed by Damm and Schultz-Nielsen (2008), which are retroactively matched to 1980–1984 housing registers; see Section B.4. Mean income is measured as the mean household income within the neighborhood between 1980–1993, which are partitioned into $1,000 bins (while neighborhoods at the extreme tails are grouped with their nearest bin); data available in IND. Urbanicity is defined as whether the majority share of household addresses belonging to a neighborhood block belongs to a municipality that is urban ($\geq$50,000 residents), suburban (1,000–49,999 residents), or rural (<1,000 residents); data available in the Housing Census (*Boligforhold*) register of 1992.

B.4 Geographic Identifiers

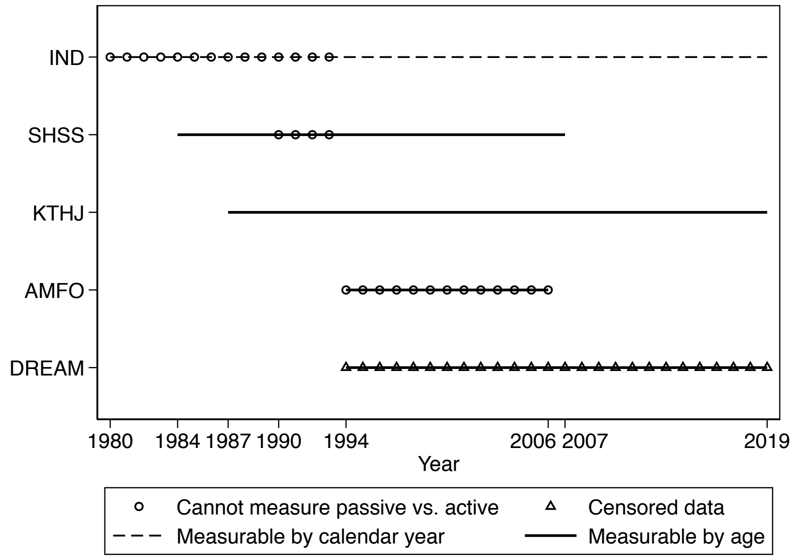
I use municipality codes in 2003 to maintain consistency in the geographic boundaries of municipalities over time.[3] Municipality of residence of each residential address are available beginning in 1980 through the Households and Families registers (*Husstande og Gamilier*, FAIN).

While each individual's residential addresses are available beginning in 1980, parish codes are matched with addresses through the Housing Census registers (*Boligtælling*, BOL) beginning in 1982. In an effort to identify as many addresses to parish in 1980 and 1981, I compile the universe of over 3 million addresses matched with parish codes from the 1982–2004 Housing Census registers.[4] I use parish codes in 2007 to maintain consistency in the

3. After 2003, there are a few changes in the organization of municipal boundaries, followed by the 2007 Municipal Reform, which reduced the number of municipalities from 275 to 98.

4. Addresses with missing municipalities are removed, since addresses are only unique up to the municipal

Figure B.1: Availability and quality of registers measuring SA participation



geographic boundaries of parishes over time. I then retroactively match missing parishes to the 1980 and 1981 addresses that also exist in the 1982–2004 universe. I apply a similar process with neighborhood block identifiers constructed by Damm and Schultz-Nielsen (2008), which are available starting only in 1985.

B.5 Measuring SA Participation over Time

Data available over time. I primarily use the Cash Assistance register (*Kontanthjælp*, KTHJ) to measure duration of SA participation at each age. There are a number of alternative registers, but each has major drawbacks. Figure B.1 summarizes the availability and quality of these registers. To describe their drawbacks:

- The Income register (*Indkomst*, IND) provides annual SA benefit levels each calendar year. Before 1994, IND provides only a single measure of SA participation that does not distinguish between passive and active benefits. Measuring duration of SA partic-

level.

ipation during the calendar year can only be approximated using SA benefit formulas; measuring participation by ages requires more approximations.

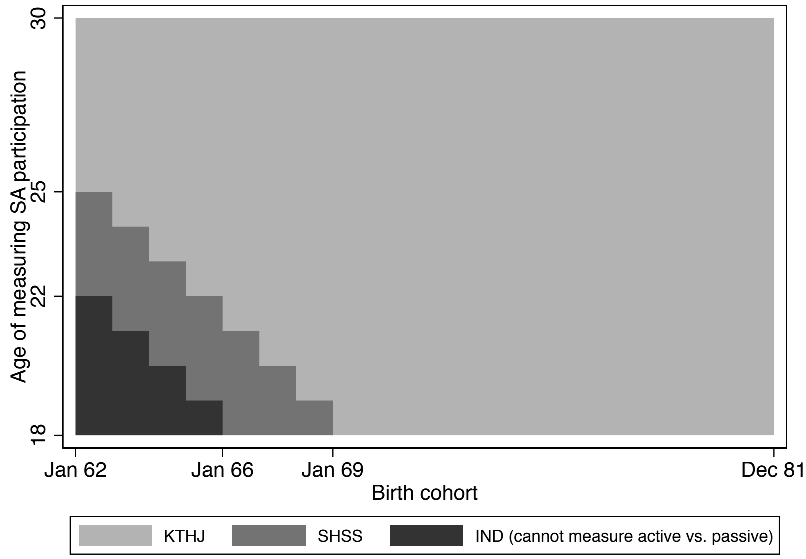
- The Coherent Social Statistics register (*Sammenhængende Socialstatistik*, SHSS) cannot measure passive vs. active between 1990 and 1993 is that the registers combine passive benefit payments under Section 37 of the Development Assistance Act with active benefit payments under Section 49a (the "youth benefit scheme") of the Development Assistance Act. Moreover, measures of activation benefits from voluntary training and education programs (Section 42 of the Development Assistance Act) are replaced with individuals participating in rehabilitation programs beginning in 1990.

- The Labor Market Measures register (*Arbejdsmarkedsforanstaltninger*, AMFO) only provides information on participation in activation programs, not passive SA benefit receipt.

- The DREAM register (maintained by the Ministry of Employment) provides data on participation of a single social benefit/insurance program per week; many programs are prioritized above SA, leading to censored measures of SA participation. Thus, measuring SA participation in DREAM will be inaccurate for individuals who participate in SA and other transfer programs simultaneously.

The earliest year that SA participation can be measured by KTHJ is 1987. As a result, between 1984–1986, I use SHSS, which is of comparable quality to KTHJ during this time period; between 1980–1983, I use IND, since it is the only register dataset available during this time period that provides some measure of SA participation. Figure B.2 illustrates which registers are used to measure SA participation over the life-cycle for each birth cohort in the main sample. Since the IND registers provide annual SA benefit levels by calendar year, I impute the number of months an individual participates in SA based on the SA benefit formula; see details below. Figure B.3 shows there are no breaks in SA participation trends

Figure B.2: Registers used to measure SA participation over life-cycle, by birth cohort

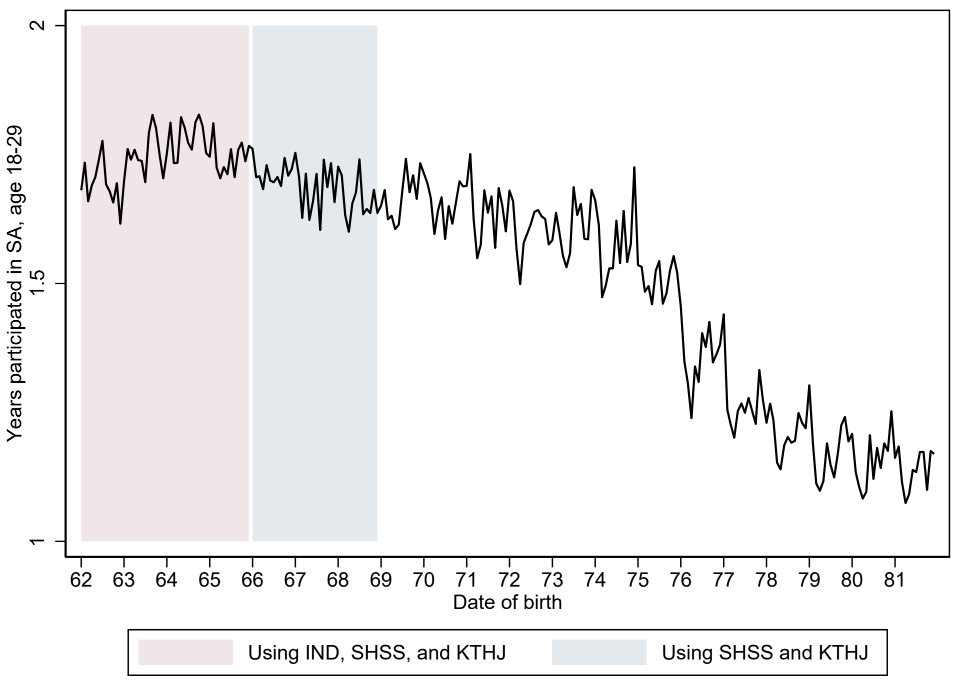


across cohorts that are based on different register data sources.

Imputation for 1980–1983 data. I impute the duration of SA participation at ages between 1980–1983 using annual SA benefit data from the IND register. I approximate the number of months of SA participation in each calendar year using the maximum monthly SA benefit using SA benefit rules, which depend on individual demographic characteristics including age, marital status, household structure, and dependent children; see Hansen and Schultz-Nielsen (2017) for more details. For the calendar year where an individual turns 18 years old, I set the maximum possible number of months of SA participation to the number of months that an individual is age 18 during the calendar year; for other ages, I set the maximum possible number of months to 12.[5] I further adjust this approximation in 1983 based on monthly participation data from SHSS for the observed fraction of age in 1984. I then assign individuals to the calendar year approximations based on the age they turn in the calendar year.

5. In general, the youngest age that any individual can participate in SA is age 18, though there are some exceptions.

Figure B.3: No breaks in SA participation over birth cohorts based on register data source



APPENDIX C

ADDITIONAL DETAILS

C.1 Risk Groups

As discussed in the main text, risk groups are defined by two criteria: (1) whether the individual's household participated in SA and (2) whether their neighborhood block of residence's SA participation rate was above the median (conditional on neighborhood mean income and urbanicity bins). These two risk characteristics should be measured before an individual becomes eligible for SA (age 18) and before they might reflect behavioral responses due to exposure to the welfare-to-work reforms. Section C.1.1 examines the implications of measuring risk characteristics at different ages during the life-cycle. Section C.1.2 describes how these specific measurement choices are used to construct the risk groups used in my main analysis. Section C.1.3 provides plots of background characteristics by risk group over the life-cycle.

C.1.1 Timing of the Measurement of Characteristics used to Construct Risk Groups

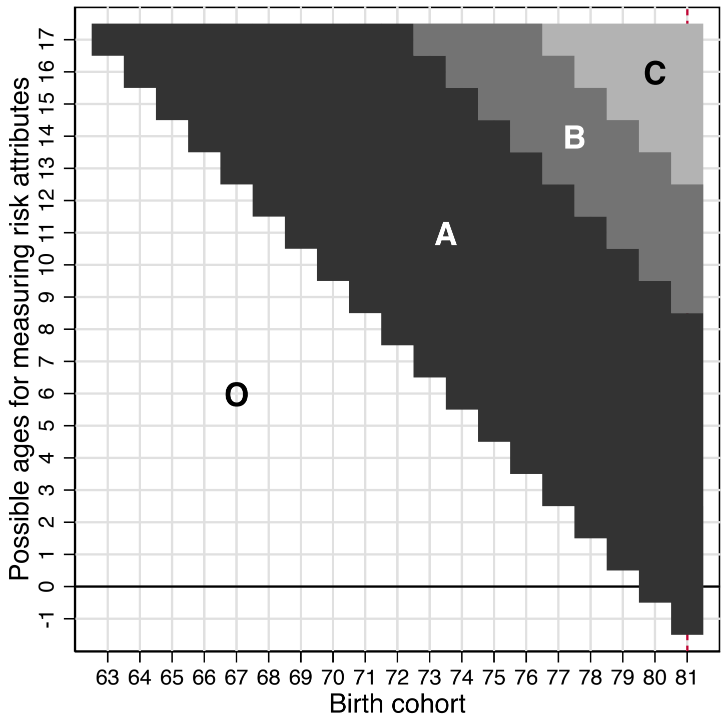
Constraints on the timing of measurement across cohorts. Ideally, one should avoid using risk characteristics measured after exposure to a welfare-to-work reform. This could occur in two distinct ways. First, an *individual*'s exposure to the reforms may *indirectly* affect their household members' behavior, which would in turn affect the risk characteristics measured at later ages, even though their household members are not directly affected by the reforms. In particular, if an individual's (cohabiting) parents possess altruistic motives, they may wish to make additional investments in the individual's human capital, which could incentivize them to accumulate more resources (by, e.g., leaving SA for work) or moving to a neighborhood with higher quality schools (which are associated with lower SA participation rates). This issue implies that the maximum age that risk characteristics can be measured decreases for later birth cohorts (since they are exposed to the reforms earlier in the life-cycle). For example, the 1981 cohort is exposed to the July 1990 reform starting at age 8 or 9, so avoiding these indirect behavioral responses would imply that risk characteristics can only be measured up to age 8; meanwhile, the 1971 cohort is exposed to this reform starting at age 18 or 19, but risk characteristics can be measured up to age 17 to ensure they are measured before the individual is formally eligible for SA.[1] Second, an individual's older *household members* are *directly* affected by the 1994 reform, which mandated work requirements for all SA participants above age 18. Exposure to this reform could influence the household's own SA participation and potentially their residential choice of neighborhood. For example, avoiding these direct behavioral responses means that the risk characteristics of the 1981 cohort can be measured up to age 12, while those of the 1971 cohort can still be measured only up to age 17.

An additional constraint is that the register data used to measure risk characteristics are only available beginning in 1980. This means that the minimum age that risk characteristics can be measured increases for earlier birth cohorts. Continuing the previous example, the risk characteristics of the 1981 cohort are observed beginning at age -1, while those of the 1971 cohort are observed only beginning at age 9. The constraints set by the timing of exposure of reforms and data availability mean that risk characteristics will be measured at different age ranges for different birth cohorts, as summarized by Figure C.1.

Implications of measuring risk characteristics at different ages of life-cycle. Variation in the appropriate life-cycle window for measuring risk characteristics may drive changes in the composition of risk groups across birth cohorts. This leads to two specific concerns:

1. Characteristics are measured annually at each calendar year. Thus, for the 1981 cohort, I observe characteristics at age 8 in the 1989, the year before the first reform.

Figure C.1: Available age ranges for measuring risk characteristics by birth cohort, pre- and post-reforms



Notes: O = Data unavailable. A = Data available, before individual and their parents are exposed to reform. B = Data available, after individual is exposed to reform but before their parents are exposed. C = Data available, after individual and their parents are exposed to reform. Age ranges based on individuals born on January 1 of each birth cohort.

1. Does the timing of measuring risk characteristics during the life-cycle affect the share of individuals classified in the high-risk group across birth cohorts?

2. Does the timing of measuring risk characteristics during the life-cycle affect an individual's participation in SA during young adulthood?

Such changes in the composition of risk groups across birth cohorts may suggest possible violations of the parallel trends assumption required for identification.

Figure C.2 examines the first concern by plotting the the mean risk characteristics measured at each age over the life-cycle up to age 17. The solid black lines plots the pooled means over all cohorts; the dashed black lines plots, on the available data, the means computed among cohorts exposed to the reforms starting age 14 to avoid household behavioral responses to the 1994 reform. Panel (a) reveals that household SA participation rate steadily declines over the course of the individual's life-cycle, while Panel (b) reveals that the proportion of the sample residing in high SA neighborhoods barely changes. Hence, the decline in the proportion of individuals from both households on SA and high SA neighborhoods over the life-cycle, as shown in Panel (c), is driven by household SA participation rates. Since earlier cohorts in my sample rely on measuring risk characteristics later in childhood due to data restrictions (see Figure C.1), this means that defining risk groups over different age ranges over the life-cycle could lead to too many individuals classified in the high-risk group in later birth cohorts. If the composition of the high-risk group varies over birth cohorts, this could lead to differential trends in potential outcomes that threaten my main identification strategy.

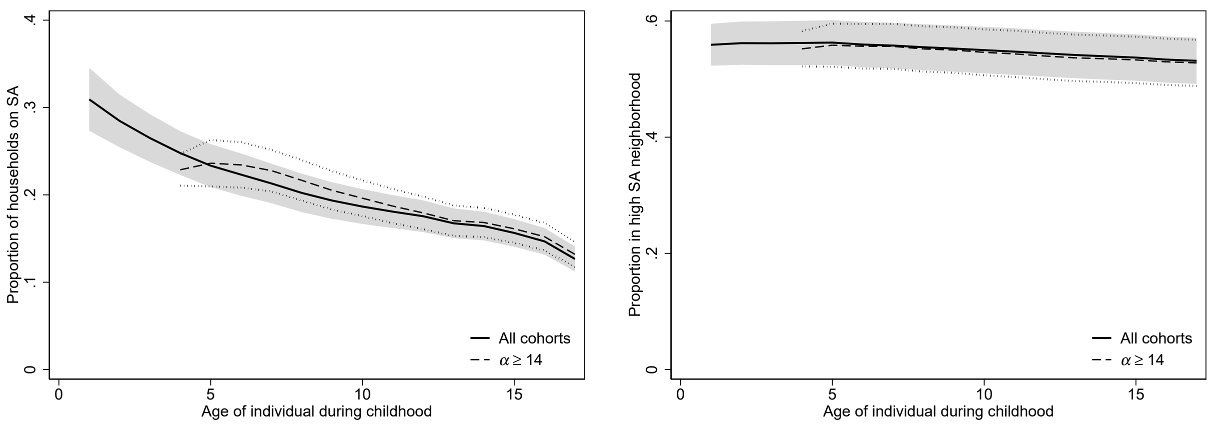
Figure C.3 investigates the second concern by illustrating the predicted increase in the duration of individuals' SA participation between ages 18–24 based on the risk characteristics. Panels (a) and (b) plot the predicted effects for each individual characteristic; Panel (c) plots the predicted effect of both characteristics. The latter Panel demonstrates that measuring risk characteristics at age 10 or above has roughly the same predicted effect of individual's duration of SA participation. Thus, as long as risk characteristics are measured at age 10 or above, one may expect similar comparisons between "treatment" and "control" groups defined by the risk characteristics.

Together, the previous two figures indicate that the best strategy is to measure risk characteristics as late as possible in the individual's life-cycle before age 18. Risk characteristics of earlier birth cohorts are only observable in late adolescence, so given Figure C.2's patterns, measuring risk characteristics among younger cohorts as late as possible—before they are exposed to any of the reforms—would help ensure that individuals classified in the high-risk group are comparable across birth cohorts. Figure C.3 helps alleviate concerns on

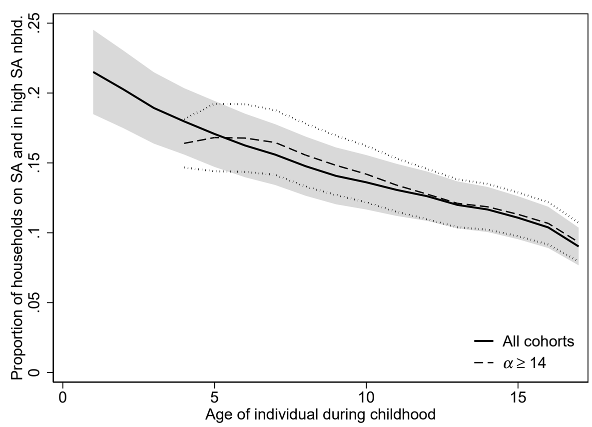
Figure C.2: Mean risk characteristics, measured by age of Individual's Life-cycle

(a) Household Participates in SA

(b) Lives in High SA Neighborhood Block



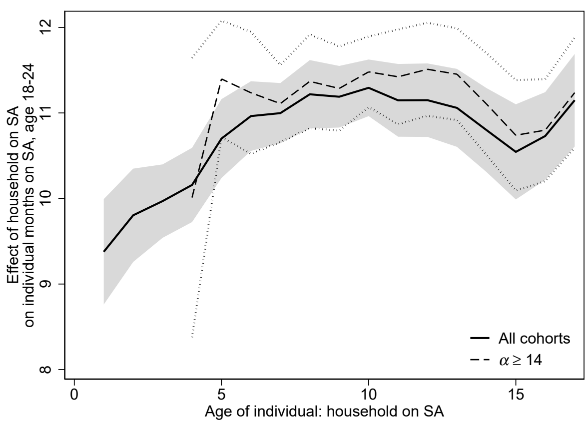
(c) Both Risk Characteristics



Notes: Solid black lines are means of risk characteristics among all birth cohorts measured at different ages of the individual's childhood. Dashed black lines are means of risk characteristics among birth cohorts exposed to the reforms starting at age 14 or later. The shaded areas and dotted lines represent their respective 95% confidence intervals. Vertical dashed red line marks age 7. Standard errors are two-way clustered over municipality of residence of residence at the age of the individual and birth quarter. All calculations performed on base sample; see main text for details.

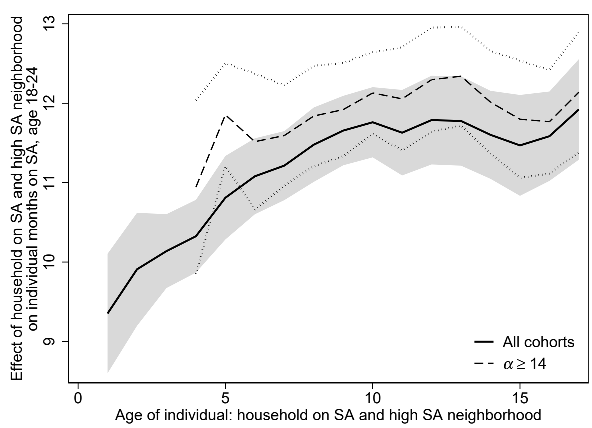
Figure C.3: Predicted Effect of Risk Characteristics on Individual's SA Participation, measured by Age of Individual's Life-cycle

(a) Household Participates in SA



(b) Resides in High SA Neighborhood Block

(c) Both Risk Characteristics



Notes: Solid black lines are the estimated coefficients from regressions of individual's duration of SA participation between ages 18–24 on an indicator of household participation in SA (Panel (a)), on an indicator of residence in high SA neighborhood block (Panel (b)), and on the interaction between these indicators (Panel (c)) measured at different ages of the individual's childhood, using all birth cohorts. Dashed black lines are analogous coefficient estimates from regressions run on birth cohorts exposed to the reforms starting at age 14 or later. The shaded areas and dotted lines represent their respective 95% confidence intervals. Vertical red dashed line marks age 7. Standard errors are two-way clustered over municipality of residence of residence at the age of the individual and birth quarter. All calculations performed on base sample; see main text for details.

comparisons between high- and low-risk groups across cohorts.

Examining household behavioral responses in risk characteristics over the life-cycle. The extent that risk characteristics can be measured later in the life-cycle among younger cohorts depends on whether older members of individuals' households respond to the individual's exposure to the 1990 reform or their own exposure to the 1994 reform. The former, more conservative assumption restricts the permissible age range to only region A depicted in Figure C.1; the latter assumption buys up to four additional ages of the younger cohorts' life-cycles, expanding the permissible age range to regions A and B.

To examine the household's behavioral responses, Figure C.4 plots the evolution of risk characteristics over the individual's life-cycle by cohort groups based on the starting age of exposure to the welfare-to-work reforms. The pieces of the series with filled markers correspond to Region B of Figure C.1 (when individuals are exposed to the reforms), while the pieces with hallow markers correspond to Region C (when older household members are exposed to the reforms). In the case of household participation in SA (solid lines), there are no significant changes in levels between the unmarked lines and the the filled marked lines but clear breaks from the filled marked lines to the hallow marked lines, suggesting an individual's household SA participation is only affected by the 1994 reform. In the case of living in a high SA neighborhood block (dashed lines), the body of evidence suggests that most households do *not* respond to the individual's exposure to the early reforms but do respond to their own exposure from the 1994 reform.[2] This implies that the permissible age range for measuring risk characteristics are Figure C.1's regions A and B.

2. There is some suggestive evidence that individuals with earlier starting ages may sort to low SA neighborhoods. However, the magnitude of these changes are very small.

C.1.2 Constructing Risk Groups from the Risk Characteristics

The analyses from the previous subsection imply that it is appropriate to measure risk characteristics starting at age 10, as late as possible in the individual's life-cycle up to age 17, and before 1994. These criteria motivate constructing risk groups using the following definitions of risk characteristics: (1) household on SA is defined by any of the individual's household members ever participating in SA during the three highest available ages between 12–17, before 1994; (2) high SA neighborhood is defined by whether the modal neighborhood block of residence between ages 12–17, before 1994, possessed an above-median participation rate (conditional on the neighborhood's mean income and urbanicity).[3] The main risk groups are defined by individuals possessing *both* or *neither* background characteristics. As a robustness check, I also construct alternative risk groups using analogously defined characteristics measured between ages 8–17 and before 1990.

I use the three highest available ages between ages 12–17, before 1994, to measure household participation in SA to ensure assignments to risk groups use similar number of years of data across cohorts. Using up to three years of data represents a compromise. On the one hand, it helps capture households who only temporarily participate in SA that using one or two years would miss and allows me to focus on measuring characteristics later in the life-cycle. On the other hand, it causes the 1963 and 1964 cohorts are limited to one and two years of data, respectively, due to data limitations. Using more than three years of data would require using ages earlier in the life-cycle and would lead to greater differences in the number of years of data available across cohorts.

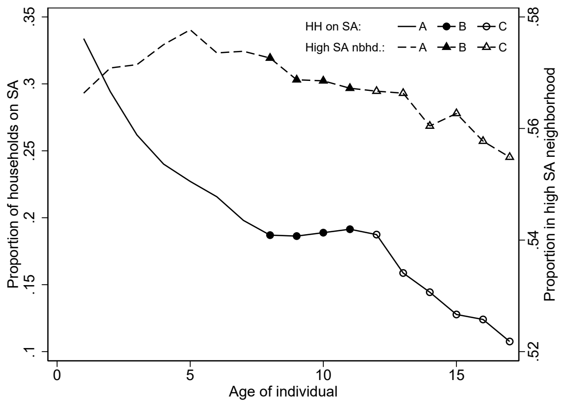
To demonstrate this point, Figure C.5, Panel (a) plots the share of high-risk groups across birth cohorts in the case where individuals are assigned to the high-risk group if their mean household participation in SA over the permissible age range falls above the pooled mean

3. Ties for the modal neighborhood are broken by assigning individuals to the neighborhood they lived in later in their life-cycle.

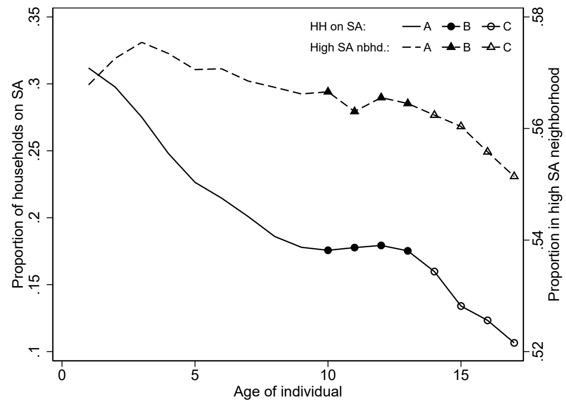
across birth cohorts, while Panel (b) plots the shares where high-risk group are defined using the three highest possible ages as described above. The series in Panel (a) are far more volatile by virtue of the different number of years available across different birth cohorts, which affect whether households with participation rates near the grand mean are assigned to the high- or low-risk group. Focusing on Panel (a), the share of high-risk group based on ages 12–17, before 1994 (solid line), is much more stable across birth cohorts while the share based on ages 8–17, before 1990 (dashed line), increases since the younger cohorts rely on earlier life-cycle data. For completeness, Figure C.6 plots the differences in the duration of SA participation between ages 18–24 between risk groups across cohorts. Different risk group definitions follow similar patterns, which is not surprising in light of Figure C.3.

Figure C.4: Mean risk characteristics over the life-cycle, by starting age cohort

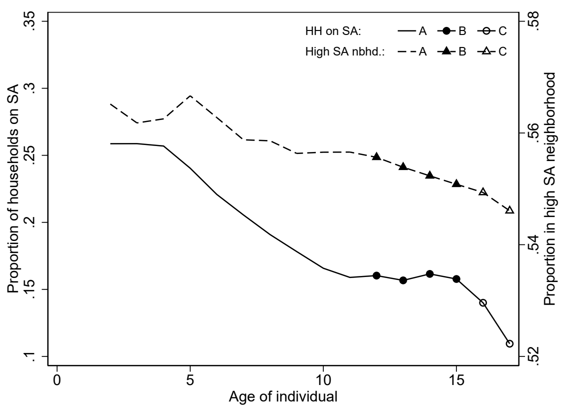
(a) $\alpha = 8$



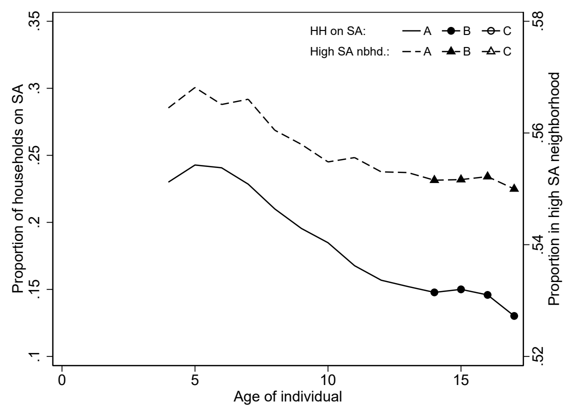
(b) $\alpha = 10$



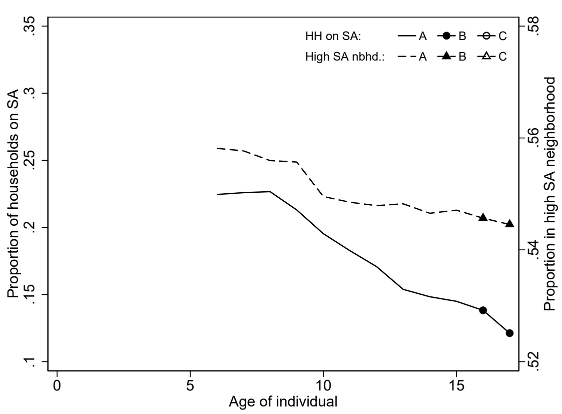
(c) $\alpha = 12$



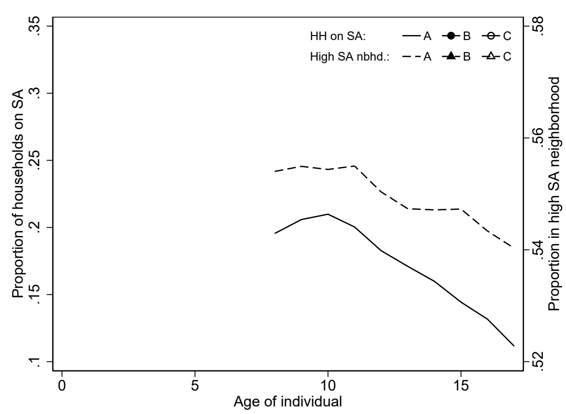
(d) $\alpha = 14$



(e) $\alpha = 16$



(f) $\alpha = 18$

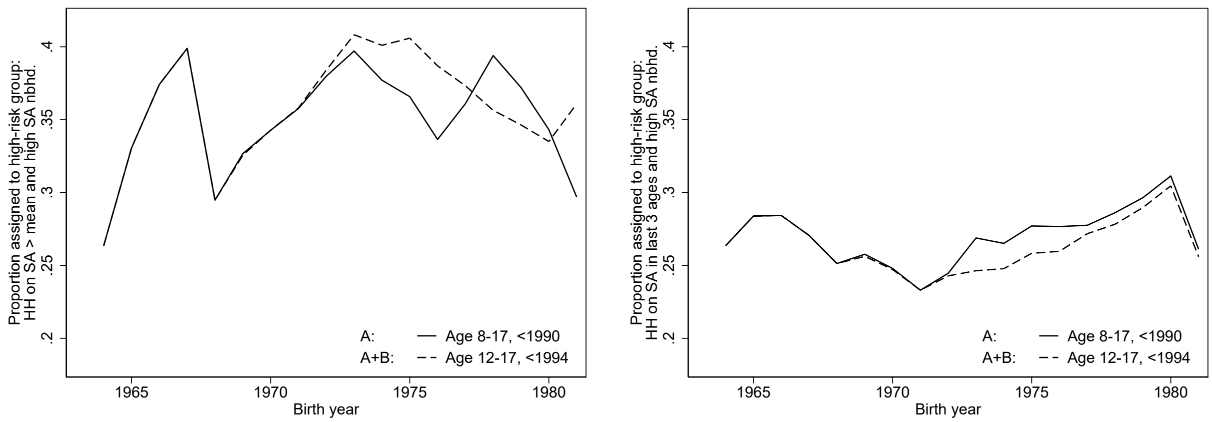


Notes: Starting age of exposure to reforms denoted by α. Line segments labeled "A," "B," and "C" correspond to the regions illustrated in Figure C.1 (missing data corresponds to region "O"). All calculations performed on base sample; see main text for details.

Figure C.5: Proportion assigned to high-risk group over birth cohorts

(a) Mean participation

(b) Three highest ages

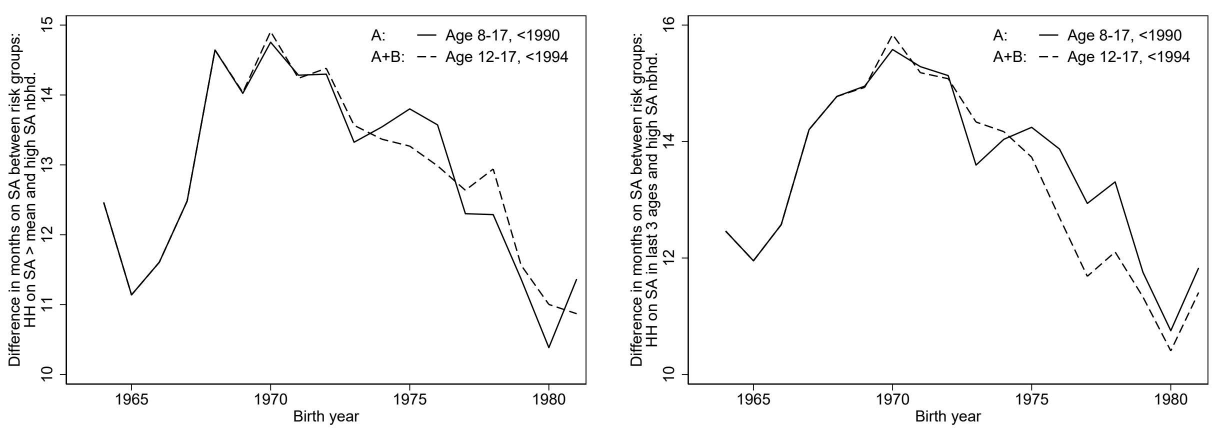


Notes: Panel (a) assigns individuals to the high-risk group if the mean household participation in SA over the permissible age range falls above the pooled mean across birth cohorts and if they live in a high SA neighborhood. Panel (b) defines high-risk group according to the main text. Solid lines define permissible age ranges based on the individual's exposure to the welfare-to-work reforms (Region A of Figure C.1); dashed lines define them based on the household's exposure (Regions A and B of Figure C.1). All calculations performed on base sample; see main text for details.

Figure C.6: Difference in duration of SA participation, age 18–24, over birth cohorts

(a) Mean participation

(b) Three highest ages



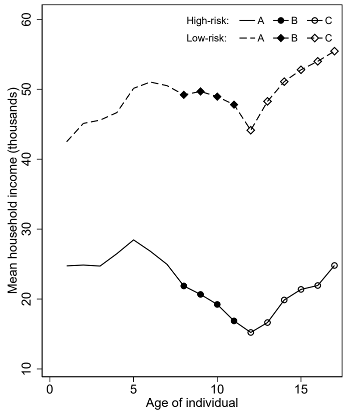
Notes: See notes of Figure C.5.

C.1.3 Background Characteristics by Starting Age and Risk Group

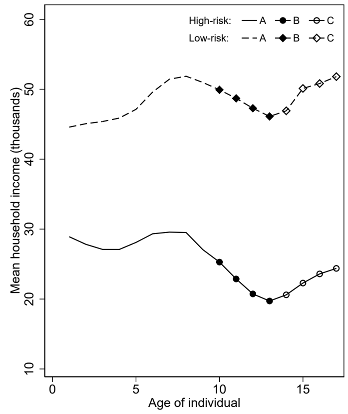
Following Figure C.4, the following figures plot the life-cycle evolution of other background characteristics by starting age cohorts and risk groups over the course of an individual's childhood. This motivates the appropriate ages used to measure background characteristics that are utilized as control variables in all my main empirical analyses.

Figure C.7: Mean household income over the life-cycle, by starting age and risk group

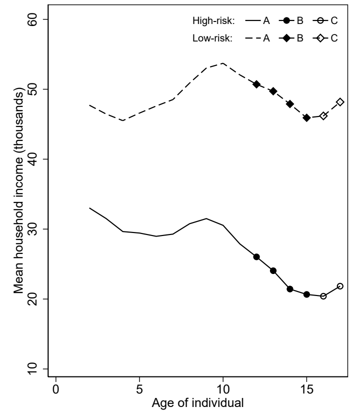
(a) $\alpha = 8$



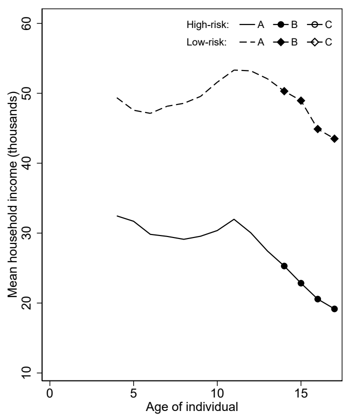
(b) $\alpha = 10$



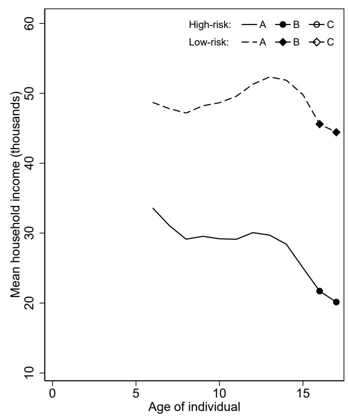
(c) $\alpha = 12$



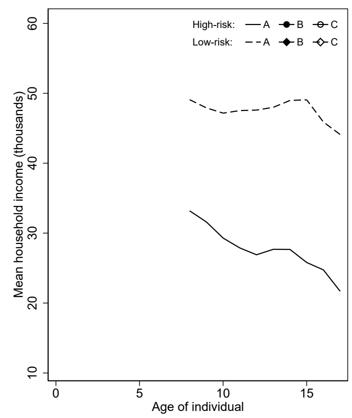
(d) $\alpha = 14$



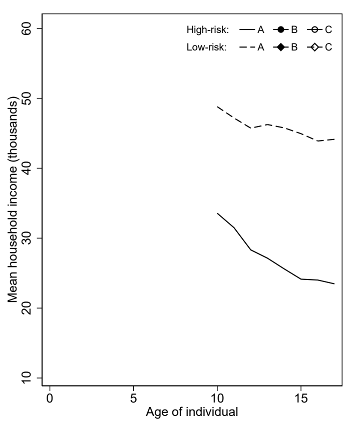
(e) $\alpha = 16$



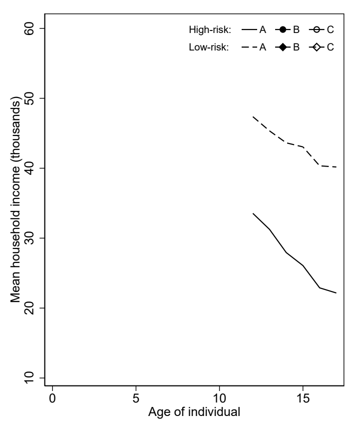
(f) $\alpha = 18$



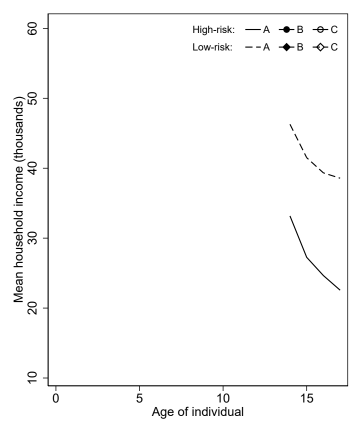
(g) $\alpha = 22$



(h) $\alpha = 24$

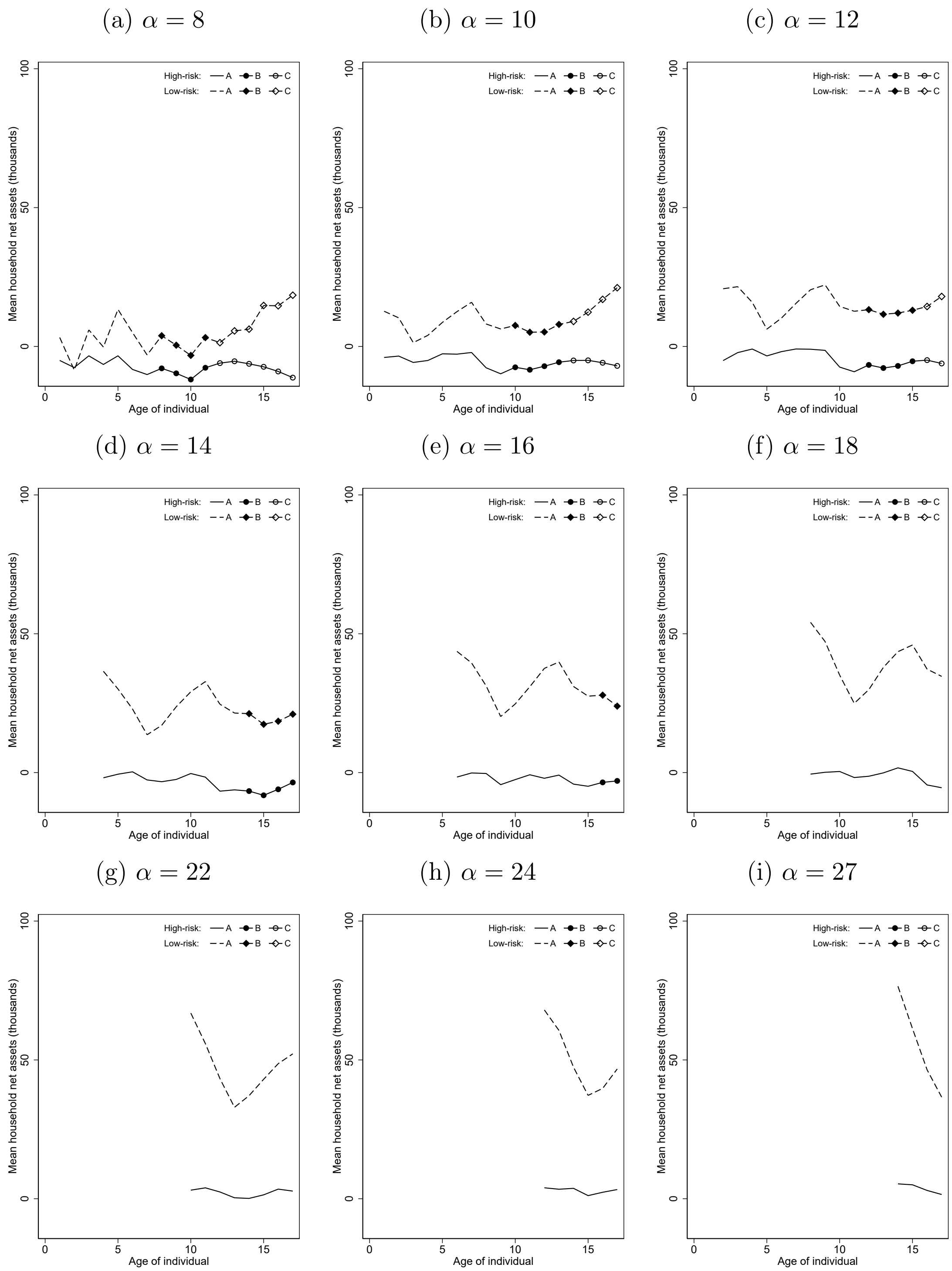


(i) $\alpha = 27$



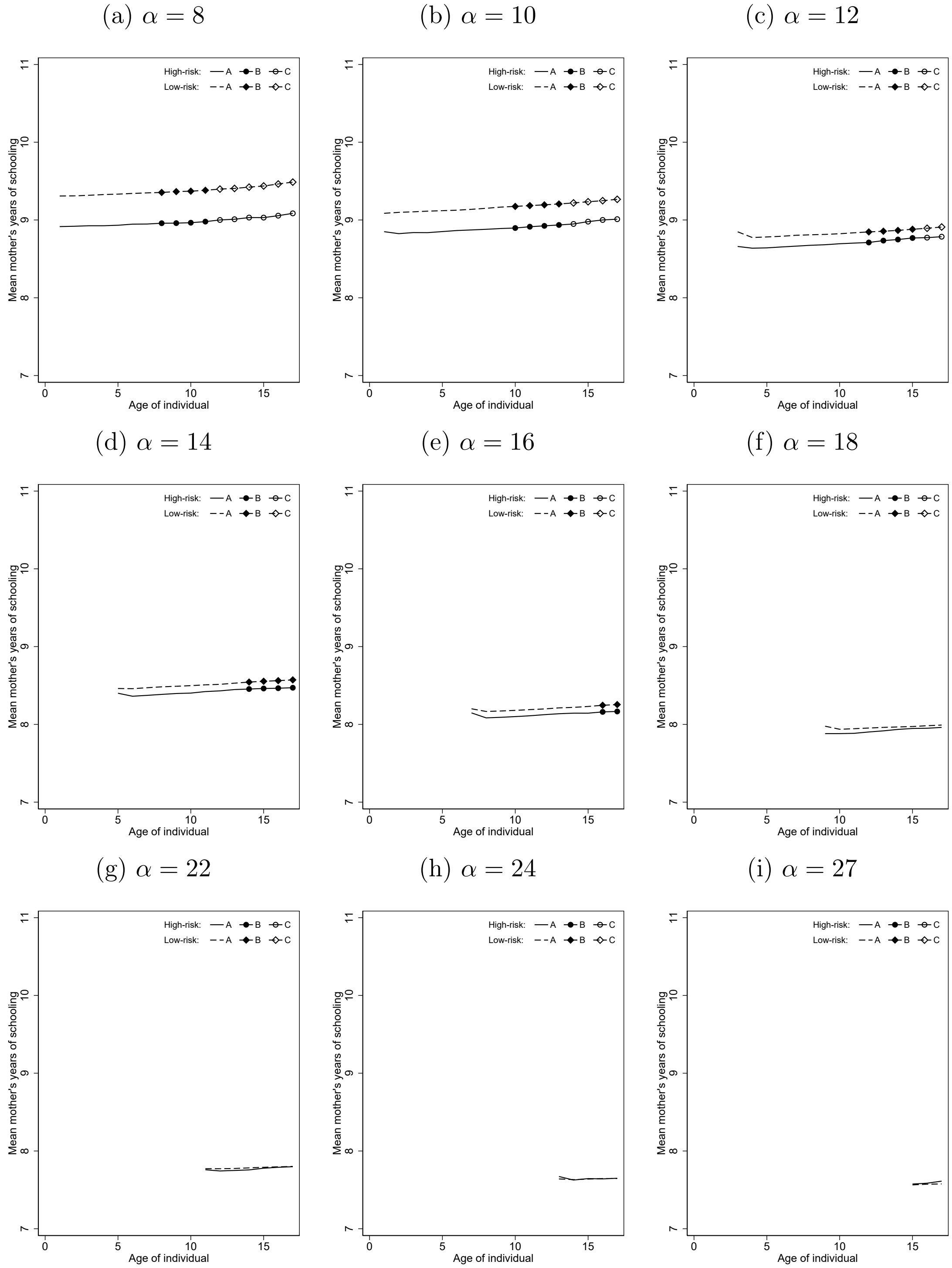
Notes: Starting age of facing reforms denoted by α. Line segments labeled "A," "B," and "C" correspond to the regions illustrated in Figure C.1 (missing data corresponds to region "O"). All calculations performed on risk groups.

Figure C.8: Mean household net assets over the life-cycle, by starting age and risk group



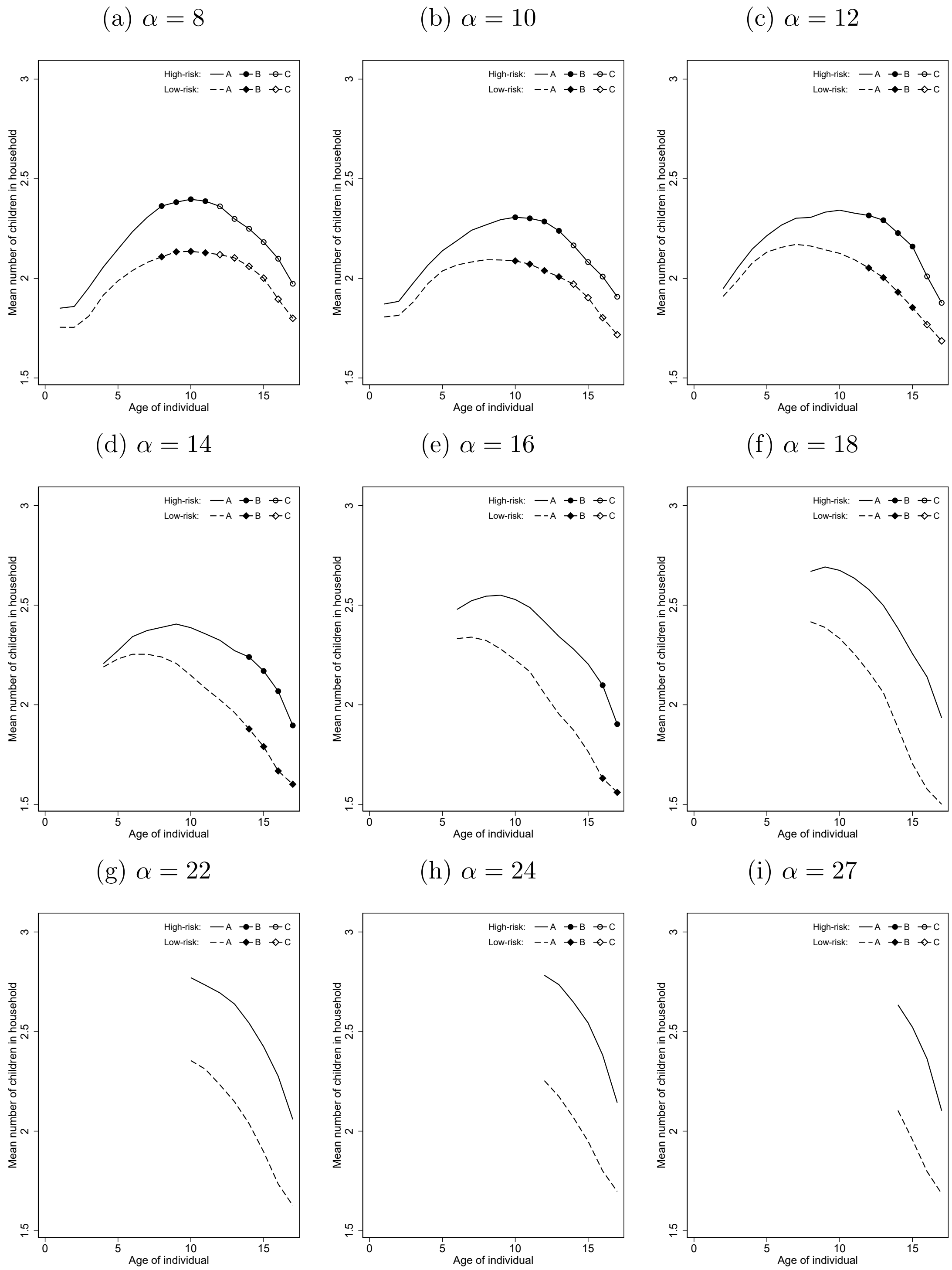
Notes: Starting age of facing reforms denoted by α. Line segments labeled "A," "B," and "C" correspond to the regions illustrated in Figure C.1 (missing data corresponds to region "O"). All calculations performed on risk groups.

Figure C.9: Mother's years of schooling over the life-cycle, by starting age and risk group



Notes: Starting age of facing reforms denoted by α. Line segments labeled "A," "B," and "C" correspond to the regions illustrated in Figure C.1 (missing data corresponds to region "O"). All calculations performed on risk groups.

Figure C.10: Number of household children over the life-cycle, by starting age and risk group



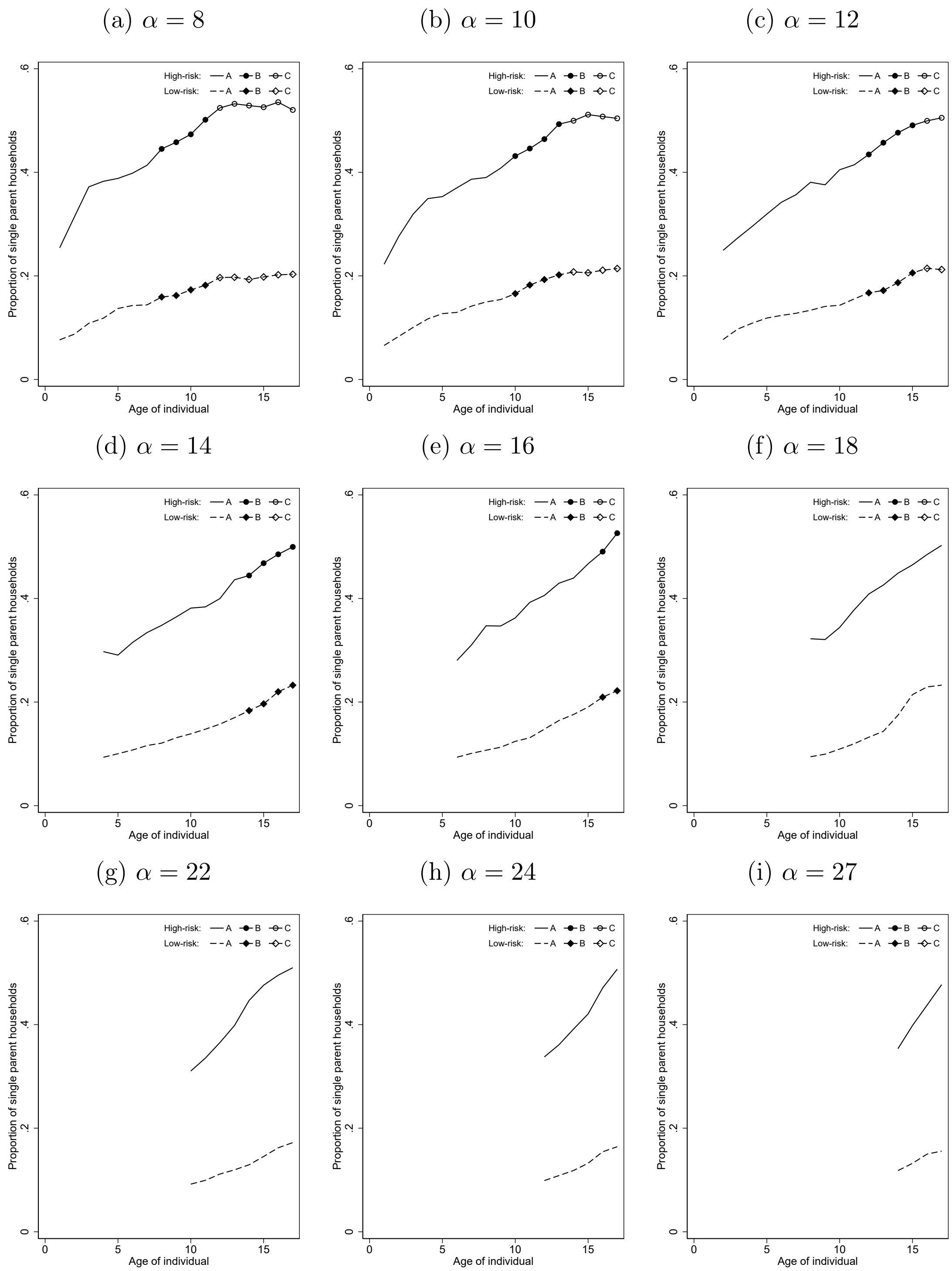
Notes: Starting age of facing reforms denoted by α. Line segments labeled "A," "B," and "C" correspond to the regions illustrated in Figure C.1 (missing data corresponds to region "O"). All calculations performed on risk groups.

Figure C.11: Single-parent household over the life-cycle, by starting age and risk group

(a) $\alpha = 8$ (b) $\alpha = 10$ (c) $\alpha = 12$

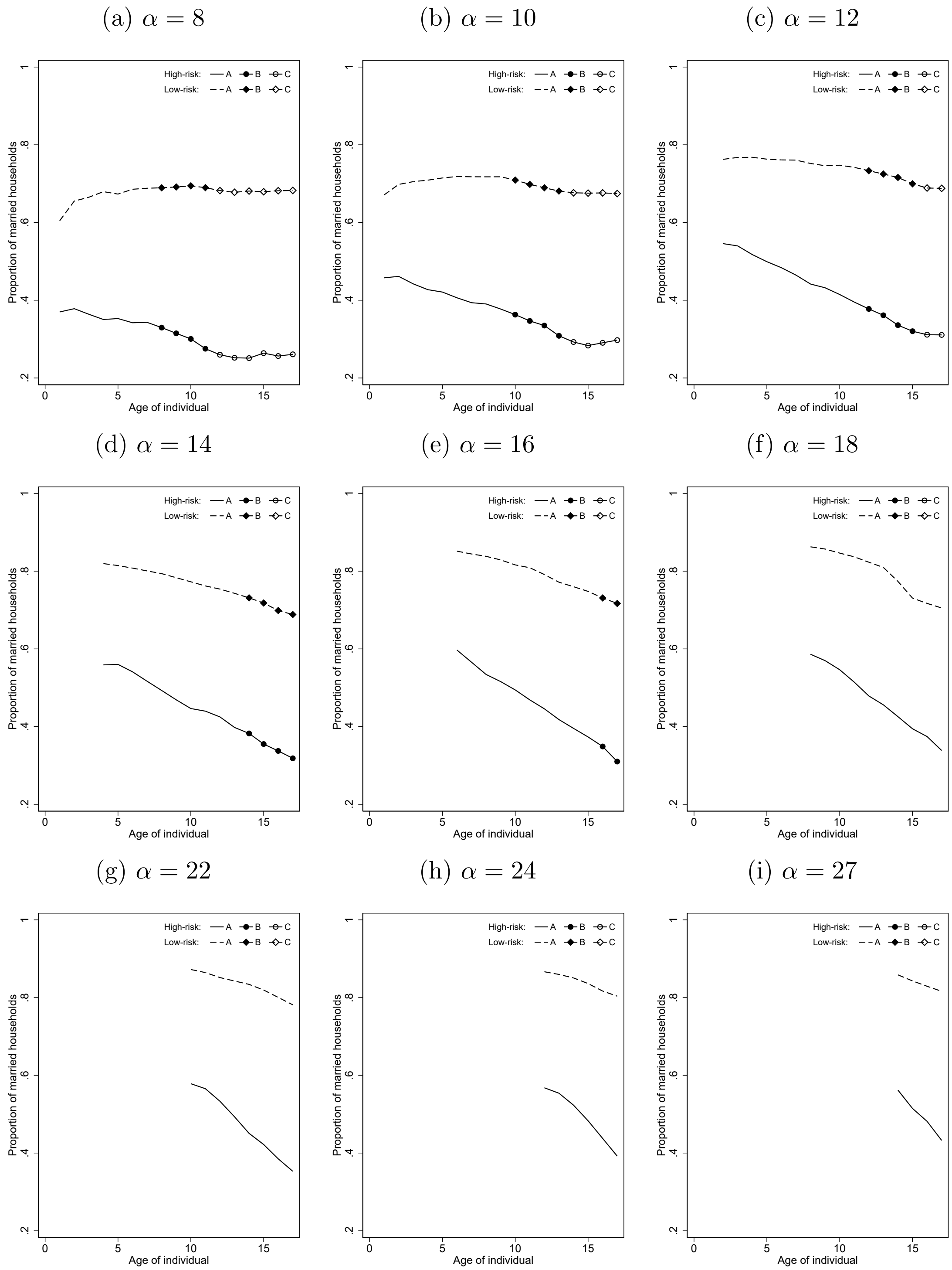
(d) $\alpha = 14$ (e) $\alpha = 16$ (f) $\alpha = 18$

(g) $\alpha = 22$ (h) $\alpha = 24$ (i) $\alpha = 27$



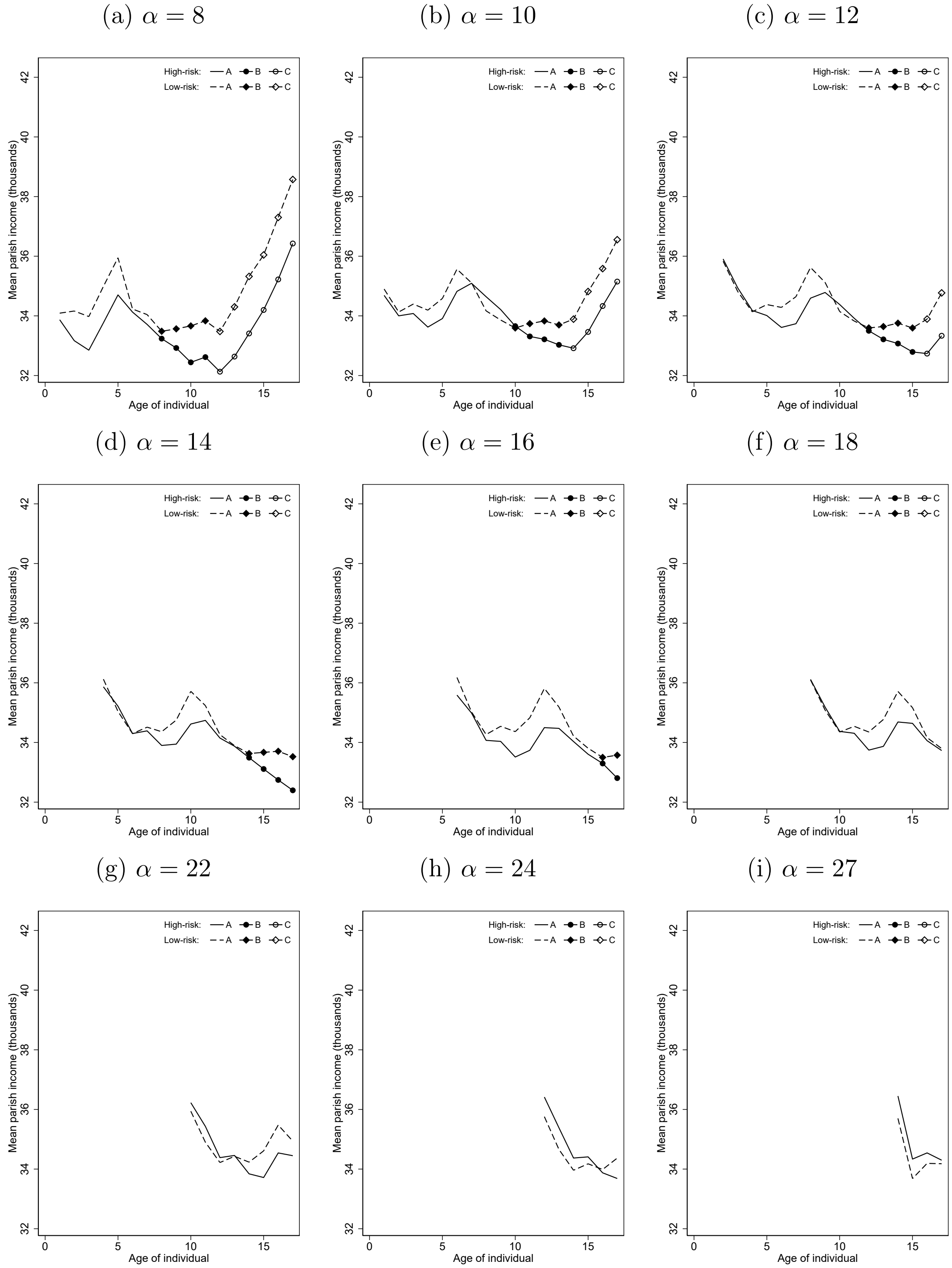
Notes: Starting age of facing reforms denoted by α. Line segments labeled "A," "B," and "C" correspond to the regions illustrated in Figure C.1 (missing data corresponds to region "O"). All calculations performed on risk groups.

Figure C.12: Married household over the life-cycle, by starting age and risk group



Notes: Starting age of exposure to reforms denoted by α. Line segments labeled "A," "B," and "C" correspond to the regions illustrated in Figure C.1 (missing data corresponds to region "O"). All calculations performed on risk groups.

Figure C.13: Mean parish income over the life-cycle, by starting age and risk group



(a) $\alpha = 8$ (b) $\alpha = 10$ (c) $\alpha = 12$

(d) $\alpha = 14$ (e) $\alpha = 16$ (f) $\alpha = 18$

(g) $\alpha = 22$ (h) $\alpha = 24$ (i) $\alpha = 27$

Notes: Starting age of exposure to reforms denoted by α. Line segments labeled "A," "B," and "C" correspond to the regions illustrated in Figure C.1 (missing data corresponds to region "O"). All calculations performed on risk groups.

C.2 Robustness Analyses

C.2.1 Changes to State Educational Grants

One of my robustness analyses compares individuals from particularly low-income households against those from higher-income households. The logic behind this comparison is to check if other policy changes unrelated to SA that may have a disproportionate effect on low-income individuals are driving my main findings.

One policy reform of potential concern is a 1988 reform to state educational grants (*Statens Uddannelsesstøtte*; SU, or student aid), which increased transfers to students attending college by at least 25%.[4] For low-income individuals, this increase in student aid could relax credit constraints and promote college attendance, which could be driving the positive effects on educational attainment and income for younger starting ages. Nevertheless, this policy change likely does not pose a threat to my analysis since SA youth benefit rates actually increased to match the new levels of student aid in 1989 (Hansen and Schultz-Nielsen 2017), which would dampen incentives to substitute from SA to higher education.[5]

C.2.2 Difference-in-Differences Exercises

In my falsification exercises on pre-treatment outcomes and validation exercises on specific outcome ages, I employ the following difference-in-differences event-study specification that

4. See Nielsen et al. (2010) for more details of the reform. Using the 1985 formula for means-tested student aid scheme they provide, I find that the vast majority of my sample that falls in the bottom quartile of the household income distribution would be eligible for the highest level of aid.

5. Another reason to be skeptical that this reform influences my results is that it actually increased student aid the most for higher-income individuals, since it transformed student aid from a means-tested scheme to a universal conditional cash transfer. This means that low-risk individuals—who are more likely to be higher-income—experienced greater changes in potential aid relative to high-risk individuals, so this reform may generate null differences in outcomes between risk groups.

exploits variation between risk groups and starting age cohorts:

$$Y_{ia} = \sum_{8 \leq \alpha \leq 30,\ \alpha \neq a+1} \beta_\alpha \left(G_i \times \mathbb{1}(D(b_i) = \alpha)\right) + \delta_1 G_i + \delta_b$$
$$+ \boldsymbol{\gamma} \boldsymbol{X}_i + \boldsymbol{\eta} \boldsymbol{X}_i \times b + \theta_{m_{D(b_i)-1}(i)} + \varepsilon_{ia}.$$

Figures associated with these falsification and validation tests plot estimates of the event-study parameters β_α grouped in consecutive pairs. This grouping is to gain greater precision. (Ungrouped estimates gives qualitatively similar but noisier results.) Note that pre-trend tests are available for starting ages $\alpha \geq a + 1$. To gain greater precision, I group α into consecutive pairs.

C.2.3 Validation Exercises

The 1994 unemployment insurance reform. The remaining rows of Figure 3.2 plot results from three validation checks.

The sixth row checks whether the presence of a 1994 UI reform is responsible for my results. This UI reform introduced work requirements to individuals who received UI benefits for four years. Recall that, in most cases, UI members do not participate in SA, so the SA and UI reforms in principle target different populations. To reduce the risk of my sample being affected by the UI reform, I restrict my main sample to individuals from households where at least 50% of cohabiting adults lack UI membership. Due to their limited exposure to the UI system, these individuals are 20% (12.4 percentage points) less likely to be UI members by age 24 compared to those from UI member households. On this restricted sample, I find qualitatively similar effects across all starting age groups. If anything, estimates for starting ages $\alpha \leq 18$ grow in magnitude; starting ages $\alpha > 18$ remain null.

I also estimate the effects of the 1994 UI reforms, which are plotted in the seventh row. This serves as another way to validate my theory of starting age heterogeneity being driven by anticipatory responses to policy reforms. UI fund membership typically begins after age 21 since membership requires vocational certification and work experience; thus, starting age cohorts $\alpha \leq 21$ could anticipate facing work requirements in the future which could induce anticipatory behavioral response consistent with evidence on "threat effects" of UI work requirements (Rosholm and Svarer 2008). In this exercise, my sample consists of households that did not participate in SA but are UI members, and I estimate effects for individuals from households that received UI benefits and, thus, more prone to being affected by the UI reform. Estimates reassuringly show positive effects up to $\alpha \leq 24$. In this way, this exercise verifies that the null or negative effects of the SA reforms for starting ages $19 \leq \alpha \leq 24$ are meaningful and not driven by confounders.

The final row of Figure 3.2 plots estimates from specifications where I use participation in SA at age 18 instead of risk groups for $\alpha \geq 19$. Just like my main estimates, I find largely null effects for these starting age cohorts, demonstrating that my results are not driven by ill-defined risk groups.

Timing of municipality implementation. The main results were based on the timing of national WtW reforms. In reality, there was heterogeneity in the timing that work requirements were implemented across municipalities. Some municipalities adopted work requirements for their SA recipients before the July 1990 reform (and, indeed, helped inspire national legislation), while others delayed adoption by a number of years.[6] As a robustness check, I estimate effects of individuals belonging to the same birth cohort who live in municipalities that implemented work requirements in different years. For example, if cohorts assigned to treatment starting age α (according to the national reforms) lived in a municipality that actually implemented the reform starting at age $\alpha + 2$, then these cohorts should experience similar treatment effects over their life-cycles compared to cohorts assigned to

6. For example, the municipality Farum instituted work requirements in the late 1980s; see Fallesen et al. (2018).

treatment starting age $\alpha + 2$ who lived in municipalities that promptly implemented work requirements.

To find the adoption date in each municipality, I estimate structural breaks in the time series of participation rates in SA work requirements in my base sample of low-SES individuals among age groups targeted by each reform, by municipality.[7,8] I use an algorithm that follows a similar spirit as the one used in Card et al. (2008). In what follows, let W_{ia} be individual i's take-up of SA work requirements at age a, $t \in \mathcal{T}_r$ index monthly calendar time over a time range relevant for reform r, $a(t)$ denote the age at time t, $r \in \mathcal{R} \equiv \{1990, 1992, 1994\}$ index the reform, and $\mathcal{A}_r$ be the set of newly treated ages in reform year r.

1. Estimate $\bar{W}_{mt,r} \equiv \mathbb{E}[W_{ia(t)} \,|\, m, t, a \in \mathcal{A}_r]$ for each municipality $m \in \mathcal{M}$ and reform year $r \in \mathcal{R}$ at at each time $t \in \mathcal{T}_r$.

2. Select the smallest root of the demeaned function $\bar{W}^d_{mt,r} \equiv \bar{W}_{mt,r} - \mathbb{E}[W_{ia(t)} \,|\, m, t \in \mathcal{T}_r, a \in A_r]$ with a positve gradient: $t^{\dagger}_{mr} \equiv \min\{t' \in \mathcal{T}_r \,|\, \bar{W}^d_{mt',r} = 0, \partial \bar{W}^d_{mt,r}/\partial t|_{t=t'} > 0\}$.

3. Select the adoption date as $t^{\star}_{mr} \equiv \max\{t' \leq t^{\dagger}_{mr} \,|\, \partial \bar{W}^d_{mt,r}/\partial t|_{t=t'} = 0 \text{ or } \bar{W}^{ma}_{mt',r} = 0\}$, where $\bar{W}^{ma}_{mt,r} \equiv \bar{W}_{mt,r} - \mathbb{E}[W_{ia(t')} \,|\, m, t' \leq t, a \in A_r]$ is the fitted function demeaned by its moving average.

In the first step, I estimate local linear fits of $\bar{W}_{mt,r}$ using an Epanechnikov kernel with bandwidth of 90 days in years with available SA work activity take-up data up to the year 2000.[9] Using a smoothed estimate of the conditional expectation function, like Card et

7. To the best of my knowledge, there are no institutional records of the exact timing that work requirements were implemented across all municipalities.

8. I verified that the base sample is well-represented in each municipality over time. This ensures that changes in take-up rates over time are not driven by changes in the relevant population.

9. There are limited measures of take-up of SA work activities before the national reforms in the registry data. I use measures of "voluntary" SA work activities from the Coherent Social Statistics and Cash Assistance registers labeled under the purview of the 1986 Development Assistance Act, Section 42, Subsection 1 (and associated pieces in subsequent revisions of the Act) whenever such data is available. Unfortunately,

al. (2008), dramatically improves the accuracy of locating adoption dates compared to other methods, especially for municipalities with relatively small sample sizes.[10] The second step, intuitively, locates the "time region" around adoption. If adoption results in a sufficiently large take-up of SA work activities and there are no large changes in take-up of SA work activities before then, then the demeaned function $\bar{W}^{d}_{mt,r}$ will lie below zero before adoption and rise above zero after adoption. The third step aims to find an earlier date within this time region when the take-up of SA work activities in the municipality begins to "ramp up," which provides a good indication of the timing of adoption of work requirements.[11]

For the 1990 and 1992 reforms, which are relevant for starting ages $\alpha \leq 24$, I categorize the 275 municipalities into two main groups: "early" adopters that adopted the work requirement policy at least 6 months before the national reform date (indicated by $E_m \in \{0, 1\}$) and "prompt or late" adopters that adopted the policy after this time. Due to data limitations of SA work activity take-up ages 25 and above—the group affected by the 1994 reform (see Footnote 9)—I use only the latter two categories for starting ages $\alpha \geq 25$.

I modify the TD specification to study heterogeneous effects for individuals residing in

"voluntary" SA work activities are not recorded after 1990, resulting in breaks in the time series of activation between 1990-1991 for ages 21–24 and 1990–1993 for ages 25 and above. Measures of "voluntary" SA work activity appears to provide an appropriate proxy for municipality-level policy for work requirements: In the case of Farum, which implemented SA work requirements in mid-1987, my algorithm applied to ages 18–19 estimates an adoption date of March 1988.

10. Fitting jump or kink discontinuity regression models with unknown thresholds (Hansen 2000, Hansen 2017) to find structural breaks do not perform well in general, especially in cases of municipalities that showcase gradual implementation, non-monotonic behavior shortly after ramp-ups, or small sample sizes with extreme outliers.

11. Using roots of $\bar{W}^{ma}_{mt,r}$ guards against cases of municipalities that experience very slow but continually increasing take-up in the years leading up to the reform (i.e., $\partial \bar{W}^{d}_{mt,r}/\partial t > 0$ for $t < t^{\dagger}_{mr}$).

the different municipality adoption groups in the year prior to the national reform:

$$
\begin{aligned}
Y_{ia} = \beta^P_\alpha & \Big(G_i \times \mathbb{1}(D(b_i) = \alpha) \times \mathbb{1}(a \geq \alpha)\Big) \\
& + \beta^E_\alpha \Big(E_{m_{\alpha-1}(i)} \times G_i \times \mathbb{1}(D(b_i) = \alpha) \times \mathbb{1}(a \geq \alpha)\Big) \\
& + \delta^P_1 G_i + \delta^P_b + \delta^P_a + \pi^P_{12}\Big(G_i \times \mathbb{1}(D(b_i) = \alpha)\Big) + \pi^P_{1,a} G_i + \pi^P_{2,a}\mathbb{1}(D(b_i) = \alpha) \\
& + E_{m_{\alpha-1}(i)} \times \Big[\delta^E_1 G_i + \delta^E_b + \delta^E_a + \pi^E_{12}\Big(G_i \times \mathbb{1}(D(b_i) = \alpha)\Big) \\
& \qquad\qquad + \pi^E_{1,a} G_i + \pi^E_{2,a}\mathbb{1}(D(b_i) = \alpha)\Big] \\
& + \boldsymbol{\gamma} \boldsymbol{X}_i + \boldsymbol{\eta}_1 \boldsymbol{X}_i \times a + \boldsymbol{\eta}_2 \boldsymbol{X}_i \times b + \theta_{m_{\alpha-1}(i)} + \varepsilon_{ia}.
\end{aligned}
$$

The parameters $\beta^P_{\alpha,a}$ are the effects for those residing in prompt or late adopters, while $\beta^P_{\alpha,a} + \beta^E_{\alpha,a}$ correspond to effects for those residing in early adopters.

It is worth mentioning that the magnitude of the parameter estimates for early (late) adopters is larger (smaller) than those of the prompt group. A plausible explanation for this phenomenon is that municipalities with earlier adoption times also tend to have greater enforcement and more stringent requirements.[12] Municipal agencies' responses from the Municipal Activation Scheme survey of 1996–1998 supports this explanation: Municipalities that reported to adopt work requirements for ages 25 and above before the 1994 reform activated significantly larger shares of their SA recipients and imposed more intense work requirements; see Tables C.1 and C.2.

C.2.4 Alternative measurement choices and samples.

Alternative measurement choices and samples.—I apply a battery of robustness checks to ensure my main results are unaffected by particular choices made in my main model specification; see Appendix Figures D.29–D.32. First, I examine alternative definitions of risk

12. In the case of Farum, an early municipality adopter, there was extremely strict enforcement of work requirements (Fallesen et al. 2018).

Table C.1: Differences in municipal policies for SA recipients facing only unemployment in 1996, by share of SA recipients on SA work programs before 1994 Reform

	Most activated		*Some activated*		*Few activated*	
	N	Mean [S.D.]	N	Diff. (S.E.)	N	Diff. (S.E.)
A. Coverage of work requirements						
% SA recipients activated	130	90.608	59	-9.108^{***}	39	-5.287^{*}
		[12.380]		(2.632)		(3.047)
B. Intensity of work requirements						
Hours/week of work req's.	131	34.168	62	−0.684	40	−0.268
		[4.757]		(0.672)		(0.789)
Months of work req's.	114	9.132	56	−0.149	33	0.020
		[5.706]		(0.775)		(1.139)
C. Sanction policy						
Deduct >cash benefits	130	0.846	62	−0.024	40	0.004
		[0.362]		(0.058)		(0.065)
Deduct cash benefits	130	0.915	62	−0.061	40	0.010
		[0.279]		(0.051)		(0.049)

Notes: Data from Questionnaire to Municipalities, *Municipal Activation Scheme (1996–1998)*, conducted by Det Nationale Forskningscenter for Velfaerd and provided by the Danish National Archives. The "Few activated" column includes municipalities who responded "To a lesser degree" or "Virtually none" to Question 24. Row 1 use responses to Question 19, Rows 2–3 use Question 16, and Rows 4–5 use Question 22. Differences are relative to the "Most activated" column. Standard deviations are in brackets; standard errors of t-tests are in parentheses.

Stars indicate statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.2: Differences in municipal policies for SA recipients facing additional issues in 1996, by share of SA recipients on SA work programs before 1994 reform

	Most activated		*Some activated*		*Few activated*	
	N	Mean [S.D.]	N	Diff. (S.E.)	N	Diff. (S.E.)
A. Coverage of work requirements						
% SA recipients activated	42	83.155	75	−13.955***	106	−27.466***
		[18.945]		(4.194)		(3.968)
B. Intensity of work requirements						
Hours/week (additional reasons)	42	34.810	76	−0.625	103	−3.479***
		[3.423]		(0.703)		(0.806)
Months (additional reasons)	34	9.235	68	1.765	90	0.698
		[5.052]		(1.120)		(1.045)
C. Sanction policy						
Deduct >cash benefits	43	0.837	77	−0.058	107	−0.033
		[0.374]		(0.074)		(0.069)
Deduct cash benefits	43	0.837	77	−0.006	107	−0.033
		[0.374]		(0.071)		(0.069)

Notes: See notes of Table C.1.

groups. I try defining the high-risk group as individuals whose households participated in SA between ages 12–17 only without using local neighborhood SA participation rates as a criterion. I also try defining risk groups based on measures of household and neighborhood SA participation defined between ages 8–17 and before 1990 instead of between ages 12–17 and before 1994. While observable household covariates do not appear to respond to the 1990–1992 reforms (see Appendix Figures C.7–C.13), measuring data between ages 8–17 before 1990 eliminates the possibility that my main definition of risk groups are affected by household responses to even the earliest reforms. Additionally, for starting age groups $\alpha \geq 18$, I try defining risk groups as individuals who are likely to be eligible for SA based on characteristics measured at age 17 related to liquid assets, UI membership, and employment and education enrollment.

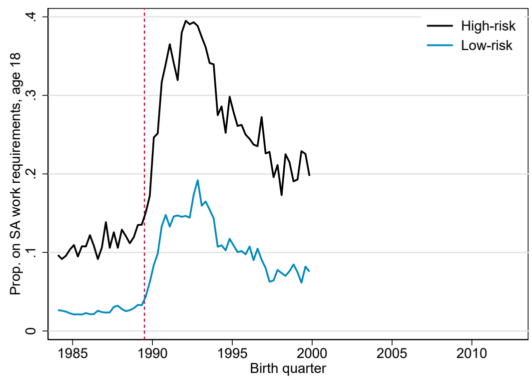
Second, I try different subsamples of my main data. I exclude individuals from the bottom decile of the household income distribution to ensure my main results are not driven by the most disadvantaged subgroup. I also try excluding the four most populous cities from the main sample (Copenhagen, Aarhus, Aalborg, Odense) to ensure that my results persist outside of urban areas with high SA participation rates. Finally, to test if results are sensitive to using significantly older starting age groups who are not-yet-treated at a given outcome age, I shorten the window of comparison cohorts to starting ages at most three years above a given outcome age. All of my results survive these alternative measurement choices and sampling schemes.

APPENDIX D

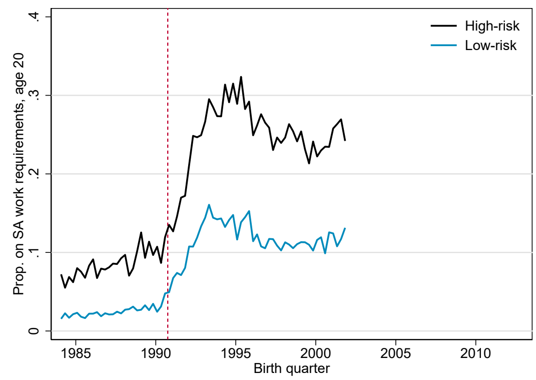
ADDITIONAL FIGURES & TABLES

Figure D.1: Take-up of SA work requirements across birth cohorts, by outcome age

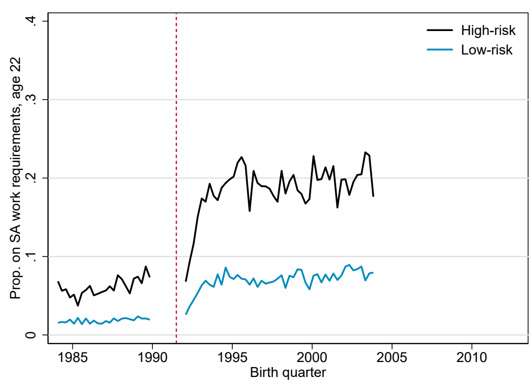
(a) Age 18



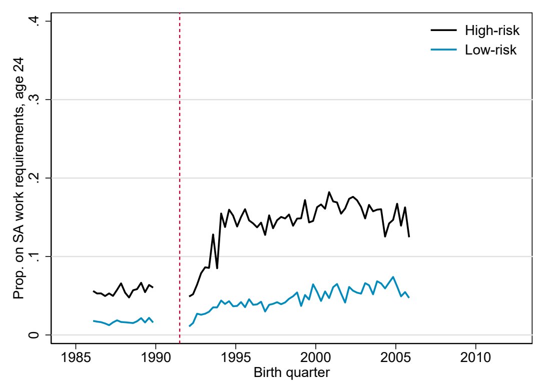
(b) Age 20



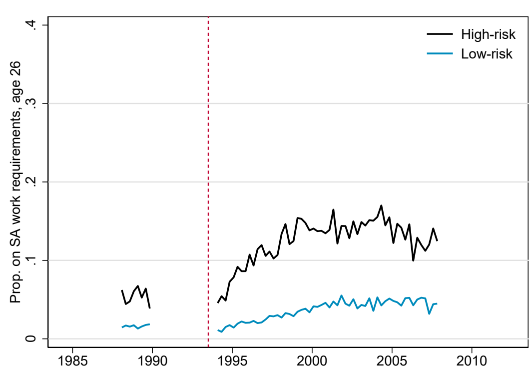
(c) Age 22



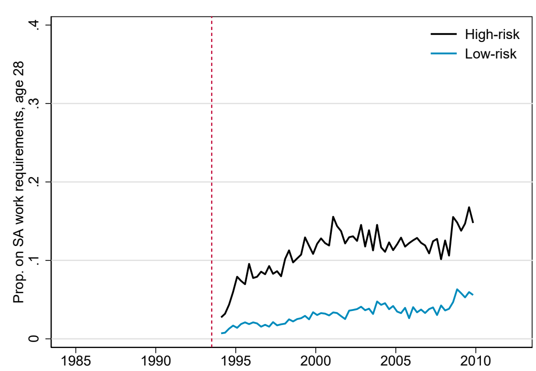
(d) Age 24



(e) Age 26



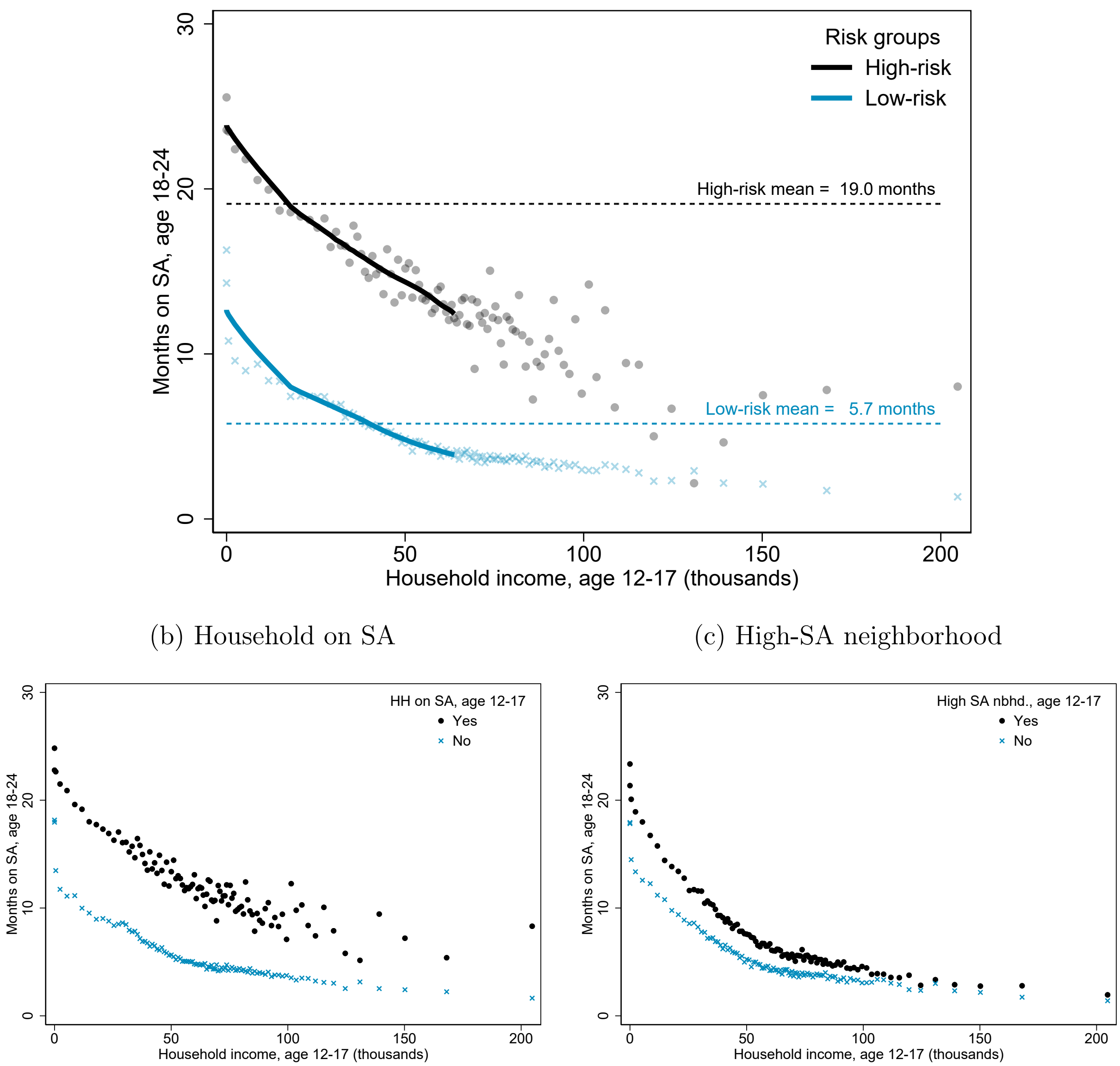
(f) Age 28



Notes: Breaks in data series due to missing register data.

Figure D.2: Duration of SA participation, age 18–24, by household income centile and risk group or risk characteristic

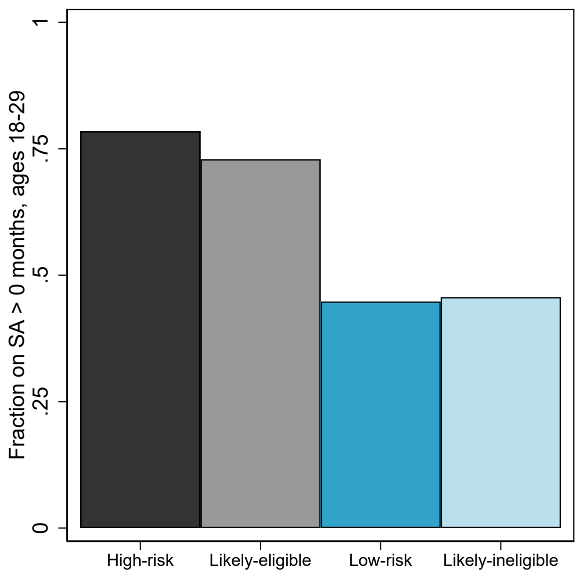
(a) Risk groups



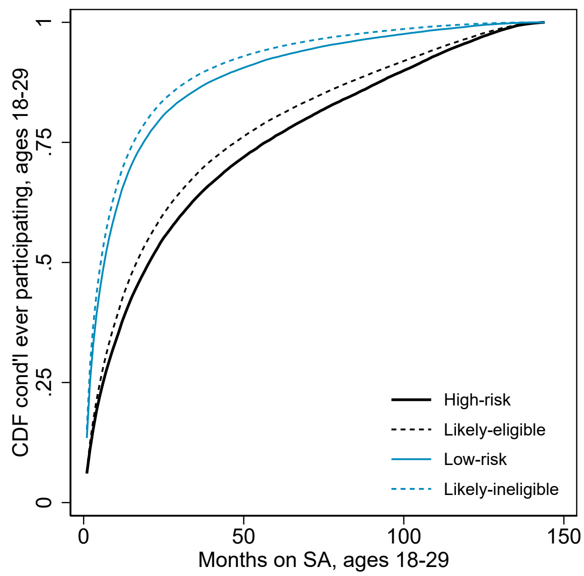
Notes: Scatter plots represent months of SA participation by household income centile bins (based on the household income distribution of the base sample before removing the top half of the distribution but after removing households with mothers with more than 12 years of schooling) and risk groups or risk characteristics defined in the main text. Solid lines are lowess fits below the 50th percentile. This figure uses all individuals whose mothers have no more than a high school education; Panel (a) further constrains the sample to individuals that fall in the high- and low-risk groups.

Figure D.3: Risk groups match SA take-up rates of best possible proxy of eligibility groups

(a) Extensive Margin

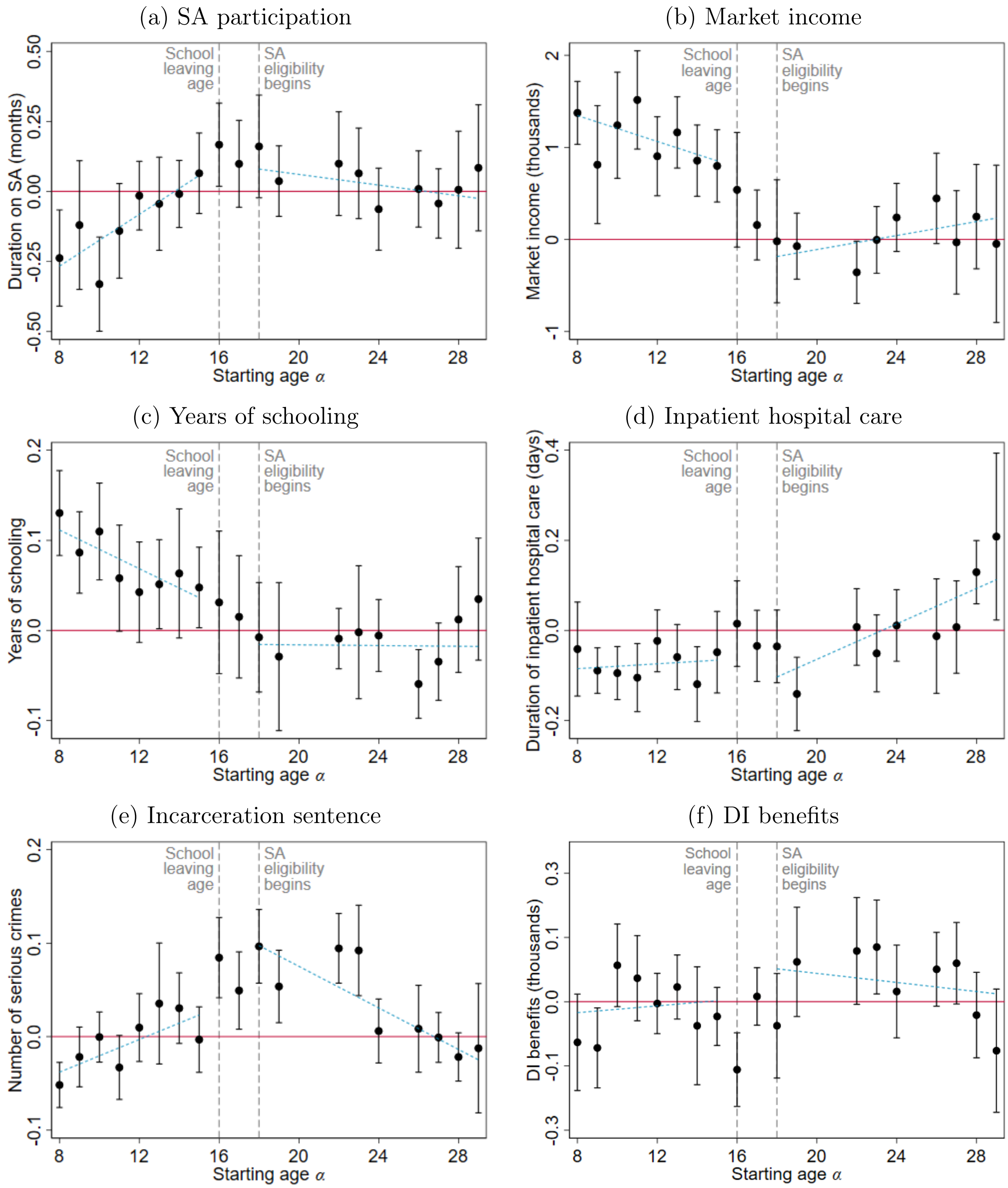


(b) Intensive Margin



Notes: This figure compares SA participation rates between risk groups and proxies of eligibility groups, "likely-eligible" and "likely-ineligible." Data constrained to individuals with starting age at least 18 to ensure reforms do not affect criteria used to construct likely-eligibility groups. Risk groups are defined by background characteristics measured between ages 12–17; see main text for details. The likely-eligible group is defined by the following criteria measured at age 17: (i) liquid assets (bank deposits and market value of bonds and shares) below \$2,000, (ii) market income below \$7,000 (the "basic amount"), (iii) not enrolled in school, and (iv) not a member of a UI fund. The likely-ineligible group is defined as the complement. Criterion (i) approximates the SA liquid asset cap, which exempts assets dedicated toward certain (unobserved) needs. Criteria (ii)–(iv) proxy experience of a social event and lack of self-sufficiency.

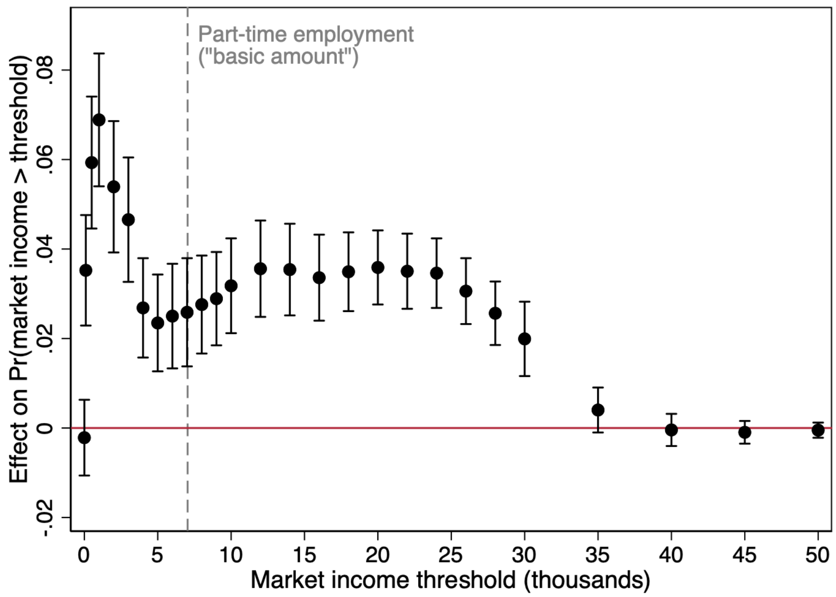
Figure D.4: Starting age heterogeneity in welfare-to-work's effects on different outcomes



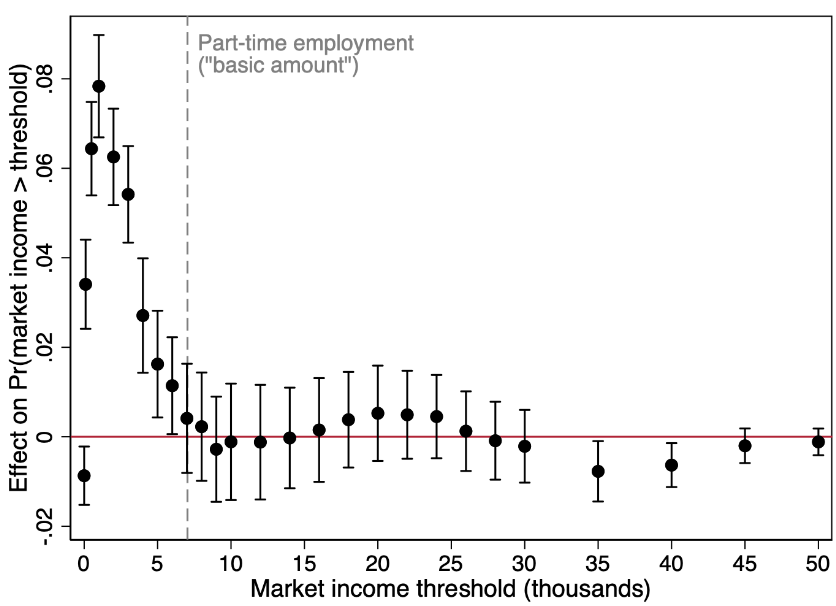
Notes: This plots estimates of β_α from (2.5) for different outcomes. Dashed blue lines are the best weighted least squares fits of β_α on starting age α, weighted by the inverse squared standard error of the estimates, between $\alpha \in [8, 15]$ and $\alpha \in [18, 29]$. Vertical bars are 90% confidence intervals based on robust standard errors two-way clustered over birth quarters and municipalities.

Figure D.5: Effect of welfare-to-work on employment, by different market income thresholds and starting age group

(a) $8 \leq \alpha \leq 15$

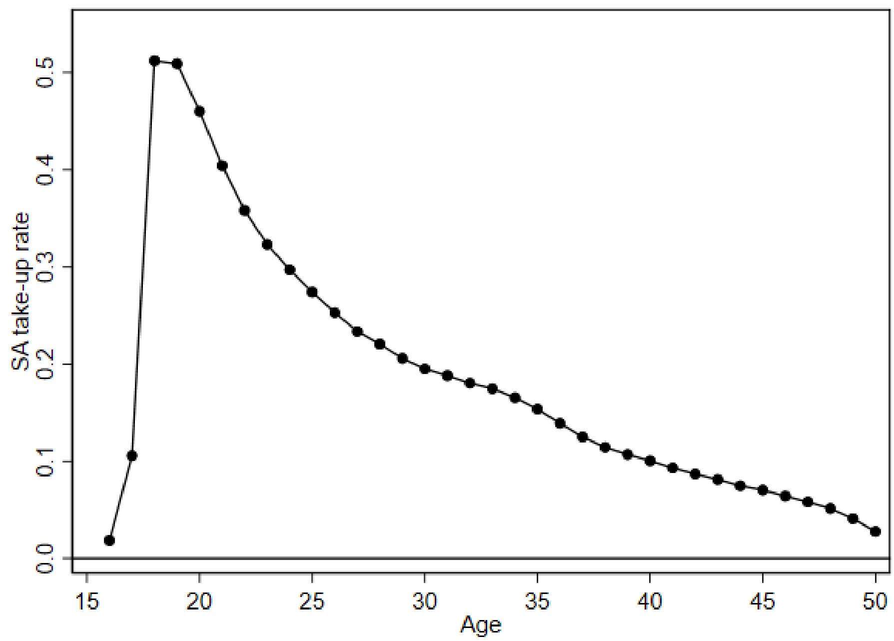


(b) $16 \leq \alpha \leq 18$



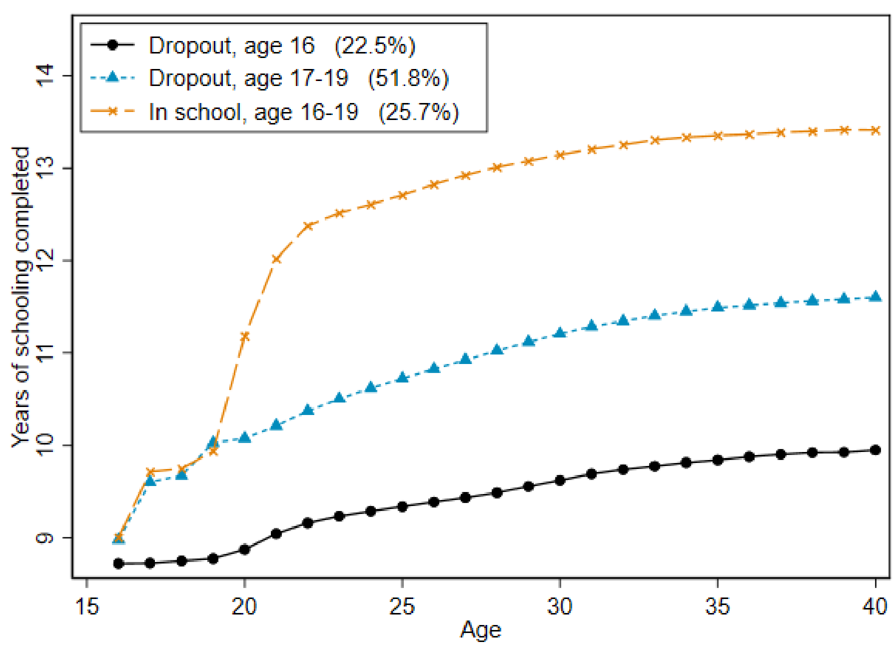
Notes: This figure plots estimates of β_α from Equation (2.5), aggregated by starting age group, where the outcome variables are indicators of market income lying above a given threshold.

Figure D.6: SA take-up rate over the life-cycle



Notes: Based on high-risk group.

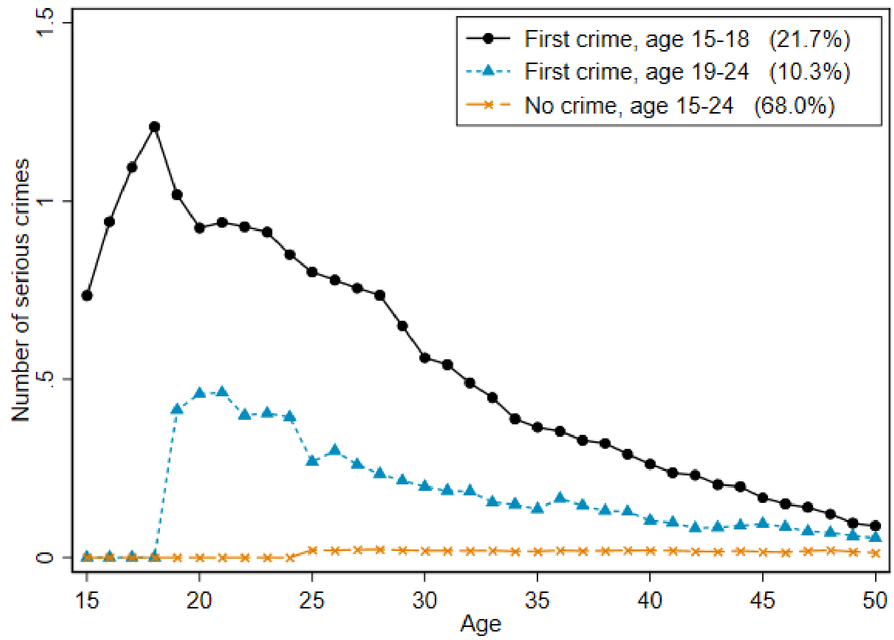
Figure D.7: Years of schooling completed over the life-cycle,
by high school dropout status between ages 16–19



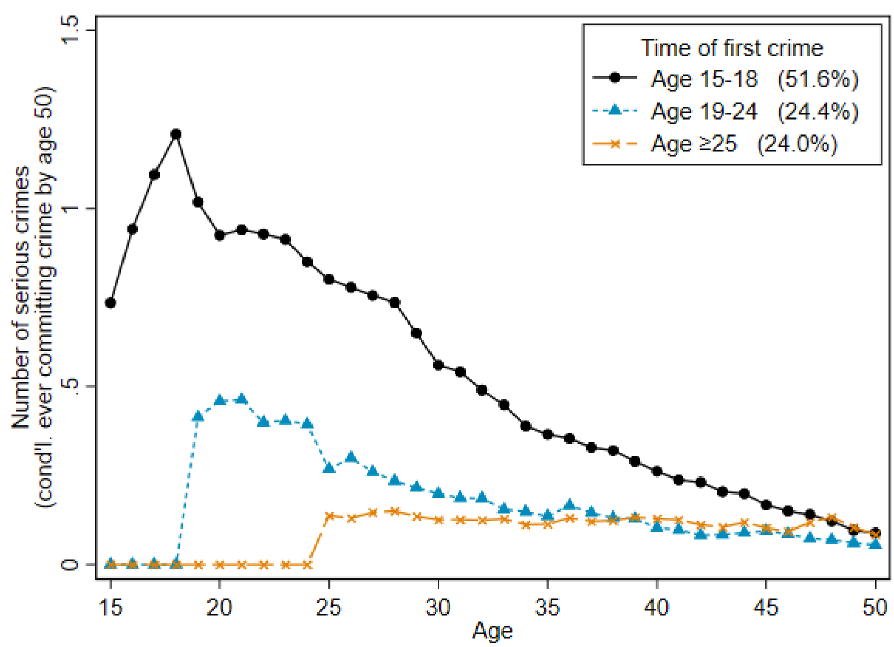
Notes: Based on high-risk group.

Figure D.8: Committed crime over the life-cycle,
by timing of first crime between ages 15–24

(a) All individuals



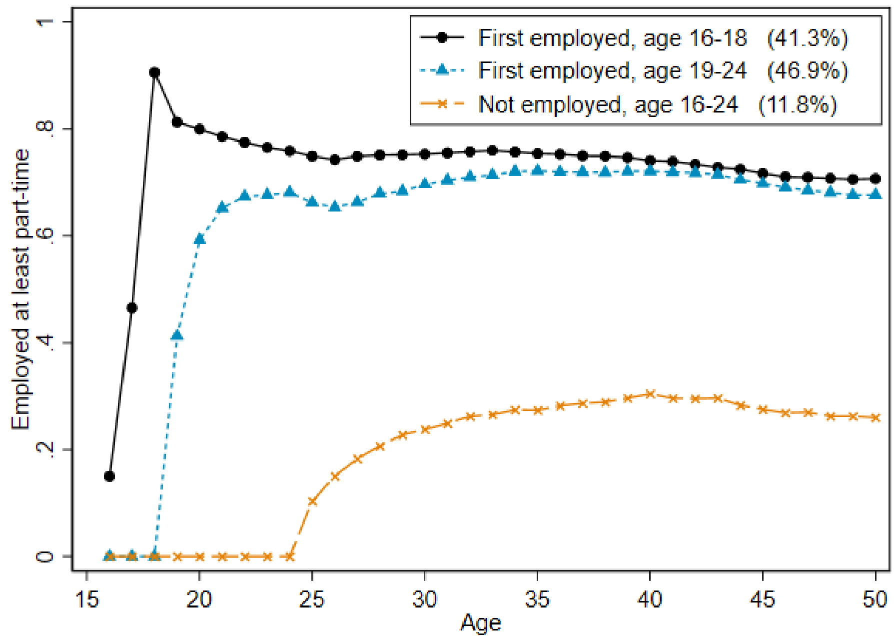
(b) Conditional on ever committing crime by age 50



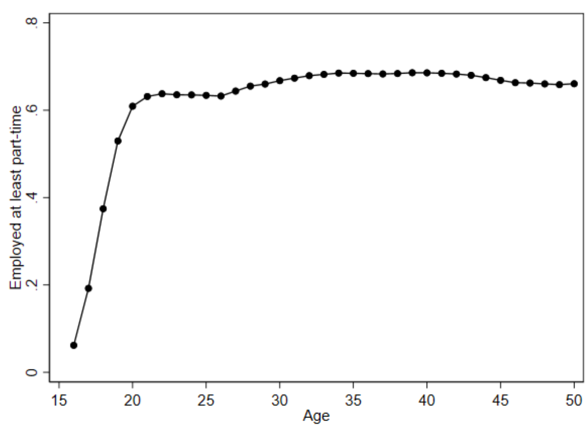
Notes: Based on high-risk group.

Figure D.9: Employed at least part-time over the life-cycle

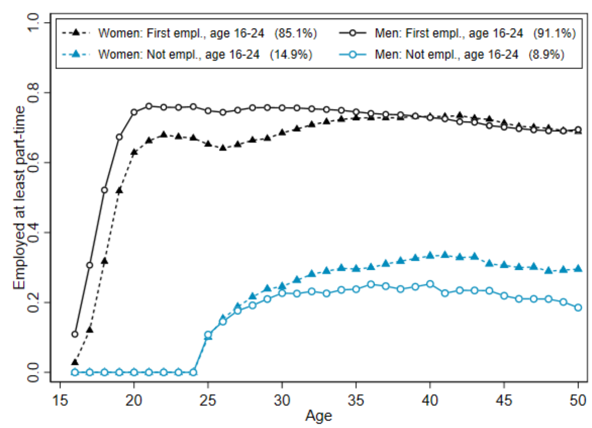
(a) By part-time employment status between ages 16–24



(b) Pooled



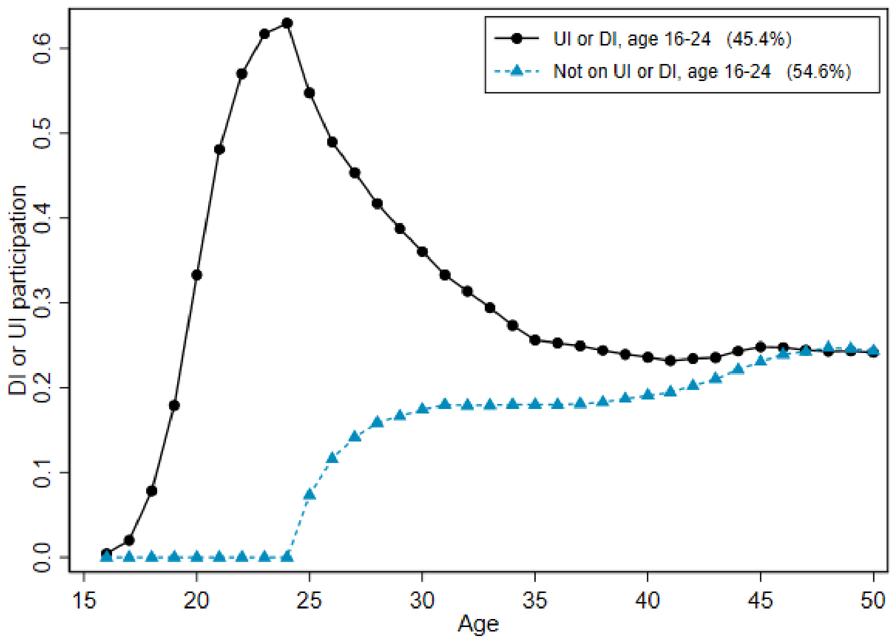
(c) By gender and part-time employment status between ages 16–24



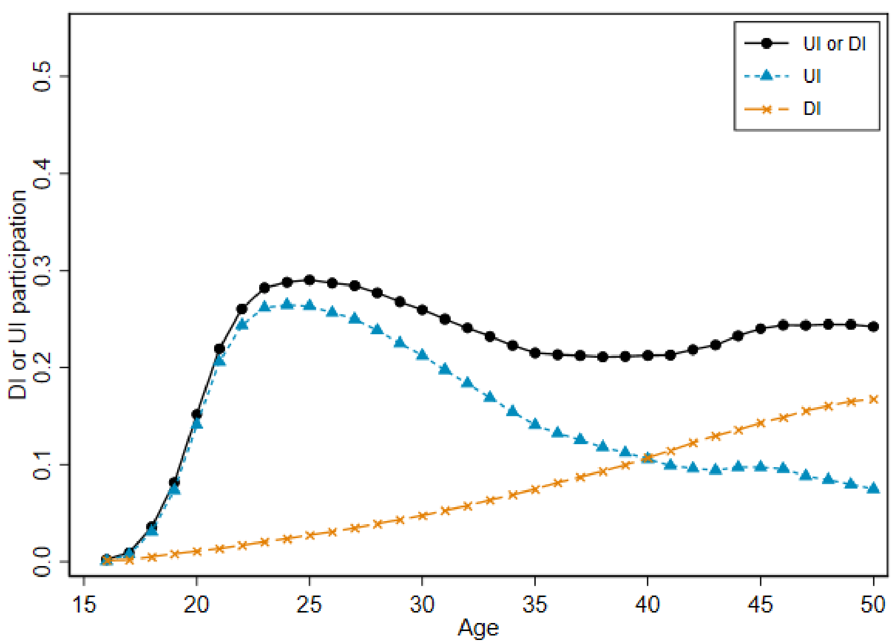
Notes: Based on high-risk group.

Figure D.10: Other social program take-up over the life-cycle

(a) UI or DI take-up, by ever taking up UI or DI between ages 16–24

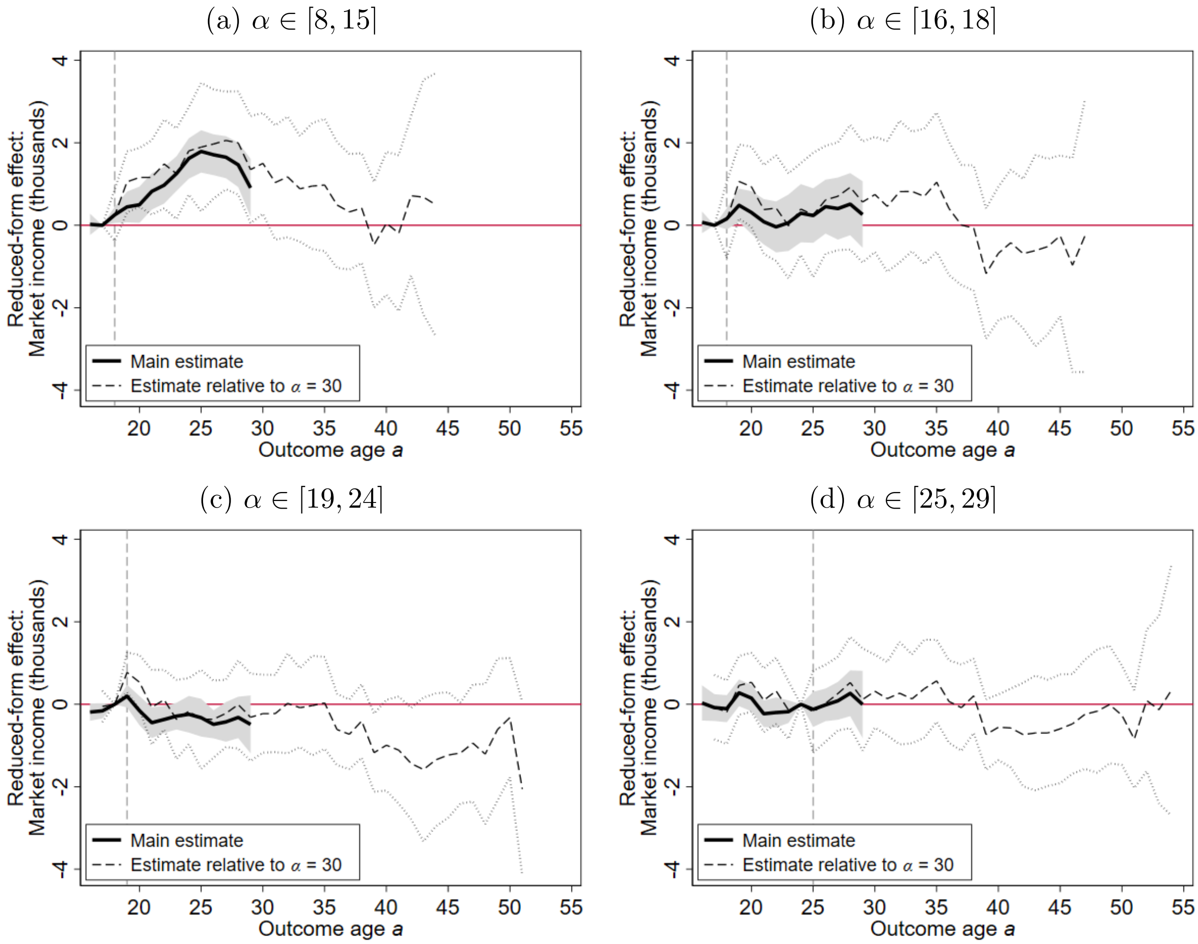


(b) Distinguishing DI and UI take-up, pooled



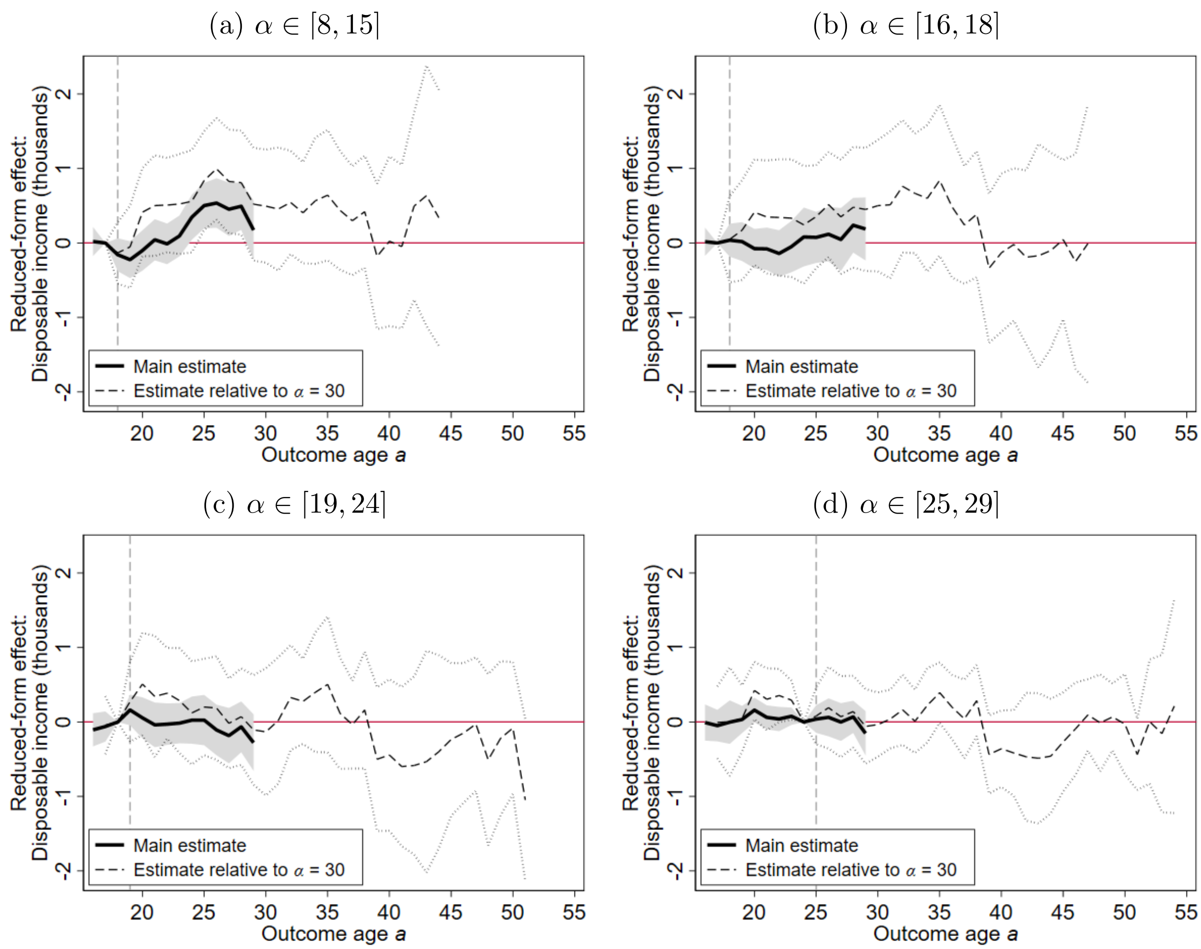
Notes: Based on high-risk group.

Figure D.11: Effects on market income over the life-cycle, by starting age group



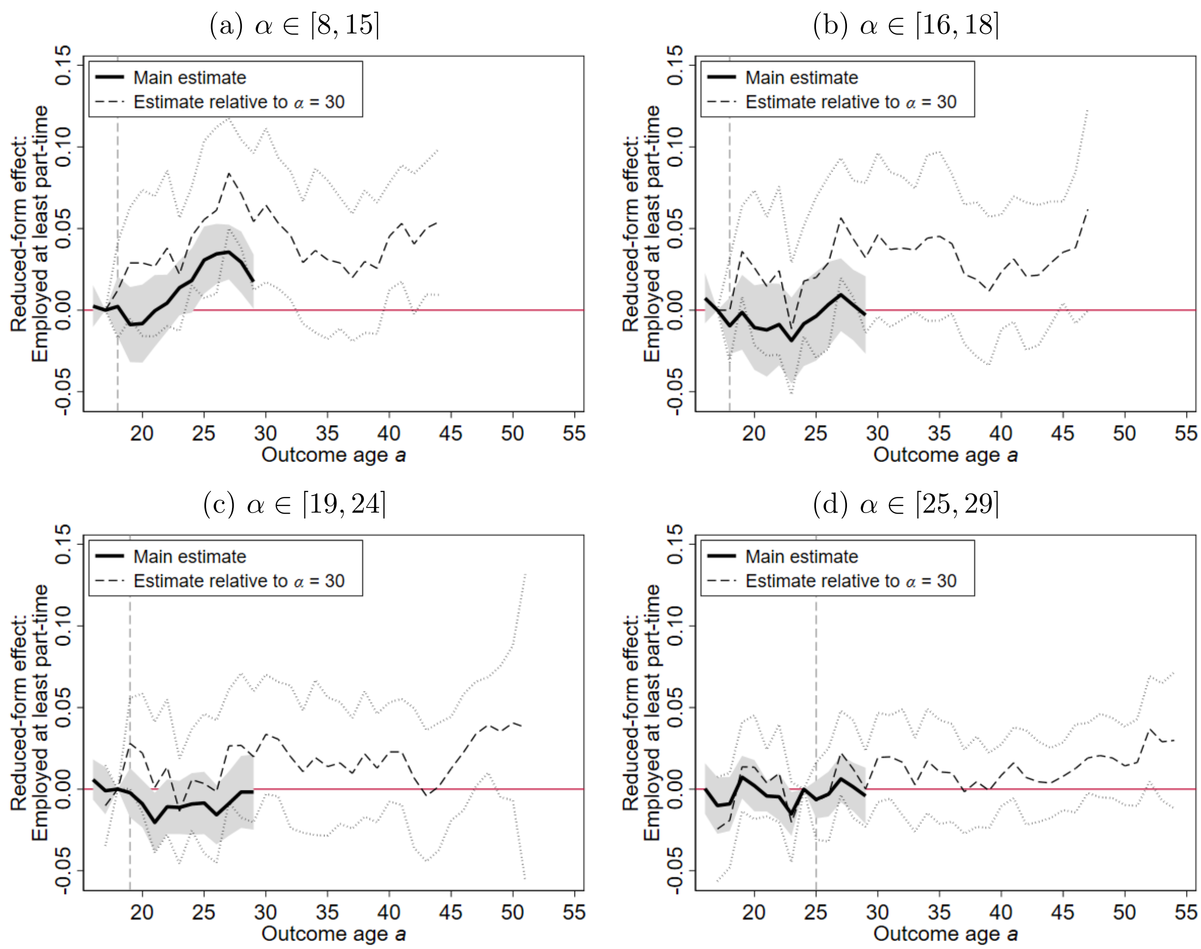
Notes: This figure plots estimates of $\beta_{\alpha,a}$ and $\beta^{30}_{\alpha,a}$ from (2.4), averaged by starting age groups. Solid lines and shaded regions correspond to estimates of $\beta_{\alpha,a}$ and their 95% confidence intervals, while the black and gray dashed lines correspond to estimates of $\beta^{30}_{\alpha,a}$ and their 95% confidence intervals.

Figure D.12: Effects on disposable income over the life-cycle, by starting age group



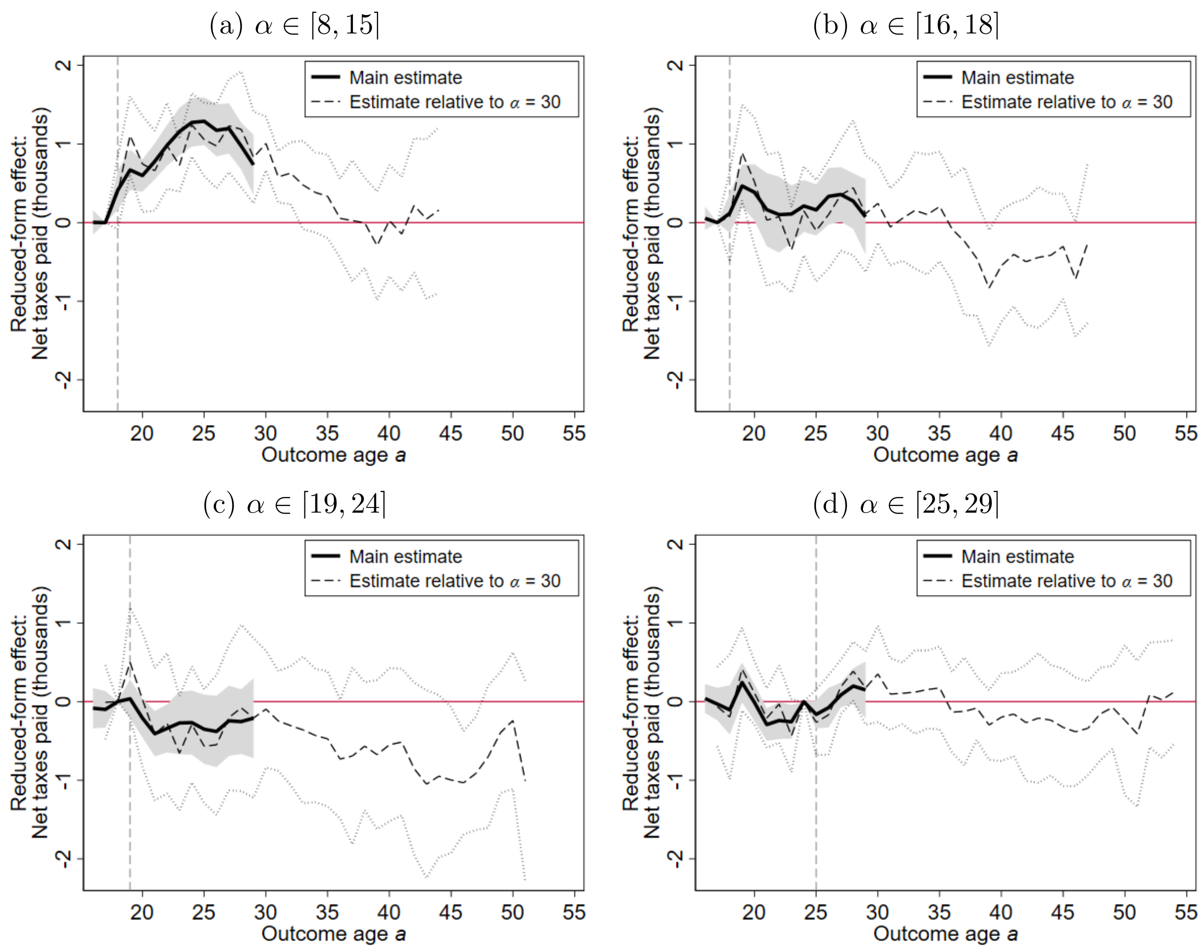
Notes: This figure plots estimates of $\beta_{\alpha,a}$ and $\beta^{30}_{\alpha,a}$ from (2.4), averaged by starting age groups. Solid lines and shaded regions correspond to estimates of $\beta_{\alpha,a}$ and their 95% confidence intervals, while the black and gray dashed lines correspond to estimates of $\beta^{30}_{\alpha,a}$ and their 95% confidence intervals.

Figure D.13: Effects on part-time employment over the life-cycle, by starting age group



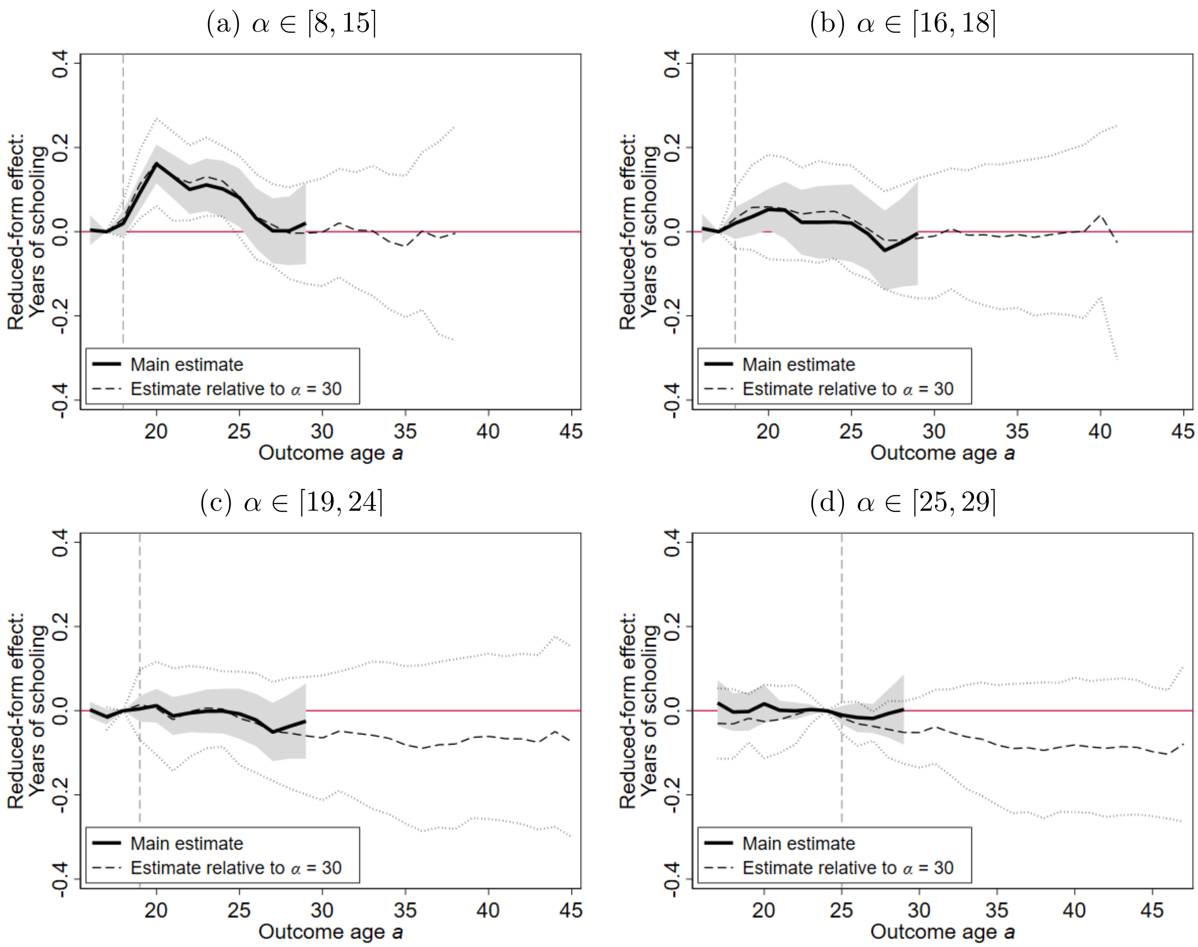
Notes: This figure plots estimates of $\beta_{\alpha,a}$ and $\beta^{30}_{\alpha,a}$ from (2.4), averaged by starting age groups. Solid lines and shaded regions correspond to estimates of $\beta_{\alpha,a}$ and their 95% confidence intervals, while the black and gray dashed lines correspond to estimates of $\beta^{30}_{\alpha,a}$ and their 95% confidence intervals.

Figure D.14: Effects on net taxes paid over the life-cycle, by starting age group



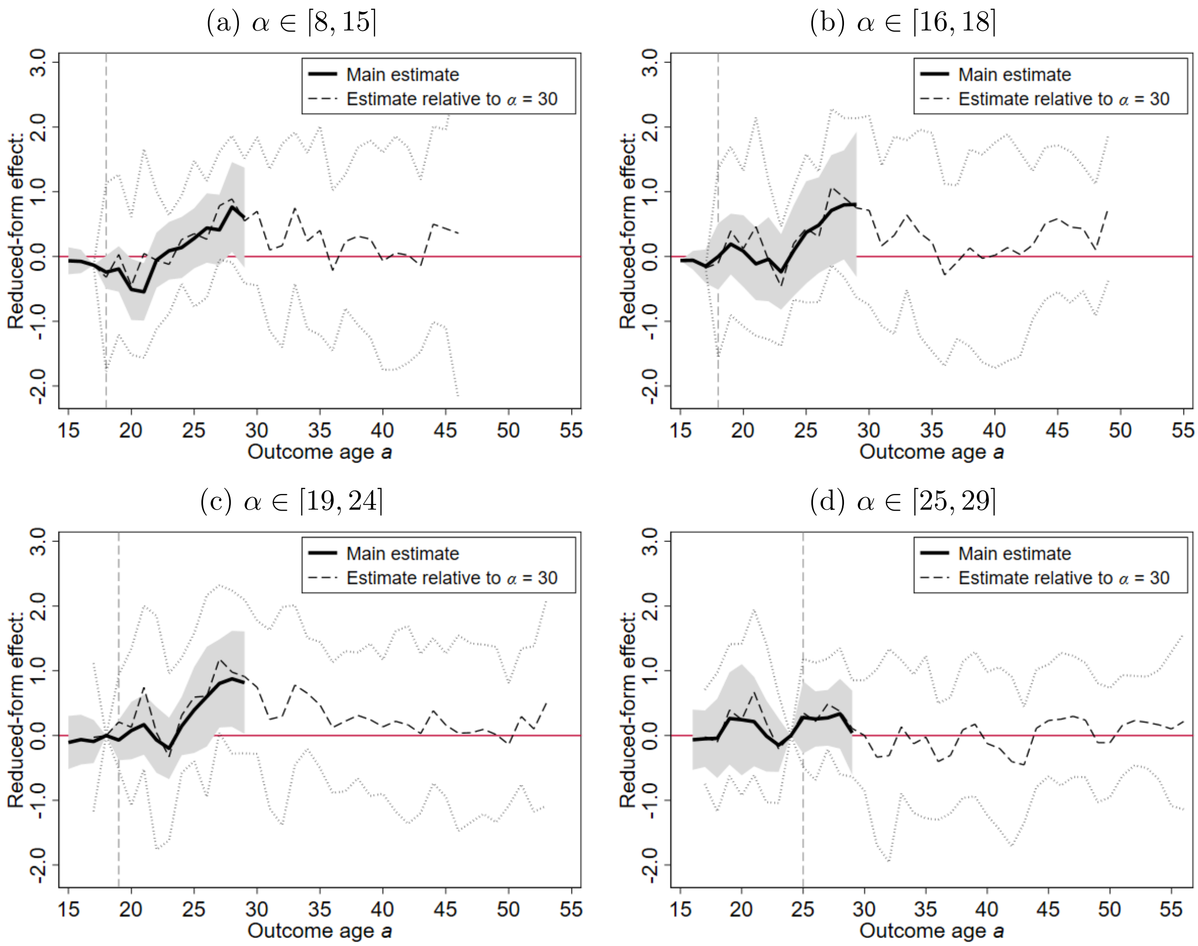
Notes: This figure plots estimates of $\beta_{\alpha,a}$ and $\beta^{30}_{\alpha,a}$ from (2.4), averaged by starting age groups. Solid lines and shaded regions correspond to estimates of $\beta_{\alpha,a}$ and their 95% confidence intervals, while the black and gray dashed lines correspond to estimates of $\beta^{30}_{\alpha,a}$ and their 95% confidence intervals.

Figure D.15: Effects on years of schooling over the life-cycle, by starting age group



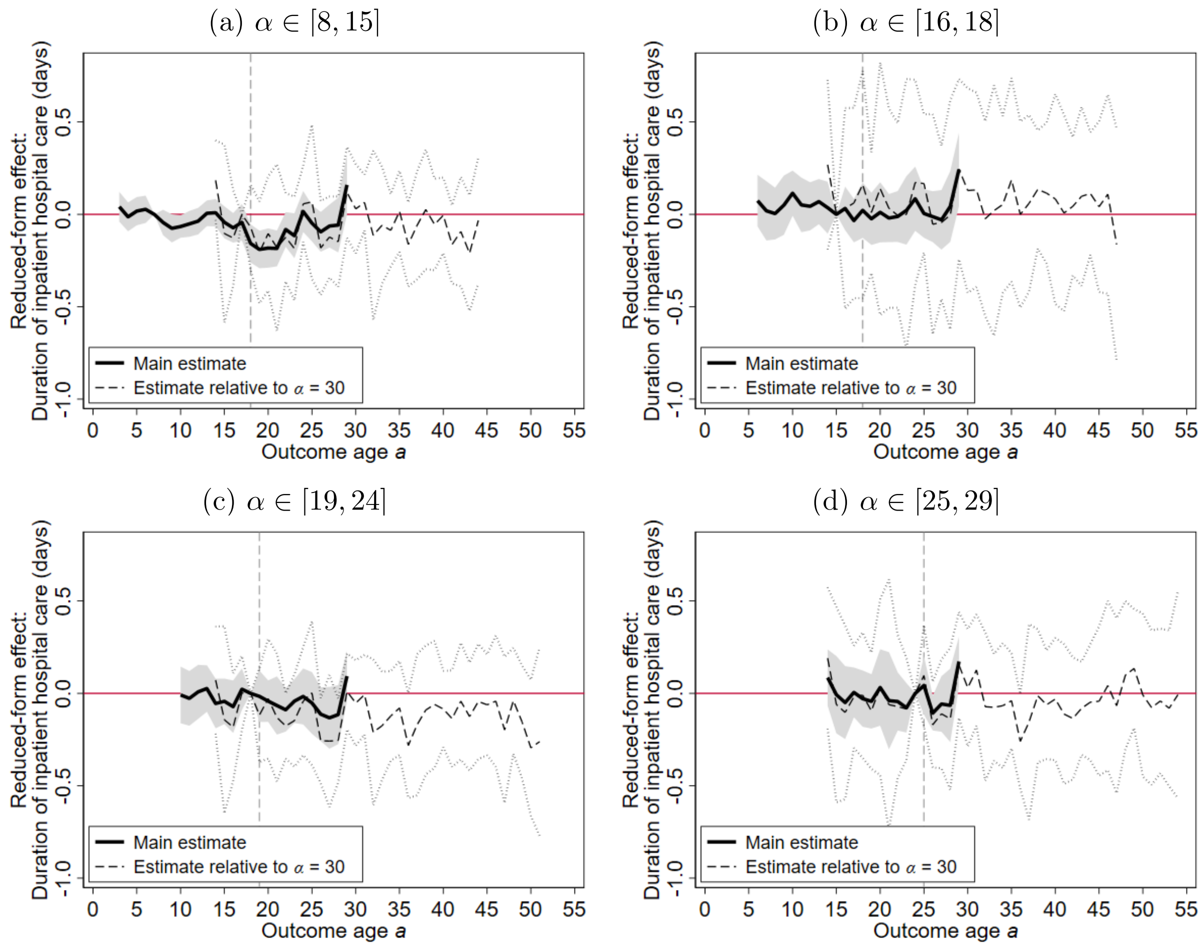
Notes: This figure plots estimates of $\beta_{\alpha,a}$ and $\beta^{30}_{\alpha,a}$ from (2.4), averaged by starting age groups. Estimates of $\beta^{30}_{\alpha,a}$ are relative to starting age $\alpha = 29$ (not $\alpha = 30$) due to data constraints. Solid lines and shaded regions correspond to estimates of $\beta_{\alpha,a}$ and their 95% confidence intervals, while the black and gray dashed lines correspond to estimates of $\beta^{29}_{\alpha,a}$ and their 95% confidence intervals.

Figure D.16: Effects on incarceration sentence over the life-cycle, by starting age group



Notes: This figure plots estimates of $\beta_{\alpha,a}$ and $\beta^{30}_{\alpha,a}$ from (2.4), averaged by starting age groups. Solid lines and shaded regions correspond to estimates of $\beta_{\alpha,a}$ and their 95% confidence intervals, while the black and gray dashed lines correspond to estimates of $\beta^{30}_{\alpha,a}$ and their 95% confidence intervals.

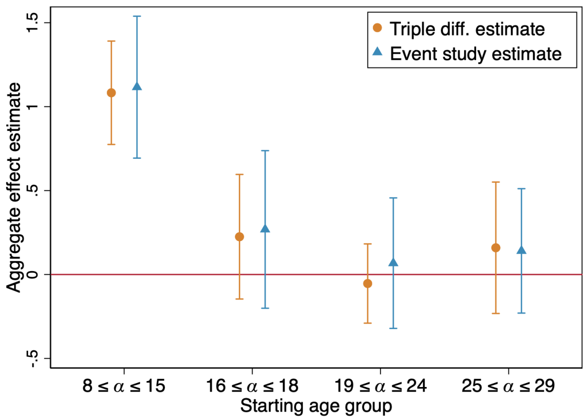
Figure D.17: Effects on inpatient hospital care over the life-cycle, by starting age group



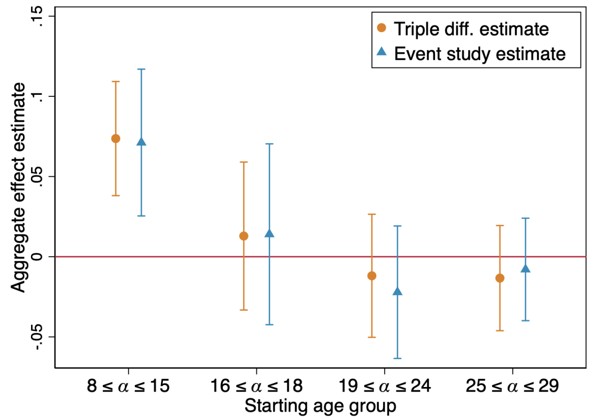
Notes: This figure plots estimates of $\beta_{\alpha,a}$ and $\beta^{30}_{\alpha,a}$ from (2.4), averaged by starting age groups. Solid lines and shaded regions correspond to estimates of $\beta_{\alpha,a}$ and their 95% confidence intervals, while the black and gray dashed lines correspond to estimates of $\beta^{30}_{\alpha,a}$ and their 95% confidence intervals.

Figure D.18: Event-study estimates averaged over outcome ages
match the triple difference estimates

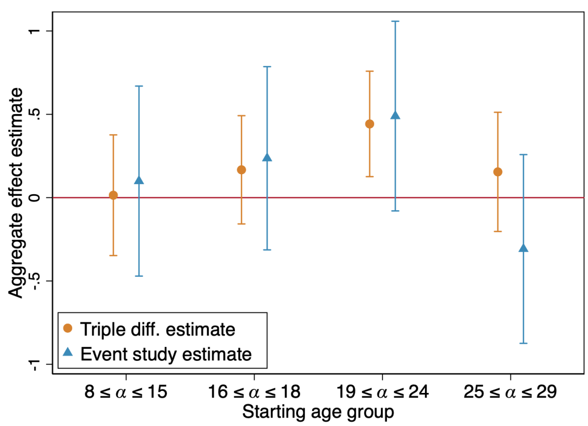
(a) Market income (thousands)



(b) Years of schooling

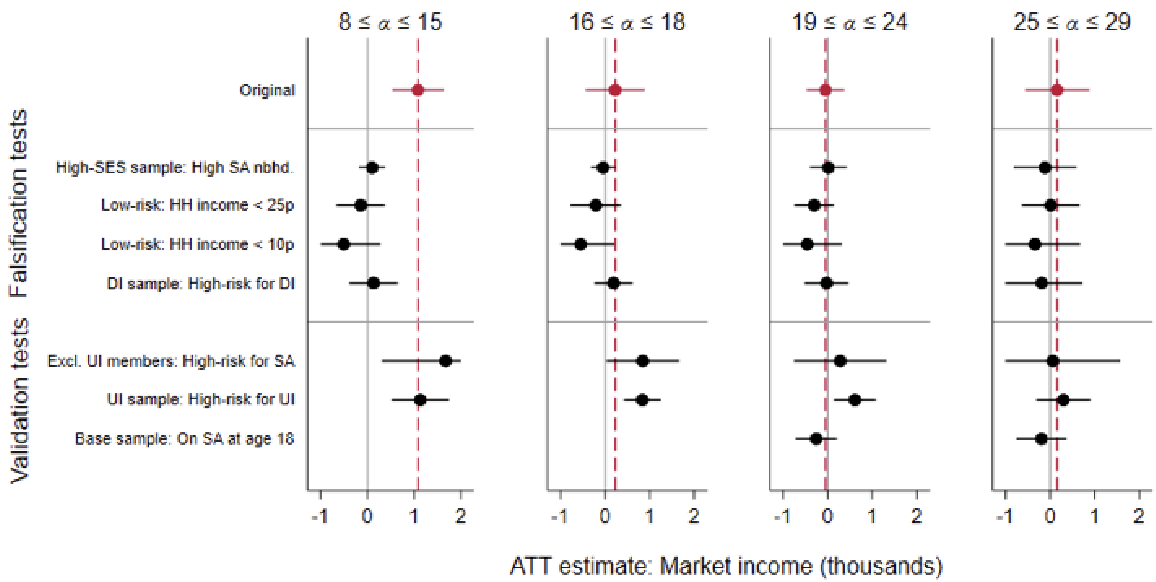


(c) Incarceration sentence (days)



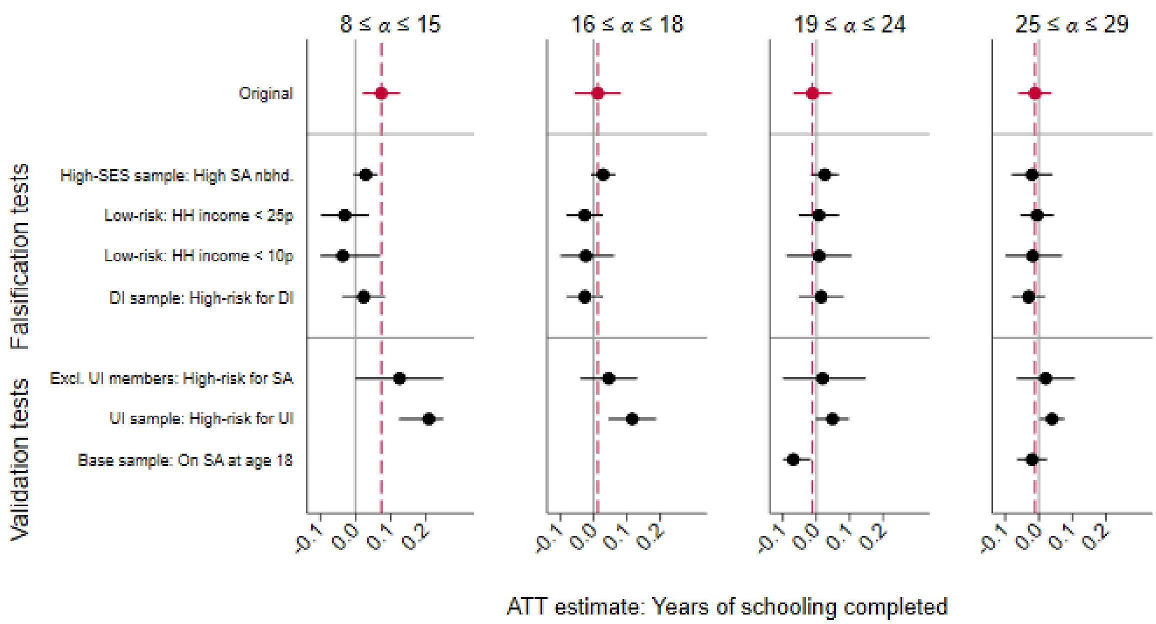
Notes: This figure compares the triple difference estimates of β_α from Equation (2.5) against the event-study estimates of $\beta_{\alpha,a}$ from Equation (2.4) averaged over the appropriate outcome ages $a \geq \alpha$, aggregated by starting age group for different outcomes. Vertical lines are 90% confidence intervals based on two-way clustered standard errors.

Figure D.19: Falsification and validation tests for market income estimates, by starting age group



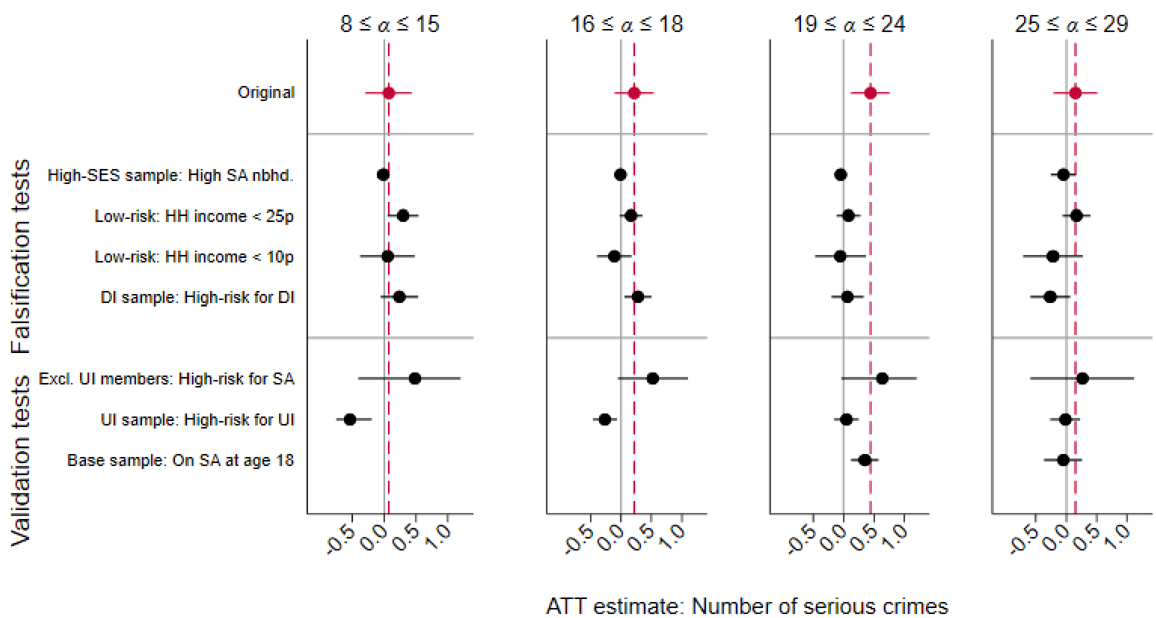
Notes: This figure plots estimates of β_α from variations of Equation (2.5), aggregated by starting age group. Row 1 plots main estimates from the original model (in red). Rows 2–5 are falsification tests and Rows 6–8 are validation tests (in black). Row 2 uses a high-socioeconomic status (SES) sample (defined by household income above median and mothers with more than 12 years of schooling) and defines treatment and control groups as individuals from high- and low-SA neighborhood blocks, respectively (see main text for more details). Row 3 uses low-risk sample and defines treatment and control groups as individuals whose household income (age 12–17) are in the first and second quartiles, respectively. Row 4 repeats the Row 3 except uses the first decile to define treatment and control groups. Rows 3–4 exclude household income and household assets as controls in the regression model to avoid overcontrolling for treatment status. Row 5 uses a subset of the base sample in which the treatment group is households that participated in disability insurance (DI) and not SA (ages 12–17) and the control group is households that participated in neither DI nor SA. Row 6 restricts the main sample to individuals whose households are not unemployment insurance (UI) fund members (which is defined as at least 50% of working-age cohabiting adults in the household between ages 12–17 are UI fund members). Row 7 uses the base sample and defines the treatment group as UI fund member households that received UI benefits but not SA benefits and the same control group as Row 6. Row 8 uses the base sample and defines treatment and control groups as whether an individual participated in SA at age 18; estimates are missing for age groups $8 \leq \alpha \leq 15$ and $16 \leq \alpha \leq 18$ due to endogeneity. Horizontal lines are 90% confidence intervals based on standard errors two-way clustered by birth quarter and municipality of residence in the age prior to the reform. The vertical dashed line signifies the magnitude of Row 1's point estimate.

Figure D.20: Falsification and validation tests for years of schooling estimates, by starting age group



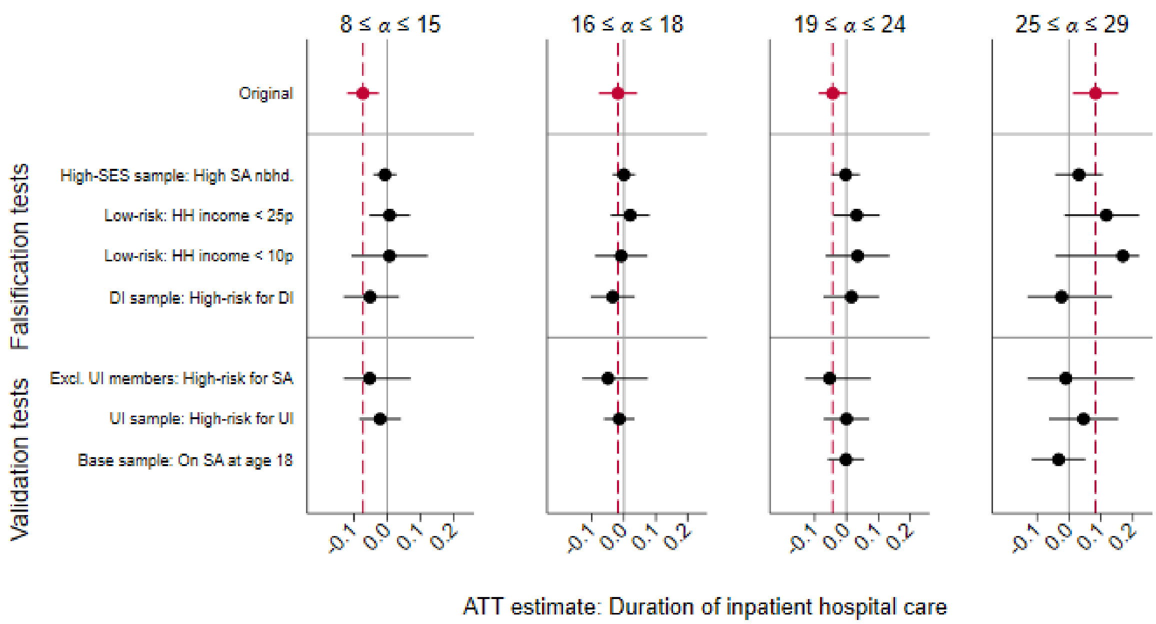
Notes: See notes to Figure D.19.

Figure D.21: Falsification and validation tests for incarceration sentence estimates, by starting age group



Notes: See notes to Figure D.19.

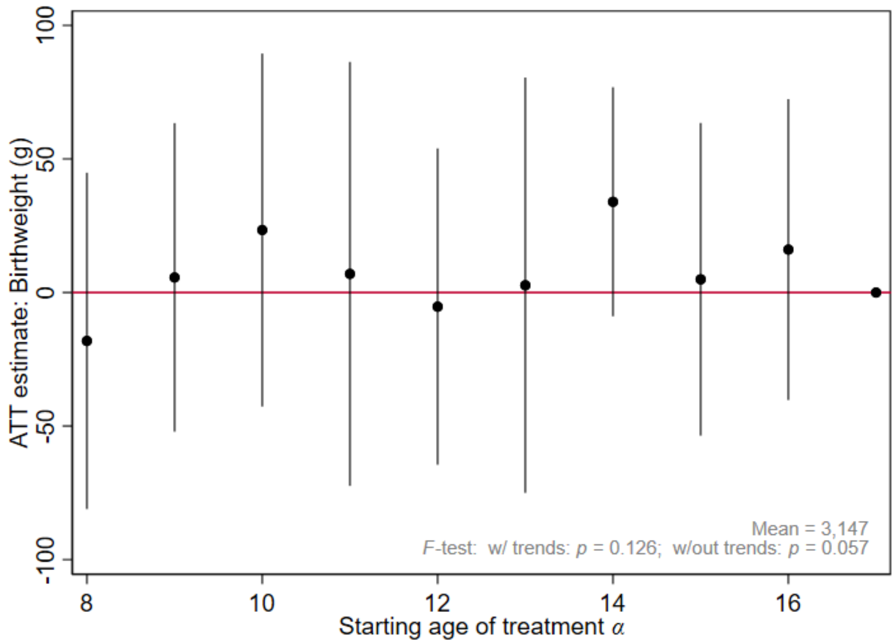
Figure D.22: Falsification and validation tests for inpatient hospital care estimates, by starting age group



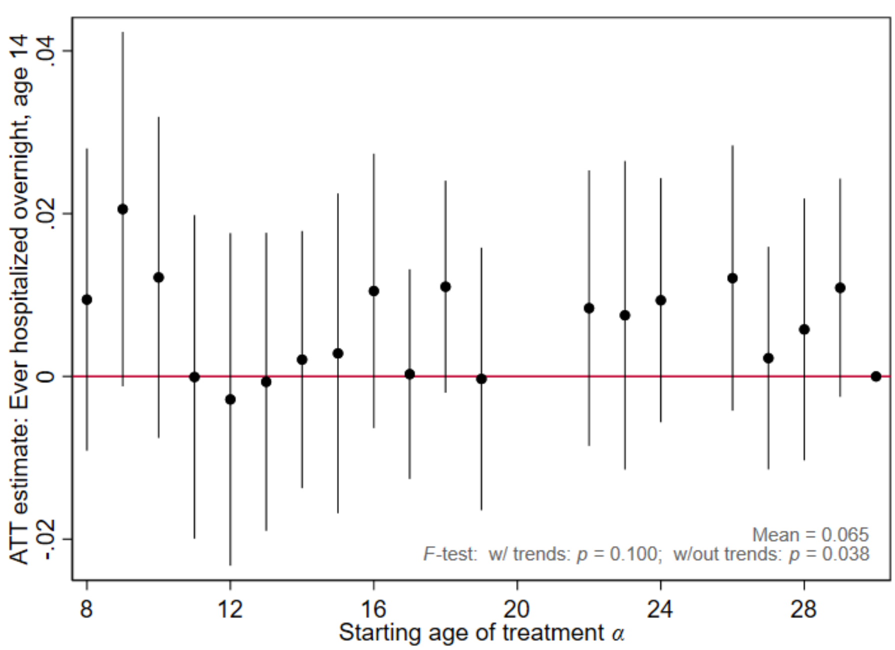
Notes: See notes to Figure D.19.

Figure D.23: Falsification tests using difference-in-difference event study on pre-treatment outcomes

(a) Birthweight

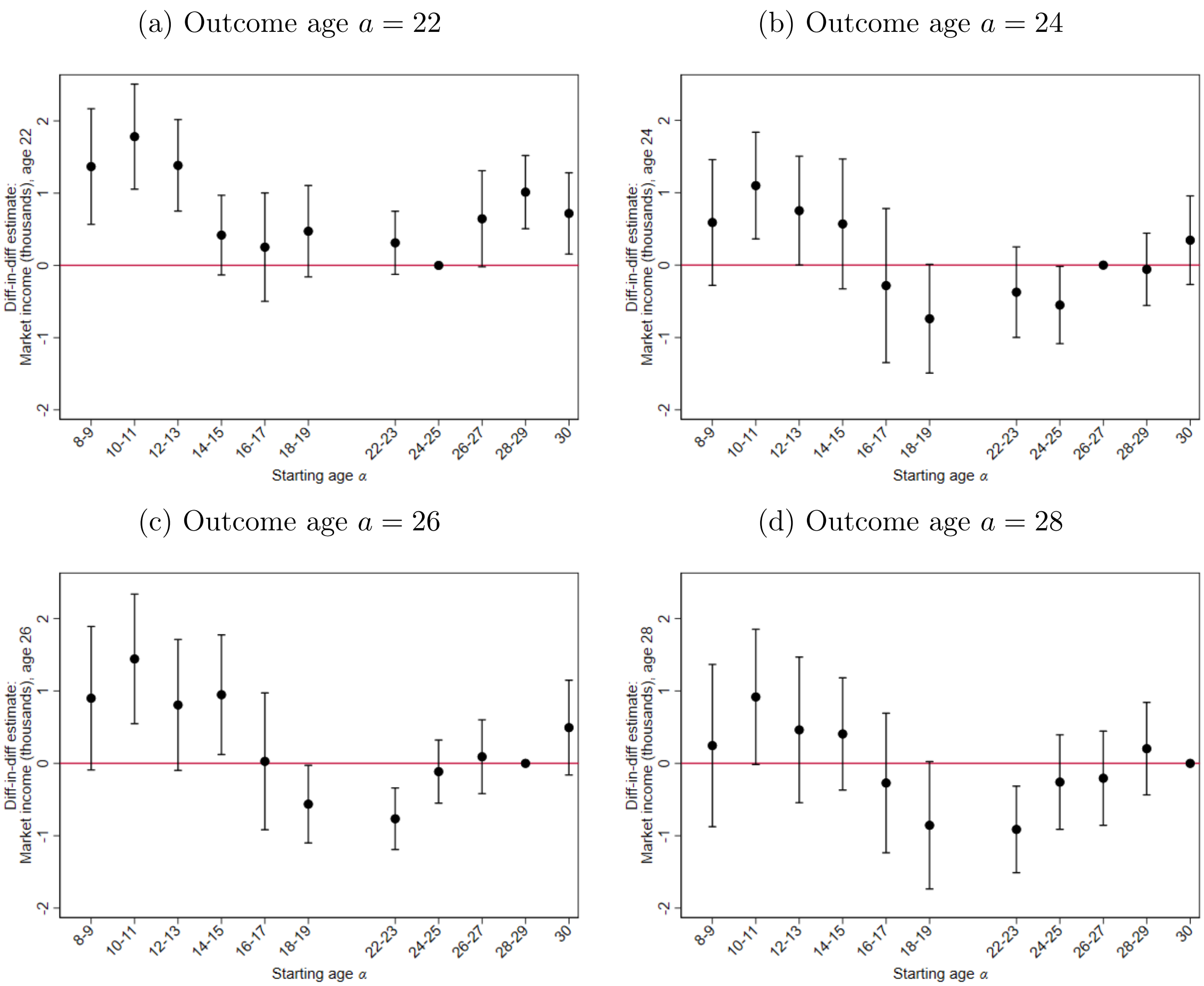


(b) Inpatient hospital care at age 14



Notes: These figures plot estimates from the difference-in-difference event-study model described in Appendix C.2.2. Outcome mean and F-tests are reported in the text below the series.

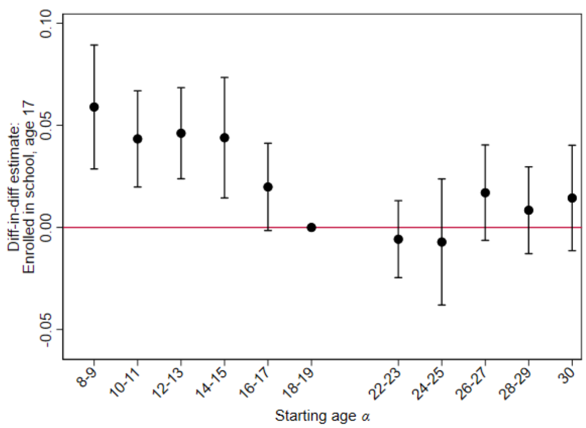
Figure D.24: Validation exercise using difference-in-difference event study on market income measured at specific outcome ages



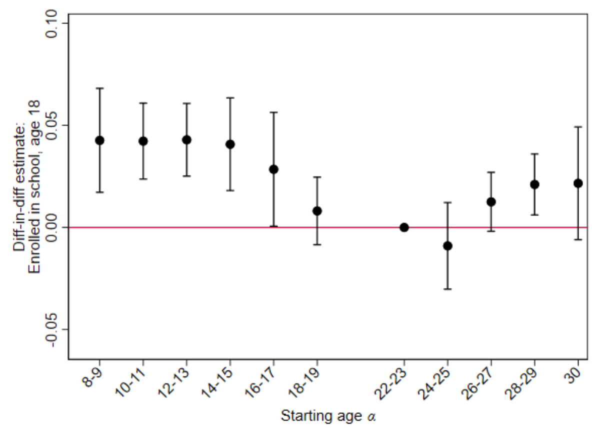
Notes: These figures plot estimates from the difference-in-difference event-study model described in Appendix C.2.2.

Figure D.25: Validation exercise using difference-in-difference event study on education outcomes measured at specific outcome ages

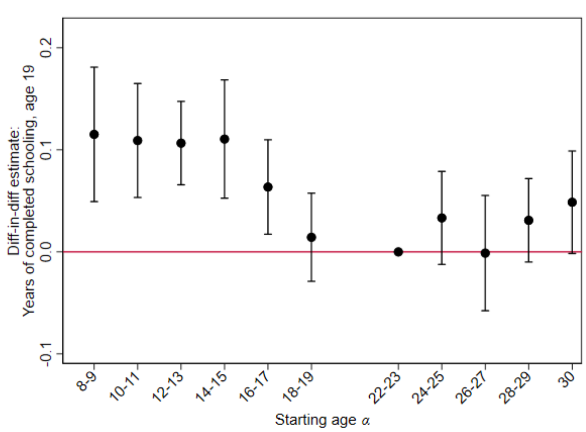
(a) Enrolled in school, outcome age $a = 17$



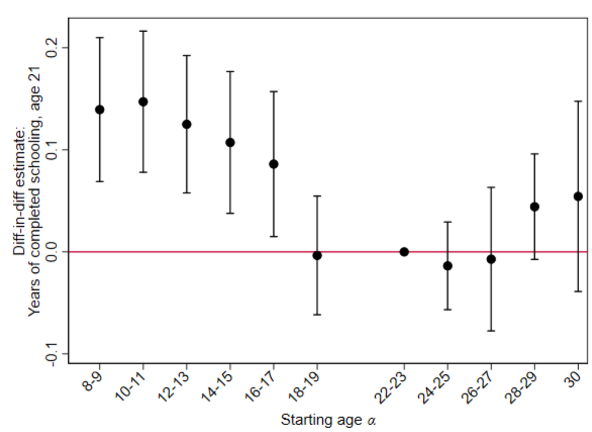
(b) Enrolled in school, outcome age $a = 18$



(c) Years of completed schooling, outcome age $a = 19$



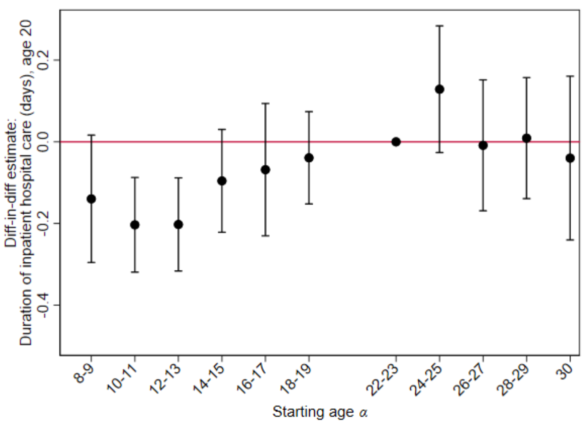
(d) Years of completed schooling, outcome age $a = 21$



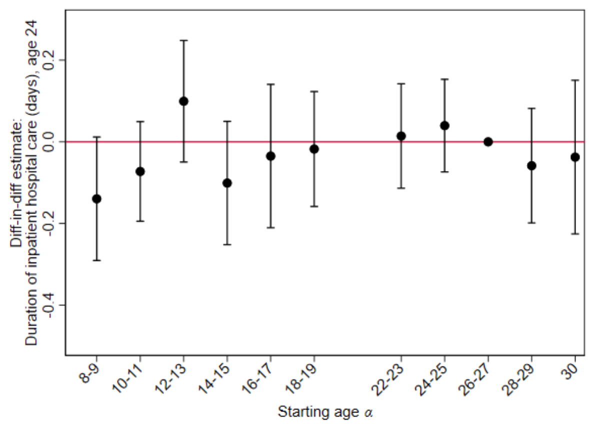
Notes: These figures plot estimates from the difference-in-difference event-study model described in Appendix C.2.2.

Figure D.26: Validation exercise using difference-in-difference event study on duration of inpatient hospital care measured at specific outcome ages

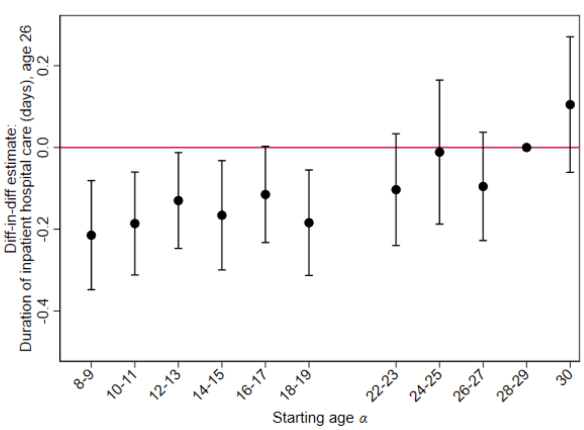
(a) Outcome age $a = 20$



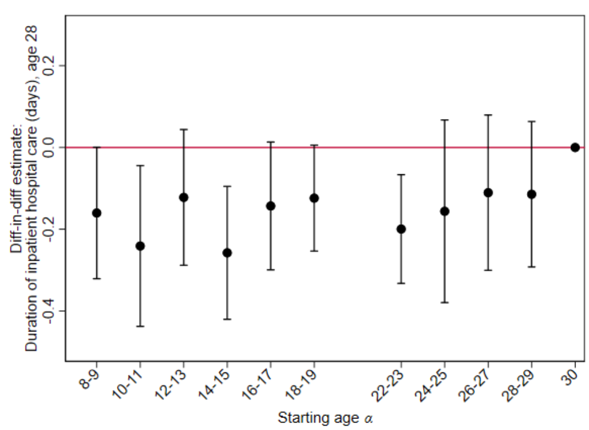
(b) Outcome age $a = 24$



(c) Outcome age $a = 26$

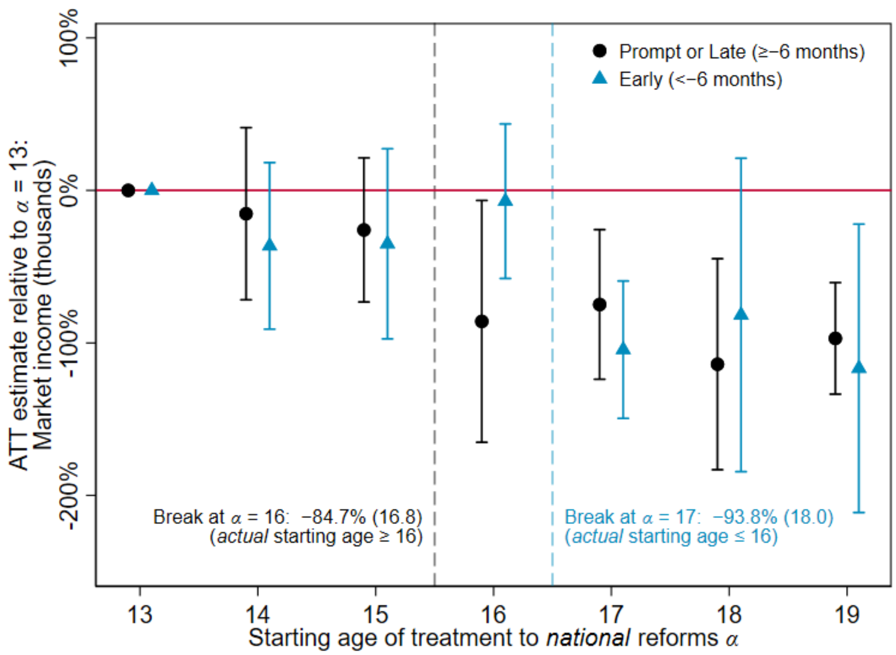


(d) Outcome age $a = 28$



Notes: These figures plot estimates from the difference-in-difference event-study model described in Appendix C.2.2.

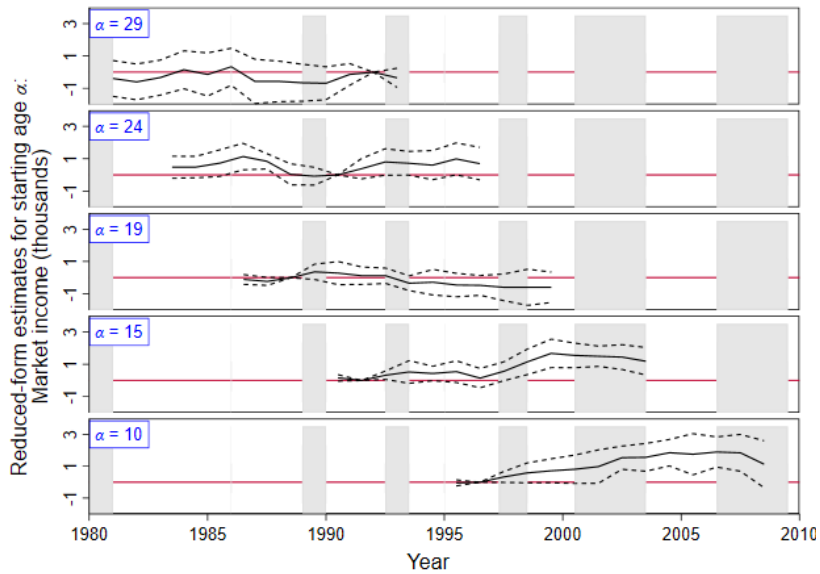
Figure D.27: Validating starting age heterogeneity in market income estimates based on the timing of municipality adoption of work requirements



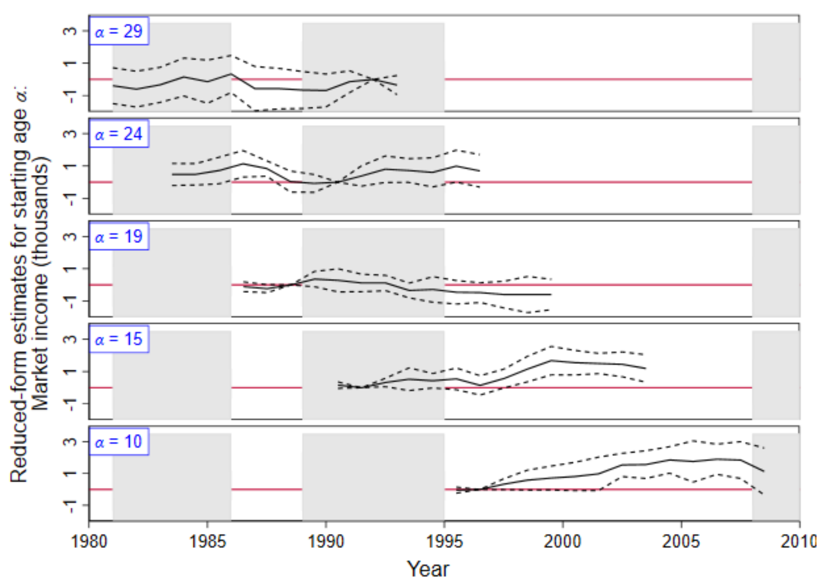
Notes: This figure plots estimates of the effect of the reforms on market income by starting age cohort α (determined by the timing of the *national* reforms) and by the timing of municipal adoption of work requirements: β_α^P and $\beta_\alpha^P + \beta_\alpha^E$ from the model described in Appendix C.2.3. Effects are normalized to zero at $\alpha = 13$. The text below the series describes the starting age α corresponding to the R^2–optimizing break point of a linear threshold model that fits estimates of β_α^P and $\beta_\alpha^P + \beta_\alpha^E$ on starting age α; the point estimate at the break point and standard error are reported in parentheses. Vertical lines are 95% confidence intervals based on the usual two-way clustered standard errors.

Figure D.28: Recessions do not explain starting age-outcome age heterogeneity in market income

(a) OECD recession indicators



(b) High unemployment indicators



Notes: This figure organizes starting age-outcome age effect estimates $\beta_{\alpha,a}$ of various starting age cohorts over calendar years. Shaded areas indicate different measures of business cycles: Panel (a) uses dates of recessions defined by the OECD, and Panel (b) uses dates where national unemployment rate exceeded 7%.

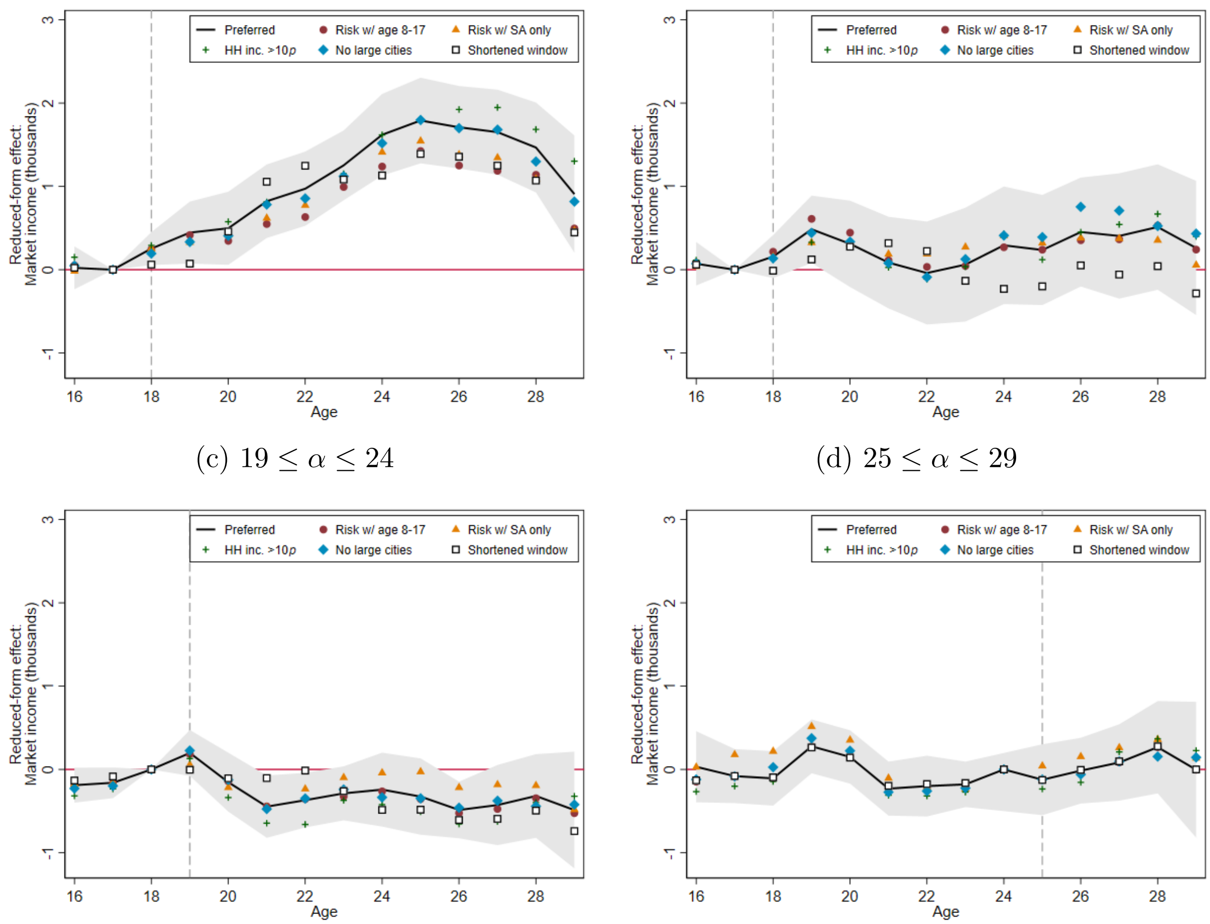
Correlations between $\beta_{\alpha,a}$ estimates and OECD recession indicators: 0.11 (unweighted), -0.00 (weighted by inverse squared standard error of $\beta_{\alpha,a}$ estimate).

Correlations between $\beta_{\alpha,a}$ estimates and unemployment rate: 0.12 (unweighted), -0.00 (weighted by inverse squared standard error of $\beta_{\alpha,a}$ estimate).

Figure D.29: Market income estimates are robust to alternative measurement choices and samples

(a) $8 \leq \alpha \leq 15$ (b) $16 \leq \alpha \leq 18$

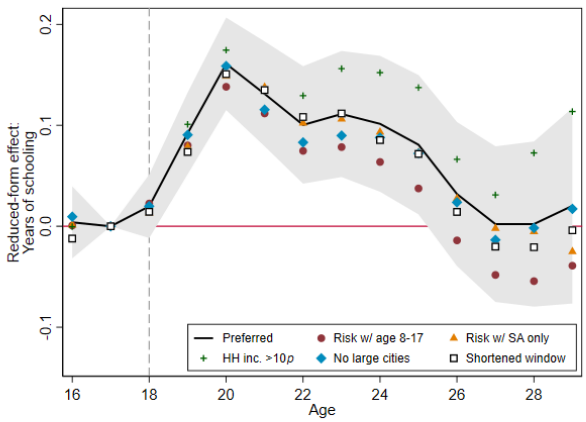
(c) $19 \leq \alpha \leq 24$ (d) $25 \leq \alpha \leq 29$



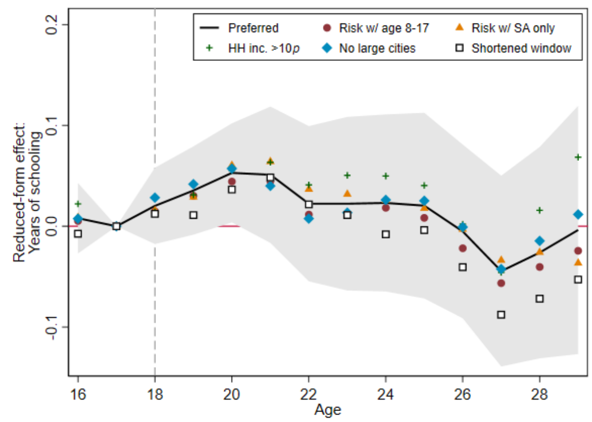
Notes: This figure plots starting age–outcome age event-study estimates $\beta_{\alpha,a}$ from specifications with alternative measurement choices and samples. The solid black line and shaded region represent my main preferred estimates and 95% confidence interval. Red circles are estimates where risk groups are defined by measuring household participation on SA and residence in high-SA neighborhood between ages 8–17 and before 1990. Orange triangles instead define risk groups using household participation on SA only (and thus uses my entire base sample). Green crosses excludes individuals whose household income between ages 12–17 are below the first decile. Blue diamonds exclude the four largest cities in Denmark (Copenhagen, Aarhus, Aalborg, Odense). White squares use a shortened window of starting ages at each given outcome age a as a comparison group, $a + 1 \leq \alpha \leq a + 3$ (instead of $\alpha \geq a + 1$).

Figure D.30: Years of schooling estimates are robust to alternative measurement choices and samples

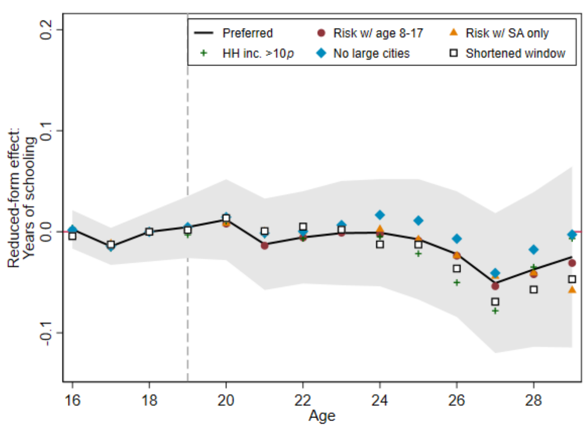
(a) $8 \leq \alpha \leq 15$



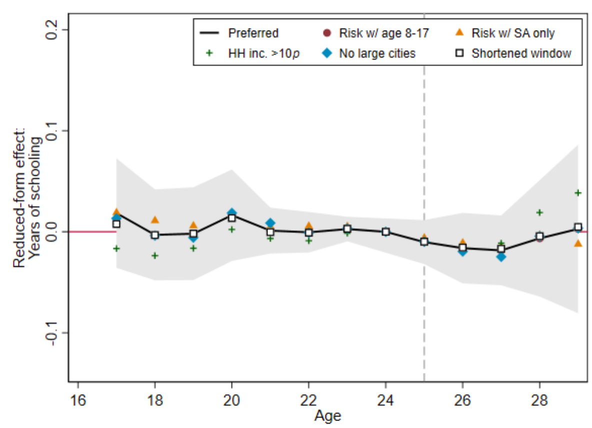
(b) $16 \leq \alpha \leq 18$



(c) $19 \leq \alpha \leq 24$



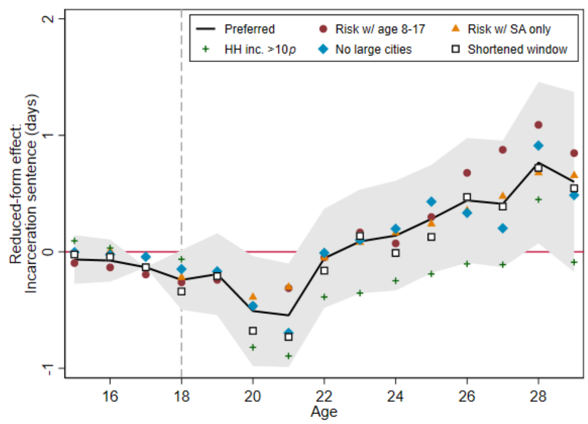
(d) $25 \leq \alpha \leq 29$



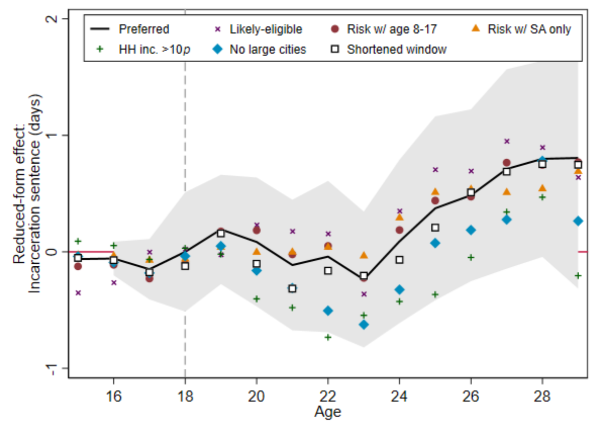
Notes: See notes to Figure D.29.

Figure D.31: Incarceration sentence estimates are robust to alternative measurement choices and samples

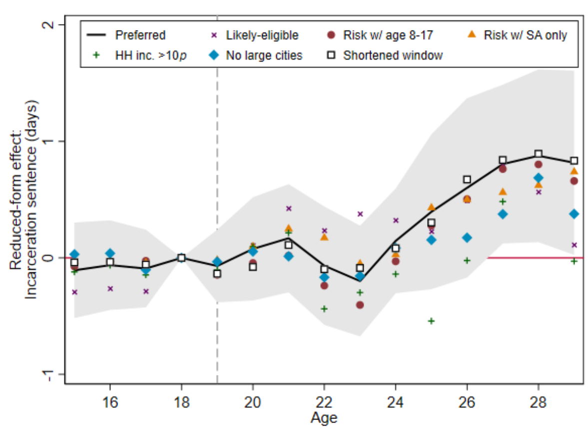
(a) $8 \leq \alpha \leq 15$



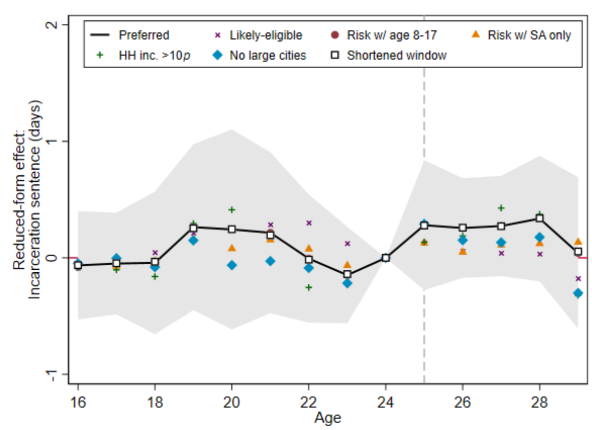
(b) $16 \leq \alpha \leq 18$



(c) $19 \leq \alpha \leq 24$



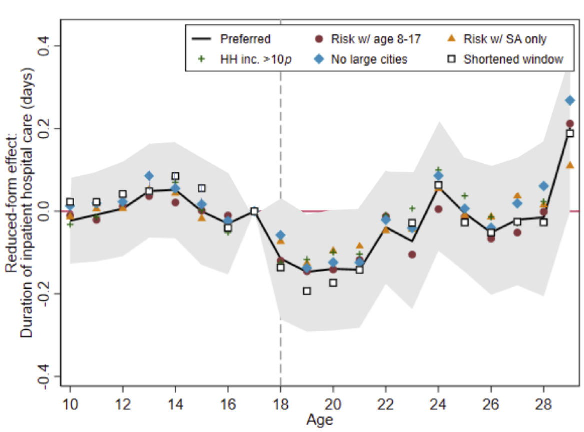
(d) $25 \leq \alpha \leq 29$



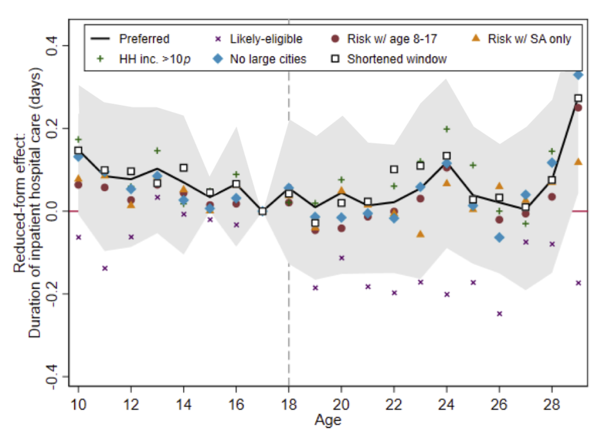
Notes: See notes to Figure D.29.

Figure D.32: Inpatient hospital care estimates are robust to alternative measurement choices and samples

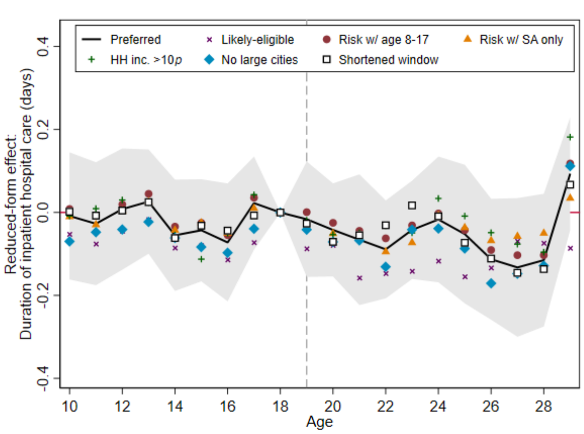
(a) $8 \leq \alpha \leq 15$



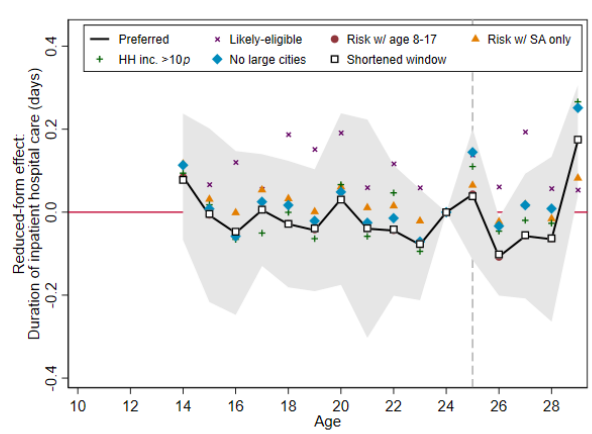
(b) $16 \leq \alpha \leq 18$



(c) $19 \leq \alpha \leq 24$

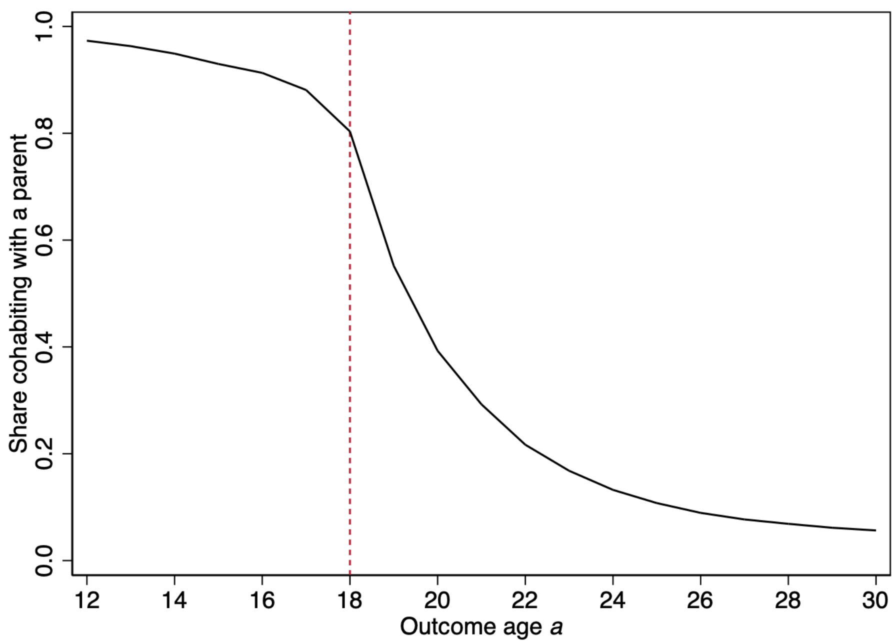


(d) $25 \leq \alpha \leq 29$



Notes: See notes to Figure D.29.

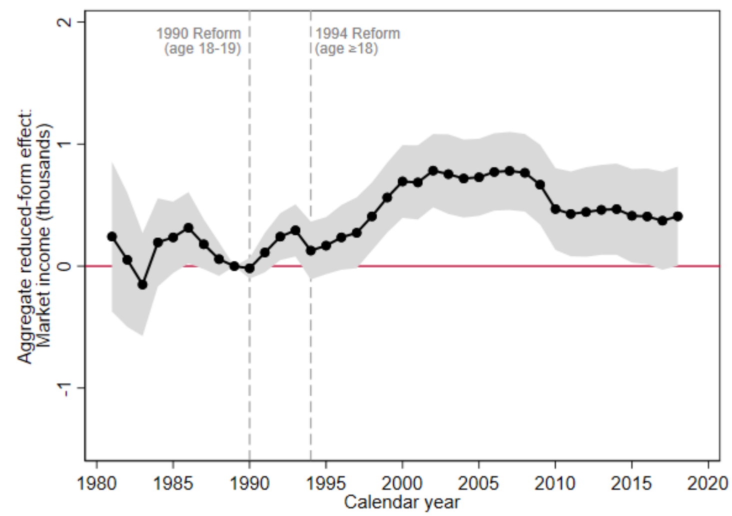
Figure D.33: Cohabitation with at least one parent over the life-cycle



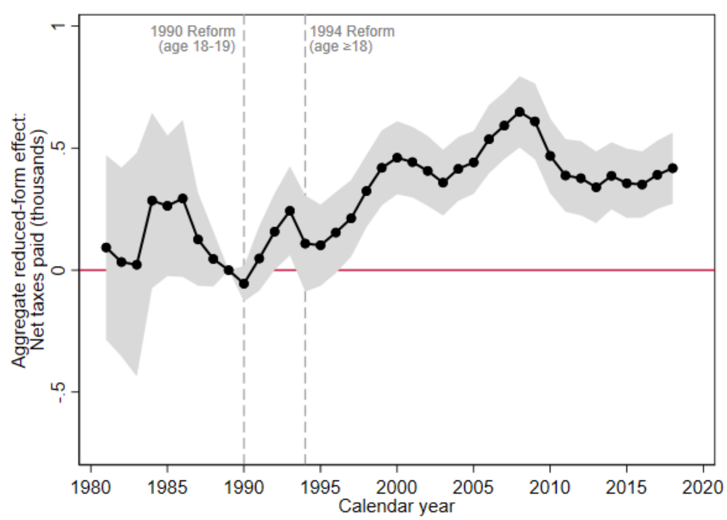
Notes: This figure plots the share of high-risk individuals who cohabit with at least one parent over the life-cycle.

Figure D.34: Aggregate effects of welfare-to-work over calendar time

(a) Market income



(b) Net taxes paid



(c) Inpatient hospital care

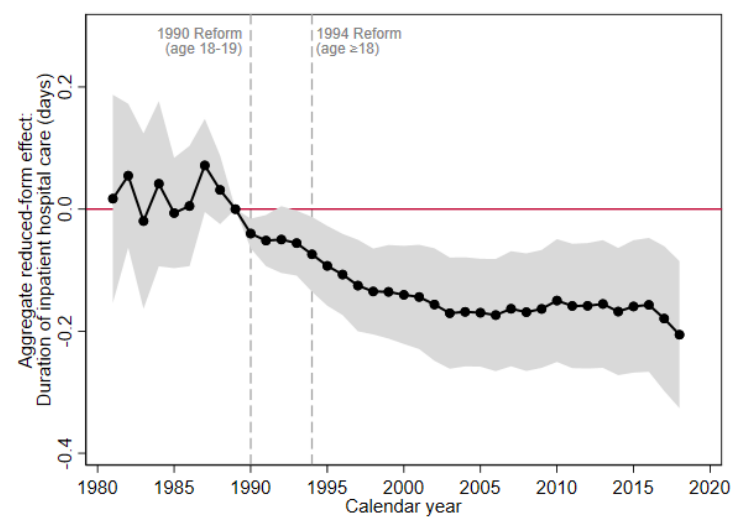


Table D.1: Summary statistics, by risk and starting age group

	All starting ages		$8 \leq \alpha \leq 15$		$25 \leq \alpha \leq 29$	
	High-risk (1)	Diff. (2)	High-risk (3)	Diff. (4)	High-risk (5)	Diff. (6)
A. Main outcome variables						
SA participation (months)	1.41	1.05	1.41	0.98	1.31	1.01
	(2.98)	[0.32]	(2.90)	[0.29]	(2.94)	[0.31]
SA work requirements (months) age 30-37	0.45	0.33	0.53	0.35	0.35	0.27
	(1.24)	[0.23]	(1.31)	[0.23]	(1.12)	[0.21]
Market income	25.57	−8.14	25.55	−8.22	25.64	−7.50
	(18.35)	[−0.32]	(18.97)	[−0.31]	(17.90)	[−0.30]
Employed at least part-time	0.68	−0.13	0.66	−0.14	0.69	−0.12
	(0.38)	[−0.27]	(0.39)	[−0.27]	(0.37)	[−0.25]
Duration of inpatient hospital care (days)	0.62	0.10	0.46	0.04	0.74	0.14
	(2.31)	[0.03]	(1.61)	[0.02]	(2.64)	[0.05]
Years of schooling, age 30	11.18	−1.33	11.48	−1.33	10.95	−1.30
	(2.21)	[−0.43]	(2.24)	[−0.43]	(2.15)	[−0.43]
Incarceration sentence (days)	1.47	1.11	1.77	1.33	0.73	0.54
	(14.85)	[0.07]	(16.42)	[0.07]	(9.65)	[0.05]
Disability insurance (ever)	0.07	0.03	0.07	0.03	0.06	0.03
	(0.23)	[0.11]	(0.24)	[0.10]	(0.23)	[0.11]
Unemployment insurance (ever)	0.17	0.01	0.13	−0.00	0.22	0.01
	(0.25)	[0.02]	(0.21)	[−0.01]	(0.29)	[0.03]
B. Control variables						
Household income	21.77	−20.46	19.16	−25.83	23.83	−14.41
	(18.48)	[−0.82]	(19.15)	[−0.97]	(17.05)	[−0.64]
Household assets	−0.33	−38.41	−5.20	−18.69	4.50	−54.91
	(31.00)	[−0.45]	(27.58)	[−0.24]	(33.87)	[−0.60]
Mother years of schooling	8.05	−0.02	8.74	−0.16	7.56	0.06
	(1.44)	[−0.01]	(1.48)	[−0.07]	(1.20)	[0.04]
Married household	0.34	−0.40	0.30	−0.38	0.42	−0.39
	(0.47)	[−0.62]	(0.46)	[−0.58]	(0.49)	[−0.61]
Single parent household	0.50	0.30	0.52	0.30	0.44	0.29
	(0.50)	[0.48]	(0.50)	[0.45]	(0.50)	[0.47]
Household number of children	1.95	0.27	2.00	0.21	2.08	0.36
	(1.21)	[0.18]	(1.12)	[0.15]	(1.28)	[0.23]
Mean parish income	34.13	−0.33	33.53	−1.07	34.87	0.43
	(8.45)	[−0.03]	(8.36)	[−0.10]	(8.50)	[0.04]
Urban municipality, age 17	0.44	0.23	0.43	0.23	0.46	0.23
	(0.50)	[0.35]	(0.50)	[0.36]	(0.50)	[0.35]
N	59,567	170,365	18,982	50,666	18,811	54,019

Notes: Odd-numbered columns report means among the high-risk group pooled over groups of starting age of treatments; standard deviations are in parentheses. Even-numbered columns report differences between the high-risk and low-risk groups; standardized differences scaled by the square root of the sum of risk group variances are in brackets. Unless otherwise noted, outcome variables are averaged between ages 30–37 and control variables are measured between ages 12–17. All monetary variables are expressed as thousands of 2010 U.S. dollars.

Table D.2: Long-run effects of welfare-to-work on labor market outcomes, by starting age group and specification

Starting age group	β_α (1)	β_α^{30} (2)	β_α (3)	β_α^{30} (4)	β_α (5)	β_α^{30} (6)	β_α (7)	β_α^{30} (8)	Mean (9)
Market income (before taxes and transfers, thousands)									
$\alpha \in [8, 15]$	0.848***	0.948**	0.960***	0.944**	0.945***	0.939**	1.083***	1.123**	21.135
	(0.177)	(0.306)	(0.177)	(0.308)	(0.182)	(0.318)	(0.181)	(0.360)	[13.961]
$\alpha \in [16, 18]$	0.020	0.245	0.113	0.221	0.099	0.209	0.225	0.246	22.252
	(0.195)	(0.337)	(0.193)	(0.335)	(0.215)	(0.360)	(0.215)	(0.430)	[14.970]
$\alpha \in [19, 24]$	−0.297***	−0.587**	−0.255**	−0.597**	−0.270**	−0.603**	−0.049	−0.614*	22.857
	(0.102)	(0.205)	(0.102)	(0.216)	(0.129)	(0.234)	(0.123)	(0.260)	[15.457]
$\alpha \in [25, 29]$	0.236	0.124	0.225	0.109	0.209	0.101	0.153	−0.235	24.811
	(0.235)	(0.172)	(0.229)	(0.185)	(0.251)	(0.221)	(0.238)	(0.227)	[16.142]
Disposable income (after taxes and transfers, thousands)									
$\alpha \in [8, 15]$	0.090	0.468**	0.141	0.468**	0.132	0.467**	0.152	0.454**	21.891
	(0.092)	(0.156)	(0.095)	(0.158)	(0.096)	(0.164)	(0.098)	(0.190)	[6.915]
$\alpha \in [16, 18]$	−0.055	0.360	−0.013	0.351	−0.022	0.347	0.018	0.286	22.734
	(0.097)	(0.206)	(0.098)	(0.205)	(0.105)	(0.213)	(0.106)	(0.238)	[7.220]
$\alpha \in [19, 24]$	−0.101*	−0.120	−0.076	−0.122	−0.084	−0.124	−0.013	−0.194	23.078
	(0.060)	(0.140)	(0.061)	(0.145)	(0.071)	(0.154)	(0.075)	(0.167)	[7.357]
$\alpha \in [25, 29]$	−0.063	0.002	−0.060	−0.006	−0.070	−0.009	−0.093	−0.236	23.758
	(0.079)	(0.097)	(0.077)	(0.103)	(0.093)	(0.125)	(0.094)	(0.140)	[7.491]
Net taxes paid (thousands)									
$\alpha \in [8, 15]$	0.758***	0.480***	0.819***	0.476***	0.814***	0.473**	0.930***	0.669***	−0.756
	(0.062)	(0.134)	(0.063)	(0.134)	(0.067)	(0.152)	(0.069)	(0.122)	[8.511]
$\alpha \in [16, 18]$	0.076	−0.115	0.126	−0.129	0.121	−0.138	0.207**	−0.040	−0.482
	(0.076)	(0.157)	(0.076)	(0.158)	(0.081)	(0.169)	(0.081)	(0.166)	[9.089]
$\alpha \in [19, 24]$	−0.196**	−0.467***	−0.179**	−0.475***	−0.186*	−0.479**	−0.036	−0.420**	−0.220
	(0.086)	(0.112)	(0.087)	(0.113)	(0.098)	(0.142)	(0.102)	(0.149)	[9.351]
$\alpha \in [25, 29]$	0.299***	0.122	0.284**	0.115	0.279**	0.110	0.246**	0.001	1.053
	(0.101)	(0.098)	(0.100)	(0.093)	(0.114)	(0.115)	(0.114)	(0.161)	[9.719]
Employed at least part-time (>$7,000)									
$\alpha \in [8, 15]$	0.009	0.044***	0.013*	0.044***	0.013*	0.044***	0.012	0.041**	0.620
	(0.007)	(0.011)	(0.007)	(0.011)	(0.007)	(0.012)	(0.007)	(0.013)	[0.322]
$\alpha \in [16, 18]$	−0.013*	0.031**	−0.009	0.031**	−0.010	0.031**	−0.009	0.029*	0.623
	(0.007)	(0.010)	(0.006)	(0.010)	(0.007)	(0.011)	(0.007)	(0.012)	[0.322]
$\alpha \in [19, 24]$	−0.002	0.019**	−0.000	0.019**	−0.001	0.019**	0.000	0.016*	0.633
	(0.004)	(0.006)	(0.004)	(0.006)	(0.005)	(0.007)	(0.005)	(0.008)	[0.320]
$\alpha \in [25, 29]$	0.003	0.017***	0.004	0.017***	0.003	0.017***	0.002	0.014**	0.666
	(0.005)	(0.004)	(0.005)	(0.004)	(0.005)	(0.004)	(0.006)	(0.005)	[0.310]
Demographics			✓	✓	✓	✓	✓	✓	
Munic. F.E.					✓	✓	✓	✓	
Trends							✓	✓	

Notes: This table reports estimates of β_α and β_α^{30} from Equation (2.5) averaged by groups of starting ages. Standard errors are two-way clustered over birth quarter and municipality of residence and reported in parentheses. Columns (1) and (2) report estimates of a triple difference regression without additional controls. Columns (3) and (4) control for the vector of demographics $\boldsymbol{X}_i$ described in the main text. Columns (5) and (6) also include municipality of residence fixed effects (measured the age before the starting age, or at age 17 for starting ages below 18). Columns (7) and (8) control for linear trends of demographics over birth quarters and over ages. Column (9) reports the mean outcomes among the high-risk group measured between ages 18–37; standard deviations in brackets.

Stars indicate statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.3: Long-run effects of welfare-to-work on other main outcomes, by starting age group and specification

Starting age group	β_α (1)	β_α^{30} (2)	β_α (3)	β_α^{30} (4)	β_α (5)	β_α^{30} (6)	β_α (7)	β_α^{30} (8)	Mean (9)
SA participation, total (months)									
$\alpha \in [8, 15]$	−0.081	−0.157*	−0.099	−0.157*	−0.098	−0.157	−0.104	−0.123	1.705
	(0.072)	(0.083)	(0.072)	(0.083)	(0.072)	(0.087)	(0.073)	(0.092)	[2.364]
$\alpha \in [16, 18]$	0.174***	−0.077	0.157**	−0.079	0.158**	−0.079	0.142*	−0.049	1.758
	(0.064)	(0.095)	(0.065)	(0.095)	(0.066)	(0.080)	(0.071)	(0.084)	[2.382]
$\alpha \in [19, 24]$	0.048	−0.155	0.040	−0.156	0.041	−0.156	0.034	−0.130	1.615
	(0.064)	(0.095)	(0.065)	(0.095)	(0.069)	(0.103)	(0.070)	(0.107)	[2.249]
$\alpha \in [25, 29]$	0.011	0.003	0.016	0.001	0.019	0.001	0.014	−0.002	1.332
	(0.049)	(0.044)	(0.049)	(0.045)	(0.054)	(0.053)	(0.055)	(0.055)	[2.008]
SA work requirements (months)									
$\alpha \in [8, 15]$	0.480***	0.289***	0.470***	0.289***	0.470***	0.289***	0.439***	0.320***	0.655
	(0.025)	(0.000)	(0.025)	(0.000)	(0.025)	(0.000)	(0.024)	(0.000)	[1.022]
$\alpha \in [16, 18]$	0.407***	0.117***	0.397***	0.116***	0.397***	0.116***	0.362***	0.136***	0.581
	(0.024)	(0.000)	(0.025)	(0.000)	(0.026)	(0.000)	(0.026)	(0.000)	[0.932]
$\alpha \in [19, 24]$	0.180***	0.063**	0.175***	0.062**	0.175***	0.062**	0.173***	0.060**	0.372
	(0.017)	(0.022)	(0.017)	(0.022)	(0.018)	(0.024)	(0.020)	(0.025)	[0.720]
$\alpha \in [25, 29]$	0.113***	0.088***	0.113***	0.089***	0.114***	0.089***	0.117***	0.095**	0.252
	(0.014)	(0.014)	(0.014)	(0.014)	(0.016)	(0.016)	(0.016)	(0.018)	[0.553]
Years of schooling									
$\alpha \in [8, 15]$	0.042*	0.033	0.048**	0.035	0.048**	0.036	0.074***	0.061	10.925
	(0.021)	(0.031)	(0.021)	(0.035)	(0.022)	(0.035)	(0.022)	(0.035)	[1.648]
$\alpha \in [16, 18]$	−0.002	−0.001	0.004	0.000	0.004	0.001	0.013	0.013	10.934
	(0.028)	(0.046)	(0.027)	(0.046)	(0.029)	(0.052)	(0.029)	(0.050)	[1.793]
$\alpha \in [19, 24]$	−0.030	−0.053	−0.031	−0.053	−0.031	−0.051	−0.011	−0.047	10.879
	(0.019)	(0.042)	(0.019)	(0.042)	(0.021)	(0.048)	(0.022)	(0.046)	[1.866]
$\alpha \in [25, 29]$	0.023*	−0.049	0.012	−0.050	0.013	−0.049	−0.012	−0.059	10.927
	(0.011)	(0.033)	(0.012)	(0.033)	(0.015)	(0.039)	(0.016)	(0.040)	[1.902]
Demographics			✓	✓	✓	✓	✓	✓	
Munic. F.E.					✓	✓	✓	✓	
Trends							✓	✓	

Notes: (see below)

Table D.3: (*continued*)

Starting age group	β_α (1)	β_α^{30} (2)	β_α (3)	β_α^{30} (4)	β_α (5)	β_α^{30} (6)	β_α (7)	β_α^{30} (8)	Mean (9)
Inpatient hospital care (days)									
$\alpha \in [8, 15]$	-0.096^{***}	-0.057	-0.099^{***}	-0.054	-0.099^{***}	-0.054	-0.072^{**}	-0.048	0.620
	(0.027)	(0.058)	(0.026)	(0.057)	(0.029)	(0.062)	(0.028)	(0.061)	[0.322]
$\alpha \in [16, 18]$	-0.044	-0.011	-0.044	-0.011	-0.044	-0.011	-0.018	-0.017	0.623
	(0.031)	(0.055)	(0.031)	(0.055)	(0.035)	(0.060)	(0.036)	(0.062)	[0.322]
$\alpha \in [19, 24]$	-0.053^{**}	-0.059	-0.051^{**}	-0.059	-0.051^{*}	-0.059	-0.043	-0.060	0.633
	(0.023)	(0.045)	(0.023)	(0.046)	(0.028)	(0.048)	(0.026)	(0.051)	[0.320]
$\alpha \in [25, 29]$	0.089^{**}	-0.013	0.086^{**}	-0.013	0.085^{**}	-0.013	0.083^{**}	-0.014	0.666
	(0.032)	(0.032)	(0.031)	(0.032)	(0.036)	(0.036)	(0.041)	(0.037)	[0.310]
Incarceration sentence (days)									
$\alpha \in [8, 15]$	0.079	0.188	0.058	0.187	0.008	0.187	0.015	0.193	1.173
	(0.119)	(0.176)	(0.120)	(0.177)	(0.145)	(0.227)	(0.147)	(0.236)	[7.918]
$\alpha \in [16, 18]$	0.231	0.242	0.220	0.247	0.172	0.247	0.167	0.280	1.319
	(0.171)	(0.186)	(0.172)	(0.189)	(0.199)	(0.229)	(0.198)	(0.240)	[9.168]
$\alpha \in [19, 24]$	0.453^{***}	0.251	0.455^{***}	0.252	0.456^{**}	0.252	0.440^{**}	0.276	1.398
	(0.146)	(0.141)	(0.146)	(0.142)	(0.176)	(0.176)	(0.181)	(0.180)	[9.671]
$\alpha \in [25, 29]$	0.143	-0.156	0.150	-0.156	0.151	-0.156	0.149	-0.146	1.010
	(0.127)	(0.134)	(0.128)	(0.135)	(0.170)	(0.165)	(0.192)	(0.176)	[7.211]
DI benefits ($)									
$\alpha \in [8, 15]$	39.4	68.1	30.4	69.1	30.1	69.7	-15.4	7.5	1,114
	(27.5)	(80.9)	(27.7)	(80.4)	(30.2)	(85.2)	(30.7)	(88.068)	[3,893]
$\alpha \in [16, 18]$	-24.0	93.3	-30.8	98.5	-31.1	100.3	-67.5^{*}	28.274	1,383
	(32.2)	(104.1)	(31.9)	(101.5)	(36.3)	(107.9)	(36.3)	(107.2)	[3,960]
$\alpha \in [19, 24]$	115.3^{***}	190.4^{*}	112.4^{***}	192.0^{*}	112.7^{***}	192.0	90.8^{**}	145.6	1,647
	(37.0)	(96.0)	(36.6)	(94.5)	(40.7)	(104.4)	(40.3)	(100.4)	[4,279]
$\alpha \in [25, 29]$	8.9	-64.6	9.4	-58.0	9.9	-56.5	5.0	-32.9	1,661
	(35.1)	(75.2)	(35.2)	(77.3)	(43.5)	(98.6)	(44.3)	(100.2)	[4,147]
Demographics			✓	✓	✓	✓	✓	✓	
Munic. F.E.					✓	✓	✓	✓	
Trends							✓	✓	

Notes: This table reports estimates of β_α and β_α^{30} from Equation (2.5) averaged by groups of starting ages. Standard errors are two-way clustered over birth quarter and municipality of residence and reported in parentheses. Columns (1) and (2) report estimates of a triple difference regression without additional controls. Columns (3) and (4) control for the vector of demographics $\boldsymbol{X}_i$ described in the main text. Columns (5) and (6) also include municipality of residence fixed effects (measured the age before the starting age, or at age 17 for starting ages below 18). Columns (7) and (8) control for linear trends of demographics over birth quarters and over ages. Column (9) reports the mean outcomes among the high-risk group measured between ages 18–37; standard deviations in brackets.

Stars indicate statistical significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table D.4: Long-run effects on educational outcomes, by starting age group

	Effect	Starting age group				High-risk mean
		$8 \leq \alpha \leq 15$	$16 \leq \alpha \leq 18$	$19 \leq \alpha \leq 24$	$25 \leq \alpha \leq 29$	[S.D.]
		(1)	(2)	(3)	(4)	(5)
1. Years of schooling	β_α	0.074***	0.013	−0.011	−0.012	10.5
		(0.022)	(0.029)	(0.022)	(0.016)	[1.54]
	β_α^{30}	0.061*	0.013	−0.047	−0.059	10.79
		(0.035)	(0.040)	(0.046)	(0.040)	[1.74]
2. Completed 10 years of schooling	β_α	0.032***	0.015*	0.010**	0.008**	0.712
		(0.006)	(0.007)	(0.005)	(0.004)	[0.453]
	β_α^{30}	0.063***	0.049***	0.020**	−0.002	0.723
		(0.010)	(0.013)	(0.007)	(0.005)	[0.447]
3. Completed 12 years of schooling	β_α	0.018***	0.004	−0.001	−0.003	0.431
		(0.006)	(0.006)	(0.005)	(0.004)	[0.495]
	β_α^{30}	0.019	0.008	0.002	−0.003	0.452
		(0.007)	(0.007)	(0.007)	(0.005)	[0.498]
4. Completed > 12 years of schooling	β_α	0.023***	0.006	0.000	−0.006	0.368
		(0.005)	(0.006)	(0.005)	(0.005)	[0.482]
	β_α^{30}	0.018	0.002	−0.009	−0.014	0.396
		(0.007)	(0.009)	(0.008)	(0.008)	[0.489]

Notes: Columns (1)–(4) report estimates of β_α and β_α^{30} from Equation (2.5), aggregated by starting age group. Column (5) reports the pooled mean outcome among the high-risk group at age 29 (for β_α) or age 31 (for β_α^{30}); standard deviations of outcomes are in brackets. Estimates of $\beta_{\alpha,a}^{30}$ are relative to starting age $\alpha = 29$ (not $\alpha = 30$) due to data constraints. All regressions estimates are based on main sample of approximately 170,000 low-risk and 60,000 high-risk individuals. Standard errors are two-way clustered over birth quarters and municipality of residence and reported in parentheses.

Stars indicate statistical significance: $^{\star}p < 0.10$, $^{\star\star}p < 0.05$, $^{\star\star\star}p < 0.01$.

Table D.5: Long-run effects on types of inpatient hospital care, by starting age group

	Effect	Starting age group				High-risk mean
		$8 \leq \alpha \leq 15$	$16 \leq \alpha \leq 18$	$19 \leq \alpha \leq 24$	$25 \leq \alpha \leq 29$	[S.D.]
		(1)	(2)	(3)	(4)	(5)
Excluding births or birth-related care	β_α	-0.095^{***}	-0.051	-0.051	0.054	0.639
		(0.031)	(0.032)	(0.031)	(0.042)	[2.003]
	β_α^{30}	-0.065	-0.021	-0.065	-0.024	0.567
		(0.051)	(0.047)	(0.042)	(0.032)	[1.653]
Accidents	β_α	-0.007	-0.005	-0.005	-0.015	0.000
		(0.010)	(0.014)	(0.017)	(0.026)	[0.000]
	β_α^{30}	-0.018	-0.008	-0.004	0.006	0.000
		(0.045)	(0.044)	(0.026)	(0.017)	[0.000]
Internal disease	β_α	-0.062^{***}	-0.018	-0.026	0.072^{**}	0.000
		(0.023)	(0.018)	(0.023)	(0.033)	[0.000]
	β_α^{30}	0.017	0.051^{*}	-0.011	0.037	0.000
		(0.031)	(0.026)	(0.027)	(0.024)	[0.000]
Mental or psychiatric	β_α	0.004	0.002	-0.008	0.005	0.000
		(0.007)	(0.007)	(0.007)	(0.016)	[0.000]
	β_α^{30}	-0.016	-0.008	-0.004	0.000	0.000
		(0.009)	(0.009)	(0.007)	(0.008)	[0.000]
Metabolic disease	β_α	0.008	0.026^{**}	0.003	-0.016	0.000
		(0.012)	(0.013)	(0.012)	(0.014)	[0.000]
	β_α^{30}	-0.028	-0.014	-0.014	-0.034	0.000
		(0.016)	(0.017)	(0.017)	(0.020)	[0.000]
Musculoskeletal disease or issues	β_α	-0.007	-0.005	-0.012	0.005	0.000
		(0.012)	(0.014)	(0.011)	(0.020)	[0.000]
	β_α^{30}	-0.013	-0.016	-0.028^{*}	-0.010	0.000
		(0.013)	(0.016)	(0.014)	(0.010)	[0.000]

Notes: Columns (1)–(4) report estimates of β_α and β_α^{30} from Equation (2.5), aggregated by starting age group. Column (5) reports the pooled mean outcome among the high-risk group between ages 18–29 (for β_α) or ages 18–39 (for β_α^{30}); standard deviations of outcomes are in brackets. All regressions estimates are based on main sample of approximately 170,000 low-risk and 60,000 high-risk individuals. Standard errors are two-way clustered over birth quarters and municipality of residence and reported in parentheses.

Stars indicate statistical significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table D.6: Long-run effects on crime outcomes, by starting age group

	Effect	Starting age group				High-risk mean
		$8 \leq \alpha \leq 15$	$16 \leq \alpha \leq 18$	$19 \leq \alpha \leq 24$	$25 \leq \alpha \leq 29$	[S.D.]
		(1)	(2)	(3)	(4)	(5)
Ever convicted	β_α	-0.021^{***}	-0.009^{***}	-0.007^{**}	-0.004	0.104
		(0.003)	(0.003)	(0.003)	(0.003)	[0.180]
	β_α^{30}	-0.036^{***}	-0.027^{***}	-0.020^{***}	-0.008^{***}	0.090
		(0.004)	(0.004)	(0.003)	(0.002)	[0.149]
Ever incarcerated (days)	β_α	0.000	0.001	0.002^{**}	0.001	0.012
		(0.001)	(0.001)	(0.001)	(0.001)	[0.071]
	β_α^{30}	0.001	0.002^{**}	0.001	-0.001	0.010
		(0.001)	(0.001)	(0.001)	(0.001)	[0.059]
Incarceration sentence (days)	β_α	0.071	0.218	0.440^{**}	0.149	1.940
		(0.151)	(0.198)	(0.181)	(0.192)	[14.275]
	β_α^{30}	0.263	0.333	0.276	-0.146	1.767
		(0.224)	(0.240)	(0.180)	(0.176)	[12.402]
Incarceration sentence, property crime (days)	β_α	-0.075	0.174	0.209	0.052	1.012
		(0.120)	(0.128)	(0.127)	(0.132)	[8.661]
	β_α^{30}	-0.061	0.056	0.050	-0.026	0.759
		(0.146)	(0.104)	(0.102)	(0.094)	[6.311]
Incarceration sentence, violent crime (days)	β_α	0.103	0.114^{*}	0.148	0.049	0.391
		(0.087)	(0.066)	(0.091)	(0.089)	[4.215]
	β_α^{30}	0.066	0.100	0.091	0.011	0.322
		(0.096)	(0.070)	(0.063)	(0.042)	[3.089]

Notes: Columns (1)–(4) report estimates of β_α and β_α^{30} from Equation (2.5), aggregated by starting age group. Column (5) reports the pooled mean outcome among the high-risk group between ages 18–29 (for β_α) or ages 18–39 (for β_α^{30}); standard deviations of outcomes are in brackets. All regressions estimates are based on main sample of approximately 170,000 low-risk and 60,000 high-risk individuals. Standard errors are two-way clustered over birth quarters and municipality of residence and reported in parentheses.

Stars indicate statistical significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table D.7: Long-run effects on other social program participation, by starting age group

	Effect	Starting age group				High-risk mean
		$8 \leq \alpha \leq 15$	$16 \leq \alpha \leq 18$	$19 \leq \alpha \leq 24$	$25 \leq \alpha \leq 29$	[S.D.]
		(1)	(2)	(3)	(4)	(5)
8. DI benefits ($)	β_α	−15.4	−67.5*	90.8**	5.0	390
		(30.7)	(36.3)	(40.3)	(44.3)	[2,296]
	β_α^{30}	7.5	28.27	145.6	−32.9	746
		(88.1)	(107.2)	(100.4)	(100.2)	[3,062]
Ever on DI	β_α	−0.004*	−0.005**	0.003	−0.002	0.023
		(0.002)	(0.002)	(0.003)	(0.003)	[0.122]
	β_α^{30}	−0.006	−0.005	0.002	−0.004	0.040
		(0.005)	(0.004)	(0.004)	(0.005)	[0.154]
Ever on UI	β_α	−0.019***	−0.019***	−0.007	0.007	0.208
		(0.006)	(0.005)	(0.005)	(0.007)	[0.249]
	β_α^{30}	−0.019***	−0.011**	0.002	−0.003	0.192
		(0.007)	(0.006)	(0.007)	(0.007)	[0.212]

Notes: Columns (1)–(4) report estimates of β_α and β_α^{30} from Equation (2.5), aggregated by starting age group. Column (5) reports the pooled mean outcome among the high-risk group between ages 18–29 (for β_α) or ages 18–37 (for β_α^{30}); standard deviations of outcomes are in brackets. All regressions estimates are based on main sample of approximately 170,000 low-risk and 60,000 high-risk individuals. Standard errors are two-way clustered over birth quarters and municipality of residence and reported in parentheses.

Stars indicate statistical significance: $^{\star}p < 0.10$, $^{\star\star}p < 0.05$, $^{\star\star\star}p < 0.01$.

Table D.8: Effects on mother's household market income from 1994 Reform for cohorts with parent ITT starting after age 19

	$15 \leq \alpha \leq 18$	$15 \leq \alpha \leq 29$
	(1)	(2)
Single mother household	−0.436	0.128
	(0.474)	(0.406)
Two-parent household	0.585	0.705
	(0.520)	(0.605)

Notes: This table reports estimates of mother's household market income from the 1994 reform for different starting age cohort groups, by individual's household structure measured in ages 12–17 (to maintain parallel trends). Column (1) are starting age cohorts used in the decomposition exercise of Table 4.1. Column (2) are all starting age cohorts corresponding to parent ITT effects that start after age 19.

Table D.9: Intergenerational spillover effects, by mother's starting age group and child gender

		$\alpha \in [8, 15]$		$\alpha \in [16, 18]$		$\alpha \in [19, 24]$		Mean		
	min{α} (1)	Son (2)	Daughter (3)	Son (4)	Daughter (5)	Son (6)	Daughter (7)	Son (8)	Daughter (9)	N (10)
A. Labor market outcomes										
Market income, age 18–20	14	−0.041	−0.402	−1.108***	−0.717***	−0.215	−0.298	11.81	8.76	52,921
(thousands)		(0.339)	(0.427)	(0.344)	(0.291)	(0.336)	(0.289)	[8.29]	[6.28]	
Disposable income, age 18–20	14	0.028	−0.175	−0.556**	−0.278	−0.140	−0.113	11.07	9.87	52,921
(thousands)		(0.208)	(0.261)	(0.245)	(0.224)	(0.234)	(0.185)	[5.18]	[4.00]	
Employed ≥part-time, age 18–20	14	0.005	−0.013	−0.047***	−0.037**	−0.004	−0.012	0.56	0.46	52,921
		(0.020)	(0.024)	(0.017)	(0.016)	(0.014)	[0.40]	[0.38]		
Net taxes paid, age 18–20	14	−0.068	−0.227	−0.552***	−0.440**	−0.074	−0.185	0.74	−1.11	52,921
(thousands)		(0.202)	(0.254)	(0.173)	(0.182)	(0.148)	(0.178)	[3.81]	[3.92]	
B. Health outcomes										
Birth weight (kg)	8	0.016	0.008	−0.032	−0.001	0.027	−0.005	3.42	3.31	64,160
		(0.002)	(0.003)	(0.004)	(0.003)	(0.002)	(0.002)	[0.57]	[0.52]	
Duration of inpatient care,	8	−0.852	−0.418	−0.235	−0.569	−0.501	0.279	9.58	8.07	65,078
age 0–6 (days)		(0.535)	(0.543)	(0.691)	(0.534)	(0.529)	(0.502)	[12.15]	[10.16]	
Duration of inpatient care,	8	0.013	−0.094	0.019	−0.117	−0.195	0.133	1.09	1.08	65,062
age 7–14 (days)		(0.120)	(0.133)	(0.141)	(0.190)	(0.122)	(0.168)	[3.28]	[3.38]	
Duration of inpatient care,	13	0.089	−0.347	−0.067	0.138	−0.158	0.103	1.21	2.40	55,705
age 15–22 (days)		(0.157)	(0.209)	(0.142)	(0.192)	(0.170)	(0.211)	[3.81]	[4.96]	
C. Education outcomes										
Grade 9 exam score, age 16 (s.d.)	15	−0.025	−0.026	−0.003	−0.009	−0.022	−0.011	−0.33	−0.13	39,194
		(0.078)	(0.051)	(0.042)	(0.050)	(0.035)	(0.044)	[0.74]	[0.74]	
Years of schooling, age 20	15	−0.093	0.191**	−0.056	0.014	−0.099	−0.020	11.11	11.03	43,291
		(0.075)	(0.087)	(0.086)	(0.064)	(0.064)	(0.057)	[1.29]	[1.16]	
Completed grade 10, age 18	13	0.001	0.024	−0.044*	−0.025	−0.010	−0.030	0.77	0.84	52,444
		(0.027)	(0.024)	(0.026)	(0.021)	(0.020)	(0.020)	[0.42]	[0.37]	
Completed grade 12, age 20	15	−0.042	0.050	−0.016	−0.008	−0.036	−0.009	0.43	0.45	43,468
		(0.029)	(0.040)	(0.028)	(0.026)	(0.023)	(0.022)	[0.50]	[0.50]	
Attended college, age 20	15	−0.025	−0.017	0.000	−0.006	−0.027**	−0.005	0.21	0.07	43,291
		(0.016)	(0.018)	(0.020)	(0.014)	(0.013)	(0.012)	[0.41]	[0.26]	

Notes: (see below)

Table D.9: (*continued*)

		$\alpha \in [8, 15]$		$\alpha \in [16, 18]$		$\alpha \in [19, 24]$		Mean		
	min{α} (1)	Son (2)	Daughter (3)	Son (4)	Daughter (5)	Son (6)	Daughter (7)	Son (8)	Daughter (9)	N (10)
D. Crime outcomes										
Number of serious crimes, age 15–17	11	−0.114**	0.014	0.097**	0.006	−0.036	0.017	0.34	0.08	60,207
		(0.047)	(0.033)	(0.047)	(0.030)	(0.050)	(0.029)	[1.08]	[0.47]	
Number of serious crimes, age 18–20	14	0.038	−0.030	0.147	0.028	−0.071	0.004	0.65	0.10	53,408
		(0.122)	(0.056)	(0.099)	(0.054)	(0.080)	(0.035)	[1.98]	[0.66]	
Incarceration sentence, age 18–20 (days)	14	1.896	0.336	5.422**	0.748	−0.399	−0.446	3.79	0.10	53,408
		(2.752)	(0.690)	(2.614)	(0.869)	(1.835)	(0.423)	[41.24]	[4.68]	
E. Social program participation										
Duration on SA, age 18–20 (months)	14	−0.887***	−1.387***	0.087	−0.319	0.285	0.107	2.56	3.38	52,803
		(0.332)	(0.325)	(0.280)	(0.321)	(0.313)	(0.336)	[6.33]	[7.71]	
Duration in SA work requirements, age 18–20 (months)	14	−0.286	−0.480***	0.298	−0.069	0.302	0.219	1.24	1.35	52,803
		(0.202)	(0.170)	(0.186)	(0.174)	(0.198)	(0.159)	[3.70]	[3.77]	
Ever on SA, age 18–20	14	−0.070***	−0.073***	−0.012	−0.016	−0.005	0.027	0.25	0.27	52,803
		(0.025)	(0.026)	(0.018)	(0.021)	(0.018)	(0.018)	[0.43]	[0.44]	
Ever on DI, age 18–20	14	−0.001	0.008	−0.001	0.003	−0.000	0.008*	0.01	0.01	52,390
		(0.006)	(0.006)	(0.006)	(0.006)	(0.005)	(0.005)	[0.11]	[0.09]	
Ever on UI, age 18–20	14	−0.002	0.003	−0.003	0.002	−0.003	−0.002	0.03	0.02	52,410
		(0.007)	(0.006)	(0.006)	(0.004)	(0.005)	(0.005)	[0.11]	[0.09]	
UI fund membership, age 20	14	0.042*	0.028	−0.010	−0.012	−0.046**	−0.026	0.19	0.15	52,803
		(0.025)	(0.022)	(0.022)	(0.018)	(0.020)	(0.017)	[0.39]	[0.36]	

Notes: This table reports estimates (relative to starting age of treatment group $\alpha \in [25, 30]$) from Equation (4.3) for children born to mothers in the main sample who are age 25 or below at the time of birth, by the mother's starting age of treatment group and child gender. Column (1) reports the smallest starting age available in the data for the first starting age of treatment group $\alpha \in [8, 15]$. Columns (2) and (3), (4) and (5), and (6) and (7) report estimates for different starting age of treatment groups, with even-numbered columns reporting estimates for sons and odd-numbered columns reporting estimates for daughters. Columns (8) and (9) report the mean outcomes among high-risk individuals with starting ages $\alpha \in [25, 30]$ for sons and daughters, respectively; standard deviations are reported in brackets. Standard errors clustered by birth quarter and municipality of residence in the age prior to treatment are reported in parentheses.

Stars indicate statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

APPENDIX E

MODEL

This section provides a microeconomic model. Section E.1 formalizes the four-period life-cycle model described in Section 2.1.3. Section E.2 provides a simple, two-period specification to clarify how anticipation effects can affect the behavior and subsequent outcomes of younger cohorts.

E.1 General Model

Preferences. Consider an agent i who lives for four periods, where $t = 1$ denotes childhood (before age 18), $t = 2$ denotes young adulthood (ages 18–24), $t = 3$ denotes adulthood (ages 25–30), and $t = 4$ denotes older adulthood (after age 30). She aims to maximize lifetime utility from private consumption c_{it} and leisure ℓ_{it}. Utility is additively separable over time with flow $U_i(c_{it}, \ell_{it})$, which is smooth, increasing, and concave in both arguments. Assume leisure is a normal good. Besides these restrictions, the model allows for unrestricted heterogeneity in preferences and parameters; I thus suppress i in all notation for the remainder of this section.

Choices. There are four mutually-exclusive binary choices available over the life-cycle: education E_t, SA participation SA_t, work W_t, and an outside option ($E_t = SA_t = W_t = 0$).[1] While continuous choices c_t and ℓ_t occur in each period, assume the set of binary choices $\mathcal{D}_t$ varies over t. In period $t = 1$ (before reaching age of SA eligibility), the agent can choose education: $\mathcal{D}_t = \{E_t\}$.[2] In period $t = 2$ (after reaching age of SA eligibility but still within

1. Legally, it was possible for individuals to work and participate in SA, so long as the earnings from work are sufficiently low (i.e., below the SA benefit level). Such earnings incurred a 100% marginal tax rate in the benefit formula. Given the most common reason for SA participation was lacking employment, my model abstracts from the possibility of simultaneously choosing SA and work.

2. It is possible for individuals to work starting at age 16. However, there are extremely few observations who work before age 18.

the age range of attending post-secondary schooling), the choice set expands to include SA or work: $\mathcal{D}_t = \{E_t, SA_t, W_t\}$. Finally, in periods $t = 3$ and 4, $\mathcal{D}_t = \{SA_t, W_t\}$.

In all periods, the agent allocates her total time (normalized to 1) between leisure ℓ_t and "labor," broadly defined, which is denoted $h_t \in [0, 1]$. Labor includes time in work under $W_t = 1$ and exogenously-determined levels of time required by schooling under $E_t = 1$ and SA participation if there are work requirements in place, indicated by $Z_t^{SA} \in \{0, 1\}$. In sum, labor is determined by

$$h_t = \begin{cases} \bar{h}^E & \text{if } E_t = 1 \\ \bar{h}^{SA} \cdot Z_t^{SA} & \text{if } SA_t = 1 \\ 1 - \ell_t & \text{if } W_t = 1 \\ 0 & \text{if } E_t = SA_t = W_t = 0, \end{cases} \tag{E.1.1}$$

where $\bar{h}^E, \bar{h}^{SA} > 0$ are the exogenous time requirements for education and SA, respectively. For concision, let $D_t \in \{E, SA, W\}$ indicate the chosen alternative in the notationally obvious way, where W subsumes the outside option. Specifically, $D_t = W$ indicates either $W_t = 1$, which implies $h_t > 0$, or $E_t = SA_t = W_t = 0$, which occurs if and only if $h_t = 0$.

Technology and prices. The agent may earn market wages w_t if $D_t = W$ and $h_t > 0$. Wages are determined by the agent's human capital θ_t,

$$w_t = \Gamma_t(\theta_t), \tag{E.1.2}$$

which may vary over the life-cycle t. Human capital evolves based on the time inputs in h_t and D_t with law of motion

$$\theta_{t+1} = f_t(\theta_t, h_t, D_t, Z_t^{SA}), \tag{E.1.3}$$

where the technology may vary in t and is smooth, increasing, and concave in θ_{t-1} and h_t and increases if $D_t = E$ or if $D_t = SA$ when $Z_t^{SA} = 1$. In other words, education, SA work requirements, and experience in the labor market increase human capital.

Agent's problem. Ignoring savings decisions and assuming a consumption floor set by a constant, exogenous level of non-labor market income $m > 0$,[3] the agent aims to solve

$$\max_{\tilde{c}^4, \tilde{\ell}^4, \tilde{D}^4} \sum_{t=1}^{4} \beta^{t-1} U(c_t, \ell_t) \quad \text{s.t.} \quad c_t = (w_t(\theta_t) h_t \times W_t) + \left(b_t^E \times E_t\right) + \left(b_t^{SA} \times SA_t\right) + m; \tag{E.1.4}$$

$$\mathcal{D}_1 = \{E_1\},\ \mathcal{D}_2 \in \{E_2, SA_2, W_2\},\ \mathcal{D}_t \in \{SA_t, W_t\} \text{ for } t = 3, 4;$$

$$\theta_1 \text{ given; and Equations (E.1.1), (E.1.2), (E.1.3),}$$

where the history of choices up to period t with tildes and superscript t (e.g., $\tilde{c}^t \equiv (c_1, \ldots, c_t)$) and β is the intertemporal discount factor. In the budget constraint, b_{SAt} denotes the level of SA benefits from selecting SA, where $b_3^{SA} = b_4^{SA} > b_2^{SA} > 0$ reflect different benefit rates for young and older adults. Meanwhile, b_t^E denotes the level of public educational benefits from selecting education, where $b_1^E = 0$ and $b_2^E = b_2^{SA}$ through state educational grants, which match the SA youth benefit rate during the 1990s.[4]

3. According to the data, these are reasonable assumptions. The average individual who is at high-risk of participating in SA possesses low-levels of debt for much of the life-cycle until age 50. Also note that to be deemed eligible for SA, an individual must possess very low levels of liquid assets. Non-labor market income constitutes less than 5% of total gross market income before age 30, a period representing when most decisions are made by the agent in the model.

4. The state educational grants (*Statens Uddannelsesstøtte*, SU) were available for students above age 18, including those who have not yet completed secondary schooling. See Hansen and Schultz-Nielsen (2017) for more details on the interaction between SA and SU benefit schemes. All (public) education is free in Denmark, so costs of choosing $D_1, D_2 = E$ do not appear in the budget constraint.

E.2 Simple Two-Period Model

Consider a simplified two-period version of the previous model. In period $t = 1$ (adolescence), $\mathcal{D}_1 = \{E_1\}$ and in $t = 2$ (adulthood), $\mathcal{D}_2 = \{SA_2, W_2\}$. Parametrize the utility flow as a constant elasticity of substitution function

$$U(c_t, \ell_t) = \left(c_t^\rho + \omega \cdot \ell_t^\rho\right)^{1/\rho}$$

and the human capital production function as

$$\theta_2 = \theta_1 + \theta_E E_1,$$

and treat human capital units as equivalent to wages, i.e.

$$w_t(\theta_t) = \theta_t.$$

This simplified model provides closed-form solutions to the agent's choice problem to readily study comparative statics between the counterfactual states of no reform ($Z_t^{SA} = 0$) and reform ($Z_2^{SA} = 1$). Figure E.1 simulates comparative statics along key parameters of the model: the returns to education in $t = 1$, θ_E (Panel (a)) and preference for leisure (relative to consumption) (Panel (b)). The movement from the black solid line to the red solid line represents how the agent's valuation for choosing $(E_1 = 0, SA_2 = 1)$ decreases as a result of losing leisure from participating in SA work requirements; as shown in Panel (b), there are greater losses as ω increases, ceteris paribus. The broken lines illustrate the choice-contingent valuations for $(E_1 = 1, W_2 = 1)$ and $(E_1 = 0, W_2 = 1)$. The green sections of these lines represent different sets of "compliers" who behaviorally respond to the reform along different choice margins.

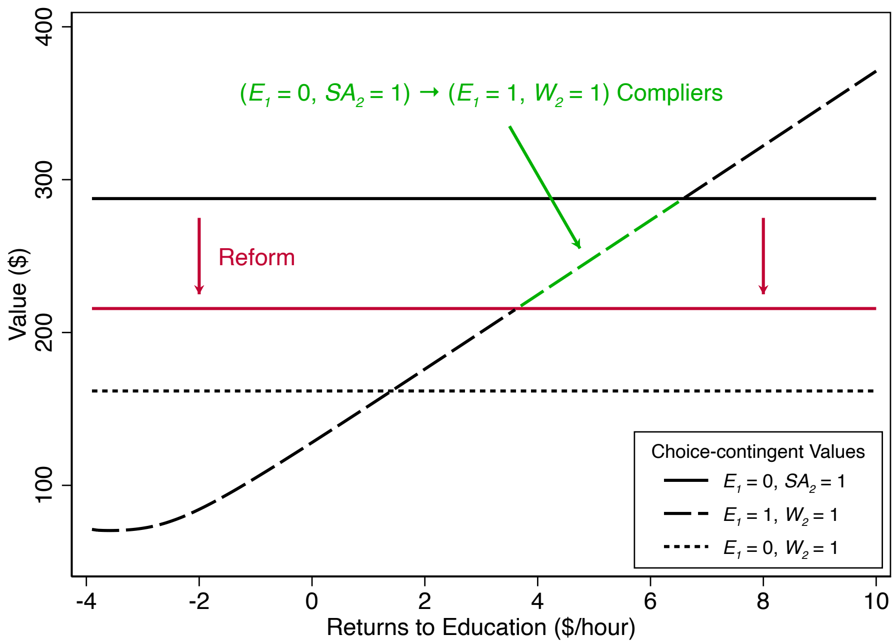
Different types of compliers emerge depending on the timing of the reform within the agent's life-cycle. Let α denote the period that the reform starts within the agent's life-cycle.

Reform starts in adulthood, $\boldsymbol{\alpha = 2}$. In this case, the reform came as a surprise to the agent at the beginning of adulthood. If the agent chose $E_1 = 0$ and would have chosen $SA_2 = 1$ under no reform ($Z_2^{SA} = 0$), then the agent may choose to continue participating in SA and fulfill its work requirements or to work in the labor market. Panel (a) depicts the case where the value of choosing work is below that of choosing SA; if the agent's initial human capital θ_1 was larger or if she possessed different preferences for leisure, the subjective gains from choosing work could instead be higher than choosing SA. Panel (b) depicts the latter case: for sufficiently large levels of preferences for leisure, agents move from SA to work (depicted by the short-dashed green line).

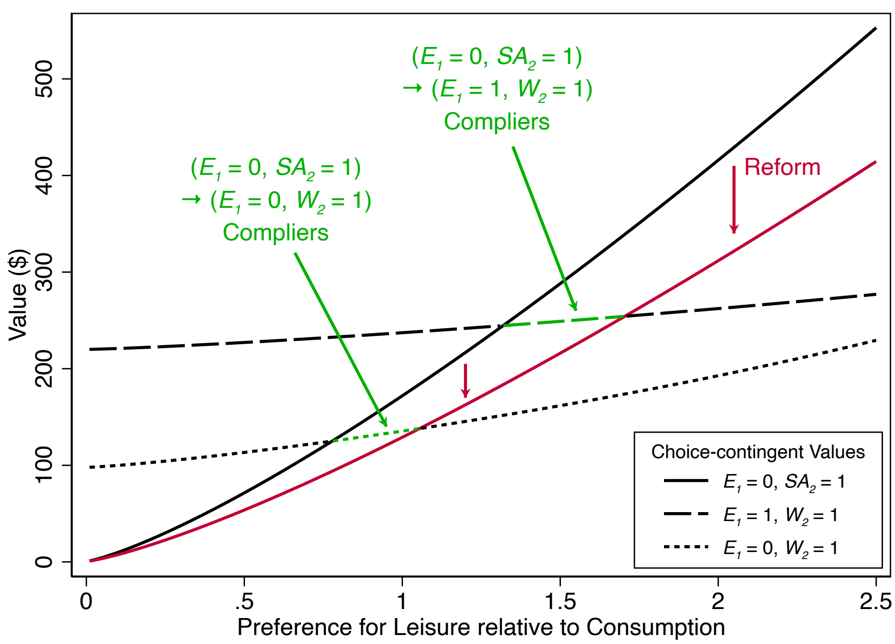
Reform starts in adolescence, $\boldsymbol{\alpha = 1}$. In this case, the agent can now *anticipate* being affected by the reform during adulthood. Agents who would have selected $E_t = 0$ under $Z_1^{SA} = 0$ may now otherwise choose to education. Intuitively, agents may be incentivized to invest in their human capital through education during adolescence in an effort to smooth their consumption and leisure in adulthood. Panel (a) shows that for sufficiently large returns to education in the wage rate, agents may shift into education and work (i.e., from $(E_1 = 0, SA_2 = 1)$ to $(E_1 = 1, W_2 = 1)$. Panel (a) illustrates these kinds of compliers also appear among agents with sufficiently high preferences for leisure relative to consumption.

Figure E.1: Comparative statics of the simple two-period model

(a) Varying returns to education



(b) Varying preferences for leisure



APPENDIX F

PROOF OF PROPOSITION 1

First, note that

$$\beta_{\alpha,a}^{Antic}(18,\infty) = \beta_{\alpha,a} - \mathbb{E}\left[Y_{ia}(\infty,18,\infty) - Y_{ia}(\infty) \,|\, D_i = \alpha, G_i = 1\right].$$

Also, note that $\mathbb{E}[Y_{i17}(\infty,18,\infty) \,|\, D_i = \alpha', G_i = 1] = \mathbb{E}[Y_{ia}(\infty) \,|\, D_i = \alpha', G_i = 1]$, $\forall \alpha' \in \mathcal{A}$, since ITT effects that start at age 18 cannot affect potential outcomes in previous ages under no anticipation. This implies that Assumption 2 may be rewritten as

$$\begin{aligned}
&\mathbb{E}\left[Y_{ia}(\infty,18,\infty) \,|\, D_i = \alpha, G_i = 1\right] \\
&= \mathbb{E}\left[Y_{i17}(\infty) \,|\, D_i = \alpha, G_i = 1\right] \\
&\quad + \left(\mathbb{E}\left[Y_{ia}(\infty) \,|\, D_i = \alpha, G_i = 0\right] - \mathbb{E}\left[Y_{i17}(\infty) \,|\, D_i = \alpha, G_i = 0\right]\right) \\
&\quad + \left(\mathbb{E}\left[Y_{ia}(\infty,18,\infty) \,|\, D_i = 18, G_i = 1\right] - \mathbb{E}\left[Y_{i17}(\infty) \,|\, D_i = 18, G_i = 1\right]\right) \\
&\quad - \left(\mathbb{E}\left[Y_{ia}(\infty) \,|\, D_i = 18, G_i = 0\right] - \mathbb{E}\left[Y_{i17}(\infty) \,|\, D_i = 18, G_i = 0\right]\right).
\end{aligned}$$

Substituting this expression into the first equation and rearranging terms yields

$$\begin{aligned}
\beta_{\alpha,a}^{Antic}(18,\infty) = \beta_{\alpha,a} - \beta_{18,a}& \\
&+ \left(\mathbb{E}\left[Y_{ia}(\infty) \,|\, D_i = \alpha, G_i = 1\right] - \mathbb{E}\left[Y_{i17}(\infty) \,|\, D_i = \alpha, G_i = 1\right]\right) \\
&- \left(\mathbb{E}\left[Y_{ia}(\infty) \,|\, D_i = \alpha, G_i = 0\right] - \mathbb{E}\left[Y_{i17}(\infty) \,|\, D_i = \alpha, G_i = 0\right]\right) \\
&- \left(\mathbb{E}\left[Y_{ia}(\infty) \,|\, D_i = 18, G_i = 1\right] - \mathbb{E}\left[Y_{i17}(\infty) \,|\, D_i = 18, G_i = 1\right]\right) \\
&+ \left(\mathbb{E}\left[Y_{ia}(\infty) \,|\, D_i = 18, G_i = 0\right] - \mathbb{E}\left[Y_{i17}(\infty) \,|\, D_i = 18, G_i = 0\right]\right).
\end{aligned}$$

Invoking Assumption 1 completes the proof.