THE UNIVERSITY OF CHICAGO

ESSAYS ON BEHAVIORAL RESPONSES TO HEALTH POLICY

A DISSERTATION SUBMITTED TO
THE FACULTY OF THE IRVING B. HARRIS
GRADUATE SCHOOL OF PUBLIC POLICY STUDIES
IN CANDIDACY FOR THE DEGREE OF
DOCTOR OF PHILOSOPHY

BY
REBECCA MYERSON

CHICAGO, ILLINOIS
JUNE 2016

CONTENTS

LIST OF FIGURES

LIST OF TABLES

ACKNOWLEDGMENTS

I am grateful to my friends and family - especially my parents, my brother Daniel, Carolina Mendez, Huizhen Tang, and Roberto Robatto - for all your love, support, and advice along the way. You have made this journey a wonderful one. I am also grateful for the valuable guidance of my advisors David Meltzer, Dan Black, James Heckman and Elbert Huang. My dissertation was also generously supported by an R36 dissertation grant and T32 training grant from the Agency for Healthcare Research and Quality. I also want to thank everyone at the University of Chicago for their support along the way including many friends and mentors at the Harris School, the Population Health Sciences department, and economics department. Training with you has made a world of difference to me.

"Chasing the Missing Patients: Exploring the Unintended Consequences of Free Health Screenings" is co-authored work with Lisandro Colantonio, Monika Safford, and David Meltzer, and "Does Identification of Previously Undiagnosed Conditions Change Patient Care Seeking Behavior?" is co-authored work with Lisandro Colantonio, Monika Safford, and Elbert Huang. We are grateful for valuable feedback and suggestions from Roberto Robatto and Carolina Mendez as well as participants of the Health Economics Workshop of the University of Chicago, the Harris School PhD Workshop, the Chicago Center for Diabetes Translation Research (CCDTR) Workshop, the Health Economics Symposium at DePaul University, the Illinois Economic Association conference, the National Research Service Award conference, the Population Association of America conference, the Association for Public Policy and Management conference, and the Population Association of America conference.

The project "Policy Analysis with Endogenous Migration Decisions: The Case of Left-Behind Migrant Children in China," was improved by valuable feedback and suggestions from Pietro Biroli, Steven Durlauf, Sara Moreira, Jesse Naidoo, Roberto Robatto, Bradley

Setzler, and participants of the East Asia Workshop and Center for Program Evaluation workshops at the University of Chicago, the Population Association of America conference, the Michigan Development Day conference, and the Association for Public Policy and Management conference.

The projects "Chasing the Missing Patients: Exploring the Unintended Consequences of Free Health Screenings" and "Does Identification of Previously Undiagnosed Conditions Change Patient Care Seeking Behavior?" are supported by a cooperative agreement U01 NS041588 from the National Institute of Neurological Disorders and Stroke, National Institutes of Health, Department of Health and Human Service. The content is solely the responsibility of the authors and does not necessarily represent the official views of the National Institute of Neurological Disorders and Stroke or the National Institutes of Health. Representatives of the funding agency have been involved in the review of the manuscript but not directly involved in the collection, management, analysis or interpretation of the data. We thank the other investigators, the staff, and the participants of the REGARDS study for their valuable contributions. A full list of participating REGARDS investigators and institutions can be found at http://www.regardsstudy.org.

ABSTRACT

The three essays of my dissertation examine how behavioral responses to health policy can shape, and sometimes undermine, the intended impact of policies.

The first essay, *Chasing the Missing Patients: Exploring the Unintended Consequences of Free Health Screenings*, examines two possible unintended consequences of removing cost sharing for health screening for people at high risk for chronic conditions, as is done in the Affordable Care Act. First, free screenings could attract patients with lower uptake of medical treatment, reducing the impact of the policy on treatment and changing the composition of diagnosed patients. Second, expanding screening could increase adverse selection and reduce the stability of health insurance markets. Using data from three biomarker studies reflecting different populations affected by the Affordable Care Act, we find evidence for the former prediction but not the latter. This essay is joint work with Lisandro Colantonio, Monika Safford, and David Meltzer.

The second essay, *Does Identification of Previously Undiagnosed Conditions Change Patient Care Seeking Behavior?,* shows that screening leads to doctor visits for previously undiagnosed conditions for many but not all patients, with marginally lower effects among patients lacking a usual healthcare provider. To identify the effects of screening, we exploit the REasons for Geographic And Racial Differences in Stroke (REGARDS) epidemiological study as a natural experiment. This essay is joint work with Lisandro Colantonio, Monika Safford, and Elbert Huang.

The third essay, *Policy Analysis with Endogenous Migration Decisions: The Case of Left-Behind Migrant Children in China,* models parental decisions as responses to local policy to show that migration effects could undermine the benefits of place-specific government services for children. Addressing a puzzle in the empirical literature on children who are left-behind by migrant parents, I use a theoretical model to sign the effect of being

left-behind on child well-being for a policy-relevant subset of children: children who become left-behind as a result of a policy change. For these children, becoming left-behind reduces their well-being. To show that these theoretically derived effects could be empirically important, I use panel data on Chinese families before and after a health policy change.

CHAPTER 1

CHASING THE MISSING PATIENTS: EXPLORING THE UNINTENDED CONSEQUENCES OF FREE HEALTH SCREENINGS

1.1 Introduction

Many people, including some with health insurance, are not screened for conditions that can be asymptomatic and can be treated to prevent illness. For instance, diabetes, high cholesterol, and hypertension are top contributors to cardiovascular disease and end-stage renal disease in the United States and about one-fifth of cases are undiagnosed [1–6]. In order to recruit patients who have unmet health needs due to lack of awareness (i.e., "missing" patients) as an entry point for treating their conditions, the Affordable Care Act (hereafter, ACA) requires health insurance plans to offer free screening for diabetes, high cholesterol, and hypertension to people at high risk.[1] This policy has already affected the health plans of an estimated 76 million people [7]. Due to the effectiveness of available treatments,[2] the policy could have a substantial health impact if this policy increases treatment of chronic conditions.

Although it may seem intuitive that subsidizing screening should increase treatment of chronic conditions, in fact the size of the effect is unclear for two reasons that we explore

1. Section 2713, "Coverage of Preventive Services," requires that health insurance issuers "not impose any cost sharing requirements for evidenced-based items or services that have in effect a rating of A or B in the current recommendations of the United States Preventive Services Task Force." Accordingly, no cost sharing is required for blood pressure screening for all adults, diabetes screening for adults aged 40-70 who are overweight or obese, and cholesterol screening for all men aged 35+ (or at lower age with elevated risk) and all women at elevated risk aged 20+. Sections 4103 and 4104 of the Affordable Care Act also eliminate cost sharing for annual wellness visits for Medicare beneficiaries.

2. See [8–17] for evidence on the effectiveness of treatments.

in this paper. The first reason is that patients with undiagnosed conditions may differ from other patients in their demand for health care after diagnosis. Our logic is as follows. Patients who are unaware of some of their conditions are less likely than patients who are aware of all their conditions to have been screened recently. This lack of screening may have resulted from their lower perceived benefits of medical treatment or higher perceived barriers to medical treatment.[3] These barriers to care could include out-of-pocket costs, or non-pecuniary costs such as distance to a physician, language barriers, or psychological costs [18,25–28]. These same barriers to medical treatment could then translate to lower treatment rates for newly detected conditions.

Second, increased screening could make medical treatment less affordable due to indirect effects. If patients switch into more generous health plans after learning that they are sick and health plans cannot use pre-existing conditions in deciding the premium price, then generous health plans must increase premium prices for all patients to cover the additional costs. This would be an example of adverse selection, the process by which patients sort into more generous insurance based on anticipated health needs that are known to them but are not incorporated into premium prices [29–31]. When adverse selection compounds over time, there can be major implications for how well a health insurance market can function [32,33]. Previous studies have shown that increases in adverse selection can unravel the benefits to providing patients with information [34].

This paper contributes to the literature by providing the first empirical assessment, to our knowledge, of these two concerns. First, we examine whether people who had undiagnosed conditions prior to low-cost screenings differ from others in their uptake of physician-recommended treatment for diagnosed conditions. This is important for providers to know whether additional outreach could be necessary to engage patients whose chronic

3. For evidence on screening, see [18–21]. For a discussion of self-selection into treatment and related econometric approaches, see, e.g., [22–24].

conditions become diagnosed as a result of a change in the price of screening. We examine this issue using data from three biomarker studies that paid participants to collect their biomarkers. These include two national biomarker studies, the National Health and Nutrition Examination Survey (hereafter, NHANES) and the Reasons for Geographic and Racial Differences in Stroke study (hereafter, REGARDS), and one regional biomarker study, the Oregon Health Insurance Experiment (hereafter, OHIE) [35–37]. These three studies reflect different groups affected by the ACA policy: all people with health insurance, people with Medicare insurance, and people who would apply for expanded Medicaid, respectively. We find that people who were not recently screened for undiagnosed conditions are less likely to receive recommended medical treatment for their previously diagnosed conditions; likewise, people with some undiagnosed conditions are less likely to treat their previously diagnosed conditions.[4] We therefore project that marginally screened people may also be less likely to receive recommended medical treatment after their currently undiagnosed conditions are diagnosed. This could reduce the health benefits of subsidizing screening and affect physician practice.

Second, we use the REGARDS study as a natural experiment to examine whether people select health plans with lower cost-sharing after learning of a previously undiagnosed condition. Understanding how low-cost screening affects health care markets is important to predict the impacts of changes to screening policy in health insurance markets with many undiagnosed patients, such as the health insurance marketplaces established under the ACA. We implement the test using panel data on Medicare plan selection that have been merged with the survey and biomarker data of 9,990 REGARDS participants, 20% of whom had an undiagnosed case of diabetes, high cholesterol and/or hypertension. Because

4. The former finding relates more directly to our goal of understanding how the marginally screened patients differ from previous patients. It is also slightly more robust, showing statistical significance at the 5% level in 28 out of 30 tests and at the 10% level in 30 out of 30 tests. Screening histories are not available in all three datasets, however, and therefore we include both analyses.

these REGARDS participants were recruited and screened on a rolling basis over 2003-2007, we can compare not-yet-recruited participants to recently-recruited participants to determine the impact of participation in low-cost screening on enrollment in Medicare Advantage while adjusting for secular trends. We find no evidence that participants with undiagnosed conditions enrolled in Medicare Advantage, health plans with lower overall cost-sharing than fee-for-service Medicare, after low-cost screening. This may indicate that participants with undiagnosed conditions did not anticipate spending much money on the new conditions. This result should be interpreted with caution, however, as our data do not include details on cost sharing for specific health plans and we do not observe switches across Medicare Advantage plans. If generalizable, the results would imply that subsidizing screening will not increase sorting of sicker patients into more generous health plans, i.e., adverse selection. This is important because adverse selection is a prominent concern when health status cannot be incorporated into premium prices, as is the case under the ACA [38].

An association between demand for medical treatment and demand for screening could explain both findings. In particular, patients with lower demand for medical treatment would have lower treatment of existing conditions and also less reason to adversely select after learning of a previously undiagnosed condition. To show that such an association is plausible, we analyze a theoretical model based on the Picone interpretation of the Grossman model [39,40]. In the model, agents can consume medical treatment to ameliorate the negative health effects of a chronic health condition, but only if they have been screened and diagnosed for the condition. Agents who have been recently screened know whether they have a chronic health condition, whereas agents who have not been recently screened hold beliefs about the probability that they have a condition. Agents differ only in the costs they face for medical treatment. We use the model to show that, as intuited above,

costs both reduce patients' demand for medical treatment after diagnosis and reduce their demand for screening.

The paper proceeds as follows. Section 1.2 compares our study with previous literature. In section 1.3, we analyze a theoretical model of demand for health screening and demand for medical treatment to show why the two could be linked. Section 1.4 shows empirical evidence of a relationship between use of medical treatment for previously diagnosed conditions and use of prior screening (directly measured, or proxied by presence of undiagnosed conditions). Section 1.5 shows no evidence of adverse selection after patients learn of a previously undiagnosed condition, and describes how we use the REGARDS epidemiological study as a subsidy to screening with random roll-out to identify this relationship. Section 3.7 concludes.

1.2 Comparison with the literature

Anticipated costs and benefits of health care can differ across individuals, influencing individuals' willingness to pursue care [23,24,41]. This premise underlies commonly used public health models such as the health belief model.[5] It follows that anticipated net benefits of particular health services can vary across individuals [44]. In certain cases, distributions of these individual-level net benefits can be estimated [22,23,45]. These distributions are useful because changes to out-of-pocket costs of health care will attract different patients to use the treatment based on their anticipated cost and benefit [46–48]. A number of recent papers use new econometric methods to estimate distributions of net benefits of specific health services. These papers typically focus on how patients choose between treatments for their conditions (i.e., the intensive margin) [45,49–54]. In contrast, our theo-

5. See [42] for a review of commonly used health behavior models in the public health field. The health belief model includes perceived benefits and perceived barriers as a key construct, and these are the constructs that are most strongly predictive of behavior in empirical tests [25,43].

retical model considers distributions of anticipated net benefits of screening, a determinant of which conditions are *not* treated (i.e., the extensive margin).

When analyzing adverse selection, we use panel data to avoid conflating adverse selection with other factors [55–59]. In particular, the observed correlation between health care claims and insurance generosity in cross-sectional data reflects two effects: (a) sicker individuals select more generous insurance based on their private knowledge of their healthcare needs (adverse selection), and (b) generous insurance reduces incentives to avoid claims due to the lower out-of-pocket price of curative care (moral hazard). Although both forces can cause a positive correlation between claims and insurance generosity, the two problems have different implications for public policy [60]. We sidestep this issue by using a strategy originally proposed by Abbring and co-authors, namely, using panel data combined with shocks to the cost or benefit of claims to identify the two forces separately [55,56]. Abbring and co-authors exploit shocks in the cost of claims in a bonus-malus car insurance payment scheme to identify moral hazard separately from selection; subsequently, Spenkuch exploits shocks in the cost of health insurance due to randomized roll-out of Mexico's Seguro Popular [59]. Our identification strategy exploits changes to the perceived benefits of generous insurance [57].

With respect to the model, our approach is based on the most commonly used economic framework for health investment, the Grossman health capital model [40]. In this model, agents make decisions about how much time and money to invest in health to maximize their utility given practical constraints. In Grossman's original health capital model, there was no uncertainty: agents had perfect knowledge about their health and about the health production process.[6] Previous research has incorporated uncertainty about how

6. As a result, it is difficult to use the model to discuss preventive care as distinct from curative care [61]. Perhaps because individuals demand more curative care when they are sick, the model's predictions of a positive relationship between health and demand for health care was not upheld in empirical tests [62]. This is still a critique of the Grossman model [63].

health investments translate to future health and productivity into the model using random shocks [64–66]. Many of these papers incorporate uncertainty into a Grossman model where health investment is motivated chiefly by labor market returns [67–69]. Because we want our results to generalize to agents who are not in the labor force, we follow an approach closer to that of Picone and co-authors, who simulate the effects of uncertainty about health on demand for preventive medical care among retirees [39]. We expand on this literature by focusing on endogenous lack of screening, rather than exogenous shocks, as agents' key source of uncertainty about their health.

1.3 Theoretical model

In this section, we analyze a model of demand for screening and demand for medical treatment after diagnosis to show one reason why the two could be mechanically related. In the model, agents use medical treatment to ameliorate the negative effects of chronic health conditions. Agents who have been recently screened know whether they have a chronic condition, whereas agents who have not been recently screened hold beliefs about the probability they have a chronic condition. Agents differ only in their costs of medical treatment; we separately model pecuniary and non-pecuniary costs. We analyze this model to derive predictions about which agents are willing to pay more for screening.

Agents maximize a continuously differentiable function of health (H) and consumption (C), net of disutility of medical treatment. Disutility of medical treatment due to non-pecuniary costs is linear in units of medical treatment M and the magnitude of disutility from non-pecuniary costs is captured by θ, which varies across agents.[7] The utility func-

7. Non-pecuniary costs could be related to factors such as language barriers, distance to a provider, depression symptoms or other psychological factors which provide barriers to care, etc.

tion is therefore:

$$u\left(C, H\left(M, D\right)\right) - \theta M$$

$u\left(\cdot\right)$ is concave in C and H, and agents have weakly higher marginal utility from consumption when they are healthier.

Health does not affect income, as in the pure consumption version of the Grossman model [66]. To keep notation simple, we assume that agents have assets A and receive no further income. If an agent has a chronic condition, then $D = 1$; otherwise, $D = 0$. If $D = 1$ and the agent has been diagnosed, then he must decide how to divide his funds between medical treatment ($M \geq 0$ units, purchased at a price P per unit where P can vary across agents), and other consumption (C). This yields the budget constraint:

$$C + PM = A$$

If the agent does not have a diagnosed condition, he is not eligible to receive medical treatment. In this case, therefore, the entire budget is spent on other consumption: $C = A$.

Health H is a function of medical treatment M and chronic condition status D, as follows. When agents have a chronic condition, health becomes worse: $H\left(M, 0\right) > H\left(M, 1\right) \forall M$. However, medical treatment improves health for agents with chronic conditions: $\frac{\partial H(M,1)}{\partial M} > 0 \forall M$.

Because doctors only provide medical treatment to patients who are diagnosed for a condition, an agent's utility and decision variables vary based on whether he has been screened and the results of the screening. There are three possible cases:

1. The agent has not been recently screened and does not know whether he has a chronic condition, but has (correct) beliefs about π, the probability that he has a chronic condition. Because the agent is not diagnosed, he cannot receive medical treatment

($M = 0$) and therefore uses all funds for consumption. His expected utility is therefore:

$$\pi u(A, H(0,1)) + (1-\pi) u(A, H(0,0)) \tag{1.1}$$

2. The agent has been recently screened and knows he does not have a chronic condition ($D = 0$).[8] He is not eligible for medical treatment and therefore uses all funds for consumption. His utility is:

$$u(A, H(0,0)) \tag{1.2}$$

3. The agent has been recently screened and knows he has a chronic condition ($D = 1$). Therefore, the agent can choose to use medical treatment. As such, the agent selects M and C to maximize his utility:

$$\max_{C,M} u(C, H(M,1)) - \theta M \tag{1.3}$$

subject to $C + PM = A$.

Screening moves agents from case (1) to case (2) or (3) depending on the results of the test.

Equations (1.1), (1.2), and (1.3) can be combined to describe agents' willingness to pay for screening. In particular, agents are indifferent between being screened and not being

8. For simplicity, we present the case where the test is perfectly informative. This assumption can be relaxed without altering the main results.

screened at out-of-pocket price of screening κ if:

$$\pi\left(\max_{M} u\left(A-PM-\kappa, H\left(M,1\right)\right)-\theta M\right)+\left(1-\pi\right)u\left(A-\kappa, H\left(0,0\right)\right)$$
$$-\left(\pi u\left(A, H\left(0,1\right)\right)+\left(1-\pi\right)u\left(A, H\left(0,0\right)\right)\right)=0 \qquad (1.4)$$

We can then define κ^* as the price of screening that makes any given agent just indifferent between being screened and not being screened. As such, κ^* captures the agent's willingness to pay for screening.

1.3.1 Optimal decisions after screening detects a chronic health condition

In this case, the agent is eligible for medical treatment and can choose his consumption of medical treatment and other goods. The optimal solutions, denoted M^* and C^*, are defined by the first order condition:

$$\frac{\partial u\left(C, H\left(M^*,1\right)\right)}{\partial H}\frac{\partial H\left(M^*,1\right)}{\partial M}-\theta=P\frac{\partial u\left(C^*, H\left(M^*,1\right)\right)}{\partial C} \qquad (1.5)$$

The left-hand side of equation (1.5) indicates the utility gains from consuming a unit of medical treatment. $\frac{\partial u(C,H(M^*,1))}{\partial H}\frac{\partial H(M^*,1)}{\partial M}$ is the utility benefit from improved health and $-\theta$ is the disutility of consuming a unit of medical treatment due to non-pecuniary costs. The right-hand side of equation (1.5) indicates the utility gains from spending P additional dollars on consumption rather than on medical treatment. Therefore equation (1.5) indicates that at the optimal point, the marginal benefits of purchasing a unit of medical treatment equal the marginal benefits of using the same funds for consumption.

1.3.2 Analysis of marginally screened agents and empirical predictions

We now show that agents who become willing to be screened after a decrease in the out-of-pocket price of screening use less medical treatment after diagnosis than already screened individuals. This follows from two propositions.

Proposition 1.3.1 *Willingness to pay for screening is decreasing in agents' costs of medical treatment:* $\frac{\partial \kappa^*}{\partial \theta} < 0$ *and* $\frac{\partial \kappa^*}{\partial P} < 0$*, respectively.*

The proofs are based on the envelope theorem. See Appendix 1.D.

Proposition 1.3.2 *Demand for medical treatment after diagnosis is also decreasing in agents' costs of medical treatment:* $\frac{\partial M^*}{\partial \theta} < 0$ *and* $\frac{\partial M^*}{\partial P} < 0$*.*

See Appendix 1.E for the proofs.

Based on these propositions, higher costs of medical treatment decrease agents' demand for medical treatment after diagnosis, and also agents' decrease willingness to pay for screening. The implications for a policy that decreases the out-of-pocket price of screening when costs of medical treatment vary across agents are as follows. First, decreasing the out-of-pocket price of screening will attract agents with marginally lower willingness-to-pay for screening (κ^*) to become screened. Agents with marginally lower κ^* will also face marginally higher costs (θ and/or P) by Proposition 2.1. In turn, higher costs for medical treatment imply that these agents will use less medical treatment for their diagnosed conditions than previously screened agents by Proposition 2.2. This produces the empirical prediction that patients whose conditions become diagnosed because of a decline in the out-of-pocket price of screening use less medical treatment for their conditions after diagnosis.

If we allowed the benefits of medical treatment rather than the costs of medical treatment to vary across agents, we could produce the same empirical predictions in some cases.

However, this exercise would be complicated by the fact that patients learn about the benefits of medical treatment through screening. In practice, patients are unaware of the presence or severity of their asymptomatic conditions prior to screening, and these factors determine the benefits of medical treatment. This presents the problem that patients' ex ante beliefs about the benefits of medical treatment determine their willingness to pay for screening, whereas their (different) ex post beliefs determine demand for medical treatment. If all patients hold correct beliefs about the benefits of medical treatment after screening, then gaps in care of diagnosed conditions among newly-screened and previously-screened patients would only appear if the true benefits to treatment were lower for newly diagnosed patients. This could be the case if newly diagnosed patients have less severe conditions overall. We explore this possibility in the empirical analysis to follow, and conclude that condition severity is unlikely to account for our findings.

1.4 Links between screening and treatment of previously diagnosed conditions on the individual-level

In this section we show that individuals who are not regularly screened, or whose undiagnosed conditions are detected after an epidemiological study pays them to be screened, differ from other individuals in their use of treatment for their previously diagnosed conditions. This pattern remains after adjusting for insurance status, condition severity, and prevalence of diagnosed and undiagnosed comorbid conditions. We also replicate the results using data from three groups that are likely to be affected by ACA provisions limiting cost-sharing for screening. If treatment of previously diagnosed conditions predicts care for newly diagnosed conditions, these results imply that the impact of low-cost screenings on treatment of diabetes, hypertension, and high cholesterol could be blunted by the lower use of medical treatment among patients with previously undiagnosed conditions.

1.4.1 Data

The analysis uses data from three studies: the National Health and Nutrition Examination Survey (NHANES), the REasons for Geographic and Racial Differences in Stroke study (REGARDS), and the Oregon Health Insurance Experiment baseline biomarker data (OHIE). In all three studies, participants reported their diagnosed conditions in a survey, had their biomarkers taken to identify undiagnosed conditions, and were paid for their time. Table 3.1 summarizes the sample selection and characteristics of included participants for these three studies.

NHANES is a nationally representative biomarker survey run by the Center for Disease Control and Prevention. Comparable data have been collected on a rolling basis from 1999-2013, and these are the data most commonly used to track awareness of chronic conditions over time on the national level [35]. REGARDS is an epidemiological study of older adults that recruited participants across the continental United States over 2003-2007 using a commercial list of residential phone numbers [37]. The REGARDS data have been linked with administrative records of doctor visits for participants enrolled in traditional Medicare [70]. Finally, the Oregon Health Insurance Experiment (OHIE) baseline biomarker study was conducted during 2009-2010 and sampled adults who entered a lottery to apply for Medicaid in Oregon in 2008 [71,72].

These three datasets have different advantages and disadvantages for our analysis. First, the NHANES data provide self-reported information on whether a doctor recommended managing hypertension and high cholesterol using a prescription, whereas the REGARDS and OHIE data do not. This is important because national guidelines recommend treating less severe cases of these conditions with diet and exercise before prescribing medication [15,16]. By tracking medication use only among participants who report that their doctor recommended medication, we can ensure that our results are not driven by medication non-

use among patients whose doctors recommended controlling the condition through diet and exercise alone. As a nationally representative survey, the NHANES also samples the most diverse group of participants. The OHIE baseline data have a different advantage for the present analysis. Adding these data allows us to pursue a focused analysis of a group of importance for the ACA: applicants to expanded Medicaid. In Medicaid expansion states, many patients who become diagnosed due to the ACA could come from this group. Finally, the OHIE and NHANES data have the disadvantage of relying exclusively on participant self-report to measure diagnosis and treatment of conditions, which could be a source of measurement error [73]. To address this issue, we also analyze data from participants in REGARDS with merged Medicare claims from the the two years prior to participation. For this group, we can analyze claims data on doctor visits for evaluation and management of diagnosed diabetes, high cholesterol, and/or hypertension rather than relying only on self-reported data.

Identifying diagnosed and undiagnosed conditions In each of the three datasets, we code participants as having a particular chronic condition (diabetes, hypertension, and/or high cholesterol) if they report prior diagnosis for the condition at the time of participation, if their biomarkers meet standard definitions for the condition after taking their fasting status into account, or if they are taking medications that indicate the condition in a medication review (if applicable for that study) [8,15,16]. Table 1.8 in the Appendix includes details of each definition. Individuals are classified as undiagnosed for the condition if they meet the biomarker definitions for a condition, but report no prior diagnosis for that condition.

In the REGARDS data, we correct for under-reporting by also classifying participants as diagnosed if they show biomarkers relevant to the condition and their doctors have been regularly evaluating and managing their condition based on recent claims. In particular, we also classify participants as diagnosed if they meet the biomarker definitions for a condition

Table 1.1: Characteristics of included participants from the three biomarker surveys

	NHANES	**OHIE Baseline Biomarker Data**	**REGARDS Medicare Data**
Survey Inclusion Criteria	Nationally representative	Applicants to expanded Medicaid	Traditional Medicare insured prior 2 years; black or white; English speaking
Geography of Sample	National	Oregon	National
Year of Biomarker Collection	2001-2014	2009-2010	2003-2007
Age Range in Analysis	All	19+	67+
Participants with Any Condition(s) of Interest	25,332	7,108	5,884
Participants with Undiagnosed Condition(s) of Interest	8,676	4,845	1,322
Among Participants with Condition(s) of Interest:			
Average Age	55	43	75
Had Health Insurance	82%	44%	100%
African American	21%	11%	30%
Participants with Diabetes	5,627	1,007	1,309
Aware of Diabetes	4,569	847	1,166
Taking Medication for Diabetes	3,911	819	1,161
Participants with Hypertension	15,598	3,117	4,502
Aware of Hypertension	13,675	2,169	4,051
Taking Medication for Hypertension	10,154	1,530	3,846
Participants with High Cholesterol	18,384	5,714	4,268
Aware of High Cholesterol	11,876	1,511	3,394
Taking Medication for High Cholesterol	6,139	976	2,457

and had two evaluation and management visits coded as relevant to that condition in the 24 months prior to REGARDS participation, i.e., meet Chronic Conditions Warehouse definitions for the condition based on their claims data at the time of REGARDS participation. This process increases the number of diagnosed cases of high cholesterol by 148 (4%), the number of diagnosed cases of diabetes by 26 (2%), and the number of diagnosed cases of hypertension by 119 (2%).

1.4.2 Analysis

We first use NHANES data to show that use of recommended treatment for diagnosed conditions is lower among individuals with undiagnosed conditions. Bivariate regressions indicate that participants with undiagnosed conditions are less likely to report taking their prescribed medications for diagnosed hypertension or high cholesterol, or taking any medication for their diagnosed diabetes. (Doctors' recommendations to control hypertension and high cholesterol using medication are asked about in the NHANES, enabling us to track medication use only among diagnosed patients for whom medication was recommended. However, there is no comparable question for diabetes.) See Row 1 of Table 1.2.

One might argue that if people with undiagnosed conditions have less severe conditions overall, lower treatment rates and screening rates in this group would represent an appropriate allocation of resources. As noted previously, the tractability of this argument is limited by the fact that people cannot know the severity of asymptomatic conditions without screening, and most cases of hypertension and high cholesterol and many cases of diabetes are asymptomatic. Furthermore, we find in all three biomarker datasets that participants with undiagnosed conditions show more severe, not less severe, biomarkers for their diagnosed conditions. See Appendix Table 1.9.

We address this argument in the analysis by adding controls for patients' biomarkers including LDL and HDL cholesterol, HbA1c, and systolic and diastolic blood pressure to the bivariate analysis above. We also adjust for self-reported retinopathy, a diabetes symptom that is consistently measured across different waves of the NHANES survey, to account for the possibility that onset of diabetes symptoms could spur demand for treatment of diabetes and screening for other conditions. As shown in Row 2 of Table 1.2, findings are similar. Findings are also similar when we adjust for prevalence and comorbidity of other conditions rather than using continuous biomarkers, as shown in Row 3 of Table 1.2.

Another possibility is that high out-of-pocket costs for treating diagnosed conditions could reduce patients' willingness to be screened and also reduce their use of treatment of diagnosed conditions, as proposed in part 1.3. We cannot quantify the importance of this channel without knowing more about participants' out-of-pocket costs of care. Using the available data, however, we find that results remain similar when we control for predictors of out-of-pocket costs and access to care such as current health insurance status, lack of coverage at any point during the past year, current Medicaid coverage, current Medicare coverage, year of age, current calendar year, race, and current prescription coverage; we allow the effect of health insurance coverage on use of health care to vary by year by adding interactions. See Rows 4 and 5 of Table 1.2. We also find similar results when the analysis is repeated including only participants with health insurance; see Row 6 of Table 1.2.

The analysis above documents the relationships between prevalence of undiagnosed conditions and treatment of diagnosed conditions. However, prevalence of undiagnosed conditions is determined by two rates: prevalence of conditions, and frequency of screening. Therefore, we conduct an additional test to isolate the relationships between frequency of screening and treatment. In this test, we compare use of recommended treatment for diagnosed conditions among patients who do vs. do not self-report taking a blood test for diabetes or high cholesterol if not already diagnosed for these conditions in the past 3

Table 1.2: Prevalence of undiagnosed conditions and use of recommended care for diagnosed diabetes, hypertension, or high cholesterol (NHANES data)

	(1) Diabetic Eye Exam	(2) Diabetic Foot Exam	(3) Hypertension Meds	(4) Cholesterol Meds
(1) No controls				
Any undiagnosed conditions	-0.138***	-0.0813***	-0.0340***	-0.0276
	(0.0238)	(0.0229)	(0.00838)	(0.0169)
(2) Control for biomarkers				
Any undiagnosed conditions	-0.0975***	-0.0902***	-0.0162	-0.0282
	(0.0351)	(0.0335)	(0.0127)	(0.0231)
(3) Control for comorbidity				
Any undiagnosed conditions	-0.148***	-0.101***	-0.0660***	-0.0865***
	(0.0251)	(0.0239)	(0.00864)	(0.0173)
(4) Control for demographics				
Any undiagnosed conditions	-0.104***	-0.0585**	-0.0105	-0.0475***
	(0.0235)	(0.0228)	(0.00794)	(0.0164)
(5) All controls				
Any undiagnosed conditions	-0.0771**	-0.109***	-0.0129	-0.0657***
	(0.0361)	(0.0345)	(0.0127)	(0.0237)
(6) Insured only				
Any undiagnosed conditions	-0.0674*	-0.108***	-0.0100	-0.0586**
	(0.0396)	(0.0371)	(0.0126)	(0.0237)

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

This table shows the relationship between use of recommended treatment for diagnosed conditions and prevalence of undiagnosed diabetes, hypertension, and/or high cholesterol. The rows include coefficients and standard errors obtained from linear probability models after adjusting for the listed control variables. The outcomes in columns 1-2 are self-reported foot exams or eye exams in the past year among participants who reported prior diagnosis of diabetes. The outcome in column 3 is self-reported use of medication for hypertension among participants among participants who reported prior diagnosis of hypertension and reported that a doctor recommended anti-hypertensive medication. The outcome in column 4 is self-reported use of medication for high cholesterol among participants who self-reported prior diagnosis of high cholesterol and reported that a doctor recommended cholesterol-lowering medication. (Doctors' recommendations to control hypertension and high cholesterol using medication are asked about in the NHANES, enabling us to track medication use only among diagnosed patients for whom medication was recommended. However, there is no comparable question for diabetes.)

Table 1.3: Recent use of a blood test to screen for asymptomatic undiagnosed conditions and use of recommended care for diagnosed diabetes, hypertension, or high cholesterol (NHANES data)

	(1) Diabetic Eye Exam	(2) Diabetic Foot Exam	(3) Hypertension Meds	(4) Cholesterol Meds
(1) No controls				
Not screened last 3 years	-0.339***	-0.320***	-0.0372***	-0.0799***
	(0.0617)	(0.0604)	(0.0110)	(0.0142)
(2) Control for biomarkers				
Not screened last 3 years	-0.353***	-0.229***	-0.0395**	-0.0537***
	(0.0882)	(0.0871)	(0.0159)	(0.0194)
(3) Control for comorbidity				
Not screened last 3 years	-0.339***	-0.322***	-0.0460***	-0.0569***
	(0.0778)	(0.0767)	(0.0123)	(0.0152)
(4) Control for demographics				
Not screened last 3 years	-0.263***	-0.260***	-0.0391***	-0.0607***
	(0.0619)	(0.0612)	(0.0105)	(0.0137)
(5) All controls				
Not screened last 3 years	-0.322***	-0.256**	-0.0436***	-0.0385*
	(0.0982)	(0.0997)	(0.0158)	(0.0198)
(6) Insured only				
Not screened last 3 years	-0.310**	-0.300**	-0.0344**	-0.0391*
	(0.128)	(0.125)	(0.0158)	(0.0203)

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

This table shows the relationship between use of recommended treatment for diagnosed conditions and use of a blood test to check for asymptomatic diabetes or high cholesterol in the past 3 years. The rows include coefficients and standard errors obtained from linear probability models after adjusting for the listed control variables. The outcomes in columns 1-2 are self-reported foot exams or eye exams in the past year among participants who reported prior diagnosis of diabetes. The outcome in column 3 is self-reported use of medication for hypertension among participants among participants who reported prior diagnosis of hypertension and reported that a doctor recommended anti-hypertensive medication. The outcome in column 4 is self-reported use of medication for high cholesterol among participants who self-reported prior diagnosis of high cholesterol and reported that a doctor recommended cholesterol-lowering medication. (Doctors' recommendations to control hypertension and high cholesterol using medication are asked about in the NHANES, enabling us to track medication use only among diagnosed patients for whom medication was recommended. However, there is no comparable question for diabetes.)

years.[9] In other words, we compare participants who have vs. have not had a recent blood test to check for diabetes if they have not been diagnosed for diabetes, or blood test to check for high cholesterol if they have not been diagnosed for high cholesterol. (NHANES does not include data on time since last blood pressure test.) Table 1.3 shows the results. In harmony with the previous results, we find that use of recommended treatment for diagnosed conditions is lower for patients who have not taken a blood test to check for diabetes or high cholesterol in the past 3 years.[10] As in the previous analysis, we find that this relationship holds after adjusting for severity of diagnosed conditions, prevalence of comorbid conditions, and factors related to access to care such as race, age, and health insurance status, and that the relationship holds when we restrict the sample to only include participants with health insurance. Furthermore, the results are more consistently significant in Table 1.3 than Table 1.2, which would support a hypothesis that the relationship between undiagnosed conditions and treatment is driven by the underlying relationship between screening and treatment.

The analysis thus far has the shortcoming that diagnosis and treatment of chronic conditions are only measured using self-reported data. To address this shortcoming, we conduct additional checks using claims data available in the merged Medicare-REGARDS data. In this analysis, our main outcome of interest is doctor visits for evaluation and management of diagnosed conditions in the previous year, measured using Medicare claims assigned to conditions based on the Chronic Conditions Warehouse classifications. As shown in Table 1.4, we find that participants with previously undiagnosed conditions had fewer doctor

9. Similar questions are not available in the REGARDS data, and only available for cholesterol in the OHIE data. However, because the NHANES are nationally representative, results obtained using only the NHANES are still helpful for policy purposes.

10. Because we do not know the timing of diagnosis, we code the variable so that participants who have diagnosed diabetes need not be screened for diabetes, and participants with diagnosed high cholesterol need not be screened for high cholesterol.

visits for their previously diagnosed conditions. As before, this relationship holds after adjusting for severity of diagnosed conditions, prevalence of comorbid conditions, and factors related to access to care such as race, age, and Medicaid dual eligibility.

The role of health insurance is worth exploring further, because the ACA regulations that prevent health insurance plans from imposing cost-sharing for screening for patients at high risk directly change out-of-pocket costs for screening among individuals with health insurance.[11] Individuals with health insurance after the ACA can be broken into two groups: individuals who were insured prior to the ACA and remained insured, and individuals who become insured after the ACA. Some individuals in the latter group may have become insured as a result of elements of ACA implementation such as health insurance mandates, state-level exchanges, and Medicaid expansions. Table 1.5 replicates the analysis in Table 1.2 for each of these groups of interest, adjusting for prevalence of comorbid conditions and biomarker measures of condition severity. In row 1 of Table 1.5, we include data from individuals who were insured before the ACA provisions came into effect; this analysis uses the NHANES data. In rows 2 and 3 of Table 1.5, we include data from individuals who applied for expanded Medicaid in Oregon; this analysis uses the OHIE data. We separately analyze all applicants to the Oregon Medicaid program vs. applicants who were uninsured at the time of application. Finally, because additional ACA provisions eliminate cost-sharing for annual wellness visits among Medicare enrollees, row 4 of Table 1.5 includes data from Medicare enrollees prior to the ACA using the REGARDS data. The findings in Table 1.5 indicate that, among groups likely to be affected by the ACA provisions related to cost-sharing for screening, the patterns found in Table 1.2 largely persist.

---

11. An exception is the so-called "grandfathered" health plans, which covered enrollees before the ACA became law (March 23, 2010) and have not made substantial changes since that time. The 2015 Kaiser Family Foundation's Employer Health Benefits Survey found that 25% of covered workers were enrolled in a grandfathered health plan in 2015 [74].

Table 1.4: Prevalence of undiagnosed conditions and number of annual doctor visits for diagnosed diabetes, hypertension, or high cholesterol (REGARDS data)

	(1) Diabetes Claims	(2) Hypertension Claims	(3) High Cholesterol Claims
(1) No controls			
Any undiagnosed conditions	-1.089***	-0.617***	-0.446***
	(0.380)	(0.159)	(0.141)
(2) Control for biomarkers			
Any undiagnosed conditions	-1.122**	-0.743***	-0.377**
	(0.435)	(0.178)	(0.152)
(3) Control for comorbidity			
Any undiagnosed conditions	-1.260***	-0.864***	-0.543***
	(0.387)	(0.164)	(0.144)
(4) Control for demographics			
Any undiagnosed conditions	-1.396***	-0.685***	-0.408***
	(0.378)	(0.155)	(0.141)
(5) All controls			
Any undiagnosed conditions	-1.140***	-0.895***	-0.353**
	(0.435)	(0.182)	(0.155)

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

This table shows the relationship between doctor visits for diagnosed conditions and prevalence of undiagnosed diabetes, hypertension, and/or high cholesterol. Due to the use of Medicare claims data, we only include individuals who had fee-for-service Medicare insurance the two years prior to REGARDS participation. The rows indicate coefficients of a linear regression model after adjusting for the listed control variables. The outcome in column 1 is the number of evaluation and management visits from the prior year coded as relevant to diabetes among participants with prior diagnosis of diabetes. Likewise, the outcomes in columns 2 and 3 are the number of evaluation and management visits from the prior year coded as relevant to hypertension or high cholesterol for participants with prior diagnosis of hypertension or high cholesterol, respectively.

Table 1.5: Replicating the previous analysis for groups likely impacted by ACA provisions that reduce cost-sharing for screening (NHANES, OHIE and REGARDS data)

	(1) Diabetes Meds	(2) Hypertension Meds	(3) Cholesterol Meds
NHANES data:			
(1) All insured prior to ACA			
Any undiagnosed conditions	0.0004	-0.0841***	-0.0767**
	(0.0337)	(0.0212)	(0.0317)
OHIE data:			
(2) All applicants for Medicaid			
Any undiagnosed conditions	-0.0634*	-0.0975***	-0.125***
	(0.0338)	(0.0262)	(0.0461)
(3) Uninsured applicants for Medicaid			
Any undiagnosed conditions	-0.0644	-0.104***	-0.138**
	(0.0516)	(0.0358)	(0.0601)
REGARDS data:			
(4) Medicare insured prior to ACA			
Any undiagnosed conditions	0.0103	-0.0165	-0.111***
	(0.0175)	(0.0149)	(0.0321)

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

This table uses data from the NHANES, OHIE, and REGARDS studies to investigate the relationship between undiagnosed conditions and treatment of diagnosed conditions. These three data sources are used to study individuals who had any health insurance prior to the ACA, who wished to apply for expanded Medicaid insurance prior to the ACA, and who had Medicare insurance prior to the ACA, respectively. All models adjust for severity of conditions measured using biomarkers and prevalence of comorbid conditions.

The findings in this section indicate that demand for screening is related to demand for medical treatment of previously diagnosed conditions in a way not fully explained by prevalence of comorbid conditions, condition severity, or health insurance status. In the next section, we show additional evidence of low demand for medical treatment among patients with undiagnosed conditions: a lack of adverse selection after participants are informed of a previously undiagnosed condition.

1.5 Diagnosis of a chronic condition and health plan selection

When patients sort into health insurance based on health risk factors that are known to them but cannot be incorporated into premium prices, this pattern - adverse selection - can cause health insurance markets to unravel. If patients demand more generous health insurance after learning about previously undiagnosed conditions, this would comprise adverse selection because diagnoses can no longer be taken into account in premium prices after the ACA [38]. Therefore, it is important to investigate whether patients switch to more generous health insurance after learning about previously undiagnosed conditions.

1.5.1 Data

To determine the impact of learning about previously undiagnosed conditions on health insurance plan selections, we use the REGARDS study as a natural experiment. REGARDS recruited participants from across the continental United States over a period of four years (2003-2007) by making random selections using a commercial list of residential phone numbers [37]. Participants first reported their diagnosis status in a survey, and then had their fasting blood glucose, blood pressure, and lipid panel assessed in their home on a morning of their choosing. All participants were notified of their results using standard text and paid $30 as compensation for their participation. It is because of the notifications

and compensation that REGARDS can be considered an intervention that provides free screenings in addition to an epidemiological study. Additional details on REGARDS data collection procedures are included in Appendix 1.B.

Outcome of interest

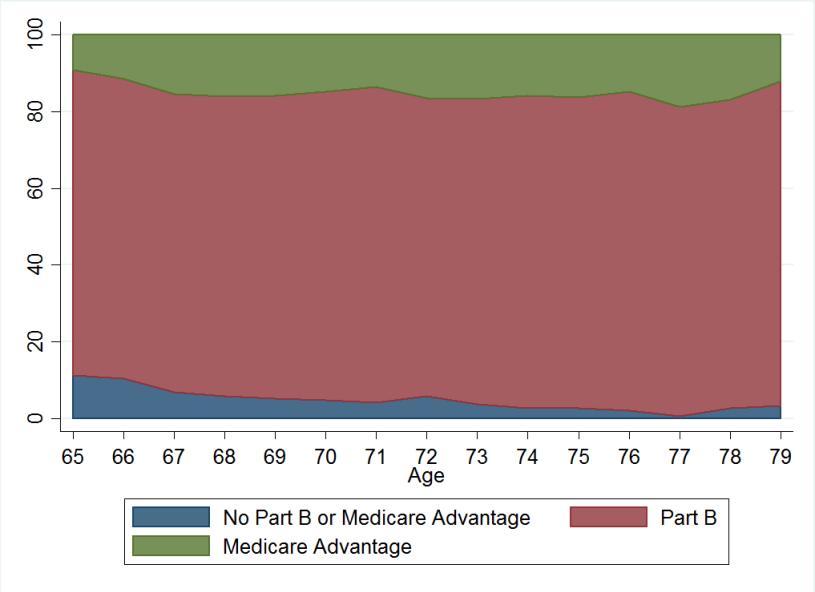
Linked Medicare data are available for REGARDS participants who were Medicare beneficiaries. These data track participants' health insurance plan selections on a monthly basis, including our outcome of interest: enrollment in Medicare Advantage, the voluntary HMO alternative to Medicare's default fee-for-service health plan (hereafter, traditional Medicare).

Enrollment in Medicare Advantage is not the only relevant margin for adverse selection in the Medicare market. Adverse selection could also involve purchasing supplemental coverage (Medigap plans), enrolling in physician coverage (Medicare Part B), or switching to a more generous Medicare Advantage plan. However, our data do not include information on Medigap plans and do not detail the generosity of Medicare Advantage plans. In addition, 95% of participants who were age-eligible for Medicare were already enrolled in Medicare Part B prior to participation in REGARDS; see Figure 1.1. Therefore, participants who were not enrolled in Medicare Part B prior to REGARDS are likely a highly selected sample which would raise concerns about including Part B enrollment as an outcome of interest.

Sample selection

This analysis uses data from REGARDS participants who were 63 years old or older at the time of REGARDS participation. As in the previous section, we only include participants who became Medicare beneficiaries as a result of turning 65 (rather than due to illness

Figure 1.1. Insurance decisions by age, measured in the month prior to REGARDS participation



This figure shows two trends in the Medicare plan selection data that inform our analytic strategy. First, almost all age-eligible Medicare enrollees were enrolled in Medicare Part B prior to REGARDS participation. (All participants in Medicare Advantage are also enrolled in Part B.) Second, Medicare Advantage enrollment varies by age: enrollment almost doubles from age 65 to age 67 and declines again after age 77.

or disability). Including people who participated in REGARDS before age 65 enables us to identify the effect of REGARDS participation on Medicare Advantage enrollment separately from age effects. This is important because age is a predictor of Medicare Advantage enrollment, as shown in Figure 1.1.

To ensure that any observed health plan switches could plausibly be attributed to REGARDS, we narrow our window of observation to the 24 months around the time of REGARDS participation. We therefore exclude participants with no health plan selection data available from the 24 months after their participation in REGARDS. These exclusion criteria yield a dataset with 9,990 REGARDS participants. See Table 1.6 for additional details.

Table 1.6: Participants cascade for analyzing the impact of learning of an undiagnosed condition on health insurance selections

Inclusion criterion	Participants
All REGARDS participants	30,183
<63 years old at REGARDS participation	-12,990
No Medicare-linked data	-3,368
Not observed for 24 months after participation	-2,391
Included	9,990

1.5.2 Context of the Medicare Advantage market and predicted effects

To provide context for the analysis to follow, it is useful to discuss the structure of the Medicare market, the role of Medicare Advantage, and how Medicare Advantage policy changed during our sampling period. Upon aging into Medicare at age 65, beneficiaries are automatically enrolled in traditional Medicare. Beneficiaries may then choose to enroll in Medicare Advantage plans in any subsequent year, including their first year of Medicare; beneficiaries may switch between Medicare Advantage and traditional Medicare as many times as they wish. Medicare Advantage plans receive capitated, risk-adjusted payments from the government and are required by law to be as generous as traditional Medicare in terms of covering a minimal suite of services, with total out-of-pocket payments (premiums and cost-sharing) not higher than traditional Medicare [75]. Plans attract customers by offering supplemental benefits such as vision or dental coverage, or by reducing patients' cost-sharing, made possible by negotiating physician payments in restricted provider networks [76].

Given the capitated-payment reimbursement system, Medicare Advantage plans can earn more money by attracting the patients with lowest health care costs within each capitated payment risk-adjustment bin. There is evidence that plans accomplished this task during the period of our data, despite requirements to accept all applicants [75]. Indeed, analysis of patients who switched plans indicate *advantageous* selection into Medicare

Advantage: that is, patients with increasing health care costs were likely to switch to traditional Medicare, and vice versa [75,77,78]. Plans reduced their exposure to high-cost patients by not entering higher-cost counties and by structuring provider networks and drug formularies so as to influence which patients wished to enroll [30,79,80].

A number of changes occurred in the Medicare Advantage market in 2006. First, Medicare Part D was introduced in 2006, allowing traditional Medicare to become more comparable to HMO coverge. Second, Medicare fought against advantageous selection into Medicare Advantage by phasing in an improved model for calculating individual-level risk-adjustment payments, the hierarchical condition categories model [75]. This model was over six times as predictive of expenditures as the previous model [81]. Third, Medicare sought to lower prices and increase generosity of Medicare Advantage plans to consumers by providing "rebates" as supplemental benefits to all enrollees in a given plan using a bid-based system [82,83]. Fourth, open enrollment and lock-in periods were enforced beginning in January 2006. Before 2006, beneficiaries could change plans once per month. In 2006, a lock-in period was introduced covering the latter six months of the year; in 2007 it was extended to 9 months [84]. This lock-in period resembles the open enrollment periods in the health insurance marketplaces established under the ACA.

Based on the particular features of the Medicare Advantage market, we hypothesize that a Medicare beneficiary who is currently enrolled in traditional Medicare will become more likely to enroll in Medicare Advantage after screening detected a previously undiagnosed chronic condition if several conditions hold: (a) she anticipates an increase in doctor visits as a result of the diagnosis; (b) there is a Medicare Advantage plan available to her that provides her anticipated bundle of services for a lower out of pocket price than that of traditional Medicare; and (c) the benefits to enrolling in Medicare Advantage are not outweighed by switching costs or hassles of a restricted provider network. If any of these conditions do not hold, the beneficiary will not wish to switch to Medicare Advantage after

learning of a screen-detected condition. Furthermore, if (b) does not hold, beneficiaries who are enrolled in Medicare Advantage may wish to switch back to traditional Medicare after learning of a screen-detected condition.

1.5.3 Analytic plan

Our empirical strategy compares health plan selections of recently screened vs. not-yet-screened REGARDS participants with vs. without undiagnosed conditions. We conduct these comparisons before and after policy changes that occurred in 2006 using the following specification of a linear probability model:

$$MA_{it} = \sum_{r \in (\text{pre } 2006, \text{ post } 2006)} \left(\mu_0^r + T_{it}\phi_1^r + T_{it}U_i\phi_2^r\right) + \mathrm{X}_{it}\mathrm{B} + \alpha_i + \omega_{it} \qquad (1.6)$$

MA_{it} indicates whether individual i was enrolled in Medicare Advantage t months before or since REGARDS, r indicates the policy regime at time t (before vs. after changes in January 2006), and U_i denotes whether the participant has any undiagnosed conditions. X_{it} denotes the control variables listed below and lower-level interaction terms, and α_i denotes an individual-level random effect. We use heteroskedasticity-robust standard errors clustered by individual.

The ϕ_2^r coefficients are the coefficients of interest in the model. If the ϕ_2^r are significantly different from zero, this would support a hypothesis that screening for diabetes, hypertension, and high cholesterol has an additional effect on plan selection for patients with previously undiagnosed conditions.

In additional analyses, we examine the influence of switching costs by controlling for polynomials of participants' log duration of enrollment in their current Medicare plan type [85]. This strategy leveraged the finding from previous literature that duration of plan

enrollment can proxy for switching costs or “status quo bias” in the Medicare market [86, 87].

As a robustness check, we also model switches into Medicare Advantage rather than modeling current enrollment. In this analysis, the specification of the right-hand side of the regression is the same as equation (1.6), but the outcome is a switch into Medicare Advantage. In this analysis, we restrict the data to only include person-months in traditional Medicare (i.e., time when participants were at "risk" for switching into Medicare Advantage).

To examine whether particular groups are driving the results, we also interact the coefficients of interest by participant characteristics relevant to plan selection, such as prior healthcare use or duration of enrollment in the current plan type. As before, all relevant lower-order interaction terms are included in the model.

Selection of control variables

Control variables are selected to address two possible biases. First, we expect that secular trends contribute to observed changes in doctor visits after REGARDS participation. For example, all participants were older after REGARDS participation than before REGARDS participation, and policy changes were implemented during our period of observation. These secular trends could bias our estimates if not controlled for in the model. The rolling nature of the implementation of screenings in the REGARDS study makes controlling for secular trends possible: there is random variation in age and calendar time of REGARDS participation because participants received their offers to participate using random phone calls over a number of years. To this end, we include a number of time-varying control variables including year dummies, interactions between region and year, and individual age, divided into 8 bins of equal size to allow for a non-linear relationship between

age and doctor visits. We also include an indicator variable for open enrollment season and an interaction with this variable with an indicator for 2006 or later.

Second, our results might be biased if the type of individual willing to participate in REGARDS changed over time. This would be problematic because the models compare not-yet-recruited participants with recently-recruited individuals to control for secular trends. Therefore, we also include a number of time-invariant control variables such as physical health measures taken at the time of REGARDS participation and a number of demographic and health-related characteristics from the REGARDS survey. In particular, we control for waist size in centimeters, BMI, glucose, lipid panel, the average of two blood pressure measures (both systolic and diastolic) and reported physical health from the SF-12; type of condition (high cholesterol, hypertension, or diabetes), and whether the condition was previously undiagnosed; race (African American or white), sex (male or female), income (less than $20,000, $20,000-<$35,000, $35-$75,000, and over $75,000), education (less than high school education, high school, some college education, or graduated from college), fair or poor self-reported health, usual health provider at the time of the interview (self-reported having or not-having a usual health provider), self-reported smoking status (current smoker, past smoker, or non-smoker), number of alcoholic drinks per week, fasting status at the time of the interview (fasting or not), cognitive status according to a short memory test (impaired or not), Medicaid dual eligibility in 2008 (eligible or not), status of county as a primary care health professional shortage area (all, part, or none of the county is a designated health professional shortage area), and the fraction of residents in poverty in the participant's county of residence. All continuous variables are binned into four categories of equal size to allow non-linearity in the relationships between these variables and health plan selections.

1.5.4 Results

We have data on Medicare Advantage enrollment status in the month prior to REGARDS participation for 9,239 included participants. 15% of these participants were enrolled in a Medicare Advantage plan and 95% of participants were enrolled in Medicare Part B. Participants who were enrolled in Medicare Advantage prior to REGARDS participation are more likely to be African American or low income than those who were not enrolled in Medicare Advantage, but have similar prevalence of undiagnosed conditions. See Table 1.10. These findings match previous published findings based on linked survey and Medicare records data from similar years [88].

The regression models show no evidence that participants switched from traditional Medicare to Medicare Advantage or vice versa after learning of a previously undiagnosed condition. These patterns hold both before and after the policy changes in 2006, and hold regardless of whether we analyze current enrollment in Medicare Advantage or analyze switches directly. The findings also do not change after we control for duration of enrollment in the current plan type, our proxy for inertia or switching costs. The coefficients of interest from these models are included in Table 1.7. The raw data show a similar pattern; see Figure 1.2.

The findings are also unchanged when we interact the coefficients of interest by participant characteristics relevant to plan selection such as prior claims or self-reported health. See Tables 1.11 and 1.12.

In summary, we find no evidence that free screenings provided by the REGARDS study resulted in adverse selection. This finding follows intuitively from our prediction in section 1.3 that marginally screened patients will have lower demand for medical treatment. If marginally screened patients have lower demand for medical treatment, they would also have less reason to adversely select after learning of a previously undiagnosed condition.

Table 1.7: Learning of previously undiagnosed diabetes, hypertension, or high cholesterol via REGARDS and subsequent enrollment in Medicare Advantage

	(1) In MA	(2) In MA	(3) Switch to MA	(4) Switch to MA
Prior to January 1, 2006				
After participation × Undiagnosed	-0.019*	-0.021	0.001	5.15e-05
	(0.010)	(0.015)	(0.002)	(0.003)
After January 1, 2006				
After participation × Undiagnosed	-0.013	-0.023	-0.000	0.003
	(0.0169)	(0.0199)	(0.003)	(0.004)
Observations	165,642	75,276	137,295	58,769
Number of participants	3,405	1,682	3,010	1,368
Control for duration dependence	N	Y	N	Y

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

MA: Medicare Advantage. This table shows that we fail to reject the null hypothesis of no significant association between learning of a previously undiagnosed condition and enrollment in, or switches into, Medicare Advantage. The rows of the table include coefficients of interest from the regression models described in equation (1.6). "Undiagnosed" denotes the participant had at least one undiagnosed condition (hypertension, high cholesterol, and/or diabetes) prior to participation in REGARDS. The results in columns 2 and 4 show that controlling for polynomials of log duration of enrollment in the current plan type, our proxy for inertia or switching costs, does not affect the results.

However, there are multiple reasons to interpret our empirical results with caution or doubt the external validity. Our data do not include details on cost sharing for specific health plans and we do not observe switches across Medicare Advantage plans. In addition, the results may be shaped by the particular environment of the Medicare Advantage market, such as the use of risk-adjusted per capita payment schemes. We conclude that although these estimates contribute to the literature by examining the relationship between screening and adverse selection in the Medicare market, this test bears repeating using richer data and data from other health insurance markets.

1.6 Conclusion

To incentivize early detection and treatment, the Affordable Care Act removes cost sharing for diabetes, high cholesterol, and hypertension screening for people at high risk. This paper explores two possible unintended consequences related to patient composition and adverse selection. In addition to suggesting new directions for health economics research, our two main findings have implications for policy-makers, health providers, and health insurance markets.

First, we find that patients whose conditions were not detected prior to free screenings seek less medical treatment than patients who were aware of all their conditions. We replicate this result using biomarker data from three datasets, with a focus on groups likely to be affected by the ACA provisions related to cost-sharing for screening. This analysis bridges the health policy literature on expanding access to care with models from economics and public health in which individuals act based on their anticipated costs and benefits. In addition, our findings are important for health providers and policy-makers. Given that health insurance expansions and changes in the price of screening should increase access to screening for a variety of patients, lower use of treatment among patients with newly diag-

nosed conditions could be an important trend in the care of chronic conditions nationwide. Furthermore, if analysts ignore these composition effects, they may incorrectly conclude that treatment and control of chronic conditions declines rather than improves as more patients become diagnosed. This point would be important to consider when designing pay for performance schemes.

Second, we find no evidence that patients switch into more generous health plans after learning of a previously undiagnosed condition. This result builds logically from the previous finding in the sense that patients who use less medical care have less reason to switch health plans after learning of a previously undiagnosed condition. However, because of the limitations of our data and the particular regulatory environment of the Medicare Advantage market, we urge that this result should be interpreted with caution and encourage replication of the analysis using data from other health insurance markets. Regardless, the analysis raises important questions for health insurance markets about the links between access to screening, patients' plan selections, and possible reactions to these selections by health insurance providers. This topic is important because sorting of sicker people into generous health plans is a prominent concern when health status cannot be incorporated into health insurance premium prices, as is the case in the ACA marketplaces.

Our findings suggest three additional directions for future research. First, as noted above, our findings have implications for pay for performance schemes. Accountable Care Organizations, established under the ACA, are health provider organizations that are allocated financial rewards based in part on their performance on quality metrics including screening and control of chronic conditions. Our results suggest that increasing performance on the screening metrics could reduce performance on the control metrics. Future research can examine whether this is the case and whether the quality metrics should be redesigned so that practices are not penalized for expanding screening. In addition, future research can build on our findings to investigate which additional engagement strategies are

most successful for increasing treatment and control among patients with previously undiagnosed conditions and consider methods to bring these strategies to scale in locations with an influx of new patients. Effective intervention design would require more detailed data on the key barriers faced by patients with newly diagnosed conditions; based on previous work on uninsured individuals, factors such as health literacy may play an important role. Finally, our findings suggest new directions for research on the economics of health care demand. In particular, classic health capital models should be revisited to see if conclusions drawn about the economics of health care demand change when agents can determine their own level of uncertainty about their health by choosing to be screened; this is a topic we plan to pursue in future work.

1.A Additional tables and figures

Table 1.8: Definitions used for diabetes, hypertension and high cholesterol

Condition	Status	Definition
Diabetes	No condition	No self-reported diagnosis of diabetes and FPG<126 mg/dl or NFPG<200mg/dl
	Undiagnosed	No self-reported diagnosis of diabetes, but FPG>126 mg/dl or NFPG>200mg/dl
	Diagnosed	Self-reported diagnosis of diabetes (when non-pregnant for women)
Hypertension	No condition	No self-reported diagnosis, SBP<140mmHg, and DBP<90mmHg
	Undiagnosed	No self-reported diagnosis of hypertension, but SBP>140mmHg or DBP>90mmHg
	Diagnosed	Self-reported diagnosis of hypertension (when non-pregnant for women)
High cholesterol	No condition	No self-reported diagnosis, total cholesterol <200 mg/dl, LDL cholesterol<160 mg/dl, and HDL cholesterol>40 mg/dl
	Undiagnosed	No self-reported diagnosis, but total cholesterol >200 mg/dl, LDL cholesterol>160 mg/dl, or HDL cholesterol<40 mg/dl
	Diagnosed	Self-reported diagnosis

Note: FPG=fasting plasma glucose; NFPG=non-fasting plasma glucose; SBP=systolic blood pressure; DBP=diastolic blood pressure; HDL=high-density lipoprotein, LDL= low-density lipoprotein. In the REGARDS data and 2013 NHANES data, we calculated LDL cholesterol using the Friedewald equation [89]. Because neither LDL cholesterol nor triglycerides were available in the OHIE data, we could not calculate LDL cholesterol and therefore defined high cholesterol using HDL and total cholesterol only.

Table 1.9: Participants with undiagnosed conditions show more severe biomarkers for their other, previously diagnosed conditions than do patients who are aware of all their conditions

	(1) HbA1c or FPG	(2) SBP	(3) DBP	(4) LDL	(5) TChol
NHANES data					
Any undiagnosed conditions	0.323***	2.073***	1.309***	18.00***	18.90***
	(0.0824)	(0.616)	(0.431)	(2.011)	(1.795)
OHIE data					
Any undiagnosed conditions	0.400***	2.181	3.077***		15.15***
	(0.129)	(1.512)	(1.007)		(5.431)
REGARDS data					
Any undiagnosed conditions	17.85***	3.473***	2.961***	13.65***	16.26***
	(4.534)	(0.827)	(0.464)	(2.766)	(3.030)

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

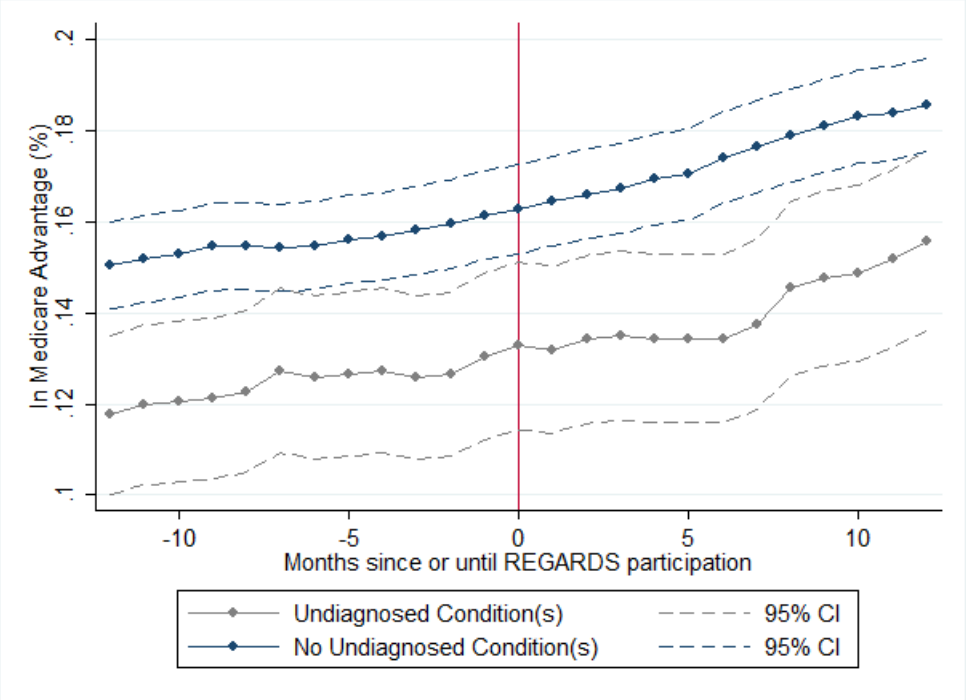
This table shows that participants with some undiagnosed conditions show more severe biomarkers for their other, previously diagnosed conditions. The models are adjusted for demographic factors and prevalence of comorbid conditions. Glycated hemoglobin (HbA1c) or fasting plasma glucose (FPG) are only included for participants with diagnosed diabetes; systolic blood pressure (SBP) and diastolic blood pressure (DBP) are included only for participants with diagnosed hypertension; and low-density lipoprotein cholesterol (LDL) and total cholesterol (TChol) are included only for participants with diagnosed high cholesterol. LDL cholesterol is not measured in the OHIE data and cannot be calculated using the Friedewald equation because data on triglycerides are also not available. We use FPG rather than HbA1c in the REGARDS data because HbA1c is not measured in these data.

Table 1.10: Characteristics of participants in traditional Medicare vs. Medicare Advantage in the month prior to screening via REGARDS

	Traditional Medicare		Medicare Advantage		Difference (MA-TM)
	N	%	N	%	
Total	7806	(85%)	1345	(15%)	
Male	3893	(50%)	615	(46%)	-4%
African American	2490	(32%)	674	(50%)	18%***
Low income	1354	(17%)	311	(23%)	6%**
Any undiagnosed conditions	1562	(20%)	232	(17%)	-3%

"MA-TM" denotes the traditional Medicare estimate is subtracted from the Medicare Advantage estimate. * denotes p<0.1, ** denotes p<0.05, and *** denotes p<0.01.

Figure 1.2. Enrollment in Medicare Advantage before and after REGARDS participation



This figure shows the fraction of participants enrolled in Medicare Advantage rather than traditional Medicare before and after participation in REGARDS, for participants who did vs. did not learn of a previously undiagnosed condition by participating in REGARDS.

1.B REGARDS data collection procedures

The REasons for Geographic and Racial Differences in Stroke (REGARDS) study recruited community-dwelling participants into an epidemiological longitudinal cohort study designed to answer questions about racial differences in stroke mortality. Recruitment was conducted from 2003-2007 and was accomplished through the use of commercially available lists of residential phone numbers and included the 48 contiguous United States (i.e., excluding Alaska and Hawaii). Sampling was stratified across African Americans and whites and three regions: the stroke belt (Alabama, Arkansas, Mississippi, and Tennessee), stroke buckle (North Carolina, South Carolina and Georgia) and all other states in the continental United States. Individuals who were under 45 years of age, did not identify as either African American or white, were non-English speaking, undergoing cancer treatment, or who resided in or were on a waiting list to enter a nursing home were excluded from the REGARDS study [37]. Figure 2.1 shows the geographic distribution of African American and white participants.

Participants were first interviewed, including questions about whether they had been diagnosed with high blood pressure, diabetes or high cholesterol by a doctor or nurse. For the in-home visit, participants were instructed to fast for 8-10 hours,[12] and had their blood glucose, blood pressure and lipid panel plus other biomarkers assessed in their home on a morning of their choosing. Blood pressure was measured as an average of two measurements taken by a trained technician using a regularly tested aneroid sphygmomanometer, after the participant was seated with both feet on the floor for 5 minutes. Glucose and the lipid panel were measured using colorimetric reflectance spectrophotometry with the Ortho Vitros 950 IRC Clinical Analyzer (Johnson and Johnson Clinical Diagnostics) after

12. About 80% of participants met the fasting requirement at the time that their labs were taken. We control for fasting status in our panel data analysis and use fasting- or non-fasting specific cutoffs where applicable when judging participants' disease status based on their biomarkers.

being shipped on ice packs overnight to a central laboratory. Participants were compensated $30 for their time, and were notified of their results and advised to seek medical care for abnormal results using the text shown in Figure 2.4.

1.C Additional robustness checks

In the main text, we report finding no significant relationship between learning of an undiagnosed condition and health plan selection. One might be concerned that null findings when all participants are pooled together might mask significant findings among certain patient sub-groups who face higher health risk.

Therefore, we also report results from an additional analysis wherein we add interactions to allow effects to vary based on participants' health care use in the past and self-rated health. As shown in Tables 1.11 and 1.12 our findings remain unchanged.

Table 1.11: No association between learning of a previously undiagnosed condition and current enrollment in Medicare Advantage

	(1)	(2)	(3)	(4)
Prior to January 1, 2006				
After participation × Undiagnosed	-0.011	-0.019*	-0.003	-0.020
	(0.012)	(0.011)	(0.016)	(0.016)
After × Undiag × High claims	-0.013		-0.0410	
	(0.015)		(0.027)	
After × Undiag × Low health		-0.002		-0.015
		(0.020)		(0.037)
After January 1, 2006				
After participation × Undiagnosed	-0.010	-0.004	-0.002	-0.011
	(0.0256)	(0.0166)	(0.028)	(0.017)
After × Undiag × High claims	-0.008		-0.041	
	(0.032)		(0.037)	
After × Undiag × Low health		-0.091		-0.091
		(0.081)		(0.090)
Observations	165,642	165,642	75,276	75,276
Number of participants	3,405	3,405	1,682	1,682
Duration dependence	0	0	1	1

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

This table shows that we fail to reject the null hypothesis of no association between learning of a previously undiagnosed condition and current enrollment in Medicare Advantage for a variety of groups. "High claims" denotes above-median outpatient claims two calendar years prior to participation in REGARDS. The rows of the table list the coefficients and standard errors from panel data models. The columns indicate four regression specifications. "Low health" denotes fair or poor self-reported health in the REGARDS participant survey. "Undiagnosed" denotes the participant was undiagnosed for hypertension, high cholesterol, and/or diabetes prior to participation. Controlling for polynomials of log duration of enrollment in the current plan type ("Duration dependence"), our proxy for inertia or switching costs, does not affect the results.

Table 1.12: No association between learning of a previously undiagnosed condition and switching into Medicare Advantage

	(1)	(2)	(3)	(4)
Prior to January 1, 2006				
After participation × Undiagnosed	0.001	0.001	-4.05e-05	0.001
	(0.002)	(0.001)	(0.00358)	(0.003)
After × Undiag × High claims	0.000		-0.000	
	(0.002)		(0.004)	
After × Undiag × Low health		0.003		-0.008*
		(0.004)		(0.005)
After January 1, 2006				
After participation × Undiagnosed	-0.001	-1.87e-05	0.000	0.003
	(0.004)	(0.002)	(0.007)	(0.004)
After × Undiag × High claims	0.001		0.006	
	(0.005)		(0.007)	
After × Undiag × Low health		-0.004		-0.008
		(0.011)		(0.015)
Observations	137,295	137,295	58,769	58,769
Number of participants	3,010	3,010	1,368	1,368
Duration dependence	N	N	Y	Y

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

This table shows that we fail to reject the null hypothesis of no association between learning of a previously undiagnosed condition and enrolling in Medicare Advantage for a variety of groups. "High claims" denotes above-median outpatient claims two calendar years prior to participation in REGARDS. The rows of the table list the coefficients and standard errors from panel data models. The columns indicate four regression specifications. "High claims" denotes above-median outpatient claims two calendar years prior to participation in REGARDS. "Low health" denotes fair or poor self-reported health in the REGARDS participant survey. "Undiagnosed" denotes the participant was undiagnosed for hypertension, high cholesterol, and/or diabetes prior to participation. Controlling for polynomials of log duration of enrollment in the current plan type ("Duration dependence"), our proxy for inertia or switching costs, does not affect the results.

1.C.1 A note on imputing missing claims data

Imputation of missing values is necessary for an analysis of prior health care claims because our data do not capture health care claims for individuals who were in Medicare

Advantage or who were younger than 65 prior to screening. In addition, this exercise has theoretical appeal. Each individual's history of medical care claims is determined not only by their health needs, but also by the out-of-pocket prices they face in their health plan. Out-of-pocket prices can vary for each type of care across plans and can vary widely in the Medicare Advantage market across time and location. We eliminate the influence of out-of-pocket prices on demand for care by imputing claims using data from a single health plan, traditional Medicare.

To impute the data, we predict the number of out-patient Medicare claims during the two years prior to participation (which were collected for some participants) using demographic and health variables (which were collected for all REGARDS participants). The predictors include blood pressure, lipid panel measurements, and blood glucose; measured waist circumference and BMI; county poverty and Health Professional Shortage Area status; age; year and region; race, sex, education, marital status, income, smoking, drinking, self-reported health, and cognitive functioning. All continuous variables are modeled as four categorical dummies to allow for non-linearity.

Health care utilization data typically have a skewed distribution and many zeroes, which makes selecting an appropriate model challenging. Although a number of modeling approaches have been explored in the literature, the current consensus is that no one approach dominates the others in all circumstances [90–94]. We follow recommendations set forth by Manning and Mullahy to decide between models [95]. The algorithm involves examining the kurtosis of residuals from a log OLS model, and then conducting a Park style test to examine the relationship between the mean and variance of residuals from a gamma GLM model if the residuals do not show substantial kurtosis [96]. Our results indicate that the log-residuals have heavy tails, with coefficients of kurtosis in the range of 3.9-4.0, so that the GLM models are likely to be problematic [95]. This is why the results shown in Tables 1.11 and 1.12 include imputed claims produced by log-transformed OLS models.

1.D Proofs: Demand for screening is (weakly) decreasing in costs of medical care

1.D.1 Demand for screening is weakly decreasing in non-pecuniary costs of medical care

When the price of screening equals willingness to pay for screening κ^*, agents are just indifferent between being screened and not being screened as follows:

$$\pi\left(\max_{M} u\left(A-PM-\kappa^*, H\left(M,1\right)\right)-\theta M\right)+\left(1-\pi\right)u\left(A-\kappa^*, H\left(0,0\right)\right) \tag{1.7}$$
$$-\left(\pi u\left(A, H\left(0,1\right)\right)+\left(1-\pi\right)u\left(A, H\left(0,0\right)\right)\right)=0$$

Differentiating (1.7) with respect to θ yields the following expression (by the envelope theorem, we can ignore the fact that the optimal M varies with θ):

$$\pi\left(-\frac{\partial u\left(A-PM^*-\kappa^*, H\left(M^*,1\right)\right)}{\partial C}\frac{\partial \kappa^*}{\partial \theta}-M^*\right)-\left(1-\pi\right)\frac{\partial u\left(A-\kappa^*, H\left(0,0\right)\right)}{\partial C}\frac{\partial \kappa^*}{\partial \theta}=0 \tag{1.8}$$

Then rearranging to solve for $\frac{\partial \kappa^*}{\partial \theta}$ yields:

$$-\left(\pi\frac{\partial u\left(A-PM^*-\kappa^*, H\left(M^*,1\right)\right)}{\partial C}\frac{\partial \kappa^*}{\partial \theta}\right)-\left(1-\pi\right)\frac{\partial u\left(A-\kappa^*, H\left(0,0\right)\right)}{\partial C}\frac{\partial \kappa^*}{\partial \theta}=\pi M^*$$
$$\frac{\partial \kappa^*}{\partial \theta}\left(-\pi\frac{\partial u\left(A-PM^*-\kappa^*, H\left(M^*,1\right)\right)}{\partial C}-\left(1-\pi\right)\frac{\partial u\left(A-\kappa^*, H\left(0,0\right)\right)}{\partial C}\right)=\pi M^*$$

$$\implies \frac{\partial \kappa^*}{\partial \theta}=-\frac{\pi M^*}{\pi\frac{\partial u\left(A-PM^*-\kappa^*, H\left(M^*,1\right)\right)}{\partial C}+\left(1-\pi\right)\frac{\partial u\left(A-\kappa^*, H\left(0,0\right)\right)}{\partial C}}\leq 0$$

We conclude $\frac{\partial \kappa^*}{\partial \theta} \leq 0$ because $\frac{\partial u}{\partial C} > 0$, $\pi > 0$ and $M^* \geq 0$.

1.D.2 Demand for screening is decreasing in out-of-pocket price of medical care

When the price of screening equals willingness to pay for screening κ^*, agents are just indifferent between being screened and not being screened as follows:

$$\pi\left(\max_M u\left(A - PM - \kappa^*, H\left(M, 1\right)\right) - \theta M\right) + \left(1-\pi\right) u\left(A - \kappa^*, H\left(0,0\right)\right) \tag{1.9}$$
$$- \left(\pi u\left(A, H\left(0,1\right)\right) + \left(1-\pi\right) u\left(A, H\left(0,0\right)\right)\right) = 0$$

Differentiating (1.9) with respect to P yields the following expression (by the envelope theorem, we can ignore the fact that the optimal M varies with P):

$$\pi\left(-\frac{\partial u\left(A - PM^* - \kappa^*, H\left(M^*,1\right)\right)}{\partial C}\frac{\partial \kappa^*}{\partial P} + \frac{\partial u\left(A - PM^* - \kappa^*, H\left(M^*,1\right)\right)}{\partial C}\right)$$
$$- \left(1-\pi\right)\frac{\partial u\left(A - \kappa^*, H\left(0,0\right)\right)}{\partial C}\frac{\partial \kappa^*}{\partial P} = 0 \tag{1.10}$$

Then rearranging to solve for $\frac{\partial \kappa^*}{\partial P}$ yields:

$$-\left(\pi\frac{\partial u\left(A - PM^* - \kappa^*, H\left(M^*,1\right)\right)}{\partial C}\frac{\partial \kappa^*}{\partial P}\right) - \left(1-\pi\right)\frac{\partial u\left(A - \kappa^*, H\left(0,0\right)\right)}{\partial C}\frac{\partial \kappa^*}{\partial P}$$
$$= \pi\frac{\partial u\left(A - PM^* - \kappa^*, H\left(M^*,1\right)\right)}{\partial C}$$
$$\frac{\partial \kappa^*}{\partial P}\left(-\pi\frac{\partial u\left(A - PM^* - \kappa^*, H\left(M^*,1\right)\right)}{\partial C} - \left(1-\pi\right)\frac{\partial u\left(A - \kappa^*, H\left(0,0\right)\right)}{\partial C}\right)$$
$$= \pi\frac{\partial u\left(A - PM^* - \kappa^*, H\left(M^*,1\right)\right)}{\partial C}$$

$$\implies \frac{\partial \kappa^*}{\partial P} = -\frac{\pi \frac{\partial u(A-PM^*-\kappa^*, H(M^*,1))}{\partial C}}{\pi \frac{\partial u(A-PM^*-\kappa^*, H(M^*,1))}{\partial C} + (1-\pi)\frac{\partial u(A-\kappa^*, H(0,0))}{\partial C}} < 0$$

We conclude $\frac{\partial \kappa^*}{\partial P} < 0$ because $\frac{\partial u}{\partial C} > 0$ and $\pi > 0$.

1.E Proofs: Demand for medical treatment is decreasing in costs of medical treatment

1.E.1 Demand for medical treatment is decreasing in non-pecuniary costs of medical treatment

We show that agents must demand less treatment when they have higher idiosyncratic non-pecuniary costs of medical treatment (captured by θ), because to do otherwise would violate the first-order conditions.

Consider the optimal decisions when agents know that $D = 1$. (This is the only case where purchase of medical care is an option, because medical care is not available without a prescription.) Now consider that θ decreases from $\overline{\theta}$ to $\underline{\theta}$. Let $M_{\overline{\theta}}$ and $C_{\overline{\theta}}$ denote the optimal decisions before the change and $M_{\underline{\theta}}$ and $C_{\underline{\theta}}$ denote the optimal decisions after the change.

$M_{\overline{\theta}}$ and $C_{\overline{\theta}}$ must fulfill the first-order conditions summarized in equation (1.5), as follows:

$$\frac{\partial u\left(C_{\overline{\theta}}, H\left(M_{\overline{\theta}}, 1\right)\right)}{\partial H}\frac{\partial H\left(M_{\overline{\theta}}, 1\right)}{\partial M} - \overline{\theta} = P\frac{\partial u\left(C_{\overline{\theta}}, H\left(M_{\overline{\theta}}, 1\right)\right)}{\partial C} \tag{1.11}$$

After non-pecuniary costs of medical treatment decreases from $\overline{\theta}$ to $\underline{\theta}$, previously optimal decisions $M_{\overline{\theta}}$ and $C_{\overline{\theta}}$ would violate equation (1.5) as follows:

$$\frac{\partial u\left(C_{\overline{\theta}}, H\left(M_{\overline{\theta}}, 1\right)\right)}{\partial H}\frac{\partial H\left(M_{\overline{\theta}}, 1\right)}{\partial M} - \underline{\theta} > P\frac{\partial u\left(C_{\overline{\theta}}, H\left(M_{\overline{\theta}}, 1\right)\right)}{\partial C} \tag{1.12}$$

To make inequality (1.12) an equality, M and C must change so that the left-hand side decreases and/or the right-hand side increases. By concavity of the utility function in H and C, the weakly positive cross-partial $\frac{\partial^2 u(C,H)}{\partial C \partial H}$, and weakly decreasing marginal returns to medical care, increasing M and decreasing C achieves both. Therefore $M_{\overline{\theta}} < M_{\underline{\theta}}$ and $C_{\overline{\theta}} > C_{\underline{\theta}}$ resolves the contradiction in the first-order conditions. We conclude that $\frac{\partial M^*}{\partial \theta} < 0$.

1.E.2 Demand for medical treatment is decreasing in out-of-pocket costs of medical treatment

We show that agents must demand less treatment when they have higher cost of medical treatment P, because to do otherwise would violate the first-order conditions.

Consider the optimal decisions when agents know that $D = 1$. (This is the only case where purchase of medical treatment is an option, because medical treatment is not available without a prescription.) Now consider that P decreases from $\overline{P}$ to $\underline{P}$. Let $M_{\overline{P}}$ and $C_{\overline{P}}$ denote the optimal decisions before the change and $M_{\underline{P}}$ and $C_{\underline{P}}$ denote the optimal decisions after the change.

$M_{\overline{P}}$ and $C_{\overline{P}}$ must fulfill the first-order conditions summarized in equation (1.5), as follows:

$$\frac{\partial u\left(C_{\overline{P}}, H\left(M_{\overline{P}}, 1\right)\right)}{\partial H}\frac{\partial H\left(M_{\overline{P}}, 1\right)}{\partial M} - \theta = \overline{P}\frac{\partial u\left(C_{\overline{P}}, H\left(M_{\overline{P}}, 1\right)\right)}{\partial C} \tag{1.13}$$

After cost of care P decreases from $\overline{P}$ to $\underline{P}$, previously optimal decisions $M_{\overline{P}}$ and $C_{\overline{P}}$ would violate equation (1.5) as follows:

$$\frac{\partial u\left(C_{\overline{P}}, H\left(M_{\overline{P}}, 1\right)\right)}{\partial H}\frac{\partial H\left(M_{\overline{P}}, 1\right)}{\partial M} - \theta > \underline{P}\frac{\partial u\left(C_{\overline{P}}, H\left(M_{\overline{P}}, 1\right)\right)}{\partial C} \tag{1.14}$$

To make inequality (1.14) an equality, M and C must change so that the left-hand side decreases and/or the right-hand side increases. As before, increasing M and decreasing C achieves both. Therefore $M_{\overline{P}} < M_{\underline{P}}$ and $C_{\overline{P}} > C_{\underline{P}}$ resolves the contradiction in the first-order conditions. We conclude that $\frac{\partial M^*}{\partial P} < 0$.

CHAPTER 2

DOES IDENTIFICATION OF PREVIOUSLY UNDIAGNOSED CONDITIONS CHANGE PATIENT CARE SEEKING BEHAVIOR?

2.1 Introduction

High cholesterol, hypertension, and diabetes are important contributors to premature death and ill-health in the United States [5,97,98]. However, many people with these conditions are unaware of them since in early stages these conditions are asymptomatic. About one-fifth of cases of high cholesterol, hypertension, and diabetes are undiagnosed among U.S. adults [1,35,99]. Lengthy gaps in diagnosis and treatment can lead to negative health consequences [8,11,12,15,16]. Therefore, increasing screening for these conditions is an important avenue to increase treatment of undiagnosed conditions and thereby improve population health [14].

The prevalence of undiagnosed high cholesterol, hypertension, and diabetes is particularly high in the Medicare population, and policy efforts to increase screening of Medicare beneficiaries have expanded in recent years [3]. Medicare offered new beneficiaries a "Welcome to Medicare" wellness visit without cost sharing starting in 2005, but uptake of this benefit seems to have been incomplete [100]. Subsequently, the Affordable Care Act (ACA) has eliminated cost-sharing for annual wellness visits for all Medicare beneficiaries and eliminated cost-sharing for high cholesterol, hypertension, and diabetes screening for patients at sufficient risk according to US Preventive Services Task Force guidelines [7,101]. The ACA also created the Center for Medicare and Medicaid Innovation which has funded two demonstration projects designed to encourage provider-to-patient outreach related to screening. Accountable Care Organizations lose their shared savings payments if they fail to achieve targets for blood pressure screening rates and other quality

metrics, and Accountable Health Communities are charged with implementing community outreach to promote awareness of clinical delivery services [102,103]. As more Medicare beneficiaries receive care under these new payment models, outreach to encourage screening may become increasingly common.

Although outreach has intuitive appeal as a strategy to increase screening among hard-to-reach Medicare beneficiaries, it is not clear how many beneficiaries who are screened as a result of outreach would visit a doctor to evaluate and initiate management of previously undiagnosed conditions. Studies of changes in self-reported treatment have shown small or non-significant effects of screening with telephone outreach among Medicare beneficiaries, but we are not aware of studies that track healthcare utilization with claims data rather than self-reported data [104]. In addition, it is not clear which participants are most likely to seek care for previously undiagnosed conditions. Prior screening interventions in vulnerable populations showed high rates of loss-to-follow-up, particularly among minority women and women with lower levels of education [105].

This study addresses these gaps in the literature by using Medicare claims data to test whether in-home screening after telephone outreach translates to doctor visits for evaluation and management of previously undiagnosed conditions among Medicare beneficiaries. We used data from a geographically and demographically diverse sample of Medicare beneficiaries and separately track the impact for high-priority groups such as women, African Americans, beneficiaries who are dually eligible for Medicaid, beneficiaries without a usual healthcare provider, beneficiaries with less than high school education, and beneficiaries living in a Health Professional Shortage Area. In particular, we exploited an epidemiological study (the REasons for Geographic And Racial Differences in Stroke study, or REGARDS) that recruited participants from across the continental United States using residential telephone calls and paid them to be screened for high cholesterol, hypertension, and diabetes [106]. We compared doctor visits for evaluation and management of these

conditions before and after each participant was screened by REGARDS, using the rolling recruitment into the study to tease out the impact of screening from the impact of secular trends.

2.2 Methods

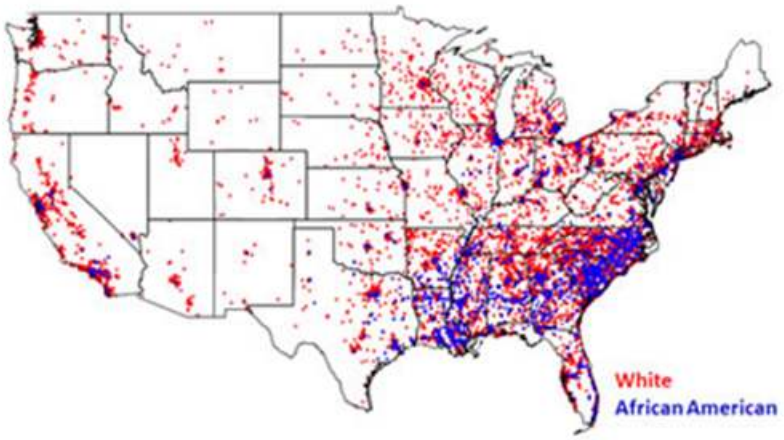
2.2.1 Data

Study population

The REGARDS study was designed to answer questions about racial and geographic differences in risks for stroke and stroke mortality. Recruitment was conducted on a rolling basis over 2003-2007 and was accomplished through the use of commercially available lists of residential phone numbers in the 48 contiguous United States. Sampling was stratified across African Americans and whites and three regions: the stroke belt (Alabama, Arkansas, Mississippi, Tennessee, and non-coastal North Carolina, South Carolina and Georgia), stroke buckle (coastal plains of North Carolina, South Carolina and Georgia) and elsewhere. Individuals who did not identify as either African American or white, or who were non-English speaking, under 45 years of age, undergoing cancer treatment, or in a nursing home were excluded from the REGARDS study [106]. Figure 2.1 shows the geographic distribution of participants by race. Data from the REGARDS study have been linked to Medicare claims. Details of the linking process are described elsewhere [70].

We limited the analysis to REGARDS study participants who (a) were aged 67 or older at the time or REGARDS participation, (b) had Medicare linked data, (c) were enrolled in Medicare fee-for-service insurance coverage (Parts A and B but not Medicare Advantage/Part C) throughout the 24 months before through 24 months after the REGARDS study, (d) had one or more of our three conditions of interest. 5,884 met all exclusion cri-

Figure 2.1. Location of REGARDS participants (Source: Howard et al, 2005)



teria and therefore had a complete panel of Medicare claims from the 24 months before through 24 months after the REGARDS study. Table 2.1 details the step-wise exclusion of participants.

Table 2.1: Participants cascade

Exclusion criterion	Participants
All REGARDS participants	30,183
< 67 years old at REGARDS participation	-17,651
No Medicare-linked data	-2,488
No Medicare Part A+B-C between 2 years before and 2 years after baseline	-3,473
No high cholesterol, hypertension, or diabetes	-687
Included	5,884

Study procedures

Participants first answered questions by phone, including whether they had been diagnosed with high cholesterol, hypertension, or diabetes by a health professional, and questions about their age, race, sex, income, education, self-reported health, smoking status, number

of alcoholic drinks per week, and whether they had a usual health provider. During the interview, participants also completed a short memory test to assess their cognitive functioning and the Short Form 12 (SF-12) questionnaire to assess their physical and mental health.

Participants were instructed to fast for an in-home visit. During the in-home visit, trained health professionals measured participants' blood pressure and collected blood samples which were shipped on ice packs overnight to a central laboratory. Blood pressure was measured twice using an aneroid sphygmomanometer, after the participant was seated with both feet on the floor for 5 minutes. The 2 blood pressure measurements were averaged for the analysis. Serum glucose, triglycerides, total and high-density lipoprotein cholesterol were measured from blood samples using colorimetric reflectance spectrophotometry with the Ortho Vitros 950 IRC Clinical Analyzer from Johnson and Johnson Clinical Diagnostics. Low-density lipoprotein (LDL) cholesterol was calculated using the Friedewald equation [89]. Participants were compensated $30 for their time. They were notified of their results and advised to seek medical care for abnormal results using letters and cards with standard text reprinted in Figure 2.4. The study received IRB approval and all participants gave informed consent [37].

We complemented the REGARDS biomarker and survey data with participants' Medicare claims. We used data from Medicare carrier and outpatient files to track doctor visits for evaluation and management of high cholesterol, hypertension, or diabetes in each six month interval during a given participant's 48-month window of observation. Codes for evaluation and management visits and International Classification of Diseases (ICD-9) diagnosis codes for high cholesterol, hypertension, and diabetes according to the Chronic Conditions Warehouse definitions are reported in Table 2.5. We also extracted data on whether each participant was dually eligible for Medicaid and data on hospitalizations during each six month interval.

We identified participants with high cholesterol, hypertension, and/or diabetes and classified each condition as diagnosed or undiagnosed using self-reported data, claims data, and biomarker data. Participants were classified as having a diagnosed condition if they positively responded to the question "Has a doctor or other health professional ever told you that you have high blood pressure/diabetes or high blood sugar/high cholesterol or an abnormal level of fats in your blood?" without a positive response to the question "Was this only when you were pregnant?" in the case of diabetes or hypertension. To correct for under-reporting of diagnosis in self-reported data, we used the Medicare claims data to identify additional diagnosed conditions [73]. In particular, biomarker-identified high cholesterol, hypertension, and diabetes were categorized as diagnosed if the claims data met Chronic Conditions Warehouse definitions for the condition, i.e., had two or more claims coded as relevant to the condition within the past 2 years. (The Chronic Conditions Warehouse definitions were designed to identify chronic conditions using claims data and correctly identify 69% of true diabetes in validation tests [107,108]. In our data, the use of this additional criterion increased the prevalence of diagnosed conditions by 4% for hypertension, and by 2% for high cholesterol and diabetes.) Biomarker-identified high cholesterol, hypertension, and diabetes that failed to meet either of these criteria were classified as undiagnosed. We used biomarker cutoffs that took participants' fasting status into account, as detailed in Table 2.6. We allowed cholesterol control cutoffs to vary by 10-year estimated risk category per national recommendations, as detailed in Table 2.7 [8,15,16,109].

Outcomes of interest

In our main analysis, the outcome of interest was a binary variable indicating whether participants with prevalent high cholesterol, hypertension or diabetes received any doctor visits for evaluation and management of their conditions in a given six month interval. This

outcome was measured on the condition-level (so that participants with multiple conditions are entered into the data multiple times) and was tracked for each six month period of the participant's 48-month period of observation. (Note that a single doctor visit could be coded as addressing multiple conditions in the Medicare data.) We also analyzed the number of doctor visits targeting for evaluation and management of each condition in each six-month interval.

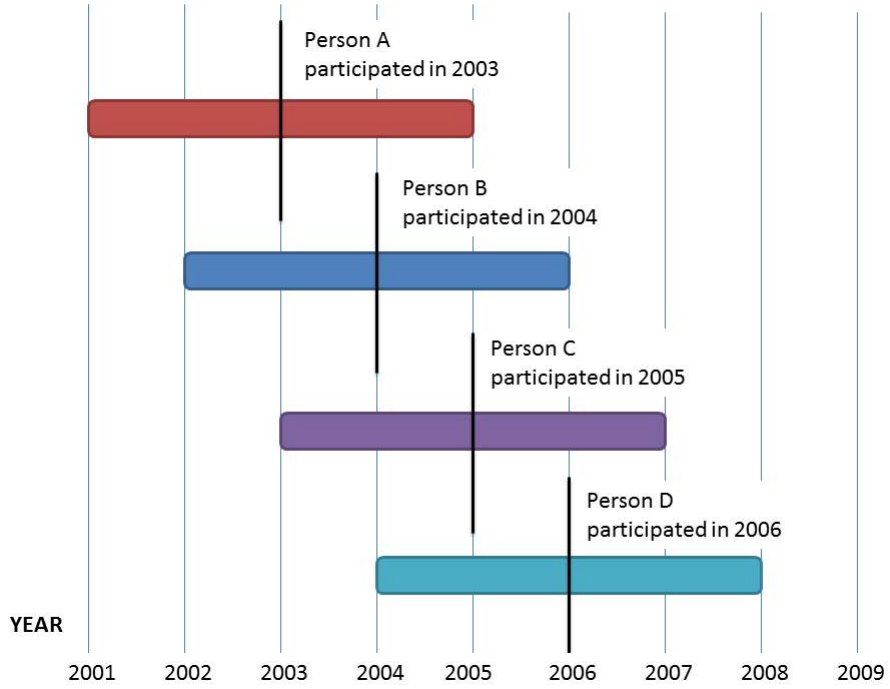
Predictors of interest

The key predictors of interest were (a) whether or not the participant had already been screened via REGARDS and (b) whether each prevalent condition was diagnosed or undiagnosed prior to REGARDS.

Control variables used in multivariate modeling

Control variables were selected to address two possible biases. First, we expected that secular trends would contribute to observed changes in doctor visits after REGARDS participation. For example, all participants were older after REGARDS participation than before REGARDS participation, and policy changes were implemented during our period of observation. These secular trends could have biased our estimates if not controlled for in the model. Two aspects of our data make it possible to control for secular trends: (a) the rolling recruitment into the REGARDS study, and (b) the availability of panel data for all participants the 24 months prior to participation. Figure 1 demonstrates this point using a graphical example: when analyzing data from the hypothetical participants in Figure 1, we could separate the effect of screening on person A in 2003 from the effect of secular trends in 2003 by using the data from person B and person C in 2003. A similar graphic could be drawn to show how we were able to identify and control for the effects of aging.

Figure 2.2. Illustration showing the months of observation for four hypothetical REGARDS participants



This figure shows an example of the periods of observation for four hypothetical individuals in the panel data who were recruited on January 1, 2003, January 1, 2004, January 1, 2005, and January 1, 2006. The center of each horizontal bar indicates the date that the individual participated in REGARDS. The length of the horizontal bar indicates the window of time over which we track each individual's Medicare claims, i.e., 24 months prior to REGARDS participation through 24 months after the individual's date of participation.

Second, our results might be biased if the type of individual willing to participate in REGARDS changed over time. This would be problematic because, as noted above, not-yet-screened individuals are compared with recently-screened individuals to control for secular trends. We addressed this concern by controlling for a number of observable characteristics in the models and, in some specifications, controlling for all time-invariant individual-level characteristics using fixed effects. (We also assessed this concern by comparing the health and biomarkers from our sample with the same characteristics from a national biomarker survey, the National Health and Nutrition Examination Survey.)

To this end, we included two main groups of control variables. Time-varying control variables included year dummies, interactions between region and year, and individual age, divided into 8 bins of equal size to allow for a non-linear relationship between age and doctor visits. Time-invariant control variables included physical health measures taken at the time of REGARDS participation and a number of demographic and health-related characteristics from the REGARDS survey. In particular, we controlled for waist size in centimeters, BMI, glucose, lipid panel, the average of two blood pressure measures (both systolic and diastolic) and reported physical health from the SF-12; type of condition (high cholesterol, hypertension, or diabetes), and whether the condition was previously undiagnosed; race (African American or white), sex (male or female), income (less than $20,000, $20,000-<$35,000, $35,000-$75,000, and over $75,000), education (less than high school education, high school, some college education, or graduated from college), fair or poor self-reported health, usual health provider at the time of the interview (self-reported having or not-having a usual health provider), self-reported smoking status (current smoker, past smoker, or non-smoker), number of alcoholic drinks per week, fasting status at the time of the interview (fasting or not), cognitive status according to a short memory test (impaired or not), Medicaid dual eligibility in 2008 (eligible or not), status of county as a primary care health professional shortage area (all, part, or none of the county is a designated health pro-

fessional shortage area), and the fraction of residents in poverty in the participant's county of residence. All continuous variables were binned into four categories of equal size to allow non-linearity in the relationship between these variables and doctor visits.

2.2.2 Analytic plan

The unit of analysis was a person with high cholesterol, hypertension, or diabetes, so that people with multiple conditions were entered into the data multiple times. We analyzed changes in doctor visits for evaluation and management of previously diagnosed vs. previously undiagnosed high cholesterol, hypertension, and diabetes after REGARDS using multivariate panel data models of the following form:

$$\begin{aligned} Y_{ijt} =& \mu + T_{it}U_{ij}\delta_1 + T_{it}U_{ij}s\delta_2 + T_{it}(1-U_{ij})\gamma_1 + T_{it}(1-U_{ij})s\gamma_2 \\ &+ \mathrm{X}_{ijt}\beta + U_{ij}\phi + \alpha_{ij} + \varepsilon_{ijt} \end{aligned} \tag{2.1}$$

where i indexes individual, j indexes condition (either high cholesterol, hypertension, or diabetes), t indexes a given 6-month time interval in individual i's 48-month period of observation, and s indicates the average time since or until individual i's REGARDS participation during interval t.

δ_1, δ_2, γ_1, and γ_2, were the coefficients of interest, indicating the changes in levels and trends in doctor visits after screening via REGARDS for undiagnosed and diagnosed conditions, respectively. We modelled changes in levels and trends of the outcome of interest separately to examine whether changes in doctor visits occurred immediately, developed over time, or both. U_{ij} was a binary variable that took the value 1 if individual i's condition j was undiagnosed prior to REGARDS, and 0 otherwise. T_{it} was a binary variable indicating whether individual i had already been screened via REGARDS at six-month interval t

(i.e., this variable took the value 1 for all six-month intervals where $s > 0$). X_{ijt} included the control variables listed above and all relevant lower-order interaction terms. The α_{ij} term captured the correlation across measures of the same person and condition over time and was modeled as a random effect in the basic specification. We modeled ε_{ijt} using heteroskedasticity-robust standard errors clustered on the individual level, to account for the heteroskedasticity that arose due the use of a binary outcome variable and account for the fact that some participants had multiple conditions [110].

To additionally control for secular trends and any changes in the composition of REGARDS participants over time, we used six different regression specifications that controlled for participants' time-invariant characteristics and participants' health trajectories in a progressively stricter fashion. In particular, we ran models with and without (a) controlling for participants' hospitalizations in the current six-month interval, (b) allowing background trends in doctor visits to vary with patients' biomarkers (i.e., interacting s with biomarkers), and (c) controlling for time-invariant characteristics using person-by-condition fixed effects (i.e., modeling α_{ij} as a fixed rather than random effect). To illuminate whether timing of REGARDS participation was indeed related to time-invariant individual-level characteristics, we conducted a Hausman test to compare the models using random vs. fixed effects.

In the main analysis, we pooled high cholesterol, hypertension, and diabetes together. In additional analyses, we restricted the data to examine changes in doctor visits for high cholesterol, hypertension, and diabetes separately.

To check whether observed changes in doctor visits after REGARDS participation could have been produced by non-linearity in the trends prior to participation, we ran placebo regressions. In the placebo regressions, we restricted the sample to only include the years prior to screening and compared participants' doctor visits two years before screening vs. one year before screening.

We also investigated the predictors of doctor visits for previously undiagnosed conditions by interacting the changes in levels and trends in doctor visits after REGARDS (the quantities with coefficients δ_1, δ_2, γ_1, and γ_2,) with characteristics of participants. These characteristics included gender, race, Medicaid dual eligibility, low income (<$20,000 per year), marital status, fair or poor self-reported health, region of residence (stroke belt vs. other), healthcare use in the 12 months prior to REGARDS, having a usual health care provider, having multiple chronic conditions, having less than a high-school education, living in a high-poverty county (>25% poverty), and living in a county that is a primary care Health Professional Shortage Area. We examined one of these variables at a time. In all cases, the relevant lower-order interaction terms were included in the regressions.

2.3 Results

Complete panel data on doctor visits were available for 6,571 participants. The REGARDS participants with merged Medicare data have been previously shown to resemble a national 5% sample of fee-for-service Medicare beneficiaries [111]. Figure 2.5 shows that our sample of participants also resembled the National Health and Nutrition Survey, a nationally representative biomarker survey, on measured and self-reported health in similar years when the REGARDS exclusion criteria were applied.

Among the 6,571 participants with complete panel data, 5,884 had one or more of our conditions of interest and were therefore included in the analysis. In total, 4,268 participants had high cholesterol, including 874 participants with undiagnosed high cholesterol; 4,502 participants had hypertension, including 451 with undiagnosed hypertension; and 1,309 participants had diabetes, including 143 with undiagnosed diabetes. Because participants with multiple conditions were entered into the data multiple times, our final dataset

comprised a panel of 10,079 prevalent conditions, including 1,468 previously undiagnosed conditions.

Table 2.2 compares the characteristics of participants with only diagnosed conditions vs. participants with one or more undiagnosed conditions. Participants with undiagnosed conditions had higher blood pressure and fasting blood glucose, higher total and LDL cholesterol, and lower HDL cholesterol than participants with only diagnosed conditions. Participants with undiagnosed conditions were more likely to be male, lack a usual health-care provider, and currently smoke than participants with only diagnosed conditions; they were less likely than participants with only diagnosed conditions to have seen a doctor for evaluation and management of any conditions in the prior year.

Table 2.3 shows the results from the six model specifications used to test for impacts of screening on the fraction of high cholesterol, hypertension, and diabetes cases that were seen by a doctor for evaluation and management per six months. The results were highly similar across the six specifications. Overall, we found no change in doctor visits for diagnosed conditions after participation, but did find changes in doctor visits for previously undiagnosed conditions after participation in REGARDS. This evidence is consistent with a hypothesis that screening changed participants' care use patterns by informing participants about previously undiagnosed conditions. In the most conservative model, the fraction of previously undiagnosed conditions that received a semi-annual doctor visit for evaluation and management increased by 15 percentage points (11 to 19) by one year after screening and by 22 percentage points (95% confidence interval: 16-28) by two years after screening. The raw data showed a similar trend; see Figure 2.

A Hausman test between the first and fourth models in Table 2.3, which were identical except for the use of fixed or random effects to model variation for each individual-condition, failed to reject the null hypothesis that both were consistent, assuming the spec-

Table 2.2: Characteristics of participants meeting all exclusion criteria, by diagnosis status at the time of REGARDS participation

	Participants w/ Only Diagnosed Conditions		Participants w/ Undiagnosed Conditions		*p*-Value of the Difference
	Mean	SE	Mean	SE	
Age	74.1	(0.1)	74.9	(0.2)	$p<0.01$
Systolic blood pressure	130.3	(0.2)	138.1	(0.5)	$p<0.01$
Diastolic blood pressure	74.6	(0.1)	78.1	(0.3)	$p<0.01$
Fasting glucose	100.6	(0.4)	109	(1.2)	$p<0.01$
Total cholesterol	185.2	(0.7)	201.2	(1.2)	$p<0.01$
Triglycerides	130.9	(1.2)	139.2	(3.9)	$p<0.01$
LDL cholesterol	106.5	(0.6)	125.8	(1)	$p<0.01$
HDL cholesterol	52.3	(0.3)	48.1	(0.5)	$p<0.01$
	N	%	N	%	
Total	4562	(69)	1322	(20)	
Male	2116	(46)	810	(61)	$p<0.01$
Any doctor visits the year before participation	4461	(98)	964	(73)	$p<0.01$
Had a usual healthcare provider	4101	(90)	964	(73)	$p<0.01$
Current smoker	324	(7)	114	(9)	$p<0.01$
African American	1367	(30)	398	(30)	$p=0.71$
Lives in stroke belt state	1583	(35)	476	(26)	$p=0.41$
Lives in stroke buckle state	1104	(24)	302	(23)	$p=0.47$
Married	2580	(57)	784	(59)	$p=0.14$

LDL: Low-density lipoprotein. HDL: High-density lipoprotein. SE: standard error of the mean. In this chart, glucose and lipid measurements are included only from participants who are fasting.

Table 2.3: Percentage point change in any semi-annual doctor visits for evaluation and management of previously diagnosed vs. undiagnosed conditions one and two years after REGARDS participation (average marginal effects from regression)

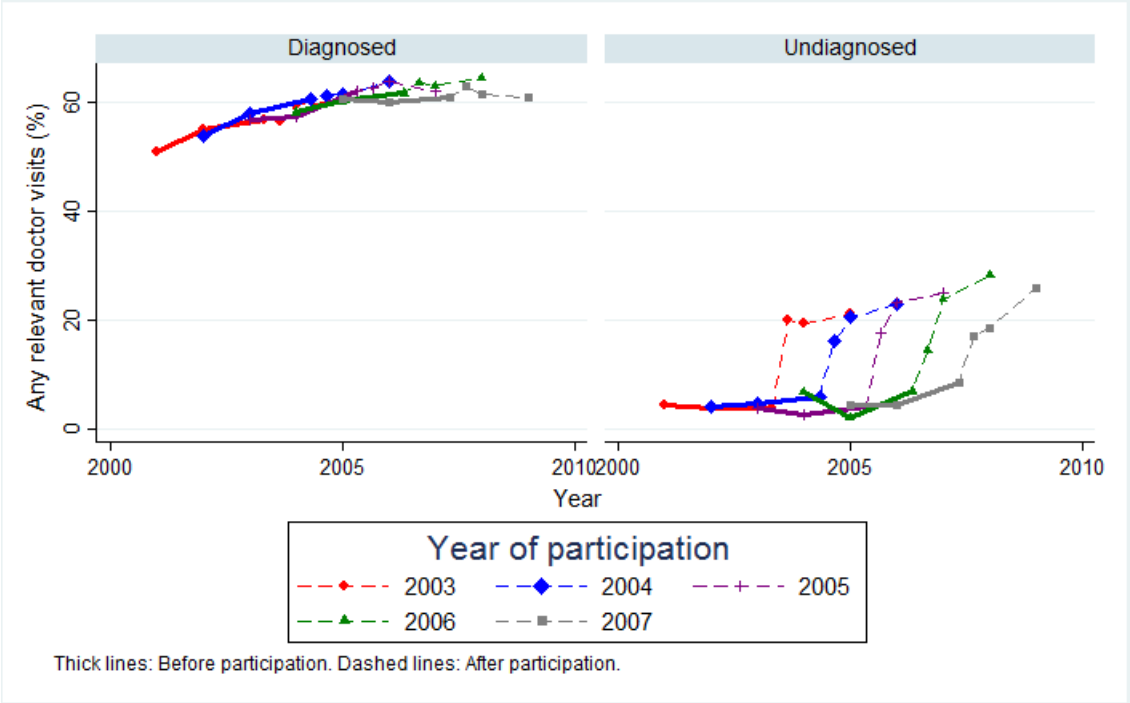
	(1)	(2)	(3)	(4)	(5)	(6)
Diagnosed conditions						
After 1 year	-1	-1	-1	-1	-1	-1
	(-4 to 1)	(-4 to 1)	(-4 to 1)	(-4 to 1)	(-4 to 1)	(-4 to 1)
After 2 years	-3	-3	-3	-2	-3	-3
	(-6 to 1)	(-7 to 1)	(-7 to 1)	(-6 to 2)	(-7 to 1)	(-7 to 1)
Undiagnosed conditions						
After 1 year	16***	15***	15***	16***	15***	15***
	(12 to 20)	(11 to 19)	(11 to 19)	(12 to 20)	(11 to 19)	(11 to 19)
After 2 years	23***	22***	22***	23***	22***	22***
	(17 to 29)	(16 to 28)	(16 to 28)	(17 to 29)	(16 to 28)	(16 to 28)
Fixed effects	N	N	N	Y	Y	Y
Control for hospitalizations	N	Y	Y	N	Y	Y
Background trends vary by biomarkers	N	N	Y	N	N	Y

95% confidence interval in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

The rows of the table include marginal effects from a multivariate panel data regression on the condition level indicating changes in healthcare utilization 1 and 2 years after REGARDS participation. The columns indicate six regression specifications. All specifications include the control variables noted in the text. In columns 2, 3, 5 and 6 estimates are adjusted for hospitalizations. In columns 4 through 6, estimates are adjusted for time-invariant individual characteristics using individual-by-condition fixed effects. In columns 3 and 6, background trends in doctor visits are allowed to vary with participants' biomarkers.

Figure 2.3. Screening via REGARDS and doctor visits for previously diagnosed and previously undiagnosed conditions in the raw data



The solid lines indicate not-yet-screened individuals and dashed lines indicate recently-screened individuals. The year of REGARDS participation is split into time points before vs. after participation.

ification of the model is correct ($F(54)$=37.77, p =0.954). The Hausman test therefore provided no evidence that the use of a fixed effects model was necessary.

Running the models separately by condition, we found that doctor visits increased for all three conditions. Over the two years after participation in REGARDS, semi-annual evaluation and management visits increased by 45 percentage points for previously undiagnosed diabetes (95% CI 30 to 60), 19 percentage points for previously undiagnosed high cholesterol (95% CI 12 to 26), and 20 percentage points for previously undiagnosed hypertension (95% CI 8 to 31). Results were similar when we examined the number of doctor visits: visits increased by 1.1 per 6-month interval for previously undiagnosed diabetes (95% CI 0.5 to 1.7), 0.3 percentage points for previously undiagnosed high cholesterol (95% CI 0.2 to 0.5), and 0.4 percentage points for previously undiagnosed hypertension (95% CI 0.2 to 0.7). The raw data showed a similar pattern, as shown in Table 2.8.

The results disappeared as expected in the placebo regressions, which used only pre-REGARDS data and test for changes in levels and trends in doctor visits the year prior to participation; see Table 2.9 and Figure 2.6.

Finally, we examined which participants were most likely to seek care for previously undiagnosed conditions. Table 2.4 shows the impact of screening on semi-annual doctor visits for previously undiagnosed conditions two years after screening by participant characteristics. We found no significant differences in rates of follow-up for previously undiagnosed conditions by gender, race, Medicaid dual eligibility, low income, marital status, fair or poor self-reported health, region of residence (stroke belt vs. other), having multiple chronic conditions, having less than a high-school education, living in a high-poverty county (>25% poverty) or a county that is a primary care Health Professional Shortage Area, doctor visits the year before participation, or failing a cognitive test. In contrast, participants who self-reported having a usual healthcare provider at the time of REGARDS participation may have been 11 percentage points more likely to seek care for a newly diag-

nosed condition (95% CI 0 to 23, two-sided p-value: 0.05) than participants who reported no usual healthcare provider at the time of REGARDS participation.

2.4 Discussion

In our national sample of Medicare beneficiaries, 15% of cases of high cholesterol, hypertension, and diabetes were undiagnosed and 20% of participants were undiagnosed for at least one of these conditions. In-home screening after telephone outreach increased use of semi-annual doctor visits for previously undiagnosed conditions by 22 percentage points after two years. Beneficiaries' reported access to a usual healthcare provider at the time of screening was more predictive of doctor visits for their previously undiagnosed conditions than factors such as gender, race, living in the stroke belt, individual-level or area-level poverty, living in a health professional shortage area, or even past use of healthcare. Indeed, the impact of screening on doctor visits for previously undiagnosed high cholesterol, hypertension, and diabetes was statistically the same for a wide variety of Medicare beneficiaries, with the possible exception of beneficiaries who reported lacking a usual healthcare provider. This possible exception is concerning because beneficiaries who reported lacking a usual healthcare provider accounted for about one-quarter of beneficiaries with undiagnosed conditions in our sample.

Our analysis builds on previous investigations on the relationship between health beliefs and healthcare seeking behaviors and provides several methodological advantages with respect to studying Medicare beneficiaries [25,104,112]. First, due to the merge of REGARDS data with Medicare claims, we were able to track participants' awareness of health conditions and healthcare utilization in the months directly before and after screening, and to measure healthcare utilization prospectively using Medicare claims rather than retrospectively using self-reported data. Second, the REGARDS study recruited Medicare

Table 2.4: Percentage point change in any semi-annual doctor visits for evaluation and management of previously undiagnosed conditions two years after REGARDS participation, by participant characteristics (average marginal effects from regression)

	Average Marginal Effect If In Group		Average Marginal Effect If Not In Group		p-Value of the Difference
	Mean	SE	Mean	SE	
Had usual healthcare provider	25	(4)	14	(5)	p=0.05
Any doctor visits the year before participation	23	(3)	24	(8)	p=0.88
Male	20	(4)	27	(4)	p=0.21
African American	18	(5)	25	(4)	p=0.29
Medicaid dual eligible	21	(8)	23	(3)	p=0.78
Income < $20,000	25	(7)	23	(3)	p=0.80
Married	25	(4)	20	(4)	p=0.39
Fair or poor self-reported health	24	(8)	23	(3)	p=0.85
Lives in a stroke belt state	25	(4)	20	(5)	p=0.45
Has multiple chronic conditions	23	(4)	22	(5)	p=0.85
Less than high school education	21	(7)	23	(3)	p=0.72
County of residence has > 25% residents in poverty	28	(11)	23	(3)	p=0.63
County of residence is primary care Health Professional Shortage Area	25	(8)	23	(3)	p=0.80
Failed cognitive test	14	(6)	24	(3)	p=0.16

beneficiaries using random phone calls from across the continental United States. This recruitment procedure produced a sample that resembled a national 5% sample of traditional Medicare beneficiaries on a variety of characteristics [111]. Third, due to the random variation in the timing of participants' recruitment into the REGARDS study, we were able to tease apart the impact of screening on doctor visits for high cholesterol, hypertension, and diabetes from the impact of aging or secular trends. In this way, our analysis addresses concerns about time-varying confounders that are important in studies with before-after designs. Fourth, we incorporate a number of control variables to address possible remaining confounders. The results did not change when we control for all time-invariant individual-level characteristics using fixed effects, although the results of our Hausman test indicate that these additional control variables are not required to produce an unbiased estimate. This result follows logically from the random, rolling nature of recruitment into the REGARDS study.

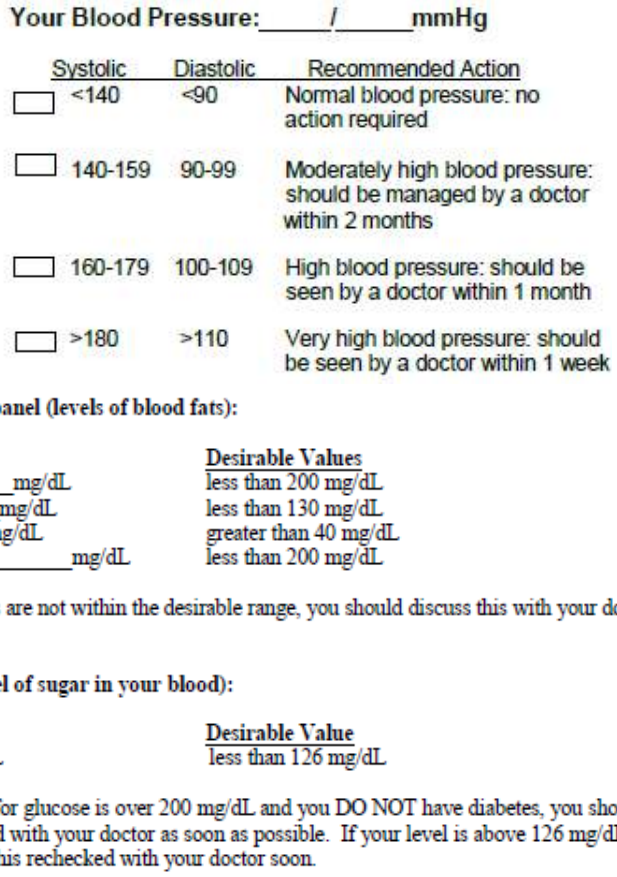
The results of our study should be interpreted with the relevant limitations in mind. Because we lack data from individuals who declined to participate in screening, we could not calculate the impact of being offered screening (i.e., the intent to treat effect). Instead, we calculated the impact of screening for individuals who are willing to be screened (i.e., the treatment on the treated effect). In addition, we cannot say whether our results will generalize beyond the group of REGARDS participants with available Medicare claims, namely, African American and white adults who were enrolled in traditional Medicare and not in Medicare Advantage.

Our findings have implications for new models of care being tested by the Center for Medicare and Medicaid Innovation. In care models such as Accountable Care Organizations and Accountable Health Communities, healthcare providers are incentivized to reach out to patients who have not recently been screened. Based on our findings, outreach to encourage screening is unlikely to exacerbate existing disparities in chronic condition care by

gender, race, region, or Medicaid dual eligibility because uptake of doctor visits after diagnosis did not vary by these factors. However, we found that the hardest-to-reach patients – those who lacked a usual healthcare provider – had marginally lower uptake of doctor visits for previously undiagnosed conditions. This result suggests that multi-pronged efforts to support and engage hard-to-reach patients, as in the Accountable Health Communities model, could become increasingly important to chronic condition care as more patients become diagnosed.

2.A Additional tables and figures

Figure 2.4. Text from the card and letter given to REGARDS participants informing them about their blood pressure and the results of their lab tests



Your Blood Pressure:_____/_____mmHg

	Systolic	Diastolic	Recommended Action
☐	<140	<90	Normal blood pressure: no action required
☐	140-159	90-99	Moderately high blood pressure: should be managed by a doctor within 2 months
☐	160-179	100-109	High blood pressure: should be seen by a doctor within 1 month
☐	>180	>110	Very high blood pressure: should be seen by a doctor within 1 week

Your Lipid panel (levels of blood fats):

Your Values	Desirable Values
Total: _____mg/dL	less than 200 mg/dL
LDL: _____mg/dL	less than 130 mg/dL
HDL: ____mg/dL	greater than 40 mg/dL
Triglycerides_____mg/dL	less than 200 mg/dL

If your values are not within the desirable range, you should discuss this with your doctor at your next visit.

Glucose (level of sugar in your blood):

Your Value	Desirable Value
_____mg/dL	less than 126 mg/dL

If your level for glucose is over 200 mg/dL and you DO NOT have diabetes, you should have this rechecked with your doctor as soon as possible. If your level is above 126 mg/dL, you should have this rechecked with your doctor soon.

Table 2.5: Chronic Conditions Warehouse ICD-9 codes related to diabetes, hypertension and high cholesterol

Condition	Included ICD-9 and CPT diagnosis codes
Diabetes	ICD-9 codes 249.00, 249.01, 249.10, 249.11, 249.20, 249.21, 249.30, 249.31, 249.40,249.41, 249.50, 249.51, 249.60, 249.61, 249.70, 249.71, 249.80, 249.81, 249.90,249.91, 250.00, 250.01, 250.02, 250.03, 250.10, 250.11, 250.12, 250.13, 250.20,250.21, 250.22, 250.23, 250.30, 250.31, 250.32, 250.33, 250.40, 250.41, 250.42,250.43, 250.50, 250.51, 250.52, 250.53, 250.60, 250.61, 250.62, 250.63, 250.70,250.71, 250.72, 250.73, 250.80, 250.81, 250.82, 250.83, 250.90, 250.91, 250.92,250.93, 357.2, 362.01, 362.02, 362.03, 362.04, 362.05, 362.06, 366.41 in any position
Hypertension	ICD-9 codes 362.11, 401.0, 401.1, 401.9, 402.00, 402.01, 402.10, 402.11, 402.90, 402.91, 403.00, 403.01, 403.10, 403.11, 403.90, 403.91, 404.00, 404.01, 404.02, 404.03, 404.10, 404.11, 404.12, 404.13, 404.90, 404.91, 404.92, 404.93, 405.01, 405.09, 405.11, 405.19, 405.91, 405.99, 437.2 in any position
High cholesterol	272.0, 272.1, 272.2, 272.3, 272.4 in any position
Face-to-face physician contact	CPT codes 99024, 99058, 99429, 99499, 99201-99288, 99291-99292, 99301-99337, 99341-99357, 99385-99387, 99395-99404

This table shows the classification algorithm from the Chronic Conditions Warehouse that is used to classify doctor visits in the Medicare data as relevant to evaluation and management of diabetes, high cholesterol and/or hypertension.
CPT: current procedural terminology. ICD-9: International Classification of Diseases, 9th Revision.
Source: Chronic Conditions Warehouse website (https://www.ccwdata.org/web/guest/condition-categories)

Table 2.6: Definitions used for diabetes, hypertension, and high cholesterol

Condition	Status	Definition
Diabetes (self-reported diagnosis, taking diabetes medication, or FPG>126 mg/dl / NFPG>200mg/dl)	No condition	No self-reported diagnosis of diabetes and FPG<126 mg/dl or NFPG<200mg/dl
	Undiagnosed	No self-reported diagnosis of diabetes, but FPG>126 mg/dl or NFPG>200mg/dl
	Diagnosed	Self-reported diagnosis of diabetes (when non-pregnant for women)
Hypertension (self-reported diagnosis, taking hypertension medication, SBP>140mmHg or DBP>90mmHg)	No condition	No self-reported diagnosis, SBP<140mmHg, and DBP<90mmHg
	Undiagnosed	No self-reported diagnosis of hypertension, but SBP>140mmHg or DBP>90mmHg
	Diagnosed	Self-reported diagnosis of hypertension (when non-pregnant for women)
High cholesterol (self-reported diagnosis, taking cholesterol medication, total cholesterol >200 mg/dl, LDL cholesterol>160 mg/dl, or HDL cholesterol<40 mg/dl)	No condition	No self-reported diagnosis, and cholesterol levels below cut-points defined based on recommendations provided by the ATP III guideline (Table 2.7).
	Undiagnosed	No self-reported diagnosis, but cholesterol levels above cut-points defined based on recommendations provided by the ATP III guideline (Table 2.7).
	Diagnosed	Self-reported diagnosis

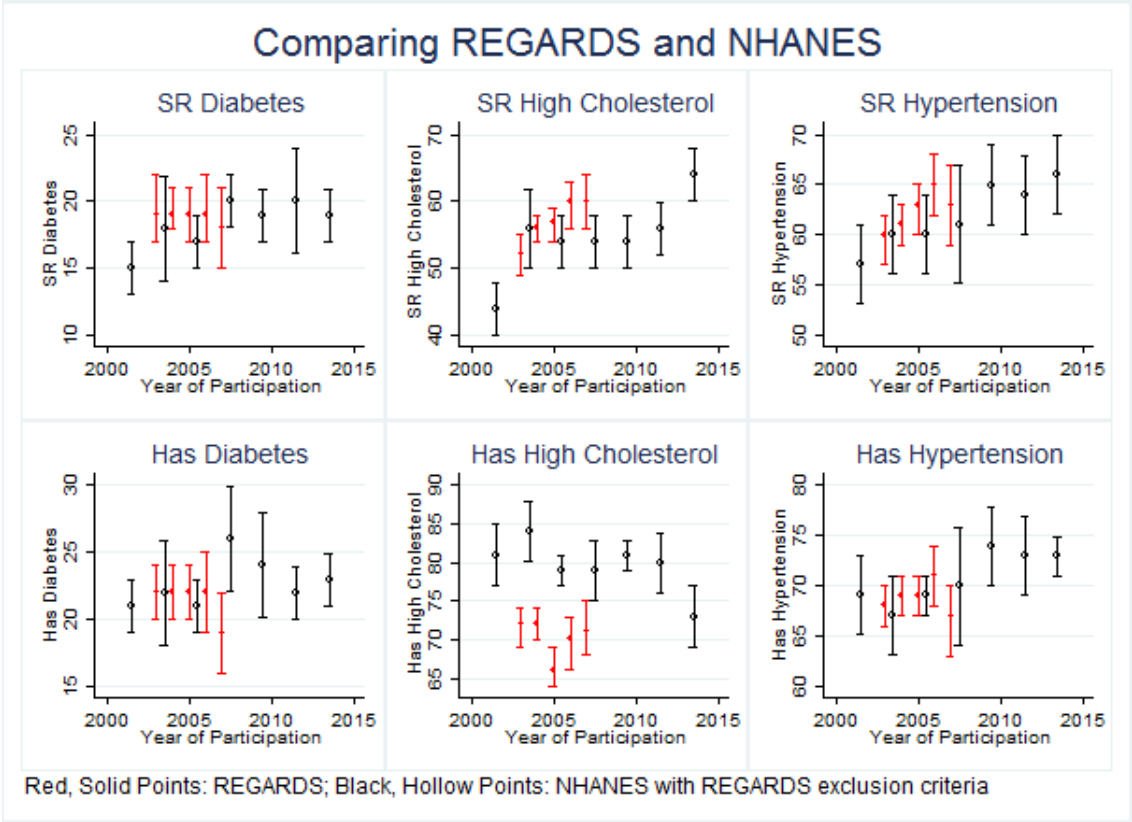
Note: ATP III: Third Report of the National Cholesterol Education Program (NCEP) Expert Panel on Detection, Evaluation, and Treatment of High Blood Cholesterol in Adults; FPG=fasting plasma glucose; NFPG=non-fasting plasma glucose; SBP=systolic blood pressure; DBP=diastolic blood pressure; HDL=high-density lipoprotein, LDL= low-density lipoprotein. Cholesterol levels recommended by the ATP III are described in Table 2.7.

Table 2.7: Cholesterol levels used as definition of high cholesterol based on target values recommended by the ATP III guideline

Participants' characteristics	LDL cholesterol for those with fasting blood sample	Non-HDL cholesterol for those with non-fasting blood sample or missing LDL cholesterol
History of CHD, CHD risk equivalents (including a history of stroke or diabetes) or 10-year CHD predicted risk > 20%	$\geq$ 100 mg/dL	$\geq$ 130 mg/dL
Multiple (2 or more) risk factors and 10-year predicted risk 10-20%	$\geq$ 130 mg/dL	$\geq$ 160 mg/dL
0-1 risk factors or multiple (2 or more) risk factors with 10-year predicted risk <10%	$\geq$ 160 mg/dL	$\geq$ 190 mg/dL

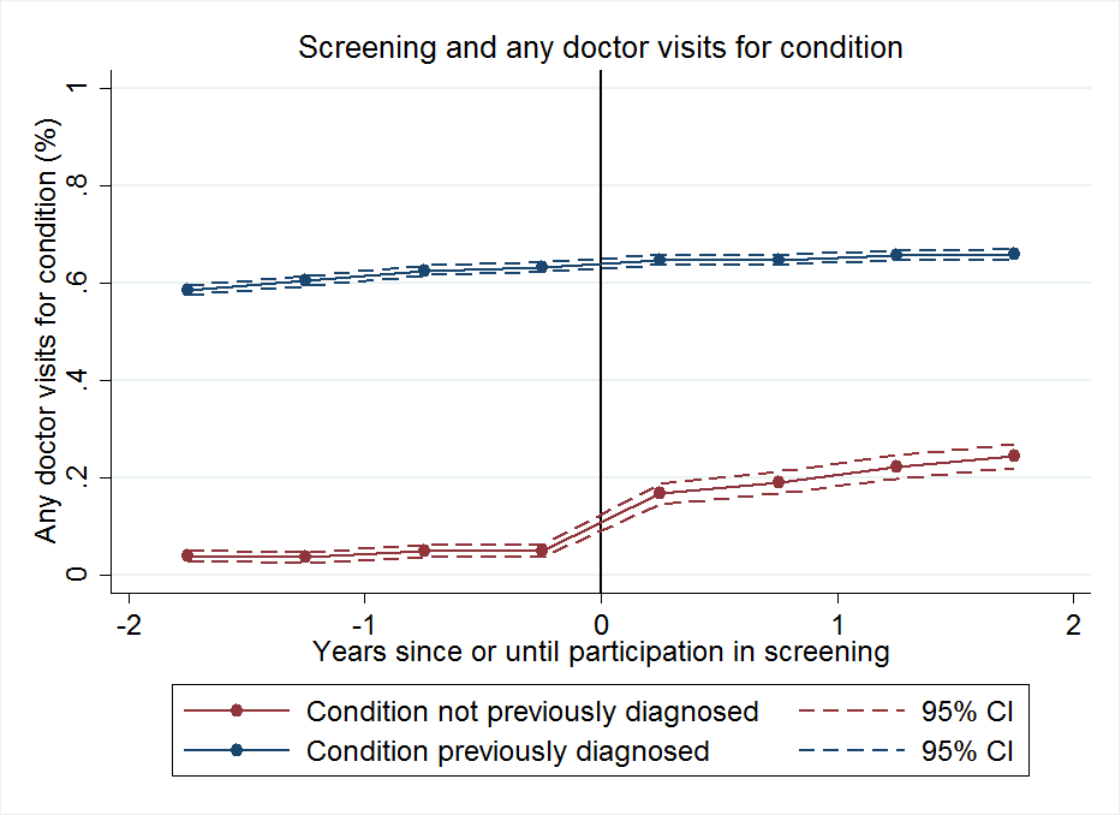
This table shows the cholesterol levels used to define high cholesterol in this paper.
ATP III: Third Report of the National Cholesterol Education Program (NCEP) Expert Panel on Detection, Evaluation, and Treatment of High Blood Cholesterol in Adults; CHD: coronary heart disease; HDL=high-density lipoprotein, LDL=low-density lipoprotein

Figure 2.5. Comparison of the REGARDS sample with the NHANES sample year by year, using comparable sample restrictions and sample weights



This figure shows that REGARDS participants have similar trends in biomarkers and levels and trends of self-reported diagnosis of high cholesterol, hypertension, and diabetes as a comparable sample of participants in the National Health and Nutrition Examination Survey in the years of interest. The comparable sample of participants was constructed by limiting the NHANES sample to only include participants who were aged 67 or older, were interviewed in English, and identified as African American or white. SR: Self Reported.

Figure 2.6. Screening by REGARDS and any doctor visits for undiagnosed and diagnosed conditions in the raw data



The figure shows that trends in doctor visits for diagnosed conditions are smooth before and after participation in REGARDS, whereas trends in doctor visits for previously undiagnosed conditions changed just after participation in REGARDS. This evidence is consistent with a hypothesis that REGARDS participation changed participants' care use patterns by informing participants about previously undiagnosed conditions.

Table 2.8: Tabulations of raw REGARDS data: Fraction of previously undiagnosed cases of diabetes, high cholesterol, or hypertension that receive a relevant evaluation and management visit from a physician each six months, according to Medicare claims data

Months since REGARDS	Has relevant claim	95% Confidence Interval
Diabetes		
<6 months before	3%	(0% to 5%)
<6 months after	26%	(18% to 34%)
6-11 months after	31%	(22% to 39%)
12-17 months after	35%	(26% to 44%)
18-23 months after	38%	(30% to 47%)
High cholesterol		
<6 months before	4%	(3% to 5%)
<6 months after	16%	(13% to 18%)
6-11 months after	16%	(14% to 19%)
12-17 months after	19%	(16% to 22%)
18-23 months after	21%	(18% to 24%)
Hypertension		
<6 months before	8%	(5% to 11%)
<6 months after	16%	(12% to 20%)
6-11 months after	21%	(16% to 25%)
12-17 months after	25%	(21% to 30%)
18-23 months after	27%	(22% to 32%)

Table 2.9: Placebo models: percentage point change in any semi-annual doctor visits for evaluation and management of previously diagnosed vs. undiagnosed conditions one year before REGARDS participation, using only data from before REGARDS (average marginal effects from regression)

	(1)	(2)	(3)	(4)	(5)	(6)
Diagnosed conditions						
After 1 year	2 (-8 to 12)	3 (-7 to 13)	3 (-6 to 13)	2 (-8 to 12)	3 (-7 to 12)	3 (-7 to 13)
After 2 years *[If no REGARDS]*	3 (-11 to 18)	4 (-10 to 18)	5 (-10 to 19)	3 (-11 to 17)	4 (-10 to 18)	5 (-10 to 19)
Undiagnosed conditions						
After 1 year	2 (-11 to 16)	2 (-11 to 16)	3 (-11 to 16)	2 (-12 to 15)	2 (-11 to 16)	2 (-11 to 16)
After 2 years *[If no REGARDS]*	3 (-17 to 23)	3 (-17 to 23)	3 (-17 to 23)	2 (-18 to 22)	3 (-17 to 23)	3 (-17 to 23)
Fixed effects	N	N	N	Y	Y	Y
Control for hospitalizations	N	Y	Y	N	Y	Y
Background trends vary by biomarkers	N	N	Y	N	N	Y

95% confidence interval in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

This table provides evidence that increases in doctor visits for undiagnosed conditions after REGARDS participation, shown in Table 2 and Table 3, were not produced by non-linearity in the trends prior to participation. The data in these placebo models only use data from prior to REGARDS. The rows of the table include average marginal effects from a multivariate panel data regression on the condition level indicating changes in healthcare utilization 1 year and (projected) 2 years after the time point 1 year prior to REGARDS participation. The columns indicate six regression specifications.

CHAPTER 3

POLICY ANALYSIS WITH ENDOGENOUS MIGRATION DECISIONS: THE CASE OF LEFT-BEHIND MIGRANT CHILDREN IN CHINA

3.1 Introduction

The well-being of children in migrant families is a topic of growing concern. China provides a case study with wider applicability because migrants within China lose access to some government social services, as many international migrants do. Inequality in economic opportunity has spurred large waves of rural-urban migration since the 1980s, in what has been called the largest rural-urban labor migration in human history [113]. The number of rural workers working in China's urban areas reached an estimated 274 million in 2014, accounting for about 36% of China's total workforce [114]. Due to the Chinese residential registration permit system (the hukou system), access to health and education services is more limited for individuals not living in their official place of residence, including family members of migrant workers [115–119]. Migrant children face difficulties enrolling in urban schools and cannot participate in urban-specific health care schemes [116,120–122]. As a result, many children remain in their place of official residence while one or both parents migrate. In 2010, there were 61 million so-called "left-behind" children in rural China who live apart from at least one parent, including 50 million under the age of 14 [123]. This represents an increase of over 2 million since 2005 and would comprise 38% of all rural children in China [123–125].

Establishing whether being left-behind by migrant parents is good or bad for children is important for shaping public policy, including targeted efforts to provide additional support for left-behind children and recent changes in hukou policy [126,127]. An extensive

body of empirical research has tested whether being left-behind by migrant parents is detrimental to Chinese children's well-being, and the results are highly mixed [119,127–144]. However, mixed results are perhaps to be expected: left-behind children experience both positive factors (parental remittances) and negative factors (parental absence), and therefore the impact of being left behind could be negative or positive depending on the balance of these two factors and their relative importance for any given child outcome [145]. For this reason, the empirical literature faces significant challenges in identifying the net impact of being left-behind by migrant parents on rural children's well-being.[1] This exemplifies a broader gap in the literature on rural-urban migration, wherein answers to policy-relevant questions such as when and to what extent such migration is desirable remain unclear [150].

This paper provides a new answer to this question by using economic theory to isolate a case where the net impact of being left-behind on children can be made clear. To provide policy-relevant conclusions, I focus on a policy-relevant subset of families. In particular, I examine whether being left-behind is good or bad for children who become left-behind by migrant parents as a result of a policy change. Under relatively parsimonious assumptions, I show that becoming left-behind decreases the well-being of children who become left-behind as a result of a policy change.

I begin by observing that if parents were previously indifferent between migrating with their child and leaving the child behind in the rural area, an increase in generosity of place-specific government services would determine their decision as to where their child should live. I then model the impact of policies designed to help children from migrant-sending regions in a framework where government services vary by migration status and place of

1. Self-selection into migration is a separate but also important concern for empirical studies. In particular, individuals self-select into migration based on factors such as health and social networks that can also determine their children's well-being [146–149]. The studies of left-behind migrant children cited above use various empirical tools to control for self-selection into migration, and imperfect corrections for selection may also account for some of the mixed results.

residence. In the model, parents are the sole decision-makers and care about their own consumption and their children's well-being [151–155]. Parents can choose between multiple migration scenarios (migrating and bringing their children, migrating and leaving their children behind, or not migrating), and they can choose their levels of time and monetary investments in their children.

I unpack the effects, including migration effects, of two hypothetical policy changes designed to help children from rural areas, which broadly reflect two recent policy changes in China. The first hypothetical policy change increases government services for rural non-migrants; this resembles the New Cooperative Medical Scheme, a government-sponsored health insurance scheme for rural residents introduced in 2003 [122,156]. The second hypothetical policy change involves increasing government services for rural migrants; this resembles ongoing reforms to the hukou system [157–160].

Both policies increase government services, which directly benefits children. However, additional effects related to migration can augment or undermine these benefits. In particular, when children become left-behind due to an increase in government services in rural areas, this decreases their well-being. (This holds both for children who previously lived in rural areas with parents and for children who previously lived in urban areas with their migrant parents.) In contrast, when children become no longer left-behind due to an increase in government services in urban areas, this increases their well-being. In summary, although increases in the generosity of government services for rural children directly help children, indirect effects can reduce or augment these benefits because of the harm to newly left-behind children or benefit to children no longer left-behind.

If few children become left-behind as a result of policy changes (for example, because few parents are near a margin of migrating and leaving children behind), these indirect effects will not be important in practical terms. I therefore test whether there was a significant increase in the fraction of rural children left-behind children by parents coinciding

with the roll-out of a government health insurance program for rural residents, the New Cooperative Medical Scheme, over 2003-2009. Employing panel data to run a within-child (fixed effects) analysis, I estimate that this policy change resulted in a 5 percentage point increase in the fraction of rural children left-behind by migrant parents. This finding supports a hypothesis that a significant number of parents are at or close to a margin of leaving children behind.

In addition to addressing an apparent puzzle in the empirical literature on left-behind children, this paper contributes to an economic literature on migration responses to public policy dating back to Tiebout's classic analysis [161]. (See [162] for a summary of empirical work on internal migration in response to policy in developing countries.) My results imply that "welfare magnet" migration effects can undermine the effects of a welfare policy if the people making decisions about migration (in my model, parents) are not the intended beneficiaries of the policy (in my model, children) [163–167]. This insight contributes to discussions about the optimal design of government policies. My findings are distinct from findings of previous studies indicating that sorting of heterogeneous agents across locations can undermine the observed effect of local policies and programs, because agents are not heterogeneous in my model [168–172].

This analysis also demonstrates a new approach to signing difficult-to-sign comparative statics in policy analysis. I sign a difficult-to-sign comparative static by restricting the focus to marginal treatment effects, the impact of taking a certain path of action for individuals at the margin of doing so [22,24,173–175]. A number of papers analyze marginal treatment effects by empirically estimating how marginal treatment effects vary across agents [22, 45,176]. My analysis differs from these in that I focus on broad conclusions that can be drawn about all marginal agents. In particular, I use the fact that parents were at the margin of leaving children behind as an additional equation to sign a comparative static that is otherwise difficult to sign using theory alone (the effect of being left-behind on child

well-being). A disadvantage of this approach is that I sign the quantity of interest without pinning down its magnitude or variability. However, this strategy simplifies the application of marginal treatment effects to policy-relevant questions and is also, to my knowledge, novel.

The rest of the paper proceeds as follows. Section 3.2 outlines the justification for the theoretical model and relates this paper to the existing empirical literature on the topic. Section 3.3 presents the model. Section 3.4 derives the result that being left-behind is harmful for children who become left-behind in rural areas as a result of a policy change and related results. Section 3.5 discusses the implications of the theoretical results for policy analysis. Section 3.6 presents an empirical test showing a increase in the fraction of children left-behind by migrant parents after a policy change in rural China, indicating that the migration effects analyzed in the model could be important in practice. Section 3.7 concludes.

3.2 Justification for the theoretical framework

In this section, I argue that two key factors should be taken into account in modeling the direct and indirect impacts of changes to Chinese government policy on rural *hukou* children: (1) parental money and time investments in children, and how these change as a result of migration decisions, and (2) location-specific government investment in children in China. These factors will be central to the theoretical model presented in the following section.

3.2.1 Changes in parental money and time investments in children, and implications for left-behind children

Many have argued that because left-behind children experience both positive factors (increased parental spending on children via remittances) and negative factors (decreased

parental time with children), the impact of being left behind could be negative or positive, depending on the balance of these two factors [140,144,145,177]. Remittances can account for a third of migrants' labor earnings in China and can increase the consumption of rural households by 20 percent, with the amount of remittances varying based on the needs of family members left behind [178–181]. Although remittances support children's education, these funds are garnered at the cost of parental time with children, which is also an important determinant of academic performance [140,182–184]. Loss of parental time can also increase the need for children's labor at home, which may interfere with schooling [142,185,186]. Accordingly, the observed impact of being left-behind by migrant parents on children's school attendance and performance has been mixed [144,187–189]. A similarly mixed effect has been shown for left-behind children's nutrition, perhaps because remittances support the purchase of nutritious food such as meat for left-behind children, while the lack of an educated caregiver is a risk factor for anaemia and underweight [127,137,190–192]. In contrast, the loss of parental time seems detrimental to children's mental health, with higher rates of psychological distress observed among children who were left-behind at an earlier age or who lack social capital to buffer negative impacts [128,133,193–196]. Similarly, lack of parental supervision and parental remittances may contribute to higher rates of unhealthy behaviors such as smoking and drinking and therefore to lower health-related quality of life among left-behind adolescents [129,132].

The theoretical model in this paper is informed by two patterns evident in this empirical literature. First, the empirical literature indicates that parental spending on children and parental time with children both contribute to child welfare and are differentially impacted by migration. Accordingly, I track both factors in the model. Second, the empirical literature indicates that parental spending on children and parental time with children differentially impact various domains of child development such as physical health, mental health, and educational progress which contribute to children's well-being. This raises

concerns about how to aggregate the mixed impacts of being left-behind on these various domains to address whether being left-behind is good or bad for children overall. To sidestep this issue, I use a single summary metric of child welfare in the model, as is done in the theoretical literature on intergenerational mobility, rather than modeling the impacts on each domain separately and then aggregating the results [151,152,154,155].

3.2.2 Location-specific government investment in children in China

Social services provided by the Chinese government follow a pattern wherein official residents (i.e., people with local hukou) are eligible for more benefits than people with non-local hukou [115,116,160,197]. In the empirical section, I discuss the New Cooperative Medical Scheme in China as an example of a policy which increases government services only for rural citizens who live in their place of official residence [122,198]. Although the hukou system is particular to China, lessons drawn from this context could be relevant for government policies in other contexts that use place-specific government benefits as incentives to reduce urban crowding, stem persistent rural-to-urban migration, or attract a particular type of resident to a community [161,171,199,200].

The most recent hukou reform, in July 2014, selectively increased availability of hukou in urban areas. The selective nature of the reform implied that, for many rural children, availability of government services may remain limited in many urban areas. In the reforms, policy-makers set a goal of granting hukou from urban locations to 100 million people who currently hold hukou from rural locations. These additional hukou were to be allocated based on size of the destination city: small cities should be "fully open," mid-sized cities should be opened in an "orderly" fashion, and hukous in the largest cities should remain "strictly controlled" [126]. As such, the reforms did not change two characteristics of the existing system: (1) hukou in more desirable cities are available only to those who

meet criteria set by the city (often, a points system based on factors like wealth, occupation, and education),[2] and (2) individuals who forgo their rural hukou forgo the land entitled to them in their home village. For many rural people, obtaining one of the urban *hukou* that are within their reach may continue to not be worth the cost [159,160,202].[3] Therefore, the pattern wherein rural people receive lower levels of government services in the city is expected to persist for many families and should be taken into account in modeling the response of the Chinese family to changes in government services.

3.3 Model

See Figure 3.1 for a summary of the model including policy inputs, parental decisions, and consequences for parents and children. The model is constructed to be as simple and general as possible while incorporating the key factors discussed above. In doing so, it incorporates some elements of classic models of labor migration and parental investment in children [152,155,204].

Each household contains one parent and one child, both with rural hukou.[4] Parents are the sole decision-makers in the household and maximize a smooth function of their own consumption (C) and children's welfare or expected utility (ψ):

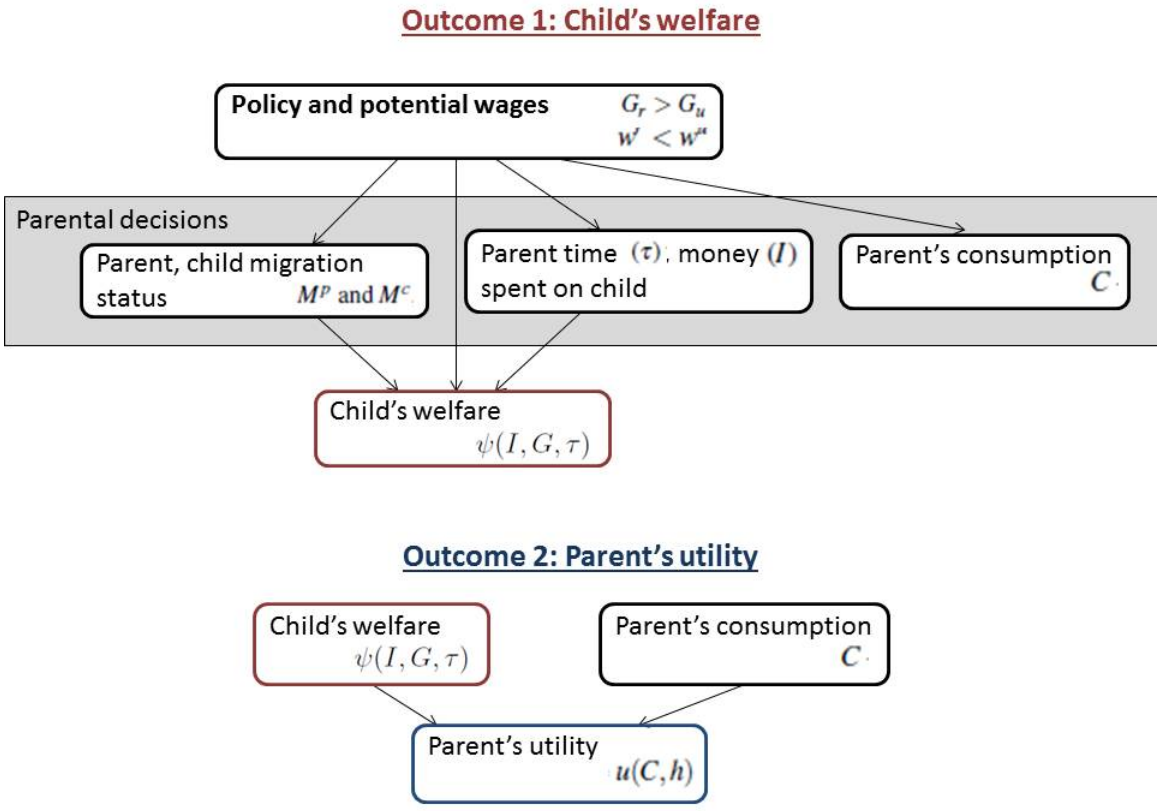
$$U = u(C, \psi)$$

2. Typically, desirable locations are larger cities providing more generous benefits [201].

3. This prediction is borne out by earlier hukou reforms. After hukou reform initiatives in 1997, only 1.39 million migrants (about 1%) had changed their hukou by 2002. In the desirable city of Harbin, only 200 migrants out of the 1 million migrants in the city were named "advanced workers" and given Harbin hukou [203]. National policy to increase offers of urban hukou to rural people, if implemented at the city level as noted above, are unlikely to give rural people equal access to urban public services in practice [197].

4. The one-parent, one-child setup helps to simplify the analysis for expositional purposes and has been previously employed in the theoretical literature [151,154].

Figure 3.1. Summary of the model including policy inputs, parental decisions, and child welfare



This setup follows the theoretical literature on parental investments in children and intergenerational mobility [151,152,154].

Parental migration and child migration are denoted as M^p and M^c, respectively. $M^p = 1$ if the parent migrates and $M^p = 0$ if the parent does not migrate. Likewise, $M^c = 1$ if the child migrates and $M^c = 0$ if the child does not migrate.

Each parent is endowed with one unit of time. When the child and parent live in the same location, the parent has the option of spending some fraction of their time (τ) with the child. The remainder of the time parents work, earning wage w which varies by location:

$$w = \begin{cases} w_u & \text{if parent migrates (that is, } M^p = 1) \\ w_r & \text{if parent doesn't migrate (that is, } M^p = 0) \end{cases}$$

Wages in these two locations are not altered by individual agents' choices [204].

In addition, parents have the option of investing money (of amount I) in their children. Therefore, parents face the budget constraint:

$$w(1 - \mathbb{I}[M^p = M^c]\tau) = C + I$$

where $\mathbb{I}$ denotes the indicator function.

Each child's welfare is determined by (1) the amount of time spent with the parent (τ);[5] and (2) monetary resources spent on him or her (m).[6] Spending on the child comes from the parents (I) and from the government (G); $m(I,G)$ is an aggregate of spending on the

5. In practice, when children are left-behind by (one or more) migrant parents they are cared for by an alternate caregiver, such as (only one parent or) grandparents. In the model, the value of alternative caregiver time is normalized to 0.

6. These inputs increase the child's welfare by increasing the child's health, psychological well-being, and educational performance - broadly, his skills or human capital. To link with the literature on intergenerational mobility, it is worth noting that these skills can increase his potential income in the future.

child so that $m_I > 0$ and $m_G > 0$. Government spending on the child varies based on the child's migration status: G_r or G_u in rural or urban areas, respectively. Child welfare can therefore be written as $\psi(m(I,G),\tau)$ where $G = M^c G_u + (1 - M^c) G_r$.

3.3.1 Assumptions

I assume that:

Assumption 3.3.1 *Parents care about their child's welfare and their own consumption, but with decreasing marginal returns to each.*[7]

$$u_\psi(C,\psi) > 0;\ u_C(C,\psi) > 0$$

$$u_{\psi\psi}(C,\psi) < 0;\ u_{CC}(C,\psi) < 0$$

Assumption 3.3.2 *$u(C,\psi)$ is additively separable in C and ψ:*

$$u(C,\psi) = \mu(C) + \nu(\psi)$$

Assumption 3.3.3 *Time and money investments in children increase child welfare.*[8] *However, there are decreasing marginal returns to each:*

$$\psi_m(m,\tau) > 0;\ \psi_\tau(m,\tau) > 0$$

7. Note that I am introducing simplifying notation of the form $g_x(x,y) = \frac{\partial g(x,y)}{\partial x}$ and $g_{xy}(x,y) = \frac{\partial^2 g(x,y)}{\partial x \partial y}$.

8. For example, these inputs can increase the child's human capital which in turn increases his earnings and consumption in the future and thereby his welfare.

$$\psi_{mm}(m,\tau) < 0;\ \psi_{\tau\tau}(m,\tau) < 0$$

These properties and the chain rule, plus $m_I > 0$ *and* $m_G > 0$ *(from the definition of m), imply* $\psi_I > 0$*,* $\psi_G > 0$*, and* $\psi_{IG} < 0$*.*

Assumption 3.3.4 *Parental time weakly increases the productivity of parental monetary investments in the child:*

$$\psi_{I\tau} \geq 0$$

For example, the books or educational toys may have more impact if the parent spends time teaching the children how to use them. Likewise, parental time with the child may have larger impact if the parent has purchased educational books or toys that they can use together.

Assumption 3.3.5 *The government spends more money on children who live in their official place of residence:*

$$G_r > G_u$$

As noted in section 3.2, this assumption is consistent with the hukou system in China, including recent and previous hukou reforms.

Assumption 3.3.6 *Wages for parents are higher in the urban area than in the rural area:*

$$w_r < w_u$$

This assumption is broadly consistent with the scale of rural-urban labor migration in China, including an estimated 270 million migrant workers in 2014 [114].

3.3.2 Parental decisions

3.3.2.1 Migration decisions

Given the above framework, each parent is faced with the following discrete choice problem:

- $M^p = 1$ and $M^c = 1$ (parent and child migrate together)
- $M^p = 0$ and $M^c = 0$ (neither parent nor child migrates)
- $M^p = 1$ and $M^c = 0$ (parent migrates and child is left behind)

Within each of these scenarios, the parent can select the optimal I, C, and perhaps τ, to maximize her utility. When the child is left behind, the parent is not able to spend time with the child and therefore τ is not a choice variable.

To define notation, let C^*_{10} and $\psi\left(m\left(I^*_{10}, G_r\right), 0\right)$ denote the parent's optimal C and ψ conditional on $M^p = 1$, $M^c = 0$ (that is, the first subscript in C^*_{10} indicates $M^p = 1$ and second indicates $M^c = 0$). Then $U^*_{10} = u\left(C^*_{10}, \psi\left(m\left(I^*_{10}, G_r\right), 0\right)\right)$ is the utility of the parent at the optimum conditional on $M^p = 1$, $M^c = 0$. The optimal choices and utility for the other migration scenarios are similarly denoted using stars and subscripts. With this notation, we can summarize parental choices and the corresponding utility as follows:

$$U = \max_{M^p, M^c} \begin{cases} U^*_{11} = & u\left(C^*_{11}, \psi\left(m\left(I^*_{11}, G_u\right), \tau^*_{11}\right)\right) \\ U^*_{00} = & u\left(C^*_{00}, \psi\left(m\left(I^*_{00}, G_r\right), \tau^*_{00}\right)\right) \\ U^*_{10} = & u\left(C^*_{10}, \psi\left(m\left(I^*_{10}, G_r\right), 0\right)\right) \end{cases}$$

Any of the three migration scenarios could be most appealing to parents, and the key tradeoffs are as follows. First, parents make more money in urban areas than rural areas by Assumption 3.3.6. Second, children receive more government services in the rural areas by

Assumption 3.3.5. Third, migrating parents are unavailable to spend time with left-behind children. If the importance of parental time is sufficiently large then migrating parents will not wish to leave their children behind.

3.3.2.2 Optimal decisions if the child and parent live in the same location

When the child and parent live in the same location ($M^p = M^c$), the parent has the option of forgoing some income to spend time with the child. In this case, the maximization problem can be summarized as:

$$\max_{I,\tau,C} \{u(C, \psi(m,\tau)) + \lambda (w(1-\tau) - C - I)\}$$

where λ is the Lagrange multiplier. The first order conditions are as follows:

$$\begin{aligned}
\{\lambda\} \quad & w(1-\tau) - C - I = 0 \\
\{I\} \quad & u_\psi \psi_I = \lambda \\
\{\tau\} \quad & u_\psi \psi_\tau \frac{1}{w} = \lambda \\
\{C\} \quad & u_C = \lambda
\end{aligned}$$

At the parent's preferred point, the marginal utility of the money spent on the parent's consumption, the parent's monetary investments in children, and forgone earnings due to time spent with children are equated.

3.3.2.3 Optimal decisions if the parent migrates and child is left-behind

When the child is left-behind by their migrant parent ($M^p = 1,\ M^c = 0$), the parent doesn't have the option of spending time with the child on a regular basis; that is, $\tau = 0$ and τ is no longer a decision variable. Therefore, the parent's maximization problem becomes:

$$\max_{I,C} \{u(C, \psi(m,0)) + \lambda(w - C - I)\}$$

The first order conditions are:

$$\begin{aligned} \{\lambda\} \quad & w - C - I = 0 \\ \{I\} \quad & u_\psi \psi_I = \lambda \\ \{C\} \quad & u_C = \lambda \end{aligned}$$

In this case, the marginal utility of the parent's monetary investments in children is only equated with the marginal utility of money spent on the parent's consumption.

3.4 Deriving parental responses to policy changes and implications for child welfare

3.4.1 Two policy changes to be considered

I consider parental responses to two hypothetical policy changes. For each change, I will apply the model to analyze the impact of the policy change on children's welfare.

3.4.1.1 First policy change: Increase G_r only.

First, the government could increase spending on rural hukou children who live in their rural, official place of residence (that is, increase G_r without changing G_u).

One example of such a policy is the New Cooperative Medical Scheme, which provided government-subsidized health insurance for rural Chinese people living in their official place of residence starting in 2003 [122,205]. Migrants from rural areas could not bring their benefits with them in practice [206]. Therefore, the policy essentially increased G_r without increasing G_u.

3.4.1.2 Second policy change: Increase G_u only.

Second, the government could increase spending on rural hukou children who have migrated away from their official place of residence (that is, increase G_u without changing G_r). As discussed in section 3.2, recent reforms to the hukou system will increase availability of government services in urban areas for some rural children [157,159].

3.4.2 Parental reactions to policy changes and implications for child welfare

I now analyze the policy changes and their impacts on parental utility and child welfare. First, I focus on the case in which a policy change does not trigger a change in migration. Second, I focus on the case where parents were previously on the margin of leaving children behind, so that a policy change determines whether or not children are left behind. In the next section, I will combine the results to discuss how these migration effects shape the net impact of a policy on the welfare of children who become left-behind (or no longer left-behind) after the policy is implemented.

3.4.2.1 Case 1: Migration decisions do not change as a result of the policy change.

I first consider the case where G does not change for the child as a result of the policy (i.e., G_r changes but the child lives in the urban area, or G_u changes but the child lives in the rural area), and show that children and parents are unaffected by the policy change in this case. I next consider the case where G changes for the child as a result of the policy (i.e., G_r changes and the child lives in the rural area, or G_u changes and the child lives in the urban area), and show that both children and parents benefit from the policy change in this case.

If G does not change

Result 3.4.1 *If a policy change does not affect G given a child's current place of residence and migration decisions do not change as a result of the policy change, then neither child welfare nor parental utility will be affected by the policy change.*

Without loss of generality, consider the example of a child who lives in an urban area ($M^c = 1$); say that G_r increases due to a policy change but the change is not large enough to provoke the parent of the child to send him home. For this child, G does not change because $G = G_u$, and there is no change in G_u.

If G does not change and no other inputs relevant to parental optimization change, then C, I and τ also do not change. Thus, without any change in any of the inputs, child welfare ($\psi(m(I,G),\tau)$) and parental utility $u(C,\psi)$ also do not change. This is Result 3.4.1.

If G changes

Result 3.4.2 *If a policy change increases (decreases) G given a child's current place of residence and migration decisions do not change as a result of the policy change, then child welfare and parental utility will increase (decrease).*

As a first step toward proving this result, I show that governmental investment of level G is equivalent to having the parents receive a monetary transfer of a certain amount, $k(G)$.

Adding notation to fix ideas, say that if G is set to 0 but the parent is given a transfer of size $k(G)$ then the parent will spend $I^*_{k(G)}$ on the child. If instead the government directly invests amount G in the child and gives no transfer to the parent, the parent will choose to spend I^*_G on the child. I want to show that for any amount of government spending on children G, there exists a direct transfer to parents of amount $k(G)$ that would produce the same ψ^* and C^*, thereby producing identical levels of child welfare and utility for parents.

I will work with the optimization problem for children who live in the same place as parents, but the Equations I obtain will also hold for left-behind children. This is because the two first-order conditions that both equal λ and are set to equal each other are equivalent to two first-order conditions for the left-behind child case, and the third first-order condition for the left-behind child case is unaffected by the change to the budget constraint (see section 3.3.2.3).

With the hypothetical transfer $k(G)$ included and G eliminated, the budget constraint would become:

$$w(1-\tau)+k(G)=C+I_{k(G)}$$

and therefore the parent's maximization problem would become:

$$\max_{I_{k(G)},\tau,C}\left\{u(C,\psi(m,\tau))+\lambda\left(w(1-\tau)+k(G)-C-I_{k(G)}\right)\right\}$$

where $m = m\left(I_{k(G)}, 0\right)$ because $G = 0$. This maximization problem yields the first-order conditions:

$$\begin{aligned}
\{\lambda\} \quad & w(1-\tau) + k(G) - C - I_{k(G)} = 0 \\
\{I\} \quad & u_\psi \psi_I = \lambda \\
\{\tau\} \quad & u_\psi \psi_\tau \frac{1}{w} = \lambda \\
\{C\} \quad & u_C = \lambda
\end{aligned}$$

The second and fourth first-order conditions can be combined to yield:

$$u_\psi \psi_{I_{k(G)}} = u_C$$

The comparable condition for the cases without transfer $k(G)$ and where $G > 0$ is:

$$u_\psi \psi_{I_G} = u_C \tag{3.1}$$

Therefore if we define the relationship between G and $k(G)$ so that:

$$\psi_{I_G} = \psi_{I_{k(G)}} \tag{3.2}$$

then the transfer $k(G)$ will produce an identical parental optimization problem and identical outcomes to governmental investment in the child of amount G.

I now establish $\frac{\partial k(G)}{\partial G} > 0$ by examining what changes are needed to maintain equality 3.2 when G changes. First, I examine the left hand size of equality 3.2. $\psi_{IG} < 0$ (see Assumption 3.3.3) implies that a decrease in G would increase ψ_I, i.e., increase the left-

hand side of equality 3.2. The right-hand side of equality 3.2, $\psi_{I_{k(G)}}$, must therefore also increase. $\psi_{II} < 0$ (Assumption 3.3.3) implies that increasing $\psi_{I_{k(G)}}$ requires decreasing I. Decreasing I can be accomplished by decreasing the parent's budget - that is, by decreasing the transfer to parents $k(G)$. In summary, if Equation 3.2 holds, a decrease in G implies an decrease in $k(G)$. By similar logic, an increase in G would imply an increase in $k(G)$. I conclude that in terms of impacts on parental utility and children's welfare, an increase in G produces an effect equivalent to a monetary transfer to parents, i.e., an increase in $k(G)$. The result that government spending on children is equivalent to a monetary transfer to parents is also discussed in previous literature, such as Becker's Treatise on the Family (p.192) [207].

Having established that an increase in G is equivalent to a transfer of money to parents of amount $k(G)$, I next consider how ψ^* changes when parents receive such a transfer. By Assumption 3.3.1, parental utility is increasing in both consumption C and welfare of children ψ, but with decreasing marginal returns to each. As a result, the transfer $k(G)$ must be split so that both C^* and ψ^* increase, because to do otherwise would violate the first-order conditions as combined in Equation (3.1). In summary:

$$\frac{\partial C^*}{\partial k(G)} > 0 \text{ and } \frac{\partial k(G)}{\partial G} > 0 \implies \frac{\partial C^*}{\partial G} > 0$$

$$\frac{\partial \psi^*}{\partial k(G)} > 0 \text{ and } \frac{\partial k(G)}{\partial G} > 0 \implies \frac{\partial \psi^*}{\partial G} > 0$$

I have shown that ψ^* is increasing in G and $u(C^*, \psi^*)$ is increasing in G. This is Result 3.4.2.

3.4.2.2 Case 2: Parents' decisions about leaving children behind change as a result of the policy change.

Children become left-behind after their parents cross one of the following two margins:

$$U_{10}^* = U_{11}^*$$
$$U_{10}^* = U_{00}^*$$

To consider why a policy change might make a parent cross one of these margins, note that Results 3.4.1 and 3.4.2 imply:

$$\frac{\partial U_{11}^*}{\partial G_r} = 0;\ \frac{\partial U_{10}^*}{\partial G_r} > 0;\ \frac{\partial U_{00}^*}{\partial G_r} > 0 \tag{3.3}$$

$$\frac{\partial U_{11}^*}{\partial G_u} > 0;\ \frac{\partial U_{10}^*}{\partial G_u} = 0;\ \frac{\partial U_{00}^*}{\partial G_u} = 0 \tag{3.4}$$

Based on Equations (3.3) and (3.4), a policy that only increased G_r could increase the number of left-behind children because a parent that previously had $U_{10}^* = U_{11}^*$ (and possibly, also $U_{10}^* = U_{00}^*$) would now prefer to send their child back to the rural area. Likewise, a policy that only increased G_u could reduce the number of left-behind children because parents at $U_{10}^* = U_{11}^*$ would now prefer to bring their child to live with them in the urban area. (Results 3.4.1 and 3.4.2 imply that a policy that only increases G_u would not affect parents at the $U_{10}^* = U_{00}^*$ margin.)

I therefore consider parents at the two margins of leaving children behind and compare ψ_{10}^* with ψ_{11}^* and ψ_{10}^* with ψ_{00}^* to show the following:

Result 3.4.3 *$\psi^*_{10} < \psi^*_{11}$ and $\psi^*_{10} < \psi^*_{00}$ for families at the margins $U^*_{10} = U^*_{11}$ and $U^*_{10} = U^*_{00}$, respectively.*

Result 3.4.4 *An incremental increase in G_r will decrease the welfare of children who become left-behind by parents as a result of this change. In contrast, an incremental increase in G_u will increase the welfare of left-behind children who become migrant children as a result of this change.*

In order to prove Result 3.4.3, I first note that for a parent at $U^*_{10} = U^*_{11}$, three scenarios exist:

1. $C^*_{10} = C^*_{11}$ and $\psi^*_{10} = \psi^*_{11}$
2. $C^*_{10} < C^*_{11}$ and $\psi^*_{10} > \psi^*_{11}$
3. $C^*_{10} > C^*_{11}$ and $\psi^*_{10} < \psi^*_{11}$

These are the only three possible scenarios because utility takes the form $U = u(C, \psi)$ where $u_C > 0$ and $u_\psi > 0$. Therefore, a scenario such as $C^*_{10} < C^*_{11}$ and $\psi^*_{10} < \psi^*_{11}$ would violate $U^*_{10} = U^*_{11}$.

For similar reasons, three scenarios exist for a parent at $U^*_{10} = U^*_{00}$:

1. $C^*_{10} = C^*_{00}$ and $\psi^*_{10} = \psi^*_{00}$
2. $C^*_{10} < C^*_{00}$ and $\psi^*_{10} > \psi^*_{00}$
3. $C^*_{10} > C^*_{00}$ and $\psi^*_{10} < \psi^*_{00}$

In both cases, Scenario 1 and Scenario 2 are inconsistent with the first-order conditions of the model. The proofs are included in Appendix 3.A, 3.B, 3.C and 3.D. Therefore, it must be the case that $\psi^*_{10} < \psi^*_{11}$ for parents at $U^*_{10} = U^*_{11}$ and $\psi^*_{10} < \psi^*_{00}$ for parents at $U^*_{10} = U^*_{00}$. This is Result 3.4.3. (Result 3.4.3 signs marginal treatment effects, i.e.,

the impact of becoming left-behind on children who are at the margin of becoming left-behind [175].)

By Equations 3.3 and 3.4, migrant parents sufficiently close to the margin of $U^*_{10} = U^*_{11}$ would send migrant children to live in the rural area (i.e., to become left-behind) if G_r increased, and would bring left-behind children to live with them in the urban area (i.e., to become migrant children) if G_u increased. Likewise, non-migrant parents sufficiently close to the margin of $U^*_{10} = U^*_{00}$ might migrate without their children and leave children behind if G_r increased, etc. Result 3.4.3 also states that $\psi^*_{10} < \psi^*_{11}$ for families with $U^*_{10} = U^*_{11}$ and $\psi^*_{10} < \psi^*_{00}$ for families with $U^*_{10} = U^*_{00}$. By continuity of ψ, changes near the optimum will be small; so when we compare ψ^*_{10} with ψ^*_{11} or ψ^*_{00} for families at the two relevant margins, the relationships $\psi^*_{10} < \psi^*_{11}$ and $\psi^*_{10} < \psi^*_{00}$ should still hold. This is Result 3.4.4. (This signs the effect of a small policy change, or a marginal policy-relevant treatment effect, for the subset of families sufficiently close to the margin of leaving children behind [22].)

3.4.3 Extending and generalizing the model

The model I present is general and simple for tractability and ease of interpretability, but I believe that several extensions could be readily accomodated. First, although I present a simple model of the family with only one parent and one child, the notation could be re-interpreted to describe a two-parent family where one or both parents are close to a margin of migration. If one or more parents decide to migrate without bringing the child, this would increase the household's total earning power while limiting total parental time with the child, the same trade-offs analyzed in the current setup. Second, the availability of grandparents to care for the children could be modeled explicitly; in the current setup, the value of caregiver time is normalized to zero for the non-parental outside option for simplicity. Third, although I present a one-period model, C and ψ could be reinterpreted as

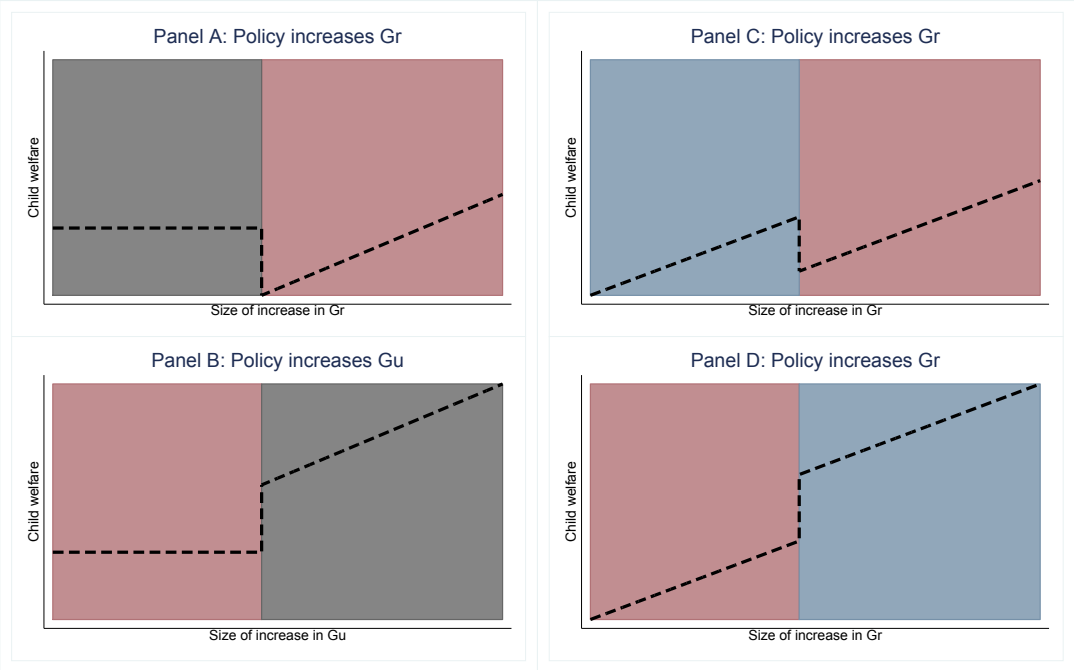
lifetime consumption and lifetime welfare of the child, with a focus on the aspects of child welfare that are sensitive to parental and government inputs during the child's formative years. Finally, C could be reinterpreted as a bundle of all parental spending that increases the parent's utility without affecting the child's welfare, including the "purchase" of non-child care leisure time at the cost of additional forgone wages.

3.5 Combining the results for policy analysis

I discuss four general cases that exemplify how policy changes can place children at risk for being left-behind and implications for child welfare. These four cases capture how, based on Equations (3.3) and (3.4), children can become left-behind or no longer left-behind as a result of policy changes that increase either G_r or G_u. The first two cases focus on children whose parents cross the $U^*_{11} = U^*_{10}$ margin as a result of a policy change, and the second two cases focus on children whose parents cross the $U^*_{00} = U^*_{10}$ margin as a result of a policy change. I combine Result 3.4.4 with Results 3.4.1 and 3.4.2 to capture the net impact of the policy on the welfare of children who become left-behind (or no longer left-behind) as a result of the policy.

In the first case, a child lives with his parent in the urban area before a policy that increases G_r is implemented. A small increase in G_r would not change the parent's decision about the child's migration status and as such would not affect the child's welfare by Result 3.4.1. If the increase in G_r were sufficiently large, however, the parent would reach and cross the $U^*_{11} = U^*_{10}$ margin and leave the child behind in the rural area, which would harm the child by Result 3.4.4. This can be considered an indirect effect of the policy on the child. After becoming left-behind, the child would only benefit from any further increases in G_r by Result 3.4.2, as a direct effect of the increase in government spending on children. Therefore, a sufficiently large increase in G_r could overwhelm the harmful migration ef-

Figure 3.2. Policies that increase government spending in rural or urban locations can affect whether children are left-behind by migrant parents, which shapes the impact of the policy change on child welfare: Four representative examples



The four panels demonstrate how increases in place-specific government spending on children can change whether children are left-behind by migrant parents, thereby augmenting or undermining the impact of changes in government spending on child welfare. The horizontal axis of each graph indicates the size of the increase in government spending on children in urban areas (G_u) or in rural areas (G_r), and the vertical axis indicates child welfare. Red areas denote that a child is left-behind by their migrant parent in the rural area, gray areas denote that a child lives with their migrant parent in the urban area, and blue areas denote that the child lives with their parent in the rural area.

fects so that the net impact of the policy on child welfare is positive. A graphical depiction is shown in Panel A of Figure 3.2.

In the second case, a child is left-behind in a rural area by her migrant parent before a policy change increasing G_u is implemented. As before, a small increase in G_u would not change the parent's decision about the child's migration status and as such would not affect the child's welfare by Result 3.4.1. If the increase in G_u were sufficiently large, the parent would reach and cross the cross the $U^*_{11} = U^*_{10}$ margin and bring the left-behind child to

the urban area as a migrant, which would benefit the child by Result 3.4.4. Any further increases in G_u would further benefit the child by Result 3.4.2. A graphical depiction is shown in Panel B of Figure 3.2.

The above examples focus on children whose parents cross the $U_{11}^* = U_{10}^*$ margin of leaving them behind. Based on Equations (3.3) and (3.4), these examples capture the two ways a policy change can affect the number of left-behind children by this margin. However, children can also become left-behind after parents cross the $U_{00}^* = U_{10}^*$ margin, as I now consider.

Equations (3.3) and (3.4) imply that changes to G_u cannot cause parents to cross the $U_{00}^* = U_{10}^*$ margin but changes to G_r might. I cannot know without making further assumptions which direction parents might move across this margin as a result of an increase to G_r, and therefore I consider both cases. First, a child living in the rural area with his parent might see the parent migrate and leave him behind after a sufficiently large increase in G_r. Second, a left-behind child living in the rural area might see his migrant parent return to the rural area after a sufficiently large increase in G_r. In both cases, the children live in the rural areas and therefore benefit from the increase in G_r by Result 3.4.2. The changes to the child's left-behind status would then reduce or augment these benefits in these two cases, respectively, by Result 3.4.4. These impacts on child welfare are graphically depicted in Panels C and D of Figure 3.2.

These examples demonstrate two general predictions about the impact of increasing place-specific government spending on child welfare. An increase in G_u has direct effects that are beneficial for children and could have indirect effects that are also beneficial for children, if children become no longer left-behind as a result of the policy. In this case, it is clear that children's welfare can only be increased by the policy. In contrast, an increase in G_r has direct effects that are beneficial for children and could have indirect effects that are harmful if children become left-behind as a result of the policy. The extent to which indirect

effects undermine the benefits of a policy that increases G_r will depend on the size of the increase in G_r[9] and on how many parents are close to a margin of leaving children behind. In the following section, I conduct an empirical investigation of whether additional children became left-behind by migrant parents after a recent increase in government services in rural China.

3.6 Empirical test: Do children become left behind after an increase in rural government services?

In the previous section, I analyzed how policy changes can influence parents' decisions about whether to migrate and leave children behind, and showed that these migration effects can reduce the benefits to children of increasing government spending. However, whether parents actually leave children behind as a result of policy changes is an empirical question. If no parents are near the margin of leaving children behind, then a policy change cannot increase the number of children left-behind. In contrast, if many parents are near a margin of leaving children behind, then implementation of a policy that increases governmental spending in rural areas but not urban areas (i.e., increases G_r but not G_u) could increase the number of left-behind children.[10]

The section addresses this issue by examining whether more children become left-behind after an increase in rural government services. The sub-sections below detail the

9. As noted above, a large enough increase in G_r could create positive effects that outweigh the negative effects associated with an increase in the number of left-behind children.

10. To be precise, testing whether children become left-behind after an increase in rural government spending is not the same as testing whether a significant fraction of parents are near a margin of leaving children behind. The fact that parents near the $U^*_{00} = U^*_{10}$ margin might return home after a generous increase in G_r would work against finding significant changes in the fraction of children left-behind after a policy change; this would also countervail the harmful effects of increasing the number of left-behind children by the $U^*_{11} = U^*_{10}$ margin. More broadly, the change in number of left-behind children is the main indirect effect that I have mapped to child welfare in this paper and therefore I believe it is the relevant quantity to measure.

policy used, the panel data on Chinese families used to conduct the test, and the results of the test.

3.6.1 Policy change: The New Cooperative Medical Scheme

The New Cooperative Medical Scheme (NCMS) was a locally implemented, voluntary government-sponsored health insurance scheme for China's rural population. As I argue below, it was a significant policy change which exemplified the Chinese social policy framework wherein government services are targeted to citizens according to their place of residence and *hukou*.

The introduction of NCMS represented an important increase in availability of health insurance in rural areas. Before the implementation of the NCMS in 2003, the majority of the rural population in China had no health insurance and poor health was a leading cause of household poverty in rural areas [208]. The Cooperative Medical System of the Mao era, which had once covered up to 90% of Chinese peasants, disappeared after the introduction of the Household Responsibility System in 1979 and a city-based social health insurance scheme took its place, offering no coverage to rural households or migrant laborers. As a result, many families had to pay for health services out of pocket. Out of pocket payments accounted for 20% of total health expenditure in China in 1978 but reached almost 60% in 2002 [209,210]. An estimated 16% of rural households incurred catastrophic medical spending the year the NCMS was introduced [211]. NCMS premiums were 10 yuan per year per individual enrolled; combined with government contributions of about 40 yuan, funding per enrollee equalled about a third of typical annual medical expenditure for a rural person in western or central China at the time [212]. The scheme was rolled-out aggressively, covering 86% of the rural population in 2007 and 90% in 2011 [211,213], including more than 95% of rural counties [211,214].

The benefits of the health insurance scheme, however, were only available to individuals living in their official place of residence. Targeted government insurance programs in the urban areas, including the Urban Employee Basic Medical Insurance and the newer Urban Resident Basic Medical Insurance plan, covered urban hukou residents but not migrant workers or their children [205]. In addition, although migrant workers could opt to enroll in the NCMS, the majority opted not to participate because reimbursement for non-local hospitals - if local governments chose to provide it - was often slow and inconsistent [198].[11] Therefore, the introduction of NCMS in rural areas effectively increased government benefits for non-migrants without significantly increasing benefits for migrants.

3.6.2 Data: The China Health and Nutrition Survey

The China Health and Nutrition Survey (CHNS) is an excellent resource for testing whether parents become more likely to leave children behind in rural areas after the introduction of NCMS. The CHNS is a longitudinal survey with nine waves of data collection over 1989 to 2011. The survey follows families in rural and urban areas of nine Chinese provinces (Guangxi, Guizhou, Heilongjiang, Henan, Hubei, Hunan, Jiangsu, Liaoning, and Shandong) and the baseline sample of the CHNS included 3,795 households or 15,917 individuals. The data include information on local government services (including availability of NCMS) and family composition. Tracking changes in family members' migration status after the introduction of NCMS is also possible due to the longitudinal nature of the study and detailed survey questions about migration of parents and children in the past and

11. Also, few employers offer migrant workers health insurance that could compensate for missing NCMS benefits [206].

Table 3.1: Characteristics of the rural villages sampled related to rural wages, child care, and migration

	Mean	SD
Typical daily wage, male worker	16.8 yuan	7 yuan
Typical daily wage, female worker	13.2 yuan	6 yuan
Typical daily wage, unskilled farm laborer	14.5 yuan	8 yuan
Monthly cost of child care for young children	84 yuan	57 yuan
% adults in this village who migrate for work, 1+ months	26%	
% adult interviewees caring for children in past week	63%	
	N	%
Total villages	150	
Village has child care for children under 3 years?	31	21%
Village has child care for children aged 3-6 years?	52	35%
Village has public primary school	122	81%
Village has public lower-middle school	38	25%
Village has public upper-middle school	17	11%

present.[12] Selected characteristics of rural communities and adults sampled are listed in Table 3.1.

3.6.3 Results of the test

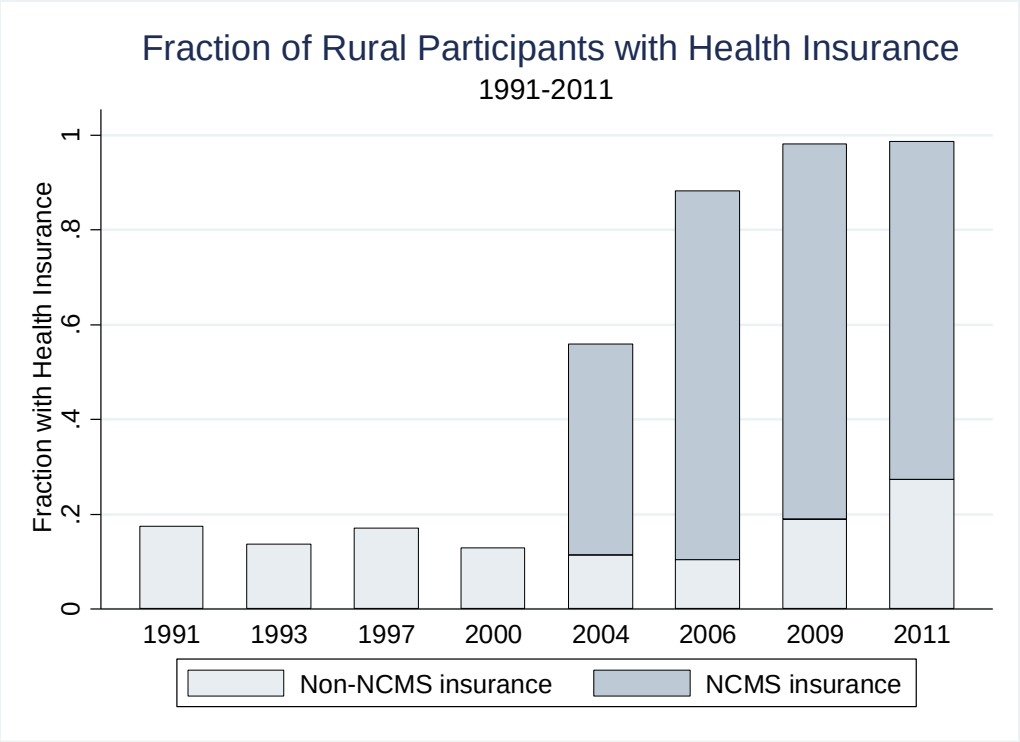
This empirical analysis uses the plausibly exogenous roll-out of the NCMS to test whether increasing government services in rural areas (increasing G_r but not G_u in the parlance of the model) significantly increases the fraction of children who are left-behind in rural

12. Questions related to migration include:

- *(For all people present in previous waves; parents and children can be linked)* Is this person still a household member? If no: What year and month did the person move out of the house? Where does this person live now? If yes: Does this person still live in the household? If not, how long has this person been away from home?
- *(For all children, to link the responses)* Who is the child's father? Who is the child's mother?

Matching standard definitions for two-parent households in China, I consider a child to be left-behind if there is evidence from any of the questions above that the child lives in the rural area and one or both of their parents currently lived elsewhere.

Figure 3.3. The New Cooperative Medical Scheme increased the availability of health insurance for rural residents starting in 2003



Source: Author's analysis of China Health and Nutrition Survey panel data

areas. A positive finding would indicate that there are a significant number of parents near the margin of leaving children behind, so that endogenous migration effects are important to consider in policy analysis.

If parents were not aware of the CHNS policy, then migration impacts of the policy would be implausible. Therefore, I first provide evidence that CHNS represented a noticable change to available health benefits in rural China. Figure 3.3 shows rates of health insurance coverage in rural China and the contribution of NCMS, according to rural Chinese people surveyed by the CHNS. The data show that health insurance coverage in rural China increased substantially over 2003 to 2011 and that survey respondents attributed much of this change to NCMS.

The rural locations sampled in the CHNS show a gradual roll-out of NCMS, such that about 20% of survey respondents in rural areas had access to NCMS in their community in 2004, as compared with 64% in 2006 and over 90% in 2009 and 2011. To examine whether the roll-out of NCMS is plausibly exogenous to parental migration decisions, I look for relationships between NCMS roll-out and observed determinants of migration from the Chinese literature [149,179]. Selected pre-NCMS predictor variables for this analysis include cost and availability of local child care services for very young children (aged <3 years) and young children (aged 3-6 years); average years of education among adults; average household size; health[13]; size of local migrant network[14]; remoteness[15]; and local wages for male and female workers.

As shown in Table 3.2, I find that NCMS may have been more likely to be implemented rapidly (by 2004) in communities with higher wages for a typical local male worker, although this was not significant at the 5% level. I do not observe associations between timing of NCMS roll-out and measures of size of the migrant network, availability or price of child care for young children, remoteness of the community, wealth and education of local adults, location in a model township, or health prior to NCMS. There was no evidence of selection in 2009 because roll-out of the program was nearly ubiquitous at that time. Despite these favorable results, I add community-level and sometimes child-level fixed effects as a conservative approach.

Limiting the sample to children born in rural areas, I examine whether the implementation of NCMS is associated with an increase in the fraction of rural children left-behind by one or more migrant parents. I use a panel-data regression following children over multiple

13. Measured as prevalence of disease or injury in the last four weeks.

14. Measured as fraction of adults who migrate for work for at least a month at a time.

15. Measured as distance to the closest train station.

Table 3.2: Predictors of NCMS Roll-Out

	(1) By 2004	(2) By 2006	(3) By 2009
Distance: village to township seat (km)	-0.042	0.016	0.000
	(0.029)	(0.023)	(0.000)
Monthly cost of child care (children <6 years)	-0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Child care available (children <3 years)	-0.006	-0.002	0.000
	(0.168)	(0.138)	(0.000)
Child care available (children 3-6 years)	0.140	-0.032	0.000
	(0.127)	(0.102)	(0.000)
Typical daily wage (male workers, yuan)	0.026*	0.021	0.000
	(0.015)	(0.013)	(0.000)
Typical daily wage (female workers, yuan)	-0.024	-0.013	0.000
	(0.018)	(0.014)	(0.000)
% adults migrating for work,	-0.002	0.004	0.000
1+ months	(0.003)	(0.003)	(0.000)
Distance to nearest train station (km)	0.001	-0.000	0.000
	(0.001)	(0.001)	(0.000)
Average household income in 2009 (yuan)	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Average years of education (age 18+)	-0.028	-0.002	0.000
	(0.030)	(0.022)	(0.000)
% self-report sick/injured,	0.908	0.662	0.000
last 4 weeks	(1.112)	(0.898)	(0.000)
Located in a model township	0.072	0.104	0.000
	(0.162)	(0.120)	(0.000)
Communities	71	77	91

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.3: Impact of NCMS implementation on whether children are left-behind in rural areas

	(1)	(2)	(3)	(4)
Village has NCMS	0.11***	0.05***	0.05***	0.05***
	(0.01)	(0.01)	(0.01)	(0.01)
Number of rural children	7,028	7,028	7,028	5,117
Control for child's age	Y	Y	Y	Y
Community fixed effects	Y	Y	Y	Y
Child-level fixed effects	N	Y	Y	Y
Control for community-specific time trend	N	N	Y	Y
Control for parents' education and experience	N	N	N	Y

Clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

years and estimate four specifications of a linear probability model. From least conservative to most conservative, the specifications are: (a) a model with community-level fixed effects and child-level random effects; (b) a model with community-level fixed effects and child-level fixed effects; (c) model b but with community-specific time trends; and (d) model c but with controls for parents' years of potential work experience and years of education, two factors widely used as predictors of potential wages. In all cases, I cluster standard errors at the community level and control for the age of the child.

Table 3.3 shows the coefficient of interest from the regressions, which indicates that the number of left-behind children increased by about 5 percentage points as a result of NCMS implementation. This result implies that a significant fraction of rural hukou parents are near the margin of leaving a child behind, so that migration effects after policy changes may be significant.

3.7 Concluding remarks

This paper addresses a puzzle in the empirical literature about the effects of being left-behind by migrant parents on children's well-being. By exploiting the fact that children can only become left-behind after their parents cross a margin of indifference, I am able to sign the effect of being left-behind for children who become left-behind as a result of a small change in the policy environment. In particular, I use the margin of indifference as an additional equation to sign an otherwise difficult-to-sign comparative static. Therefore, in addition to addressing an empirical puzzle, this paper expands the field of policy analysis by demonstrating that focusing on marginal treatment effects can make it easier to sign otherwise difficult-to-sign comparative statics.

The effects I find have significant implications for policy analysis in China and other contexts where government dollars for social services do not systematically follow children who move. I find that although any increase in government spending on children directly helps children, these beneficial direct effects could be undermined by harmful indirect effects if parents respond by leaving children behind when they migrate. Because my empirical results indicate that a significant number of parents responded in this way after a recent policy change, it follows that the indirect effects I derive could be empirically important. These results add an important caveat to welfare magnet analyses of public policy: generous place-specific government services could have harmful effects for vulnerable individuals who cannot decide on their own migration.

3.A Proof that $C^*_{10} = C^*_{11}$ leads to a contradiction for parents at the margin of $U^*_{10} = U^*_{11}$

First, I can prove that by contradiction that in this scenario, it must be that $\tau^*_{11} > 0$ and therefore $I^*_{10} > I^*_{11}$. If $C^*_{10} = C^*_{11}$ and $\tau^*_{11} = 0$, this would yield $I^*_{10} = I^*_{11}$ by the budget constraint, but $I^*_{10} = I^*_{11}$ and $\tau^*_{11} = 0$ would imply $\psi^*_{10} > \psi^*_{11}$ which contradicts the premise of this case. In particular, $I^*_{10} = I^*_{11}$ and $\tau^*_{11} = 0$ imply:

$$\begin{aligned} \psi^*_{11} &= \psi\left(m\left(I^*_{11}, G_u\right), \tau^*_{11}\right) \\ &= \psi\left(m\left(I^*_{10}, G_u\right), 0\right) \end{aligned} \tag{3.5}$$

Which would then imply $\psi^*_{10} > \psi^*_{11}$ because:

$$\psi\left(m\left(I^*_{10}, G_r\right), 0\right) > \psi\left(m\left(I^*_{10}, G_u\right), 0\right) \tag{3.6}$$

by Assumption 3.3.5 ($G_r > G_u$) and Assumption 3.3.3 ($\psi_m > 0$).

In turn, $\psi^*_{10} > \psi^*_{11}$ and $C^*_{10} = C^*_{11}$ implies:

$$u\left(C^*_{10}, \psi^*_{10}\right) > u\left(C^*_{11}, \psi^*_{11}\right)$$

which would contradict $U^*_{10} = U^*_{11}$, the premise of this case. Therefore, marginal families must have $\tau^*_{11} > 0$ meaning that if the child is left-behind, parental income will be higher by $\tau^*_{11} w_u$. By the budget constraint and premise of this case ($C^*_{10} = C^*_{11}$), this implies $I^*_{10} > I^*_{11}$.

I now show that $I^*_{10} > I^*_{11}$, $C^*_{10} = C^*_{11}$, $\tau^*_{11} > 0$, and $\psi^*_{10} = \psi^*_{11}$ would violate the first-order conditions. Together with $\psi_{II} < 0$ and $\psi_{IG} < 0$ (Assumption 3.3.3), $\psi_{I\tau} \geq 0$ (Assumption 3.3.4), $G_r > G_u$ (Assumption 3.3.5), these properties would imply:

$$\psi_I\left(m\left(I_{10}^*, G_r\right), 0\right) < \psi_I\left(m\left(I_{11}^*, G_u\right), \tau_{11}^*\right) \tag{3.7}$$

However, according to the first order conditions in sections 3.3.2.2 and 3.3.2.3, the optimal decisions if children are left-behind or migrate with their parents must satisfy, respectively:

$$u_\psi\left(C_{10}^*, \psi_{10}^*\right)\psi_I\left(m\left(I_{10}^*, G_r\right), 0\right) = u_C\left(C_{10}^*, \psi_{10}^*\right) \tag{3.8}$$

$$u_\psi\left(C_{11}^*, \psi_{11}^*\right)\psi_I\left(m\left(I_{11}^*, G_u\right), \tau_{11}^*\right) = u_C\left(C_{11}^*, \psi_{11}^*\right) \tag{3.9}$$

These first order conditions cannot both hold if $C_{10}^* = C_{11}^*$, $\psi_{10}^* = \psi_{11}^*$, and inequality (3.7) hold. Starting with Equation (3.8) and substituting using these properties yields the inequality:

$$u_\psi\left(C_{11}^*, \psi_{11}^*\right)\psi_I\left(m\left(I_{11}^*, G_u\right), \tau_{11}^*\right) > u_C\left(C_{11}^*, \psi_{11}^*\right)$$

which violates Equation (3.9).

I conclude that this scenario is not possible, because it would involve taking optimal decisions that violate the first order conditions and would therefore not be optimal. As a result, I can rule out $\psi_{10}^* = \psi_{11}^*$ for families at the $U_{10}^* = U_{11}^*$ margin.

3.B Proof that $C_{10}^* = C_{00}^*$ leads to a contradiction for parents at the margin of $U_{10}^* = U_{00}^*$

First, I show that $I_{10}^* > I_{00}^*$. Because $w_u > w_r$ by Assumption 3.3.6, $C_{10}^* = C_{00}^*$ implies $I_{10}^* > I_{00}^*$ by the budget constraint for any $\tau_{00}^* \geq 0$. (The budget constraints are listed below to make this point.)

$$w_r\left(1-\tau_{00}^*\right)=C_{00}^*+I_{00}^*$$
$$w_u=C_{10}^*+I_{10}^*$$

The remainder of the proof proceeds along similar lines to that in section 3.A. I now show that $I_{10}^*>I_{00}^*$, $C_{10}^*=C_{00}^*$, and $\psi_{10}^*=\psi_{00}^*$ would violate the first-order conditions. Together with $\psi_{II}<0$ and $\psi_{IG}<0$ (Assumption 3.3.3) and $\psi_{I\tau}\geq 0$ (Assumption 3.3.4), these properties would imply:

$$\psi_I\left(m\left(I_{10}^*,G_r\right),0\right)<\psi_I\left(m\left(I_{00}^*,G_r\right),\tau_{00}^*\right) \tag{3.10}$$

However, according to the first order conditions in sections 3.3.2.2 and 3.3.2.3, the optimal decisions if children are left-behind or live in rural areas with their parents must satisfy, respectively:

$$u_\psi\left(C_{10}^*,\psi_{10}^*\right)\psi_I\left(m\left(I_{10}^*,G_r\right),0\right)=u_C\left(C_{10}^*,\psi_{10}^*\right) \tag{3.11}$$

$$u_\psi\left(C_{00}^*,\psi_{00}^*\right)\psi_I\left(m\left(I_{00}^*,G_r\right),\tau_{00}^*\right)=u_C\left(C_{00}^*,\psi_{00}^*\right) \tag{3.12}$$

These first order conditions cannot both hold if $C_{10}^*=C_{00}^*$, $\psi_{10}^*=\psi_{00}^*$, and inequality (3.10) hold. Starting with Equation (3.11) and substituting using these properties yields the inequality:

$$u_\psi\left(C_{00}^*,\psi_{00}^*\right)\psi_I\left(m\left(I_{00}^*,G_r\right),\tau_{00}^*\right)>u_C\left(C_{00}^*,\psi_{00}^*\right)$$

which violates Equation (3.12).

I conclude that this scenario is not possible, because it would involve taking optimal decisions that violate the first order conditions and would therefore not be optimal. As a result, I can rule out $\psi^*_{10} = \psi^*_{00}$ for families at the $U^*_{10} = U^*_{00}$ margin.

3.C Proof that $C^*_{10} < C^*_{00}$ leads to a contradiction for parents at the margin of $U^*_{10} = U^*_{11}$

In order to satisfy both $U^*_{10} = U^*_{11}$ and $C^*_{10} < C^*_{11}$, it must be the case that $\psi^*_{10} > \psi^*_{11}$. This case can be also ruled out by a violation of the same first order conditions as in Appendix 3.A.

First, $C^*_{10} < C^*_{11}$ implies $I^*_{10} > I^*_{11}$ by the budget constraint and $\tau^*_{11} \geq 0$. (The budget constraints are listed below to make the point clear.)

$$C^*_{10} + I^*_{10} = w_u$$
$$C^*_{11} + I^*_{11} = w_u (1 - \tau^*_{11})$$

Then $I^*_{10} > I^*_{11}$ plus Assumptions 3.3.3, 3.3.4, and 3.3.5 imply Equation 3.7 as in Appendix 3.A. In addition, $u_{CC} < 0$ and $u_{\psi\psi} < 0$, and additive separability of the utility function (Assumption 3.3.2), $C^*_{10} < C^*_{11}$ and $\psi^*_{10} > \psi^*_{11}$ imply $u_C\left(C^*_{10}, \psi^*_{10}\right) > u_C\left(C^*_{11}, \psi^*_{11}\right)$ and $u_\psi\left(C^*_{10}, \psi^*_{10}\right) < u_\psi\left(C^*_{11}, \psi^*_{11}\right)$.

As before, I examine the first-order conditions by starting with a first-order condition for left-behind children, Equation 3.8:

$$u_\psi\left(C^*_{10}, \psi^*_{10}\right) \psi_I\left(m\left(I^*_{10}, G_r\right), 0\right) = u_C\left(C^*_{10}, \psi^*_{10}\right)$$

starting with this equation and applying Equation 3.7, $u_C\left(C_{10}^*, \psi_{10}^*\right) > u_C\left(C_{11}^*, \psi_{11}^*\right)$ and $u_\psi\left(C_{10}^*, \psi_{10}^*\right) < u_\psi\left(C_{11}^*, \psi_{11}^*\right)$ yields the inequality:

$$u_\psi\left(C_{11}^*, \psi_{11}^*\right) \psi_I\left(m\left(I_{11}^*, G_u\right), \tau_{11}^*\right) > u_C\left(C_{11}^*, \psi_{11}^*\right)$$

which would violate the first-order conditions (Equation 3.9).

I conclude that this scenario is also not possible, because the optimal decisions would violate the first order conditions and would therefore not be optimal.

3.D Proof that $C_{10}^* < C_{00}^*$ leads to a contradiction for parents at the margin of $U_{10}^* = U_{00}^*$

The proof proceeds along similar lines to that in section 3.B. In order to satisfy both $U_{10}^* = U_{00}^*$ and $C_{10}^* < C_{00}^*$, it must be the case that $\psi_{10}^* > \psi_{00}^*$.

First, $C_{10}^* < C_{00}^*$ implies $I_{10}^* > I_{00}^*$ by the budget constraint, $w_u > w_r$ (Assumption 3.3.6) and $\tau_{00}^* \geq 0$. (The budget constraints are listed below to make the point clear.)

$$C_{10}^* + I_{10}^* = w_u$$
$$C_{00}^* + I_{00}^* = w_r\left(1 - \tau_{00}^*\right)$$

Then $I_{10}^* > I_{00}^*$ plus $\psi_{II} < 0$ and $\psi_{I\tau} \geq 0$ (Assumptions 3.3.3 and 3.3.4) imply:

$$\psi_I\left(m\left(I_{10}^*, G_r\right), 0\right) < \psi_I\left(m\left(I_{00}^*, G_r\right), \tau_{00}^*\right) \tag{3.13}$$

In addition, $u_{CC} < 0$ and $u_{\psi\psi} < 0$, and additive separability of the utility function (Assumption 3.3.2), $C^*_{10} < C^*_{00}$ and $\psi^*_{10} > \psi^*_{00}$ imply $u_C\left(C^*_{10}, \psi^*_{10}\right) > u_C\left(C^*_{00}, \psi^*_{00}\right)$ and $u_\psi\left(C^*_{10}, \psi^*_{10}\right) < u_\psi\left(C^*_{00}, \psi^*_{00}\right)$.

As before, I examine the first-order conditions by starting with a first-order condition for left-behind children, Equation 3.8:

$$u_\psi\left(C^*_{10}, \psi^*_{10}\right) \psi_I\left(m\left(I^*_{10}, G_r\right), 0\right) = u_C\left(C^*_{10}, \psi^*_{10}\right)$$

starting with this equation and applying Equation 3.13, $u_C\left(C^*_{10}, \psi^*_{10}\right) > u_C\left(C^*_{00}, \psi^*_{00}\right)$ and $u_\psi\left(C^*_{10}, \psi^*_{10}\right) < u_\psi\left(C^*_{00}, \psi^*_{00}\right)$ yields the inequality:

$$u_\psi\left(C^*_{00}, \psi^*_{00}\right) \psi_I\left(m\left(I^*_{00}, G_r\right), \tau^*_{00}\right) > u_C\left(C^*_{00}, \psi^*_{00}\right)$$

which would violate the first-order conditions (Equation 3.9).

I conclude that this scenario is also not possible, because the optimal decisions would violate the first order conditions and would therefore not be optimal.

REFERENCES

[1] Catherine Cowie, Keith Rust, Earl Ford, Mark Eberhardt, Danita Byrd-Holt, Chaoyang Li, Desmon Williams, Edward Gregg, Kathleen Bainbridge, Sharon Saydah, and Linda Geiss. Full accounting of diabetes and pre-diabetes in the US population in 1988-1994 and 2005-2006. *Diabetes Care*, 32(2):0–7, 2009.

[2] Global Burden of Disease Collaborators. The state of US health, 1990-2010: burden of diseases, injuries, and risk factors. *JAMA*, 310(6):591–608, August 2013.

[3] Margaret McDonald, Robin Hertz, Alan Unger, and Michael Lustik. Prevalence, awareness, and management of hypertension, dyslipidemia, and diabetes among United States adults aged 65 and older. *Journals of Gerontology - Series A Biological Sciences and Medical Sciences*, 64(2):256–263, 2009.

[4] Casey Olives, Rebecca Myerson, Ali H Mokdad, Christopher J L Murray, and Stephen S Lim. Prevalence, awareness, treatment, and control of hypertension in United States counties, 2001-2009. *PloS one*, 8(4):e60308, January 2013.

[5] Shivani Patel, Munir Winkel, Mohammed Ali, K M Venkat Narayan, and Neil Mehta. Cardiovascular Mortality Associated With 5 Leading Risk Factors: National and State Preventable Fractions Estimated From Survey Data. *Annals of Internal Medicine*, 163(4):245–253, June 2015.

[6] Richard Zweifler, Leslie McClure, Virginia Howard, Mary Cushman, Martha Hovater, Monika Safford, George Howard, and David Goff. Racial and geographic differences in prevalence, awareness, treatment and control of dyslipidemia: the reasons for geographic and racial differences in stroke (REGARDS) study. *Neuroepidemiology*, 37(1):39–44, January 2011.

[7] Amy Burke and Adelle Simmons. Increased Coverage of Preventive Services with Zero Cost Sharing under the Affordable Care Act. Technical report, Department of Health and Human Services, 2014.

[8] American Diabetes Association. Standards of Medical Care in Diabetes–2014. *Diabetes Care*, 37 Suppl 1(October 2013):S14–80, January 2014.

[9] Cholesterol Treatment Trialists' Collaborators. Efficacy and safety of cholesterol-lowering treatment: prospective meta-analysis of data from 90,056 participants in 14 randomised trials of statins. *Lancet*, 366(9493):1267–78, October 2005.

[10] Blood Pressure Lowering Treatment Trialists' Collaboration. Effects of ACE inhibitors, calcium antagonists, and other blood-pressure-lowering drugs: results of

prospectively designed overviews of randomised trials. *Lancet*, 356:1955–1964, 2000.

[11] Andrew Bindman, Kevin Grumbach, Dennis Osmond, Miriam Komaromy, Karen Vranizan, Nicole Lurie, John Billings, and Anita Stewart. Preventable hospitalizations and access to health care. *JAMA*, 274(4):305–11, 1995.

[12] Neil M Bressler, Rohit Varma, Quan V Doan, Michelle Gleeson, Mark Danese, Julie K Bower, Elizabeth Selvin, Chantal Dolan, Jennifer Fine, Shoshana Colman, and Adam Turpcu. Underuse of the health care system by persons with diabetes mellitus and diabetic macular edema in the United States. *JAMA ophthalmology*, 132(2):168–73, February 2014.

[13] Ralph B D'Agostino, Ramachandran S Vasan, Michael J Pencina, Philip a Wolf, Mark Cobain, Joseph M Massaro, and William B Kannel. General cardiovascular risk profile for use in primary care: the Framingham Heart Study. *Circulation*, 117(6):743–53, February 2008.

[14] Thomas Farley, Mehul Dalal, Farzad Mostashari, and Thomas Frieden. Deaths Preventable in the U.S. by Improvements in Use of Clinical Preventive Services. *American Journal of Preventive Medicine*, 38(6):600–609, 2010.

[15] Paul James, Suzanne Oparil, Barry Carter, William Cushman, Cheryl Dennison-Himmelfarb, Joel Handler, Daniel Lackland, Michael LeFevre, Thomas MacKenzie, Olugbenga Ogedegbe, Sidney Smith, Laura Svetkey, Sandra Taler, Raymond Townsend, Jackson Wright, Andrew Narva, and Eduardo Ortiz. 2014 evidence-based guideline for the management of high blood pressure in adults: report from the panel members appointed to the Eighth Joint National Committee (JNC 8). *JAMA*, 311(5):507–20, February 2014.

[16] Neil Stone, Jennifer Robinson, Alice Lichtenstein, C Noel Bairey Mertz, Conrad Blum, Robert Eckel, Anne Goldberg, David Gordon, Daniel Levy, Donald Lloyd-Jones, Patrick McBride, J Sanford Schwartz, Susan Shero, Sidney Smith, Karol Watson, and Peter Wilson. 2013 ACC/AHA Guideline on the Treatment of Blood Cholesterol to Reduce Atherosclerotic Cardiovascular Risk in Adults: A Report of the American College of Cardiology/American Heart Association Task Force on Practice Guidelines. *Journal of the American College of Cardiology*, 63(25 Pt B):2889–934, July 2014.

[17] Pamela Sytkowski, William Kannel, and Ralph D'Agostino. Changes in risk factors and the decline in mortality from Cardiovascular Disease: The Framingham Heart Study. *New England Journal of Medicine*, 323(24):1704–6, December 1990.

[18] Ruth Bernstein Hyman, Stephen Baker, Robert Ephraim, Alyson Moadel, and Joanne Philip. Health Belief Model variables as predictors of screening mammography utilization. *Journal of Behavioral Medicine*, 17(4):391–406, August 1994.

[19] Lourdes Lostao, Thomas E. Joiner, Jeremy W. Pettit, Paloma Chorot, and Bonifacio Sandín. Health beliefs and illness attitudes as predictors of breast cancer screening attendance. *European Journal of Public Health*, 11(3):274–279, 2001.

[20] Emily Oster, Ira Shoulson, and E. Ray Dorsey. Optimal Expectations and Limited Medical Testing: Evidence from Huntington Disease. *American Economic Review*, 103(2):804–830, April 2013.

[21] Nicholas Wilson. Antiretroviral Therapy and Demand for HIV Testing: Evidence from Zambia. *SSRN Electronic Journal*, August 2011.

[22] Pedro Carneiro, James Heckman, and Edward Vytlacil. Estimating marginal returns to education. 101(October):2754–2781, 2010.

[23] Philipp Eisenhauer, James Heckman, and Edward Vytlacil. Generalized Roy Model and the Cost-Benefit Analysis of Social Programs. *Policy Analysis*, 2010.

[24] James Heckman. Building Bridges Between Structural and Program Evaluation Approaches to Evaluating Policy. *Journal of Economic Literature*, 48(2):356–398, June 2010.

[25] Christopher J Carpenter. A meta-analysis of the effectiveness of health belief model variables in predicting behavior. *Health Communication*, 25(8):661–9, December 2010.

[26] Donald Kenkel. The demand for preventive medical care. *Applied Economics*, 26(4):313–325, 1994.

[27] Willard Manning, J P Newhouse, N Duan, E B Keeler, a Leibowitz, and M S Marquis. Health insurance and the demand for Medical Care: evidence from a randomized experiment. *The American economic review*, 77(3):251–77, June 1987.

[28] Donald Musa, Richard Schulz, Roderick Harris, Myrna Silverman, and Stephen B Thomas. Trust in the health care system and the use of preventive health services by older black and white adults. *American Journal of Public Health*, 99(7):1293–9, July 2009.

[29] Mark Pauly. Overinsurance and public provision of insurance: the roles of moral hazard and adverse selection. *The Quarterly Journal of Economics*, 88(1):44–62, 1974.

[30] Michael Rothschild and Joseph Stiglitz. Equilibrium in competitive insurance markets: An essay on the economics of imperfect information. *The Quarterly Journal of Economics*, 90(4):629–649, 1976.

[31] Charles Wilson. A model of insurance markets with incomplete information. *Journal of Economic Theory*, 1977.

[32] David Culter and Sarah Reber. Paying for Health Insurance : The Trade-Off between Competition and Adverse Selection. *The Quarterly Journal of Economics*, 113(2):433–466, 2011.

[33] David Cutler and Richard Zeckhauser. Adverse selection in health insurance. 1998.

[34] Ben Handel. Adverse selection and switching costs in health insurance markets: When nudging hurts. 2011.

[35] Centers for Disease Control and Prevention. National Health and Nutrition Examination Survey, 1999-2014.

[36] Amy Finkelstein. Oregon Health Insurance Experiment Public Use Data, 2013.

[37] Virginia Howard, Mary Cushman, LeaVonne Pulley, Camilio Gomez, Rodney Go, Ronald Prineas, Andra Graham, Claudia Moy, and George Howard. The reasons for geographic and racial differences in stroke study: objectives and design. *Neuroepidemiology*, 25:135–143, 2005.

[38] Ben Handel, Igal Hendel, and Michael D. Whinston. Equilibria in Health Exchanges: Adverse Selection versus Reclassification Risk. *Econometrica*, 83(4):1261–1313, 2015.

[39] Gabriel Picone, Martín Uribe, and R. Mark Wilson. The effect of uncertainty on the demand for Medical Care, health capital and wealth. *Journal of Health Economics*, 17(2):171–185, 1998.

[40] Michael Grossman. On the Concept of Health Capital and the Demand for Health. *Journal of Political Economy*, 80(2):223, 1972.

[41] Mark Egan and Tomas Philipson. Health Care Adherence and Personalized Medicine. NBER Working Papers 20330, National Bureau of Economic Research, Inc, July 2014.

[42] Karen Glanz and Donald Bishop. The role of behavioral science theory in development and implementation of public health interventions. *Annual Review of Public Health*, 31:399–418, January 2010.

[43] Irwin Rosenstock, Victor Strecher, and Marshall Becker. Social Learning Theory and the Health Belief Model. *Health Education & Behavior*, 15(2):175–183, January 1988.

[44] David Vanness and John Mullahy. Moving Beyond Mean-based Evaluation of Health Care. In *The Elgar Companion to Health Economics, Second Edition*, Chapters, chapter 52. Edward Elgar, March 2012.

[45] Anirban Basu and James Heckman. Use of instrumental variables in the presence of heterogeneity and self selection: an application to treatments of breast cancer patients. *Health Economics*, 1157(October):1133–1157, 2007.

[46] Anirban Basu and David Meltzer. Value of information on preference heterogeneity and individualized care. *Medical Decision Making*, 27(2):112–27, 2007.

[47] Dana Goldman and Tomas Philipson. Insurance Design in the Presence of Integrated Medical Multiple Technologies. *American Economic Review*, 97(2):427–432, 2007.

[48] Mark Pauly and Fredric Blavin. Moral hazard in insurance, value-based cost sharing, and the benefits of blissful ignorance. *Journal of Health Economics*, 27(6):1407–17, December 2008.

[49] Anirban Basu and Willard Manning. Issues for the next generation of health care cost analyses. *Medical Care*, 47(7), 2009.

[50] Anirban Basu. Economics of individualization in comparative effectiveness research and a basis for a patient-centered health care. *Journal of Health Economics*, 2011.

[51] Anirban Basu. Estimating person-centered treatment effects using instrumental variables: an application to evaluating prostate cancer. *Journal of Applied Econometrics*, 2013.

[52] Elbert S Huang, Morgan Shook, Lei Jin, Marshall H Chin, and David O Meltzer. The impact of patient preferences on the cost-effectiveness of intensive glucose control in older patients with new-onset diabetes. *Diabetes Care*, 29(2):259–64, February 2006.

[53] David Meltzer and E Huang. Effects of patient self-selection on cost-effectiveness of diabetes therapy by age. *iHEA 2007 6th World . . .* , pages 2–3, 2007.

[54] Mark Sculpher. Subgroups and heterogeneity in cost-effectiveness analysis. *PharmacoEconomics*, 26(9):799–806, January 2008.

[55] Jaap Abbring, Pierre-Andre Chiappori, and Jean Pinquet. Moral Hazard and Dynamic Insurance Data. *Journal of the European Economic Association*, 1(4):767–820, 2003.

[56] Jaap Abbring, James Heckman, Pierre-André Chiappori, and Jean Pinquet. Adverse Selection and Moral Hazard in Insurance: Can Dynamic Data Help to Distinguish? *Journal of the European Economic Association*, 1(2/3):512–521, 2003.

[57] Alma Cohen. Asymmetric information and learning: Evidence from the automobile insurance market. *The Review of Economics and Statistics*, 87(May):197–207, 2005.

[58] Martin Hackmann, Jonathan Kolstad, and Amanda Kowalski. Health Reform, Health Insurance, and Selection: Estimating Selection into Health Insurance Using the Massachusetts Health Reform. *American Economic Review*, 102(3):498–501, May 2012.

[59] Jörg Spenkuch. Moral hazard and selection among the poor: evidence from a randomized experiment. *Journal of Health Economics*, 31(1):72–85, January 2012.

[60] Liran Einav and Amy Finkelstein. Selection in insurance markets: Theory and empirics in pictures. 2011.

[61] Donald Kenkel. Prevention. *Handbook of Health Economics*, 1(PART B):1675–1720, 2000.

[62] Adam Wagstaff. The demand for health: some new empirical evidence. *Journal of Health Economics*, 5(3):195–233, 1986.

[63] Peter Zweifel. The Grossman model after 40 years. *European Journal of Health Economics*, 13(6):677–682, 2012.

[64] Bengt Liljas. The demand for health with uncertainty and insurance. *Journal of Health Economics*, 17(2):153–170, 1998.

[65] Michael Grossman. The demand for health after a decade. *Journal of Health Economics*, 1(1):1–3, 1982.

[66] Michael Grossman. The human capital model. *Handbook of Health Economics*, 1(Part A):347–408, 2000.

[67] Fwu Ranq Chang. Uncertainty and investment in health. *Journal of Health Economics*, 15(3):369–376, 1996.

[68] Valentino Dardanoni and Adam Wagstaff. Uncertainty, inequalities in health and the demand for health. *Journal of Health Economics*, 6:283–290, 1987.

[69] Thomas Selden. Uncertainty and health care spending by the poor: The health capital model revisited. *Journal of Health Economics*, 12(1):109–115, 1993.

[70] Paul Muntner, Lisandro Colantonio, Mary Cushman, David Goff, George Howard, Virginia Howard, Brett Kissela, Emily Levitan, Donald Lloyd-Jones, and Monika Safford. Validation of the atherosclerotic cardiovascular disease Pooled Cohort risk equations. *JAMA*, 311(14):1406–15, April 2014.

[71] Heidi Allen, Katherine Baicker, Amy Finkelstein, Sarah Taubman, and Bill J Wright. What the Oregon health study can tell us about expanding Medicaid. *Health Affairs (Project Hope)*, 29(8):1498–506, August 2010.

[72] Katherine Baicker, Sarah L Taubman, Heidi L Allen, Mira Bernstein, Jonathan H Gruber, Joseph P Newhouse, Eric C Schneider, Bill J Wright, Alan Zaslavsky, Amy N Finkelstein, Matt Carlson, Tina Edlund, Charles Gallia, and Jeanene Smith. The Oregon experiment–effects of Medicaid on clinical outcomes. *The New England journal of medicine*, 368(18):1713–22, May 2013.

[73] Bruce Meyer, Wallace Mok, and James Sullivan. Household Surveys in Crisis. *Journal of Economic Perspectives*, 29(4):1–29, 2015.

[74] Gary Claxton, Matthew Rae, Michelle Long, Nirmita Panchal, Anthony Damico, Kevin Kenward, and Heidi Whitmore. Employer Health Benefits: 2015 Annual Survey. Technical report, Kaiser Family Foundation and Health Research and Educational Trust, 2015.

[75] Joseph Newhouse, Mary Price, and J. Michael McWilliams. How Much Favorable Selection Is Left in Medicare Advantage? 2014.

[76] Joseph Newhouse. *Pricing the priceless: A health care conundrum.* MIT Press, Cambridge, MA, 2002.

[77] J. Michael McWilliams, John Hsu, and Joseph Newhouse. New risk-adjustment system was associated with reduced favorable selection in medicare advantage. *Health Affairs*, 31(12):2630–2640, 2012.

[78] Robert Morgan, Beth Virnig, Carolee Devito, and Nancy Persily. The Medicare-HMO revolving door - the healthy go in and the sick go out. *The New England Journal of Medicine*, 337(3):169–175, 1997.

[79] Zhun Cao and Thomas G. McGuire. Service-level selection by HMOs in Medicare. *Journal of Health Economics*, 22(6):915–931, 2003.

[80] Richard Frank, Jacob Glazer, and Thomas McGuire. Measuring adverse selection in managed health care. *Journal of Health Economics*, 19(6):829–854, November 2000.

[81] Gregory Pope, John Kautter, Randall Ellis, Arlene Ash, John Ayanian, Lisa Iezzoni, Melvin Ingber, Jesse Levy, and John Robst. Risk adjustment of Medicare capitation payments using the CMS-HCC model. 25(4):119–141, 2004.

[82] Joseph Newhouse and Thomas McGuire. How Successful is Medicare Advantage? *Milbank Quarterly*, 92(2):351–394, 2014.

[83] Zirui Song, Mary Beth Landrum, and Michael Chernew. Competitive bidding in Medicare Advantage: Effect of benchmark changes on plan bids. *Journal of Health Economics*, 32(6):1301–1312, 2013.

[84] Thomas McGuire, Joseph Newhouse, and Anna Sinaiko. An economic history of Medicare Part C. *Milbank Quarterly*, 89(2):289–332, 2011.

[85] Curtis Eberwein, John C Ham, and Robert J LaLonde. Alternative methods of estimating program effects in event history models. *Labour Economics*, 9(2):249–278, apr 2002.

[86] William Samuelson and Richard Zeckhauser. Status quo bias in decision making. *Journal of Risk and Uncertainty*, 1(1):7–59, 1988.

[87] Anna Sinaiko, Christopher Afendulis, and Richard Frank. Enrollment in medicare advantage plans in Miami-Dade County: Evidence of status quo bias? *Inquiry (United States)*, 50(3):202–215, 2014.

[88] J. Michael McWilliams, Christopher Afendulis, Thomas McGuire, and Bruce Landon. Complex Medicare Advantage Choices May Overwhelm Seniors - Especially Those With Impaired Decision Making. *Health Affairs*, 30:1786–1794, 2011.

[89] William Friedewald, Robert Levy, and Donald Fredrickson. Estimation of the concentration of low-density lipoprotein cholesterol in plasma, without use of the preparative ultracentrifuge. *Clinical Chemistry*, 18(6):499–502, 1972.

[90] Melinda Beeuwkes Buntin and Alan M Zaslavsky. Too much ado about two-part models and transformation? Comparing methods of modeling Medicare expenditures. *Journal of Health Economics*, 23(3):525–42, May 2004.

[91] Naihua Duan, Willard Manning, Carl Morris, and Joseph Newhouse. Comparison of Alternative Models for the Demand of Medical Care. *Journal of Business & Economic Statistics*, 1(2):115–126, 1983.

[92] Willard. Manning. The logged dependent variable, heteroscedasticity, and the retransformation problem. *Journal of Health Economics*, 17(3):283–295, June 1998.

[93] Willard Manning, Anirban Basu, and John Mullahy. Generalized modeling approaches to risk adjustment of skewed outcomes data. *Journal of Health Economics*, 24(3):465–88, May 2005.

[94] Willard Manning. Modeling Cost and Expenditure for Healthcare. *Encyclopedia of Health Economics*, 2:299–305, 2014.

[95] Willard Manning and John Mullahy. Estimating log models: to transform or not to transform? 20(June 1999):461–494, 2001.

[96] Rolla Edward Park. Estimation With Heteroscedastic Error Terms. *Econometrica*, 34(4):888, 1966.

[97] Goodarz Danaei, Eric L Ding, Dariush Mozaffarian, Ben Taylor, Jürgen Rehm, Christopher J L Murray, and Majid Ezzati. The preventable causes of death in the United States: comparative risk assessment of dietary, lifestyle, and metabolic risk factors. *PLoS Medicine*, 6(4):e1000058, April 2009.

[98] US Burden of Disease Collaborators. The state of US health, 1990-2010: burden of diseases, injuries, and risk factors. *JAMA*, 310(6):591–608, August 2013.

[99] BM Egan, Y Zhao, and RN Axon. US trends in prevalence, awareness, treatment, and control of hypertension, 1988-2008. *JAMA*, 303(20):2043–2050, 2010.

[100] Frank Sloan, Kofi Acquah, Paul Lee, and Devdutta Sangvai. Despite 'Welcome To Medicare' Benefit, One In Eight Enrollees Delay First Use Of Part B Services For At Least Two Years. *Health Affairs*, 31(6):1260–1268, 2012.

[101] Healthcare.gov. Preventive Services Covered Under the Affordable Care Act, 2014.

[102] Dawn E. Alley, Chisara N. Asomugha, Patrick H. Conway, and Darshak M. Sanghavi. Accountable health communities - addressing social needs through medicare and medicaid. *New England Journal of Medicine*, 374(1):8–11, 2016. PMID: 26731305.

[103] Center for Medicare and Medicaid Services. Guide to Quality Performance Scoring Methods for Accountable Care Organizations, 2011.

[104] Rd Edwards. If My Blood Pressure Is High, Do I Take It To Heart? Behavioral Impacts of Biomarker Collection in the Health and Retirement Study. 2013.

[105] Eric a Finkelstein, Olga Khavjou, and Julie C Will. Cost-effectiveness of WISE-WOMAN, a program aimed at reducing heart disease risk among low-income women. *Journal of Women's Health (2002)*, 15(4):379–389, 2006.

[106] VJ Howard, M Cushman, L Pulley, Camilio Gomez, Rodney Go, Ronald Prineas, Andra Graham, Claudia Moy, and George Howard. The reasons for geographic and racial differences in stroke study: objectives and design. *Neuroepidemiology*, 25:135–143, 2005.

[107] Yelena Gorina and Ellen A. Kramarow. Identifying chronic conditions in medicare claims data: Evaluating the chronic condition data warehouse algorithm. *Health Services Research*, 46(5):1610–1627, 2011.

[108] Joseph W Sakshaug, David R Weir, and Lauren H Nicholas. Identifying diabetics in Medicare claims and survey data: implications for Health Services Research. *BMC Health Services Research*, 14(1):150, 2014.

[109] National Cholesterol Education Expert Panel. Detection, evaluation, and treatment of high blood cholesterol in adults (Adult Treatment Panel III). (02), 2001.

[110] James Stock and Mark Watson. Heteroskedasticity-robust standard errors for fixed effects panel data regression. *Econometrica*, 76(1):155–174, 2008.

[111] F Xie, L Colantonio, JR Curtis, MM Safford, EB Levitan, G Howard, and P Muntner. Linkage of a Population-Based Cohort with Primary Data Collection to Medicare Claims: The Reasons for Geographic and Racial Differences in Stroke (REGARDS) Study. *American Journal of Epidemiology*, In Press, 2016.

[112] N. K. Janz and M. H. Becker. The Health Belief Model: A Decade Later. *Health Education & Behavior*, 11(1):1–47, January 1984.

[113] Kenneth Roberts. China's "Tidal Wave" Of Migrant Labor: What Can We Learn From Mexican Undocumented Migration to the United States? *International Migration Review*, 31(2):249–293, 1997.

[114] National Bureau of Statistics of People's Republic of China. National Monitoring Survey of China's Migrant Workers. Technical report, Beijing, 2015.

[115] Tiejun Cheng and Mark Selden. The Origins and Social Consequences of China's Hukou System. *The China Quarterly*, 139:644–668, 1994.

[116] F Lai, C Liu, R Luo, L Zhang, and X Ma. Private migrant schools or rural/urban public schools: where should China educate its migrant children? Technical report, Center for Education Innovations, April 2009.

[117] Yehua Dennis Wei, editor. *Regional Development in China: States, Globalization, and Inequality*. Routledge, 2000.

[118] Xiaogang Wu and Donald J Treiman. The Household Registration System and Social Stratification. *Demography*, 41(2), 2004.

[119] Jingzhong Ye, James Murray, and Wang Yihuan. *Left-Behind Children in Rural China: An Impact Study of Rural Labour Migration on Left-Behind Children in Mid-Western China*. Social Sciences Academic Press, China, 2005.

[120] China Labor Bulletin. Paying the Price for Economic Development: The Children of Migrant Workers in China. Technical report, 2009.

[121] Wei Jianwen and Hou Jiawei. The Household Registration System, Education System, and Inequalities in Education for Migrant Children. *Chinese Education & Society*, 43(5):77–89, September 2010.

[122] Adam Wagstaff, Magnus Lindelow, Gao Jun, Xu Ling, and Qian Juncheng. Extending health insurance to the rural population: An impact evaluation of china's new cooperative medical scheme. *Journal of Health Economics*, 28(1):1 – 19, 2009.

[123] All China Women's Federation. Research report on the current status of rural left-behind and urban migrant children in China. Technical report, Beijing, 2013.

[124] All China Women's Federation. National survey of left-behind children in rural areas. Technical report, 2008.

[125] China Youth Research Center. China's Children and Juveniles Statistical Handbook. Technical report, 2006.

[126] Li Zhang, Richard LeGates, and Min Zhao. Evolution, status, and reform of hukou. In *Understanding China's urbanization: the great demographic, spatial, and social transformation*, pages 47–87. Edward Elgar Publishing, 2016.

[127] Chengchao Zhou, Sean Sylvia, Linxiu Zhang, Renfu Luo, Hongmei Yi, Chengfang Liu, Yaojiang Shi, Prashant Loyalka, James Chu, Alexis Medina, and Scott Rozelle. China's Left-Behind Children: Impact Of Parental Migration On Health, Nutrition, And Educational Outcomes. *Health Affairs (Project Hope)*, 34(11):1964–71, nov 2015.

[128] Fang Fan, Linyan Su, Mary Kay Gill, and Boris Birmaher. Emotional and behavioral problems of Chinese left-behind children: a preliminary study. *Social Psychiatry and Psychiatric Epidemiology*, 45(6):655–64, June 2010.

[129] Yang Gao, Li Ping Li, Jean Hee Kim, Nathan Congdon, Joseph Lau, and Sian Griffiths. The impact of parental migration on health status and health behaviours among left behind adolescent school children in China. *BMC Public Health*, 10:56, January 2010.

[130] Bingyan He, Jingyi Fan, Ni Liu, Huijuan Li, Yanjun Wang, Joshua Williams, and Kaishing Wong. Depression risk of 'left-behind children' in rural China. *Psychiatry Research*, 200(2-3):306–12, December 2012.

[131] Hongwei Hu, Shuang Lu, and Chien-Chung Huang. The psychological and behavioral outcomes of migrant and left-behind children in China. *Children and Youth Services Review*, 46:1–10, November 2014.

[132] Yun Huang, Xiao-Ni Zhong, Qing-Ying Li, Dan Xu, Xuan-Lin Zhang, Chao Feng, Guo-Xiu Yang, Yun-Yun Bo, and Bing Deng. Health-related quality of life of the rural-China left-behind children or adolescents and influential factors: a cross-sectional study. *Health and Quality of Life Outcomes*, 13:29, January 2015.

[133] Zhaobao Jia and Wenhua Tian. Loneliness of left-behind children: a cross-sectional survey in a sample of rural China. *Child Care, Health and Development*, 36(6):812–7, November 2010.

[134] Li-Juan Liu, Xun Sun, Chun-Li Zhang, Yue Wang, and Qiang Guo. A survey in rural China of parent-absence through migrant working: the impact on their children's self-concept and loneliness. *BMC Public Health*, 10:32, January 2010.

[135] Wei Lu. *Left-Behind Children in Rural China: Research Based on the Use of Qualitative Methods in Inner Mongolia*. Phd dissertation, University of York, 2011.

[136] Yao Lu. Education of Children Left Behind in Rural China. *Journal of Marriage and Family*, 74(2):328–341, April 2012.

[137] Yao Lu. The Gender-Differentiated Impact of Father Migration on Left-Behind Children Evidence from Gansu Province, China. 2012.

[138] J Luo, X Peng, R Zong, K Yao, and R Hu. The status of care and nutrition of 774 left-behind children in rural areas in China. *Public Health Reports*, 123, June 2008.

[139] Sophie Xuefei Wang. The Effect of Parental Migration on the Educational Attainment of Their Left-behind Children in Rural China. *The B.E. Journal of Economic Analysis and Policy*, 14(3):1037–1080, 2014.

[140] Ming Wen, Shaobing Su, Xiaoming Li, and Danhua Lin. Positive youth development in rural China: the role of parental migration. *Social Science and Medicine*,

132:261–9, may 2015.

[141] Hongwei Xu and Yu Xie. The Causal Effects of Rural-to-Urban Migration on Children's Wellbeing in China. *European Sociological Review*, January 2013.

[142] Jingzhong Ye and Lu Pan. Differentiated childhoods: impacts of rural labor migration on left-behind children in China. *Journal of Peasant Studies*, 38(2):355–377, March 2011.

[143] Xue Zhao, Jian Chen, Ming-Chun Chen, Xiao-Ling Lv, Yu-Hong Jiang, and Ye-Huan Sun. Left-behind children in rural China experience higher levels of anxiety and poorer living conditions. *Acta Paediatrica*, 103(6):665–70, June 2014.

[144] M Zhou, R Murphy, and R Tao. Effects of Parents' Migration on the Education of Children Left Behind in Rural China. *Population and Development Review*, 40:273–292, June 2014.

[145] F Antman. The impact of migration on family left behind. In *International Handbook on the Economics of Migration*, pages 1–34. 2012.

[146] Xiaojiang Hu, Sarah Cook, and Miguel a Salazar. Internal migration and health in China. *Lancet*, 372(9651):1717–9, November 2008.

[147] Yao Lu and Lijian Qin. Healthy migrant and salmon bias hypotheses: a study of health and internal migration in China. *Social Science and Medicine*, 102:41–8, March 2014.

[148] Jacob L Vigdor. Locations, outcomes, and selective migration. *Review of Economics and Statistics*, 84(November):751–755, 2002.

[149] Yaohui Zhao. The Role of Migrant Networks in Labor Migration: The Case of China. *Contemporary Economic Policy*, 21(4):500–511, October 2003.

[150] Somik V Lall, Harris Selod, and Zmarak Shalizi. Rural-Urban Migration in Developing Countries: A Survey of Theoretical Predictions and Empirical Findings. *Journal of Urban Economics*, 63(1):56–73, 2006.

[151] Gary Becker, Scott Duke Kominers, Kevin Murphy, and Jorg Spenkuch. A theory of intergenerational mobility. (66334):0–40, 2015.

[152] GS Becker and Nigel Tomes. An equilibrium theory of the distribution of income and intergenerational mobility. *The Journal of Political Economy*, 87(6):1153–1189, 1979.

[153] Gary S Becker and Nigel Tomes. Human Capital and the Rise and Fall of Families. *Journal of Labor Economics*, 4(3):S1–39, July 1986.

[154] Glenn C Loury. Intergenerational Transfers and the Distribution of Earnings. *Econometrica*, 49(4):843–867, 1981.

[155] Gary Solon. A model of intergenerational mobility variation over time and place. In *Generational income mobility in North America and Europe*. 2004.

[156] Xiaoyan Lei and Wanchuan Lin. The New Cooperative Medical Scheme in Rural China: Does more coverage mean more service and better health? *Health Economics*, (10):1–36, 2009.

[157] The Guardian. China reforms hukou system to improve migrant workers' rights. pages 1–4, July 2014.

[158] State Council of the People's Republic of China. Thoughts on Hukou Reform, 2014.

[159] Wall Street Journal. China's Hukou Reform Plan Starts to Take Shape. 2014.

[160] Kam Wing Chan and Will Buckingham. Is China Abolishing the Hukou System? *The China Quarterly*, 195, September 2008.

[161] Charles M Tiebout. A pure theory of local expenditures. *The Journal of Political Economy*, 64(5):416–424, 1956.

[162] Robert E. Lucas. Internal Migration in Developing Countries. *Handbook of Population and Family Economics*, 1:721–798, 1997.

[163] Scott W. Allard and Sheldon Danziger. Welfare Magnets: Myth or Reality? *The Journal of Politics*, 62(02):350–368, July 2008.

[164] WD Berry, RC Fording, and RL Hanson. Reassessing the "race to the bottom" in state welfare policy. *Journal of Politics*, 65(2):327–349, 2003.

[165] GJ Borjas. Immigration and welfare magnets. *Journal of Labor Economics*, 17(4), 1999.

[166] Phillip B. Levine and David J. Zimmerman. An empirical analysis of the welfare magnet debate using the NLSY. *Journal of Population Economics*, 12(3):391–409, August 1999.

[167] Aaron L Schwartz and Benjamin D Sommers. Moving for medicaid? Recent eligibility expansions did not induce migration from other states. *Health Affairs (Project*

Hope), 33(1):88–94, January 2014.

[168] Dennis Epple and Thomas Romer. Mobility and Redistribution. *Journal of Political Economy*, 99(4):828, 1991.

[169] Leonardo A. Lanzona. Migration, self-selection and earnings in Philippine rural communities. *Journal of Development Economics*, 56(1):27–50, 1998.

[170] Mark R Rosenzweig and Kenneth I Wolpin. Migration selectivity and the effects of public programs. *Journal of Public Economics*, 37(3):265–289, December 1988.

[171] Michael P Todaro. A Model of Labor Migration and Urban Unemployement in Less Developed Countries, 1969.

[172] PE Todd. Evaluating social programs with endogenous program placement and selection of the treated. *Handbook of Development Economics*, 4(07), 2007.

[173] James J. Heckman and Edward J. Vytlacil. Local instrumental variables and latent variable models for identifying and bounding treatment effects. *Proceedings of the National Academy of Sciences of the United States of America*, 96(8):4730–4734, 1999.

[174] James J. Heckman and Edward J. Vytlacil. Policy-relevant treatment effects. *The American Economic Review*, 91(2):107–111, 2001.

[175] James J. Heckman and Edward J. Vytlacil. Econometric Evaluation of Social Programs, Part II: Using the Marginal Treatment Effect to Organize Alternative Econometric Estimators to Evaluate Social Programs, and to Forecast their Effects in New. In J.J. Heckman and E.E. Leamer, editors, *Handbook of Econometrics*, chapter 71. Elsevier, September 2007.

[176] Arild Aakvik, James J. Heckman, and Edward J. Vytlacil. Estimating treatment effects for discrete outcomes when responses to treatment vary: an application to Norwegian vocational rehabilitation programs. *Journal of Econometrics*, 125(1-2):15–51, March 2005.

[177] Albert Park and Leng Lee. Parental Migration and Child Well-Being in Developing Countries With Some New Evidence from China. (July), 2010.

[178] Alan De Brauw and Scott Rozelle. Migration and household investment in rural China. *China Economic Review*, 19(2):320–335, June 2008.

[179] Yang Du, Albert Park, and Sangui Wang. Migration and rural poverty in China. *Journal of Comparative Economics*, September 2005.

[180] Qiming Liu and Barry Reilly. Income transfers of Chinese rural migrants: some empirical evidence from Jinan. *Applied Economics*, 36(12):1295–1313, aug 2006.

[181] JE Taylor, Scott Rozelle, and A De Brauw. Migration and incomes in source communities: A new economics of migration perspective from China. *Economic Development and Cultural Change*, 2003.

[182] Philip Brown. Parental Education and Investment in Children's Human Capital in Rural China. *Economic Development & Cultural Change*, 54(4):759–789, 2006.

[183] Feng Hu. Migration, remittances, and children's high school attendance: The case of rural China. *International Journal of Educational Development*, 32(3):401–411, May 2012.

[184] John Knight, Li Shi, and Deng Quheng. Education and the Poverty Trap in Rural China: Setting the Trap. *Oxford Development Studies*, 37(4):311–332, December 2009.

[185] Hongqin Chang, Xiao-yuan Dong, and Fiona MacPhail. Labor Migration and Time Use Patterns of the Left-behind Children and Elderly in Rural China. *World Development*, 39(12):2199–2210, December 2011.

[186] David McKenzie and Hillel Rapoport. Can migration reduce educational attainment? Evidence from Mexico. Policy Research Working Paper Series 3952, The World Bank, 2006.

[187] Xinxin Chen, Qiuqiong Huang, Scott Rozelle, Yaojiang Shi, and Linxiu Zhang. Effect of Migration on Children's Educational Performance in Rural China. *Comparative Economic Studies*, 51(3):323–343, May 2009.

[188] Leng Lee and Albert Park. Parental Migration and Child Development in China. Technical report, Gansu Survey of Children and Families, November 2010.

[189] K Zhu, C Li, and S Zhou. Analysis and suggestion on the academic performance of the left-behind children. *Education Science [in Chinese]*, 18(4):21–24, 2002.

[190] A DeBrauw and R Mu. Migration and the overweight and underweight status of children in rural china. *Food Policy*, 36:88–100, 2011.

[191] Elspeth Graham and Lucy P Jordan. Does Having a Migrant Parent Reduce the Risk of Undernutrition for Children Who Stay Behind in South-East Asia? *Asian and Pacific Migration Journal*, 22(3):315–348, December 2013.

[192] Renfu Luo, Linxiu Zhang, Chengfang Liu, Qiran Zhao, Yaojiang Shi, Grant Miller, Elaine Yu, Brian Sharbono, Alexis Medina, Scott Rozelle, and Reynaldo Martorell. Anemia in Rural China's Elementary Schools: Prevalence and Correlates in Ningxia and Qinghai's Poor Counties. *Journal of Health, Population and Nutrition*, 2011.

[193] J Cheng and Y-H Sun. Depression and anxiety among left-behind children in China: a systematic review. *Child Care, Health and Development*, 41(4):515–23, July 2015.

[194] Elspeth Graham and Lucy P Jordan. Migrant Parents and the Psychological Well-Being of Left-Behind Children in Southeast Asia. *Journal of Marriage and the Family*, 73(4):763–787, August 2011.

[195] Zhengkui Liu, Xinying Li, and Xiaojia Ge. Left too early: the effects of age at separation from parents on Chinese rural children's symptoms of anxiety and depression. *American Journal of Public Health*, 99(11):2049–54, November 2009.

[196] Qiaobing Wu, Deping Lu, and Mi Kang. Social capital and the mental health of children in rural china with different experiences of parental migration. *Social Science and Medicine*, 132:270 – 277, 2015.

[197] Fei-ling Wang. Brewing tensions while maintaining stabilities: The dual role of the hukou system in contemporary China. In *Discontented miracle: Growth, conflict, and institutional adaptations in China.* World Scientific, 2007.

[198] J Guo, X Liu, and X Zhu. Migrant workers are not covered by NCMS. *Outlook Weekly [in Chinese]*, 47(12):S12, 2007.

[199] AS Oberai. An overview of migration-influencing policies and programmes. In AS Oberai, editor, *State policies and internal migartion: studies in market and planned economies*, pages 11–26. St. Martins Press, New York City, 1983.

[200] AB Simmons. A review and evaluation of attempts to constrain migration to selected urban centres and regions. In *Population distribution policies in development planning*, pages 87–100. United Nations, New York, 1981.

[201] Dorothy J. Solinger and Yiyang Hu. Welfare, wealth and poverty in urban china: The dibao and its differential disbursement. *The China Quarterly*, 211:741–764, 9 2012.

[202] Shannon Tiezzi. Urbanization With Chinese Characteristics. *The Diplomat*, December 2015.

[203] Q Guo and C. J. Liu. Haerbin government loosening the conditions of hukou transfer for model rural migrant laborers [in Chinese], 2009.

[204] John Harris and Michael P Todaro. Migration, unemployment and development: A dynamic two-sector analysis. *American Economic Review*, 60(1):126–142, 1970.

[205] Wanchuan Lin, Gordon Liu, and Gang Chen. Urban Resident Basic Medical Insurance: A Landmark Reform Toward Universal Coverage in China. *Health Economics*, 18:S83–96, 2009.

[206] China Labor Bulletin. China's migrant workers and their children. Technical report, 2013.

[207] Gary Becker. Family background and the opportunities of children. In *A Treatise on the Family*, pages 179–237. Harvard University Press, Cambridge, MA, 1993.

[208] Yuanli Liu, Keqin Rao, and William C Hsiao. Medical expenditure and rural impoverishment in China. *Journal of Health, Population, and Nutrition*, 21(3):216–22, September 2003.

[209] Björn Gustafsson and Shi Li. Expenditures on education and health care and poverty in rural China. *China Economic Review*, 15(3):292–301, January 2004.

[210] Winnie Yip and William C Hsiao. The Chinese health system at a crossroads. *Health Affairs (Project Hope)*, 27(2):460–8, 2008.

[211] Shanlian Hu, Shenglan Tang, Yuanli Liu, Yuxin Zhao, Maria-Luisa Escobar, and David de Ferranti. Reform of how health care is paid for in China: challenges and opportunities. *Lancet*, 372(9652):1846–53, November 2008.

[212] Ministry of Health of People's Republic of China. Research on Health Services in the Western Part of China. Technical report, Beijing, 2004.

[213] Xin Meng. Children of Migrants: The Impact of Parental Migration on their Children's Education and Health Outcomes. 2012.

[214] Ministry of Health of People's Republic of China. Annual Report on the Health Reform and Development in China. Technical report, Beijing, 2008.