Essays on the Production of Primary Health Care

Scott Patterson Laughery
Seattle, WA

B.A., The George Washington University, 2007
M.A., University of Virginia, 2011

A Dissertation presented to the Graduate Faculty of the
University of Virginia in Candidacy for the Degree of
Doctor of Philosophy

Department of Economics

University of Virginia
May, 2016

Abstract

Expanding access to primary health care is a longstanding objective of government policy. One persistent impediment to access is the relative scarcity of primary care physicians and other health care professionals in rural and low-income urban areas. In response, numerous federal programs have been administered over several decades to attract health professionals to regions where they are otherwise scarce. These programs have been expanded considerably in recent years. Areas to be targeted under the programs are designated as Health Professional Shortage Areas (HPSAs). The first three chapters focus on the effects of these programs on different measures of population health as well as health care access and quality. The fourth chapter is a coauthored paper on the relationship between health information technology adoption in the ambulatory care setting and the quality of ambulatory care.

In the first chapter, I describe the HPSA program and its associated benefits, and discuss my identification strategy. Because HPSA designation must be applied for through a burdensome application process, designation may be endogenous to unobserved attributes of the local health care system. Results from a fixed effects and paired event study specification indicate selection bias, so in my main specifications I estimate the effect of HPSA designation on the various outcomes by fuzzy regression discontinuity, exploiting a cutoff in the HPSA eligibility criteria.

In the second chapter, I estimate the effect of HPSA designation on the rate of ambulatory care sensitive hospitalizations (ACSH), a measure of the effectiveness of local primary care for which a lower value suggests better care. I calculate ACSH rates at the county level using data on all hospital stays of Medicare fee-for-service beneficiaries between 2003 and 2012. I find that designation substantially reduces hospitalizations for acute ACS conditions. Designation might reduce ACSH for chronic conditions as well, but those findings are statistically insignificant. I then test whether HPSA designation affects measures of primary care utilization, provider availability, Medicare reimbursements, and process of care. I find no effect of HPSA designation on any of these outcomes. This, together with a number of empirical challenges related to the institutional complexity of the HPSA program, argues for caution before using the large point estimates in cost-benefit analysis. Nevertheless, the analysis in this chapter is a first step in providing evidence of the effectiveness of the HPSA program.

In the third chapter, I use data from the National Vital Statistics System to study the relationship between HPSA designation and outcomes such as mortality, fertility, and prenatal care. I estimate these relationships by fuzzy regression discontinuity, as described in Chapter 1. The analysis yields no evidence of

an effect of HPSA designation on mortality, prenatal care utilization, birth weight, or gestation. However, the results do suggest that designation reduces fertility among women aged 15-24. I then test whether HPSA designation affects abortion rates using data from nine states and find no effect. The pattern of results suggests that the interventions attached to HPSA designation may increase access to contraception.

The fourth chapter on the effects of health IT adoption in the ambulatory setting is coauthored with Carole Roan Gresenz, Amalia Miller, and Catherine Tucker. US government investments in health information technology (IT) have focused on giving incentives for digital health records in hospital settings and by individual physicians. We evaluate the omission of ambulatory care centers, by studying the effects of healthcare IT on ambulatory care quality, which we measure using the rate of hospital admissions for conditions identified as sensitive to ambulatory care quality, using data from Medicare and the Nationwide Inpatient Sample. Results from difference-in-differences models that control for location and time fixed effects, as well as observable factors related to healthcare quality and population demographics, indicate that increased ambulatory IT adoption lowers local area ambulatory care sensitive (ACS) hospitalizations, suggesting quality improvements. The magnitudes imply that a 45% increase in ambulatory IT adoption in a county (the average increase over our sample period 2003-2012) lowers the ACS admission rate in that county by about 1.6%.

Acknowledgements

First and foremost, this project quite simply would not have been possible without the guidance, advocacy, encouragement, constructive feedback, and patience of my advisors, Amalia Miller and Leora Friedberg. They take mentorship very seriously, and for that I will always be grateful. Thanks also to Chris Ruhm and Ed Olsen for their service on my committee and their valuable input on this project. Many other professors have contributed to this project, in particular Sarah Turner who offered feedback in biweekly research group meetings. In fact, I sought input from several members of the faculty, in economics and elsewhere, and I was never refused a meeting and a helpful, attentive ear.

I am grateful to the Bankard Fund for Political Economy for the funds to purchase the Medicare hospitalization data used in two chapters of this dissertation, to the University of Virginia Library for securely housing these data, and to staff at the Health Resources and Services Administration for furnishing data on National Health Service Corps placements. Moreover, I will always be grateful to the Department of Economics at UVA for more than five years of generous financial support of my graduate studies. I am still in awe of the fact that I can be paid to attend an excellent university and avail myself of its tremendous research infrastructure for several years for the purpose of developing my own human capital.

Of course, I am grateful to my Chapter 4 coauthors, Carole Roan Gresenz, Amalia Miller, and Catherine Tucker. Additionally, chapter 4 benefited from the financial support of the Agency for Healthcare Research and Quality and helpful comments by Mirielle Jacobson.

I also want to thank the many great friends that I have made in graduate school who have helped me in various ways as I have worked on this project. While there are too many to name, I want to at least mention Ignacio Martinez and Tianying He. I lived with each for three years and gained a lot from their experience and camaraderie.

Finally, I extend my heartfelt thanks to my family: my parents Kathy and Rory, and my brothers Todd, Tyler, and Mark. Their influence on me has been the most glaring unfair advantage I have enjoyed in my life. My father in particular inspired my interest in this topic by sharing with me some of his experiences with delivering primary care to underserved populations as I was growing up.

Contents

Chapter 1

Health Professional Shortage Areas (HPSAs): Background

1.1 Introduction

Expanding access to primary health care is a longstanding objective of government policy. While many people face financial barriers to access, some face physical and geographic barriers as well, resulting from disparities in the geographic concentration of health care professionals. Since the 1960s, federal and many state policies have sought to entice primary care physicians and other health care professionals to move to rural and inner-city areas where their concentration is low. These enticements include scholarships and student loan repayment for clinicians who commit to work in underserved communities, relaxation of visa requirements for foreign physicians who do the same, and a 10% bonus payment from Medicare for delivering care in a designated underserved area. To identify underserved areas, these programs rely on federal designation as a primary care Health Professional Shortage Area (HPSA).

While the programs tied to HPSA designation are not on the scale of Medicaid or Medicare, they uniquely target geographic access and they are increasingly being looked to by policymakers to reduce geographic disparities in primary care access and outcomes. The National Health Service Corps (NHSC, the largest and oldest HPSA-tied program) offers scholarships and repays loans for certain health care professionals. The NHSC more than doubled in size between 2009 and 2011, and the president's proposed 2016 budget called for again rapidly expanding the program (Pathman and Konrad, 2012; Health Resources and Services Administration, 2015). Aside from the NHSC expansions, the ACA expanded eligibility for physicians practicing in HPSAs to earn bonus payments from Medicare (Centers for Medicare and Medicaid Services, 2014). Overall, the pattern in recent years has been a rapid increase in resources tied to the Health Professional Shortage

Area designation.

Yet, despite their long history and recently renewed attention, the effectiveness of these programs for improving access to quality care remains poorly understood. This is likely due in part to a lack of data on primary medical care and in part to the difficulty in isolating the effects of HPSA-tied policies given the complexity of the program and policy environment. In this dissertation, I use multiple large national data sets to estimate the effects of HPSA designation on a number of outcomes related to population health and the quality of health care. First, in chapter 2, I examine how HPSA designation affects the rate of hospitalizations for ambulatory care sensitive (ACS) conditions among the Medicare population. ACS conditions, such as hypertension, urinary tract infection, and diabetes, are those for which hospitalization is typically preventable through timely and effective outpatient care. A high rate of hospitalization for such conditions in a population suggests that its primary health care needs are not being met. Using individual-level data on every hospitalization of a Medicare fee-for-service beneficiary in the U.S. between 2003 and 2012, I measure the ACS hospitalization (ACSH) rate in each county using the Prevention Quality Indicators (PQI) module developed by the Agency for Healthcare Research and Quality. Second, in chapter 3, I measure the effects of HPSA designation on fertility, mortality, and on a variety of measures related to health care utilization during pregnancy as well as prenatal and neonatal health. I also estimate the effect of HPSA designation on rates of abortion in a sample of nine states.

I employ two distinct methods to estimate the effects of HPSA designation. The first (only estimated with ACSH as the outcome) is a linear differences-in-differences specification with county and year fixed effects, paired with an event study around HPSA designation. The event study entails regressing ACSH on a vector of binary variables indicating the number of years since initial designation as a HPSA. Each coefficient estimate captures the average difference between the ACSH rate between counties a certain number of years before and after designation and those that are never designated, controlling for county and year fixed effects and county-level control variables. The event study results reveal pre-designation trends in that counties are later designated. Counties that obtain HPSA designation exhibit falling ACSH rates prior to designation relative to other counties, after controlling for observable county-level covariates. This finding suggests that there are unobservable time-varying county attributes that both make a county more likely to obtain a HPSA designation and reduces ACSH in the county. The finding of pre-designation trends (which are not eliminated after including linear county time trends to the regression) indicates that the differences-in-differences specification does not identify the causal effect of HPSA designation on the ACSH rate.

In fact, the sort of unobserved heterogeneity suggested by the event study results is unsurprising in the case of HPSAs. Designation is not automatic, but instead must be applied for. The applicant incurs the cost of collecting data necessary to demonstrate eligibility. The attitudes or institutional capacity of health care providers in a county might affect both whether they apply for designation and whether they are effective in preventing ambulatory care sensitive hospitalizations, mortality, or adverse pregnancy outcomes. This finding motivates my second (and main) empirical approach: a fuzzy regression discontinuity (RD) design exploiting a cutoff in the eligibility criteria for HPSA designation. In most cases, a county is eligible for designation if primary care in adjacent counties is inaccessible or over-utilized and the ratio of inhabitants to primary care physicians in the county is greater than 3,500:1. Using annual estimates of full-time-equivalent primary care physicians, I use the 3,500:1 eligibility cutoff value to estimate a fuzzy regression discontinuity model. As Lee and Lemieux (2010) demonstrate, this approach identifies the effect of the treatment in a neighborhood around the cutoff provided that the assignment variable (the number of physicians per capita) is not precisely manipulable by the applicants for HPSA designation. This fuzzy RD analysis represents the first attempt in the literature to investigate the effects of HPSA designation on a population health outcome while accounting for the possible endogeneity of designation.

1.2 Background

1.2.1 Benefits of HPSA Designation

Designation as a Health Professional Shortage Area is made by the federal Health Resources and Services Administration (HRSA). A HPSA can be a defined geographic area (such as a county or a group of Census tracts), a specific population living in a geographic area (such as low-income or Medicaid-eligible populations), or an individual facility. I focus here on whole-county geographic HPSAs. In addition to primary care HPSAs (which are the focus of this paper), separate designations exist for dental care and mental health care as well. As of April 13, 2016, there were 6,385 designated primary care HPSAs in the United States, including 1,371 geographic HPSAs, 1,415 population group HPSAs, and 3,599 facility HPSAs. There are HPSAs in all 50 states, as well as the District of Columbia, Puerto Rico, and other U.S. dependencies. Over 50 million people live in designated HPSAs (roughly evenly split between geographic and population group designations) (Health Resources and Services Administration, 2016).

HPSA designation confers several valuable benefits upon health care providers in the designated area. By one estimate, 21 federal programs use the HPSA designation to target program resources, although most of these programs are not restricted to HPSAs (Negotiated Rulemaking Committee on the Designation of

Medically Underserved Populations and Health Professional Shortage Areas, 2011). Additionally, many state programs are also linked to HPSA designation, including scholarship and loan repayment programs for health professionals.[1] Because so many programs are attached to HPSA designation, I cannot identify the effect of any single HPSA-tied intervention and therefore estimate the combined effect of programs tied to HPSA designation. The most significant programs tied to HPSA designation are the National Health Service Corps (NHSC), the J-1 visa waiver program, and Medicare physician bonus (sometimes referred to as the Medicare Incentive Payment).

The NHSC offers scholarships and repayment of up to $50,000 in student loans for primary care physicians, physician assistants, nurse practitioners, and certified nurse midwives who commit to work in an approved facility located in a HPSA for at least two years. Approved facilities must accept Medicare and Medicaid patients, employ a sliding fee schedule for low-income patients, and meet other criteria (National Health Service Corps, 2015). To allocate NHSC members to areas with the greatest need, HPSAs are ranked by their numerical scores, which are assigned upon designation with higher scores indicating more pronounced need. HRSA determines a threshold HPSA score annually, and employers in HPSAs below the threshold can only hire NHSC members once all vacancies in HPSAs above the threshold are filled. Any NHSC members hired in a HPSA below the threshold will receive loan repayment at a reduced rate. This score threshold effectively allows the NHSC to roughly match the number of member job seekers to the number of prioritized employers based on intensity of need. When expanded dramatically under the American Recovery and Reinvestment Act, the NHSC both doubled the number of new participants with loan repayment contracts within a year and simultaneously lowered the HPSA score threshold from 17 to 10.[2]

The second major HPSA-tied government intervention is the J-1 visa waiver. Hagopian et al. (2003) report that 25 percent of U.S. physicians completed medical school overseas. Foreign physicians who come to the U.S. after medical school for residency and/or fellowship training most often use a J-1 visa. They are then typically required to return to their home country for two years before they are eligible to work in the U.S. The waiver program allows these physicians to avoid this return-home requirement if they commit to work with underserved populations for three years (Johnson et al., 2003). The large majority of waivers are allocated by the states through the Conrad State 30 program, through which each state can request waivers for 30 physicians each year.[3] Each state administers its own program, and states have

[1] Among 31 state-administered loan repayment and scholarship programs included in a list maintained by the American Association of Medical Colleges, 13 used the HPSA designation, 16 used other location criteria, and two were indeterminate.

[2] NHSC enrollment information from HRSA 2013 Congressional Budget Justification; HPSA score thresholds from Federal Register notices by HRSA dated 6/18/2008 and 5/28/2009.

[3] The total combined waiver capacity of all states is large compared to the number of physicians seeking waivers, but not all slots are filled as the popularity of different states varies widely.

considerable flexibility in setting their own criteria. For example, several states allow specialists in addition to primary care physicians, and some states restrict waiver placements to only rural or only urban areas (Hagopian et al., 2003). As a consequence of the decentralized nature of the Conrad State 30 program, there is no repository of data on visa waiver physicians and their practice locations. This has been cited as a problem for policymakers tasked with assessing local health care needs (Mueller et al., 2002; Government Accountability Office, 2006a), particularly since retention of visa waiver physicians following the completion of their commitments has been found to be substantially lower than retention of clinicians participating in NHSC and similar programs (Crouse and Munson, 2006; Kahn et al., 2010; Opoku et al., 2015).

The third major HPSA-tied intervention is a physician bonus from Medicare. The bonus payment is equal to 10% of Medicare Part B reimbursements for care delivered to Medicare beneficiaries in designated geographic HPSAs. The bonus is available both to primary care and specialist physicians. The bonus is the only federal HPSA-tied intervention that is automatic and for which (geographic) HPSA designation is the sole necessary and sufficient condition to receive the intervention (NRMC, 2011). Medicare bonus payments totaled approximately $100 million annually from the 1990s through 2002 (before the payment was made automatic), and between 2003 and 2011, total payments increased from roughly $150 million to $274 million (Hayes et al., 2013; Chan et al., 2004). The Affordable Care Act introduced an additional Surgical Incentive Payment for general surgeons in HPSAs that stacks with the HPSA bonus (Centers for Medicare and Medicaid Services, 2014).

1.2.2 HPSA Eligibility and Designation Procedure

The main empirical strategy in this dissertation entails exploiting exogenous variation in HPSA designation status generated by cutoffs in HPSA eligibility rules. To be eligible for designation as a primary care HPSA, a geographic area must satisfy three conditions. First, it must be a *rational service area* for the provision of care, such as a county or a group of Census tracts that are contiguous, economically interlinked, and distinct from neighboring areas. I perform my analysis at the county level and counties qualify as rational service areas, so this condition is trivially satisfied. Second, the area must have either (a) more than 3,500 inhabitants per full time equivalent (FTE) primary care physician, or (b) between 3,000 and 3,500 inhabitants per FTE primary care physician and also exhibit exceptionally high need based on rates of poverty, fertility, infant mortality, or excessive use of existing primary care services.[4] Physician members of the NHSC are excluded from the physician count, while J-1 waiver physicians are not excluded. Finally, primary care availability

[4] As of August 2015, 37 percent of designated geographic HPSAs met the high-need threshold (Health Resources and Services Administration, 2016).

in neighboring areas must be inaccessible or over-utilized. The empirical analysis entails using county-level data to identify which counties are subject to which cutoff value of the physician-to-population ratio and then estimating regression discontinuity around the cutoff.

HPSAs are not designated automatically, but must be applied for. The application must include all information necessary to confirm eligibility and assign a HPSA score, including most importantly the number of area primary care physicians reported using survey data. Applications can be submitted directly to HRSA, but applicants are encouraged instead to apply through the state's primary care office (PCO). The fact that a HPSA can only be designated following a burdensome application and review process introduces a possible selection effect and motivates the empirical approach taken in this paper. HRSA releases neither HPSA applications nor data on the number of applications received and evaluated, but the agency indicates on its website that they evaluate applications withing ninety days of receipt.[5] The Maryland PCO reports that application review can extend longer than a year, and also indicates that it submitted 132 HPSA applications between 2007 and 2010, of which 66 were approved (Primary Care Office (MD), 2011). HPSA designation must be renewed every three years, with the applicant demonstrating continuing eligibility using updated data. However, between 2002 and 2011, although renewal applications continued to be evaluated regularly by HRSA, the agency did not withdraw any HPSA designations for areas that could no longer demonstrate eligibility (Government Accountability Office, 2006b; Health Resources and Services Administration, 2011). The only withdrawals of designated HPSAs during that period resulted from new designations superseding previous ones.[6]

The process for designating HPSAs has long been controversial. The current eligibility criteria for primary care HPSAs have been largely unchanged since 1980. As detailed in Salinsky (2010), there have since been three official proposals to overhaul the HPSA designation process, in 1998, in 2008, and 2010-2011. None of these proposals have been implemented but they all sought to address the same widely recognized deficiencies of the HPSA designation process. These are, first, the administrative burden placed on states and on applicants to collect data; second, the inadequacy of the primary care physician-to-population ratio as a comprehensive measure of primary care resources in a community; and third, the duplicated burdens created by the simultaneous use of the HPSA designation and the Medically Underserved Area (MUA) designation (Salinsky (2010); Ricketts et al. (2007); NRMC, 2011).[7] The primary care physician supply is thought to

[5] `http://bhpr.hrsa.gov/shortage/hpsas/apply.html`

[6] Source: author's email correspondence with HRSA.

[7] The MUA is a separate, parallel designation that is primarily used to place federally qualified health centers (FQHCs). Like HPSAs, MUAs must be applied for, but eligibility is less dependent on primary care physician supply and designations do not have to be renewed once granted.

be an inadequate measure because it does not account for care provided by physician assistants or nurse practitioners, or primary care provided by specialists.

In spite of the criticisms of the HPSA program and its administration, research has found that HPSA designations are broadly well targeted, meaning that HPSA residents are indeed underserved. Parchman and Culler (1999) use the Medicare Current Beneficiary Survey to study ambulatory care sensitive hospitalizations among Medicare beneficiaries living in West Virginia in 1991 who rated their own health as fair or below. Controlling for several patient-level covariates, HPSA residents experienced significantly more ACS hospitalizations. Liu (2007) uses data from a 2001 telephone survey of the uninsured in West Virginia to show that HPSA residents are poorer, less educated, in worse physical health, and less likely to have received primary care in the last 6 months. Kohrs and Mainous III (1995) use data from a 1993 Kentucky survey and find that HPSA residents perform worse than others in measures of social functioning, mental health, and pain.

1.2.3 Literature

In these chapters, I estimate the effect of HPSA designation on several measures of population health. The only article in the literature to do this is Pathman et al. (2005). The authors measure age-adjusted mortality in nearly 1,500 rural counties in 1981–1983 and 1996–1998. The counties are grouped based on whether they have HPSA designations in 1984 and on the number of years during the interval that the county had an NHSC placement. Counties that either gain or lose designation between 1984 and 1998 are excluded. The authors measure the change in age-adjusted mortality between these groups of rural counties and find that, while mortality decreased across all groups over this period, there was no apparent difference in the rates at which mortality decreased that might suggest an effect of HPSA designation or NHSC intervention. While the authors calculate these differences in rates of mortality reduction, they do not perform any statistical tests using their findings.

While Pathman et al. (mostly) find a null result of HPSA designation, there are three key differences between their study and mine that could lead to a different finding in my case. First, I consider several other outcomes in addition to mortality. It is plausible that HPSA designation would affect, for example, ACSH without affecting mortality. Second, whereas Pathman et al. studies the period 1981–1998, I consider the period 1999–2012 (depending on the particular outcome). The benefits attached to HPSA designation have increased considerably since 1998.[8] Third, I employ an identification strategy (regression discontinuity in my

[8]Aside from the large NHSC and Medicare bonus expansions discussed in section 1.2.1, the Conrad State 30 program was expanded in 2002 from 20 to 30 waivers per state per year.

main specification) to identify the causal HPSA effect under more plausible assumptions than those required to interpret the Pathman et al. results as causal. Pathman et al. measure rates of change in mortality between different groups of counties, which could only identify the causal effect if changes in county-level mortality are unaffected by any factors that are correlated with HPSA exposure. Pathman et al. do not make this claim.

Although no other papers directly consider the effect of HPSA-tied programs on population health, there are two related literatures that inform my analysis. First, there is a body of research into the effects of location incentive programs on physician location choice. Second, there is a large literature on the relationships between primary care physician concentration and population health outcomes. While at least some of the papers in the first literature are focused on identifying causal relationships, the second literature mostly describes associations in the data without considering possibly endogenous physician choice.

In general, articles in the first literature find that physicians respond to incentives similar to those generated by the HPSA-tied programs. First, Holmes (2004) applies bivariate Probit to physician-level data to estimate the causal effect of NHSC participation on subsequent work in underserved areas. He finds that, controlling for selection into NHSC, participation in NHSC reduces the probability of working in the same location five years later. However, NHSC participation increases the probability of working in *any* underserved area after five years. This paper is particularly relevant because it is the only one to estimate a causal effect of a program tied to HPSA designation.

Other papers investigate the effects of financial incentives for physicians on their choice to practice in underserved areas. This literature is well summarized by Bärnighausen and Bloom (2009), who review and perform a meta-analysis using 43 studies evaluating such financial incentives. Among the studies reviewed, 17 compared retention in underserved areas between physicians with and without financial incentives. The findings overall corroborate Holmes (2004), though Bärnighausen and Bloom note that Holmes is the only study to address unobserved heterogeneity. An additional study not included in the review was performed by Bolduc et al. (1996) on the choice of initial practice location by new primary care physicians in Quebec. Physician choice is modeled by spacially autoregressive multinomial Probit using a sample period covering the introduction of financial incentives to work in underserved areas of the province. The authors estimate that Quebec's financial incentives resulted in a large, statistically significant increase in primary care physician availability in underserved areas. The collective finding of this literature is that physicians respond to financial incentives of the type that are attached to HPSA designation.

The second relevant literature focuses on the relationship between primary care physician supply and

population health. This literature is quite extensive, and has been synthesized in literature reviews and meta-analyses by Blumenthal et al. (1995); Starfield et al. (2005); Macinko et al. (2007); and Zerehi (2008). In general, articles in this literature employ ordinary least squares or logistic regression to measure associations between the ratio of primary care physicians to population and a chosen health outcome, typically controlling for local economic and demographic characteristics.

The findings in this literature are varied. Shi (1992) and Starfield et al. (2005) find that primary care physician density is associated with lower mortality and that specialist density is associated with higher mortality. Ricketts and Holmes (2007) dispute those findings and also find large regional variations in the relationship between primary care physician density and mortality. Chang et al. (2011) use 2007 data on a large sample of Medicare beneficiaries and find those who live in areas with more primary care physicians experience lower mortality and fewer ACS hospitalizations. Other studies on the relationship between primary care physicians and ACS hospitalizations have been mixed, with Laditka et al. (2005) and Zhan et al. (2004) finding a negative association but Krakauer et al. (1996) finding no association.

The ambiguous findings of this second literature mean that, even if HPSA designation increased the supply of primary care physicians (as predicted by the first literature), the effect on population health outcomes could not be predicted by the literature – a fact which motivates this empirical analysis.[9]

1.3 Data

The empirical analyses in Chapters 2 and 3 are performed using a panel dataset containing yearly records for every county in the United States between 1999 and 2012 (depending on the particular outcome being considered). Each record contains the county-level average rate of the outcome (e.g. ACS hospitalization rates), indicators of HPSA designation, and various county characteristics including the estimated number of full time equivalent primary care physicians and other variables upon which HPSA eligibility is based.

1.3.1 Shortage Area Designation and Eligibility

I downloaded a list of all current and former primary care HPSAs from the HRSA Data Warehouse, an online repository of up-to-date information on HRSA-related initiatives. The dataset includes the type and location of every HPSA along with its designation date and, for former HPSAs, the designation withdrawal date. Using this dataset, I generated binary variables indicating the presence of several types of HPSA designation (e.g. whole-county geographic area) in each year. In addition to the binary indicators of HPSA designation, I estimate the fraction of each county's population living in population group and geographic

[9] Of course, even if the second literature were conclusive, the empirical analysis would still be warranted because HPSA designation does more than simply introduce additional physicians.

Table 1.1: Whole-County HPSAs: Changes in Designation Status and Number of Designated Counties, by Year

Year	Gains	Losses	Designated
2003	16	2	796
2004	10	4	804
2005	16	14	816
2006	25	25	827
2007	15	17	817
2008	7	19	807
2009	10	54	798
2010	20	27	764
2011	40	84	777
2012	68	41	761
Avg.	22.7	28.7	797

Source: Author calculations based on data from Health Resources and Services Administration (HRSA).

HPSAs in each year. I do this by calculating the combined population of all HPSAs in a county in a year and expressing it as a fraction of the county population. Intercensal population estimates are taken from the Census Bureau.

In the empirical analysis, I focus on whole-county HPSA designation because I do not observe physician counts below the county level. As shown in Table 1.1, the average number of counties with a whole-county designation between 2003 and 2012 was 797 (out of the 3,147 counties and county equivalents in the U.S.). During that interval, 227 counties gained and 287 counties lost whole-county designations.

As described in Section 1.2.2, a county is eligible for HPSA designation if primary care in adjacent areas is inaccessible or over-utilized, and if the ratio of population to full-time-equivalent primary care physicians exceeds a threshold value (either 3,500:1 or 3,000:1 depending on whether the county is high-need). The regression discontinuity analysis entails comparing counties on either side of the physician ratio threshold. To determine which observations to include in the RD regressions, I identify each county's physician ratio threshold in each year. I determine high-need status using the county poverty rate reported in the Small Area Income and Poverty Estimates along with fertility and infant mortality rates calculated from restricted-access vital statistics data from the National Center for Health Statistics.[10]

To determine whether primary care in adjacent counties is over-utilized or inaccessible for each county observation, I use guidelines for assessing HPSA eligibility published by HRSA[11] and the California Office of

[10]Specifically, I calculate one-year fertility and infant mortality rates using period-linked birth/infant death records through 2009, and from the separate natality and mortality files for 2010–2012.

[11]`http://bhpr.hrsa.gov/shortage/hpsas/designationcriteria/medicaldentalhpsaguidelines.html`

Statewide Health Planning and Development[12]. For a county to be eligible for designation, each neighboring county must satisfy the "inaccessible or over-utilized" criteria. A neighbor satisfies these criteria if it is a HPSA, if its own physician ratio exceeds 2,000:1, if there are large demographic differences between the proposed HPSA and the neighbor, or if the travel time from the population center of the proposed HPSA to the nearest source of primary care in the neighbor exceeds 30 minutes. As I do not observe the precise location of primary care delivery sites, I use the travel time between the population centers of the proposed HPSA and each neighbor, using distance and travel time data from the Oak Ridge National Laboratory[13].

1.3.2 FTE Physician Count

The running variable in my regression discontinuity specifications is the number of full-time-equivalent (FTE) primary care physicians per 10,000 county inhabitants. I calculate this in each county and in each year using the Area Health Resources File (AHRF) maintained by HRSA. The AHRF is a collection of data from several original sources reported at the county level. The physician totals in the AHRF are derived from the American Medical Association (AMA) Physician Masterfile and an analogous list maintained by the American Osteopathic Association. The AMA Physician Masterfile is a widely used source of data on the characteristics and location of the physician workforce.[14] Basing local physician counts on the AMA Masterfile is attractive due to its completeness, national coverage, and long history. Records in the AMA Masterfile include self-reported physician specialty, professional activity, and office and mailing addresses. Data in the AHRF are aggregated to the county level, but are presented with sufficient granularity to allow the measurement of primary care physicians using a definition that matches the HRSA guidelines for assessing HPSA eligibility.

The literature suggests that the Physician Masterfile data provide an imperfect measure of the physician count, particularly in rural areas. Physician counts at the county level in the AHRF are based on recorded office locations but, when they are missing, recorded mailing addresses are used. Using Masterfile records for physicians in greater Chicago, McLafferty et al. (2012) find that, in 17% of cases, office addresses and mailing addresses lie in different counties. In addition, Konrad et al. (2000) survey pharmacists in a sample of rural communities to identify the physicians in each community. Comparing these figures to Masterfile data, the authors find that the Masterfile overstates the number of physicians in small towns by 20 percent on average relative to the number reported by local pharmacists. Yet, in spite of these imperfections, the

[12] `http://www.oshpd.ca.gov/HWDD/Application_Process.html`

[13] `http://cta.ornl.gov/transnet/SkimTree.htm`

[14] The AMA Masterfile is the source of physician data in Holmes (2004); Chang et al. (2011); and Starfield et al. (2005), for example.

first-stage regression discontinuity results presented in Section 2.3.1 are robust to a variety of specifications, which suggests that the AMA Masterfile-derived physician count measure I used broadly aligns with the measures used in applications for HPSA designation.

I detail the construction of the estimated number full time equivalent primary care physicians in Appendix A. The rule for counting physicians aligns closely with the definition of primary care physician used to determine HPSA eligibility. Physician FTEs are estimated using the local distribution of physicians by age and sex (reported in the AHRF), along with national estimates of physician FTEs by age and sex that I tabulated from the 2005-2009 American Community Survey. The final physician FTE variable is expressed as the number of FTE primary care physicians per 10,000 county inhabitants. The HPSA eligibility threshold of one FTE primary care physician per 3,500 county inhabitants corresponds to 2.86 per 10,000 inhabitants. Of course, HPSA applications must report the actual number of hours worked by each primary care physician in the county and, since I do not observe individual physician hours in the AHRF, my estimate of physician FTEs is approximate. To the extent that observed primary care physician FTEs differ from their true level, this will bias toward zero the estimated discontinuities in the outcome variable (the "reduced form") and the probability of HPSA designation (the "first stage"). As I describe in Section 1.4.2, the effect of HPSA designation on each outcome is estimated by a Wald statistic in which the first-stage discontinuity appears in the denominator. If the first-stage discontinuity is biased toward zero, the Wald estimate can become unstable and imprecisely estimated.

The AHRF does not report data on physicians in 2009. For the fixed effects and event study analyses, I linearly interpolate each county's 2009 FTE primary care physician ratio using the 2008 and 2010 figures, except in the 10 percent of counties experiencing the greatest change in FTE physician ratio over that period. However, in the main regression discontinuity specifications, I do not interpolate physicians in 2009 because the precise value of the physician count (relative to the eligibility threshold) is more important in that setting.

1.4 Empirical Analysis

1.4.1 Fixed Effects and Event Study Specifications

As a first approach to the data, I estimate the effect of HPSA designation on the ACSH rate by a differences-in-differences specification including county and year fixed effects, paired with event studies around new HPSA designation and loss of HPSA designation. The event studies serve, first, to check for pre-event trends in counties whose designation status changes and, second, to investigate the timing of the ACSH response

to HPSA designation. I ultimately find that these methods, despite having some attractive properties, are ill-suited to identify the causal effect of HPSA designation on the ACSH rate. For this reason, I do not repeat the analysis using vital statistics data. In the remainder of this subsection, I briefly summarize the results and implications of the fixed effects/event study estimation. A considerably more detailed exposition appears in Appendix C.

In the fixed effects model, I regress the ACSH rate on whole-county HPSA designation, controlling for county and year fixed effects as well as several county-level covariates. The covariates include (among others) the fraction of each county's population living in a population group HPSA and the fraction living in a sub-county geographic HPSA.[15] The event study specification entails regressing the ACSH rate on a vector of dummies indicating the number of years prior to or since HPSA designation. The parameters of the event study model indicate the difference in expected ACSH rates between a county that is a specified number of years from whole-county designation and a county that never becomes designated, conditional on county and year fixed effects and on covariates.

The results of this analysis suggest first that the relationship between HPSA designation and the ACSH rate differs between counties that gain designation and those that lose designation. Second, event study results reveal pre-trends in ACSH rates prior to the gain or loss of designation, suggesting that designation is correlated with unobservable time-varying county-level characteristics that affect ACSH. In response to this finding, I re-estimate the fixed effects and event study with county-specific linear time trends included. For counties that gain designation, the inclusion of the trends results in statistically imprecise, ambiguous estimates. For counties that lose designation, including the county-specific linear trend does not eliminate the evidence of a pre-trend in the relative ACSH rate prior to loss of designation.

Ex ante, the fixed effects and event study models are attractive as an empirical approach for a number of reasons. Given the longitudinal nature of the data set, it is appealing to control for county and year fixed effects. It is similarly appealing to control for the share of the population in counties without whole-county HPSA designations who are in fact covered by sub-county HPSAs. The multitude of HPSAs of different types at different levels of geography poses an empirical challenge, and the fixed effects approach offers a reasonable solution in the form of explicit controls for the fraction of the county population living in population group and sub-county geographic HPSAs. Third, the event study analysis might have revealed to what extent any effects of HPSA designation on ACSH rates are delayed. My event study specifications

[15]Other control variables include the unemployment rate from the Bureau of Labor Statistics and the median household income and poverty rate from the Small Area Income and Poverty Estimates, as well as county-level measures of per-capita (mostly federal) government transfer payments to individuals. The government transfers data are reported by the Bureau of Economic Analysis.

are based on Bailey and Goodman-Bacon (2015), who study the effect of community health centers (CHCs) on county-level mortality rates. An important finding of their paper is that the full benefits of CHCs only materialize after some years. One would expect that the same would be true of the benefits tied to HPSA designation in light of the similarities between designation and CHC introduction.[16]

However, taken together, the fixed effects and event study specifications do not appear to identify the effect of HPSA designation on ACSH rates. This is reflected in divergent trends between counties that are about to change HPSA status relative to other counties, even when county-specific linear time trends are included in the estimation. These results suggest that changes in HPSA designation are being influenced by unobserved factors that also drive ACSH rates. These factors are county-specific and vary non-linearly over time (since they are not absorbed by the fixed effects or county trends). Consequently, in spite of the advantages of the fixed effects and event study approach, my main analysis uses a regression discontinuity design.

1.4.2 Regression Discontinuity Empirical Approach

My main empirical approach is motivated by the fixed effects and event study results, which suggest that both HPSA designation and the ACSH rate respond to the same unobserved, time-varying county characteristics. The institutional context for this finding of apparent endogeneity is that HPSA designation must be applied for, which is burdensome to the applicant. The unobservable characteristics of a county influencing the decision to apply for designation could also affect ACSH. To estimate the effect of HPSA designation, then, it is necessary to identify and exploit some exogenous variation in designation status. The variation I use is the cutoff level of FTE primary care physicians per capita above which a county is not eligible for designation. Having found this evidence of endogeneity in ACSH, I proceed to exploit this same exogenous variation in HPSA designation status to estimate the effects of designation on mortality, fertility, prenatal care, and neonatal health outcomes.

More precisely, I estimate the effect of HPSA designation on each outcome using a fuzzy regression discontinuity (RD) design around the eligibility threshold number of primary care physicians per capita. The implied relationship between the variables of interest is given in Equation 1.1, where Y_{it} is the outcome, X_{it-1} is a continuous "running" variable,[17] h_{it} is an indicator of treatment that varies discontinuously at some cutoff value c of the running variable, and the unobserved error ϵ_{it} is in general correlated with the treatment status (so OLS regression would not identify β_2). In this particular setting, the assignment variable

[16] Both are federal interventions that must be applied for and that aim to expand access to basic health care among the underserved.

[17] The running variable is not lagged in general, but it is in this application.

X_{it-1} is the number of FTE primary care physicians per 10,000 county inhabitants in the previous year. The eligibility cutoff value $c = 2.86$ is the level of X that corresponds to one primary care physician per 3,500 county inhabitants, the cutoff for HPSA eligibility in low-need counties that are otherwise eligible for HPSA designation. The treatment h_{it} is whole-county designation in year t.

$$Y_{it} = \beta_0 + \beta_1 X_{it-1} + \beta_2 h_{it} + \epsilon_{it} \tag{1.1}$$

"Fuzzy" RD refers to RD designs in which the treatment status h_{it} is not determined solely by whether the assignment variable X_{it-1} exceeds c. In fuzzy RD, the treatment effect $\hat{\beta}_2$ is calculated as the estimated discontinuity in $\mathbb{E}[Y_{it}|X_{it-1}]$ at the eligibility cutoff $X_{it-1} = c$ divided by the estimated discontinuity in the probability of designation $\mathbb{E}[h_{it}|X_{it-1}]$ at the same cutoff. The discontinuity in the expected value of Y_{it} at the cutoff is the "reduced-form" discontinuity, which is the change in ACSH at the HPSA eligibility cutoff. The discontinuity in the probability of treatment is the "first-stage" discontinuity (which is the change in the HPSA designation at the eligibility cutoff), and the ratio of reduced-form to first-stage estimates is the Wald estimate of the treatment (the change in the outcome that can be attributed to the change in HPSA designation). I use these terms to refer to the estimates in this section. I estimate the fuzzy RD model by local linear regression with a triangular kernel.[18] This procedure entails estimating the reduced-form discontinuity by weighted least squares regression of ACSH on the physician ratio separately to the left (ℓ, shown in equation 1.2) and to the right (r, shown in equation 1.3) of the cutoff c.[19] The regressions are weighted by a triangular kernel function of the specified bandwidth h, denoted K_h. The reduced-form discontinuity τ_{RF} is the difference in the estimated intercepts $\gamma_{0\ell}$ and γ_{0r}, as shown in equation 1.4. The first-stage discontinuity τ_{FS} is calculated analogously, but with HPSA designation (h_i) in place of ACSH (Y_i) in equations 1.2 and 1.3. The Wald estimate for β_2 is shown in equation 1.5.

$$\left(\hat{\gamma}_{0\ell}, \hat{\gamma}_{1\ell}\right) = \underset{\gamma_{0\ell},\gamma_{1\ell}}{\arg\min} \sum_{i|c-h<X_i<c} \left(Y_i - \gamma_{0\ell} - \gamma_{1\ell} X_i\right)^2 K_h(X_i) \tag{1.2}$$

$$\left(\hat{\gamma}_{0r}, \hat{\gamma}_{1r}\right) = \underset{\gamma_{0r},\gamma_{1r}}{\arg\min} \sum_{i|c<X_i<c+h} \left(Y_i - \gamma_{0r} - \gamma_{1r} X_i\right)^2 K_h(X_i) \tag{1.3}$$

$$\hat{\tau}_{\text{RF}} = \hat{\gamma}_{0r} - \hat{\gamma}_{0\ell} \tag{1.4}$$

[18] I use the "rd" package developed for Stata by Austin Nichols.
[19] For clarity, I drop the time subscript in equations 1.2 and 1.3.

$$\hat{\beta}_2 = \frac{\hat{\tau}_{\text{RF}}}{\hat{\tau}_{\text{FS}}} \tag{1.5}$$

Unlike in the fixed effects and event study specifications, missing data on physicians in 2009 are not filled in by interpolation, so values of each outcome from 2010 are excluded from the sample. The sample used in the regressions consists of county-year observations for which the lagged FTE physician count is greater than zero and for which 2.86 physicians per 10,000 inhabitants is the relevant eligibility cutoff.[20] Counties subject to the eligibility cutoff at 2.86 are those that do not exhibit "high need" as demonstrated by high rates of poverty, fertility, or infant mortality.[21] (High-need counties are subject to a more lax eligibility cutoff of 3.33 per 10,000 population – 1 physician per 3,000 inhabitants.) As shown in Section 2.3.1, there is a discontinuity in the probability of HPSA designation for the non-high-need counties facing the cutoff value of 2.86, but there is no evidence of a discontinuity in designation probability for high-need counties at the cutoff value of 3.33.

The crucial identifying assumptions in the regression discontinuity design are first that the treatment probability is indeed discontinuous at the cutoff, and second that the underlying relationship between physicians per capita and the outcome variable conditional on designation ($\mathbb{E}[Y_{it}|X_{it-1}, h_{it}]$) is continuous through the cutoff.[22] As a consequence, any discontinuous change in the expected value of the outcome at the cutoff can be attributed to the discontinuous change in the probability of being designated. This condition might be violated if, for example, counties considering applying for HPSA designation can precisely manipulate the value of X_{it-1}. If precise manipulation were possible, the unobserved county characteristics that influence the decision to apply for HPSA designation would be discontinuous at c. If those characteristics also affected Y_{it}, then the continuity assumption would be violated. However, as Lee and Lemieux (2010) discuss, the assumption is only violated if potential applicants for designation can exert *precise* control over the number of FTE physicians. Even if counties considering applying for designation can manipulate the FTE physician count, the continuity assumption is not violated as long as the manipulation is imprecise.[23]

Despite the more robust identification in the RD setting compared to fixed effects or event study specifications, RD has limitations. Some of these are general and others are specific to this particular application.

[20] I exclude observations with zero physicians (approximately 5% of the sample) because including those observations biases the estimates. This bias appears in plots of the RD results as a visibly poor fit to the data and is confirmed by applying the bias estimation procedure developed by Calonico et al. (2014).

[21] There is an exception in specifications for which infant mortality or fertility is the outcome in the regression. See Section 3.3 for details.

[22] A further condition is monotonicity, meaning that the sign of β_2 is the same for all counties.

[23] Identification could theoretically be threatened if the entity applying for designation observes the county population and has complete control over the labor supply of all physicians in the county, but this would also imply bunching just below the eligibility threshold and the results of a McCrary test reveal no evidence of such bunching.

First, since regression discontinuity designs entail estimating the treatment effect only at the cutoff value by using only data "near" the cutoff value, RD estimates can be ill-suited for drawing inference away from the cutoff. If, for example, the effect of HPSA designation varies with the number of physicians, the RD estimate may be very different from the effect of HPSA designation on counties with substantially fewer (or more) than 2.86 physicians per 10,000 inhabitants. This is unfortunate because some HPSA-tied resources, e.g. National Health Service Corps placements, are only furnished to HPSAs that have the highest scores (indicating the most acute need), so the effect of HPSA designation may indeed differ between counties near the cutoff and those with fewer physicians.

Second, the researcher has to decide whether to estimate the discontinuities by parametric regression (and, if so, at what polynomial order) or nonparametric regression (and if so, with what kernel and at what bandwidth). These choices can affect the estimates, and there is no clear rule for making many of these decisions. In my main specifications, I generate estimates by local linear regression with a triangular kernel, because local linear regression has been found to minimize bias and the triangular kernel has been found to be the most efficient in the RD context (Fan and Gijbels, 1996; Nichols, 2007). I use the Imbens-Kalyanaraman (IK) bandwidth, which is asymptotically optimal for sharp regression discontinuity designs (Imbens and Kalyanaraman, 2012). I also report results using wider bandwidths than IK because fuzzy RD entails greater variance than sharp RD due to the first stage estimation.

A challenge posed by RD estimation that is unique to this application is that the physician-population ratio is both the running variable (and must therefore be controlled for) and, it is suspected, one of the principal channels through which HPSA designation might affect ACSH. In that sense, it is both a determinant and an outcome of designation in the RD setting. If in fact HPSA designation increases the number of physicians and more physicians in fact lead to a decrease in the outcome variable, then to control for physicians would bias the reduced-form and Wald estimates toward zero. To ameliorate this problem, I use the lagged FTE physician measure as the running variable, which allows the contemporaneous FTE physician count to vary with HPSA designation status and the outcome.[24] Still, to the extent that primary care physician supply takes longer than one year to adjust in response to HPSA designation, and that increased physician supply affects the population health outcomes, the reduced-form and Wald estimates will continue to be biased toward zero.

[24] An additional benefit of using the lagged physician ratio is that the HPSA application process can sometimes take a year or more (Primary Care Office (MD), 2011), so the lagged physician count arguably better reflects the data included in a HPSA application.

Chapter 2

HPSAs and Ambulatory Care Sensitive Medicare Hospitalizations

2.1 Introduction

In this chapter, I employ the identification strategy described in Chapter 1 to estimate the effect of HPSA designation on the rate of ambulatory care sensitive hospitalizations (ACSH) among Medicare fee-for-service beneficiaries.

The results suggest that HPSA designation reduces hospitalizations of Medicare beneficiaries for acute ACS conditions including dehydration, urinary tract infection, and bacterial pneumonia. I find that designation reduces acute ACSH by between 44% and 62% of the mean rate for beneficiaries aged 65 or older. Point estimates are also consistent with an effect of designation on rates of hospitalization for chronic ACS conditions such as hypertension and diabetes, but those findings are not statistically significant. I perform a supplementary RD analysis of ACSH in Texas and Georgia using data on all residents irrespective of age or insurer. The results of the supplementary analysis corroborate those of the RD analysis with Medicare data: point estimates are consistent with a reduction in ACSH resulting from HPSA designation, and this pattern is not restricted to the elderly.

The estimated reduction in the acute ACSH rate resulting from HPSA designation is greater in magnitude than might be expected given the modest size of the HPSA program relative to the overall health care system. In an effort to corroborate the finding, I test for effects of HPSA designation on a variety of measures of health care provider availability, Medicare reimbursements, primary care utilization, and process of care. The rationale is that, if HPSA designation indeed reduces unnecessary hospitalizations, designation must be expanding access to primary care, increasing utilization of primary care, and/or improving the quality of

care delivered to Medicare beneficiaries. However, I do not find evidence that HPSA designation affects any of the measures I examine. While this analysis does not exhaust all possible mechanisms by which HPSA designation might affect ACSH rates, it does suggest that finding of a large effect of HPSA designation should be interpreted cautiously.

2.2 Data

The data used in this analysis are described in Section 1.3 excluding the outcome measures, which are discussed below.

2.2.1 Medicare Hospitalization Data

I construct the outcome variable, county-level ambulatory care sensitive hospitalization (ACSH) rates using the Medicare Inpatient Standard Analytic File (a.k.a. Medicare Inpatient Limited Data Set) for each year from 2003 through 2012. These files contain a record for every hospital inpatient admission (hereafter "admission" or "hospitalization") of a Medicare fee-for-service beneficiary in the U.S. during those years. Among other variables, each record contains an anonymized patient identifier, the year and quarter of admission, the patient's sex, race, and age (in broad categories), along with ICD-9-CM codes for diagnoses and procedures applied to the patient during the hospital stay.[1] The dataset contains between 10.5 and 13.8 million records per year (126.7 million total).

Patient sex, age, race, and county of residence are missing for up to 86 percent of observations in the 2008 file. To recover these missing data where possible, I use the unique patient identifier to identify all hospitalization records between 2003 and 2012 for the patients with missing data in the 2008 file. Many of these patients have multiple admissions, so missing 2008 records are filled in using data from other admission records corresponding to the same patient. Race and sex are assigned if they are reported consistently in all other admissions for a patient in the ten years of the dataset. Age category and county of residence are potentially time variant. They are assigned to missing 2008 records if they are reported consistently in all other 2008 records corresponding to the patient, or if the same value is reported in the previous admission and the subsequent admission (potentially over multiple years).

Following this procedure, I reduced the percentage of 2008 records with missing county of residence from 86% to 18%, and the reduction in missing race data was similar. Missing values of age and sex were reduced from 11% to between 3 and 6 percent of 2008 records. The remaining missing values correspond to patients whose 2008 admission with missing data was their only admission in the 2003-2012 period, or patients whose

[1] ICD-9-CM stands for International Classification of Diseases, Ninth Revision, Clinical Modification. ICD-9-CM codes identify specific diagnoses and procedures.

multiple admission records do not allow the missing 2008 data to be inferred.

2.2.2 Ambulatory Care Sensitive Hospitalization Rates

The outcome variable I use is the annual rate of ambulatory care sensitive hospitalizations (ACSH) at the county level, constructed from the Medicare data using the Prevention Quality Indicators (PQI) module developed by the Agency for Healthcare Research and Quality (AHRQ). While based on hospital inpatient data, ACSH are in fact an indirect measure of ambulatory (i.e. outpatient) care quality. A condition like diabetes or bacterial pneumonia can be considered ACS if it should not result in a hospital admission when the patient receives timely, high-quality outpatient care. If an area has a high ACSH rate, it suggests that the population is not receiving high-quality primary care. This could result from low primary care utilization (perhaps because people cannot afford needed treatment, they simply dislike receiving medical care, or because they are geographically, culturally, or linguistically isolated from local health care providers). It could also result from adequate utilization but substandard care, or from unusually high patient noncompliance with the directives of providers. So, while a high ACS hospitalization rate indicates that presence of one or more of these problems, it alone does not distinguish which of these factors contribute to the deficiency.

Ambulatory care sensitive hospitalizations have become a widely used measure of health care quality outside the hospital setting. The Institute of Medicine identified the ACSH rate as one of three measures to monitor health care effectiveness (Institute of Medicine (IOM), 2009). In the economics literature specifically, Currie and Tekin (2015), Kolstad and Kowalski (2012), and Dafny and Gruber (2005) use ACSH rates in their studies of the health effects of home foreclosure, health care reform in Massachusetts, and Medicaid expansions for children, respectively.

There have been multiple attempts to identify which hospitalizations are sensitive to ambulatory care, starting with Weissman et al. (1992) and Billings et al. (1993). In close consultation with physicians, epidemiologists, and health services researchers, AHRQ developed the Prevention Quality Indicators module, a list of 14 ACS conditions. Twelve of the ACS conditions are included in three composite measures: an overall composite and composites of acute and chronic conditions. The PQIs were developed and are regularly evaluated on the basis of their sensitivity to primary care (Agency for Healthcare Research and Quality Quality Indicators (AHRQ QI), 2006; Davies et al., 2009). I use software developed by AHRQ to calculate PQIs from hospital discharge records.[2] A full list of the PQIs appears in Appendix B.

The PQIs are calculated as the number of admissions meeting the PQI inclusion criteria (based on

[2]I use version 5.0 of the PQI SAS software, modified as described below to adapt the code to the population of Medicare beneficiaries.

diagnosis and procedure codes in the hospital discharge record) per 100,000 members of the population. I define the population as the number of people enrolled in fee-for-service Medicare in each county in each year, as reported by the Centers for Medicare and Medicaid Services (CMS).[3]. The PQI software enables the user to calculate rates specific to age groups, races, and sexes within each county. Unfortunately, the enrollment data reported by CMS are not broken down by age, race, or sex. I estimate demographic-specific ACS hospitalization rates for beneficiaries over 65 using the distribution of the total county 65+ population by age, race, and sex. This approximation is appropriate as long as the demographics of fee-for-service Medicare mirror those of other elderly people within a county. For beneficiaries over 65, I compute ACS hospitalization rates by sex and age group, as well as for black and non-black populations.[4] I do not calculate demographic-specific ACS rates for Medicare beneficiaries under age 65 because the within-county demographics of that population are unobserved and cannot reasonably be inferred.

I also estimate the effect of HPSA designation on the ACSH rate using all-payer data from Texas and Georgia (see Section 2.3.3). County-level ACSH rates for Georgia are reported online by Georgia Department of Public Health Office of Health Indicators for Planning (2015) through their Online Analytical Statistical Information System (OASIS). I use ACSH rates from 1999 through 2011. ACSH are not defined using the PQI module, but rather using the definition developed by Billings et al. (1993), which overlaps considerably with the PQI definition.[5] In addition to the data from Georgia, I calculate the ACSH rate in Texas counties using the Hospital Discharge Data Public Use Data File provided online for 1999 through 2008 by the Texas Department of State Health Services (2015). For consistency with Georgia, I calculate the ACSH rate for each county using the Billings et al. definitions.

2.2.3 Additional Variables

In Section 2.4, I investigate the effects of HPSA designation on variables related to possible mechanisms through which HPSA designation might reduce ACSH rates. These variables include the number of nurse practitioners and physician assistants in the county, which are taken from the Area Health Resource File (the source of the primary care physician data) and derived from Medicare National Provider Identifier (NPI) database and figures collected by the professional associations representing nurse practitioners and physician

[3] `https://www.cms.gov/Medicare/Health-Plans/MedicareAdvtgSpecRateStats/FFS-Data.html`

[4] The Medicare data are not conducive to more specific racial or ethnic categorization because of deficiencies with the indicator of patient race included in Medicare datasets. Eicheldinger and Bonito (2008) find that, among Medicare beneficiaries who identify themselves as Hispanic, only 30% are identified as such in the Medicare databases, and Asians and American Indians are identified correctly 54% and 36% of the time, respectively. However, the analogous figure for African Americans is 97%, so I follow others in the literature in restricting measurement of race-specific outcomes to broad "Black" and "Non-Black" categories (Lauderdale and Goldberg, 1996; Arday et al., 2000).

[5] The Billings ACS rate definition used by Georgia is provided at `https://oasis.state.ga.us/oasis/oasis/help/death.html`.

assistants. I also use annual county-level data on total Medicare Part A and Part B reimbursements per enrollee, reported by by CMS.[6]

I further investigate possible mechanisms using two datasets provided by the Dartmouth Atlas of Health Care. The first of these contains Medicare claims by type of provider, adjusted for age, race, sex, and price for 2003 through 2012 (Skinner et al., 2011). The data are based on a 20% sample of beneficiaries for 2003–2009 and on all fee-for-service beneficiaries for 2010–2012. The second database contains county-level indicators of primary care utilization and process of care derived from a 20% sample of Medicare claims. The process of care measures include, for example, the fraction of diabetic patients who receive an eye exam. These measures were developed by Goodman et al. (2010) and are available for 2003 through 2012.

2.2.4 Summary Statistics

Table 2.1 reports summary statistics for several ACSH rates expressed as the number of ambulatory care sensitive hospitalizations of the specified type or for the specified population per 100,000 members of the population. These reveal that admissions for chronic ACS conditions are more common than for acute conditions. In the sample, 25% of observations have a whole-county HPSA designation and 24% are eligible for designation in the current year based on the physician ratio. I also report statistics on the fraction of each county's population that lives in any HPSA, a sub-county geographic HPSA, and a population group HPSA. Since whole-county HPSAs cover the entire population of the county, these statistics imply that, among counties without whole-county designation, the average fraction of residents living in a sub-county or population group HPSA is 0.2. This indicates the extent of HPSA exposure in non-whole-county HPSAs, which pose an empirical challenge. Two ratios of primary care physicians to population are reported: the first using the number of physicians, and the second using the adjustment for FTE. On average, the FTE adjustment reduces the estimated number of physicians in each county by more than one per 10,000 inhabitants. The other variables shown in the table are used as controls in the fixed effects and event study specifications described in Section 1.4.1 and include unemployment, median household income, poverty, and government transfers of various types.

2.3 Results

2.3.1 Regression Discontinuity Results

Identification by fuzzy regression discontinuity requires that the first-stage discontinuity exist, i.e., that the probability of HPSA designation changes discontinuously at the HPSA eligibility cutoff. The evidence

[6]The data source is the same file from which the Medicare enrollment figures are taken.

Table 2.1: Summary Statistics: 2003–2012

	Mean	Std. Dev.	Min.	Max.	Obs.
Overall Composite: All Aged 65+	7639.5	3304.1	0	66666.7	31280
Overall Composite: Aged <65	6217.6	3541.9	0	100000	31285
Acute Composite: All Aged 65+	3540.4	1656.1	0	50000	31280
Acute Composite: All Aged <65	2161.1	1999.0	0	100000	31285
Chronic Composite: All Aged 65+	4102.5	1990.7	0	60000.0	31280
Chronic Composite: All Aged <65	4076.0	2775.9	0	100000	31285
Whole-County HPSA Designated	0.25	0.43	0	1	31294
Fraction of Pop with any HPSA	0.40	0.41	0	1	31294
Fraction of Pop with non-County HPSA	0.059	0.19	0	1	31294
Fraction of Pop with Population HPSA	0.092	0.16	0	1	31294
Primary Care Physicians (Num) per 10k Pop	6.47	4.68	0	94.2	28158
Primary Care Physicians (FTE) per 10k Pop	5.31	3.97	0	79.5	30472
Poverty Rate (%)	15.0	5.88	2.39	57.8	31289
HPSA Eligible based on Physician Ratio	0.24	0.43	0	1	31294
High-Need	0.21	0.41	0	1	31274
Unemployment Rate (%)	6.70	2.89	0.80	29.9	31273
Median Household Income ($)	41614.4	10961.1	16868	121250	31289
Total County Population	96597.3	308997.6	55	9951690	31294
Total County Population Aged 65+	12367.9	35458.5	5	1144301	31294
Total Govt Transfers ($ per capita)	6876.2	6899.6	1130.3	343996.6	31279
Retirement and Disability Insurance	2609.3	2778.0	285.1	139745.4	31279
Medicare Benefits	1711.2	1686.3	152.0	79545.1	31279
Public Assistance Medical Benefits	1233.9	1022.4	0	40363.4	31251
SSI Benefits	147.8	160.8	0	5167.0	30916
Earned Income Tax Credit Payments	161.2	158.5	0	7703.6	31215
Food Stamp (SNAP) Benefits	162.5	148.8	0	5602.0	30858
Other Income Benefits e.g. TANF, WIC	231.9	279.8	18.4	12761.7	31245
Unemployment Insurance Payments	201.6	213.2	12.1	11078.7	31090

Table 2.2: First Stage: Estimated Discontinuities in Probability of Whole-County Designation, by Bandwidth, Kernel, and High-Need Status

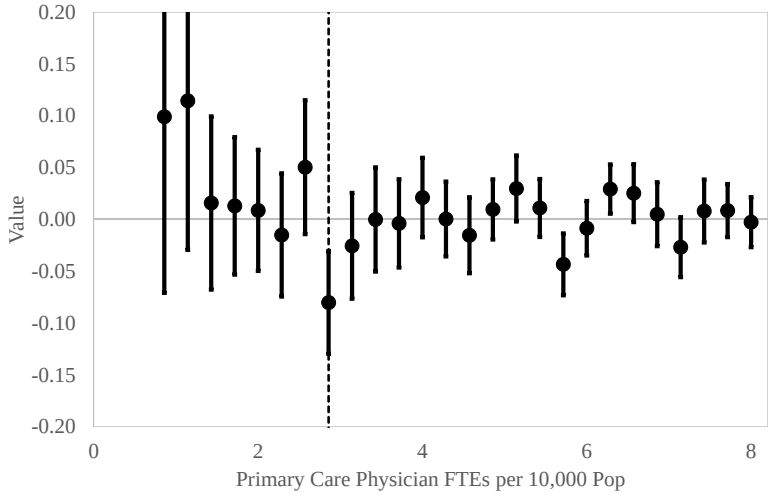
	(1) Triangular Kernel Non-High-Need	(2) Rectangular Kernel Non-High-Need	(3) Triangular Kernel High-Need
IK Bandwidth	-0.080***	-0.073***	0.013
	(0.025)	(0.026)	(0.042)
0.5x IK Bandwidth	-0.098***	-0.094***	0.027
	(0.035)	(0.036)	(0.059)
2x IK Bandwidth	-0.105***	-0.109***	-0.032
	(0.020)	(0.019)	(0.031)
4x IK Bandwidth	-0.165***	-0.177***	-0.071***
	(0.018)	(0.017)	(0.027)
IK Bandwidth	1.699	1.335	1.538

Notes: IK bandwidth is Imbens-Kalyanaraman bandwidth. Reported figures are estimates of discontinuous decrease in probability of designation at the point at which eligibility is lost. Standard errors in parentheses.
* $p < .10$; ** $p < .05$; *** $p < .01$

for such a discontinuity appears among non-high-need counties, but the discontinuity is not very large. Table 2.2 reports estimates of this discontinuity. Columns (1) and (2) report estimates generated using the triangular and rectangular kernels, respectively. Results in different rows are estimated using different bandwidths (expressed as multiples of the Imbens-Kalyanaraman bandwidth). Estimates in these columns range between $-.07$ to $-.18$ and are statistically significant at the 1% level. This suggests that the finding of a discontinuity is robust to both bandwidth and kernel. Counties are ineligible for HPSA designation if their physician ratio exceeds the cutoff, so the negative point estimates should be interpreted as the estimated decrease in probability of designation at the point at which eligibility is lost. Column (3) reports first-stage discontinuity estimates for the sample of high-need counties at the high-need eligibility threshold (3.33, as opposed to 2.86 for the non-high-need counties). Except at a very wide bandwidth, there is no evidence of a first-stage discontinuity for high-need counties. For this reason, I continue the RD analysis (both here and in Chapter 3) using only non-high-need counties.

While the first-stage discontinuity at the eligibility cutoff is robust to a variety of specifications, there is little evidence of any discontinuity in treatment probability at other levels of the physician ratio. Figure 2.1 depicts point estimates of discontinuities at various equally spaced "placebo" eligibility cutoffs. The vertical bands depict the 95% confidence interval for each point estimate. The dashed vertical line indicates the actual eligibility cutoff value of 2.86 FTE primary care physicians per 10,000 inhabitants. The chart shows that there is a discontinuity at the actual cutoff value and only one of the placebo values: 5.71 per 10,000

Figure 2.1: Point Estimates and Confidence Intervals of Estimated Discontinuity in Probability of Whole-County Designation at "Placebo" Cutoffs



Dashed vertical line indicates true HPSA eligibility cutoff for non-high-need counties. Dots indicate point estimates and bars indicate 95% confidence intervals.

inhabitants, or twice the HPSA eligibility cutoff value. The fact that designation probability is in general not discontinuous at other values of the physician ratio suggests that variation in HPSA designation is in fact being generated by the eligibility cutoff.

The magnitude of the first-stage discontinuity might seem surprisingly small. After all, how can counties to the right of the cutoff be designated at all? A HPSA designation lasts three years, so a county can be eligible one year, receive a designation, and then lose eligibility (by gaining physicians per capita) prior to losing designation. This is particularly likely between 2002 and 2011, when HRSA did not actually withdraw designations of counties that failed to demonstrate continuing eligibility. The bottom panels (c and d) of Figure 2.2 depict the first-stage estimation graphically (though at bandwidths other than those presented in Table 2.2). The scatter plot shows the share of counties with HPSA designation in each of the 86 fixed-width bins. The scatter plot and the overlaid local linear regression estimates show the discontinuity, but it appears to be fairly small relative to an overall downward trend surrounding it. The empirical challenge posed by a small first-stage discontinuity is that the first-stage estimate appears in the denominator of the Wald statistic, so a first-stage estimate close to zero can result in an imprecise Wald estimate.

Table 2.3 presents the fuzzy RD results by age, by bandwidth, and by category of ACS condition resulting in hospitalization. Results in column (1) are for Medicare fee-for-service beneficiaries under 65 (generally those on Medicare disability) and results in column (2) are for beneficiaries over 65. Columns (3) and (4)

present results separately for beneficiaries over 65 divided further by age. While the ACSH rates used in columns (1) and (2) are calculated using administrative data on fee-for-service enrollments by county and by year, the ACS rates used in columns (3) and (4) are calculated using population estimates, so greater caution is warranted in interpreting those results.[7] Columns (5) through (8) repeat the first four at double the bandwidth.[8] In the top panel of the table, the outcome variable is the total rate of hospitalization for all ACS conditions combined, as identified by the AHRQ PQI module. In the middle panel, the outcome is the rate of hospitalization for acute conditions (bacterial pneumonia, dehydration, and urinary tract infection). In the bottom panel, the outcome is the rate of hospitalizations for all chronic conditions (including complications for diabetes, heart failure, hypertension, COPD, etc.).

A negative sign indicates a lower ACSH rate. To interpret the estimates, consider the results for acute ACS conditions among Medicare beneficiaries under age 65 (column [1], middle panel). The reduced-form estimate suggests that, as the number of primary care FTE physicians increases through the threshold of 2.86 per 10,000 inhabitants, the estimated discontinuity in ACSH is 110 per 100,000 young Medicare FFS beneficiaries, and this is statistically significant at the 10% level. The first-stage estimate indicates that as the physician ratio increases through 2.86 (so the county loses eligibility), the probability of HPSA designation decreases by 8.4 percentage points,[9] and this is statistically significant at the 1% level. The Wald estimate indicates that the local average treatment effect of HPSA designation among counties whose designation status is determined by the cutoff is -1,311 ACSH per 100,000.[10] The Wald estimate represents 58% of the mean ACSH rate (but this is not statistically significant). A reduction in ACSH is consistent with an improvement in primary care quality.

All of the Wald estimates reported in Table 2.3 are negative. Results for chronic conditions are not statistically significant, but for acute conditions the reduced-form estimates are statistically significant for all specifications, and the Wald estimates are significant in the majority of specifications. In particular, the results suggest a significant discontinuity in the acute ACSH for beneficiaries over 65 at the HPSA eligibility cutoff. In Appendix D, I report the RD results for Medicare beneficiaries separated by sex and by race, and the pattern is the same – all point estimates are negative but they are only marginally statistically significant for acute conditions among women. While results are statistically significant for non-blacks, the estimates

[7] Recall that Medicare county-level enrollment figures are not broken down by age, race, or sex. So, age-specific results in Table 2.3 as well as the sex- and race-specific results in Table D.1 are calculated using population estimates.

[8] The IK bandwidth minimizes mean squared error in the so-called "sharp" (in contrast to fuzzy) RD design, in which the first-stage relationship is deterministic and does not have to be estimated. Since estimating the first stage introduces sampling variation, the most appropriate bandwidth in a fuzzy RD setting might be wider than the IK.

[9] This estimate differs from Table 2.2 because of the difference in bandwidth.

[10] Technically, this is a weighted LATE because the triangular kernel weights different observations differentially.

for black populations are too imprecise to evaluate the effects of HPSA designation on racial disparities in ACSH rates.

The estimated effects of HPSA designation are quite large relative to the mean ACS admission rates, particularly for acute conditions. At the IK bandwidth, the Wald estimate for beneficiaries over age 65 equals 62% of the mean acute ACSH rate. At twice the IK bandwidth, the corresponding Wald estimate equals 44% of the mean. Both of these are very large in light of the relatively small size of the HPSA-tied programs and the fact that many undesignated counties are in fact "partially treated" because they contain sub-county HPSAs. Still, the results, which use a plausible identification strategy to obtain causal estimates, suggest grounds for future research to explain the apparently large effects of HPSAs in reducing hospitalizations for the Medicare population.

Table 2.3: Regression Discontinuity Results by Beneficiary Age, ACS Definition, and Bandwidth

	(1) Aged <65	(2) Aged 65+	(3) Aged 65-74	(4) Aged 75+	(5) Aged <65	(6) Aged 65+	(7) Aged 65-74	(8) Aged 75+
Reduced Form	124.011	257.172*	144.974	456.188**	146.402	248.225**	153.467*	396.568**
	(142.031)	(144.093)	(101.721)	(231.046)	(117.912)	(117.374)	(85.319)	(177.028)
First Stage	-0.086***	-0.084***	-0.087***	-0.080***	-0.124***	-0.116***	-0.126***	-0.102***
	(0.023)	(0.023)	(0.022)	(0.026)	(0.019)	(0.019)	(0.019)	(0.020)
Wald Estimate	-1445.365	-3056.199	-1675.984	-5735.523	-1182.866	-2130.811*	-1214.706*	-3903.736**
	(1726.430)	(1995.332)	(1284.138)	(3667.463)	(980.360)	(1098.653)	(710.280)	(1988.288)
Mean of Outcome	6314.2	7865.7	4692.6	11729.9	6154.1	7594.0	4496.9	11314.6
Bandwidth	2.197	2.015	2.262	1.592	4.394	4.031	4.524	3.184
Acute ACS Conditions								
Reduced Form	110.260*	181.718**	94.018**	302.111**	108.282**	158.240**	73.406**	262.291***
	(65.273)	(81.771)	(39.198)	(132.987)	(53.489)	(62.772)	(34.695)	(100.751)
First Stage	-0.084***	-0.080***	-0.102***	-0.079***	-0.116***	-0.101***	-0.160***	-0.097***
	(0.023)	(0.026)	(0.020)	(0.027)	(0.019)	(0.020)	(0.018)	(0.020)
Wald Estimate	-1310.976	-2283.357*	-920.132**	-3813.383*	-931.293*	-1562.832**	-459.279**	-2703.137**
	(869.774)	(1334.862)	(432.842)	(2256.295)	(488.750)	(719.587)	(225.799)	(1241.661)
Mean of Outcome	2257.4	3709.1	1825.9	5886.6	2195.8	3587.7	1748.6	5673.0
Bandwidth	2.010	1.581	3.221	1.460	4.019	3.163	6.442	2.921
Chronic ACS Conditions								
Reduced Form	12.346	91.065	43.745	131.304	43.931	30.312	70.672	133.829
	(106.594)	(65.230)	(74.038)	(110.363)	(87.891)	(60.692)	(59.080)	(90.229)
First Stage	-0.085***	-0.135***	-0.082***	-0.085***	-0.121***	-0.197***	-0.110***	-0.121***
	(0.023)	(0.018)	(0.024)	(0.023)	(0.019)	(0.017)	(0.019)	(0.019)
Wald Estimate	-145.358	-676.481	-531.783	-1543.232	-364.150	-154.160	-645.338	-1104.379
	(1258.780)	(501.116)	(926.163)	(1405.984)	(734.656)	(310.054)	(559.083)	(780.840)
Mean of Outcome	4069.1	4001.0	2864.6	5797.8	3975.2	3889.9	2749.3	5578.0
Bandwidth	2.119	4.952	1.825	2.132	4.237	9.905	3.651	4.264
Relative to IK	IK × 1	IK × 1	IK × 1	IK × 1	IK × 2	IK × 2	IK × 2	IK × 2

Notes: All estimates generated by local linear regression with triangular kernel. IK is the Imbens-Kalyanaraman bandwidth. In columns (1) and (2), ACSH rates calculated using Medicare administrative data on the number of fee-for-service enrollees. In columns (3) and (4), rates calculated using author estimates of Medicare fee-for-service enrollment by age. Columns (5)-(8) repeat columns (1)-(4), respectively, at twice the IK bandwidth. * $p < .10$; ** $p < .05$; *** $p < .01$

Figure 2.2 depicts the RD estimation for acute conditions among Medicare beneficiaries older than 65. The reduced-form relationships between ACSH rates and physicians are plotted in the top row, and the first-stage relationships are plotted in the bottom row. The left column reports results at the IK bandwidth, while the right column reports results at twice the IK bandwidth (columns 2 and 6 of Table 2.3, respectively). The scatter plot depicts the average value of the outcome within each fixed-width bin, and the size of each marker depicts the number of observations in the bin.[11] The lagged physician ratio has a maximum value of 50 in the sample, but the graphs are truncated at a value of 10 because that includes 90% of observations and because the focus of the analysis is on a discontinuity at 2.86. The figure shows that the local linear regression procedure appears to fit the data well and does not reveal any obvious bias resulting from misspecification.

2.3.2 Robustness Checks

The reader will recall that the key identifying assumptions needed to estimate a causal effect by fuzzy regression discontinuity are the existence of the first stage, for which evidence of a relatively small but significant jump is provided above, and that the underlying relationship between the outcome (ACSH rates) and the assignment variable (lagged physicians) is continuous conditional on HPSA designation status through the eligibility cutoff. While the second assumption cannot be directly tested, as it involves counterfactuals, there are standard tools in the literature for evaluating its reasonableness (Lee and Lemieux, 2010).

One approach is to test for discontinuities in baseline county characteristics at the HPSA eligibility cutoff. Evidence of discontinuities in these variables would cast doubt on the assumed continuity of the underlying relationship between physicians and ACSH. Table 2.4 reports the (sharp) discontinuity estimates at the HPSA eligibility cutoff for 19 baseline county-level variables. The baseline variables are lagged, so that they are measured contemporaneously with the physician ratio. The only statistically significant findings are for per-capita Supplemental Security Income (SSI) transfers, public assistance medical benefits (principally Medicaid reimbursements), and medically underserved population (MUP) designations. The pattern suggests that (ineligible) counties just to the right of the cutoff may have more vulnerable populations than (eligible) counties to the left of the cutoff, which could potentially account for the discontinuity in acute ACSH.

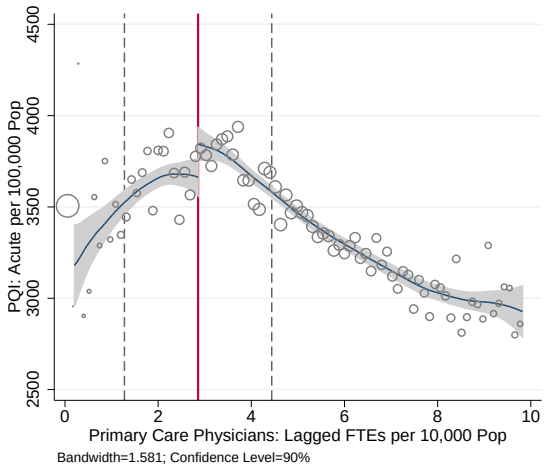
If the discontinuities in baseline characteristics result from counties precisely manipulating their physician ratios in response to the HPSA eligibility rule, that would invalidate the RD design. Since there is no reason for a county to manipulate its physician ratio to maintain *ineligibility*,[12] the manipulation must be performed

[11] The largest marker in each diagram corresponds to observations with zero primary care physicians which are excluded because they bias the discontinuity estimates when included.

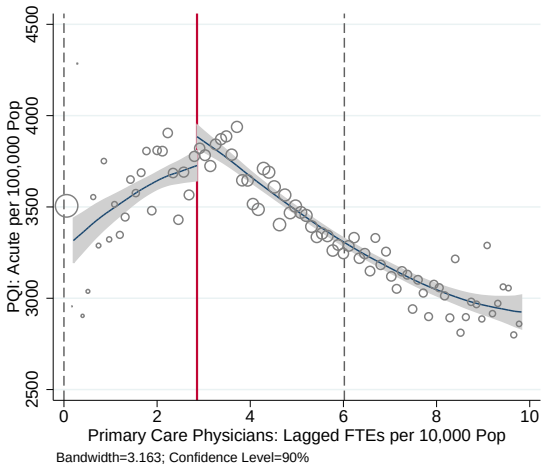
[12] Even supposing that a county did not want the benefits attached to HPSA designation, it could simply decline to apply. Nothing is gained from maintaining ineligibility.

Figure 2.2: RD Results: Acute Conditions for Beneficiaries Aged 65 and Older, by Bandwidth

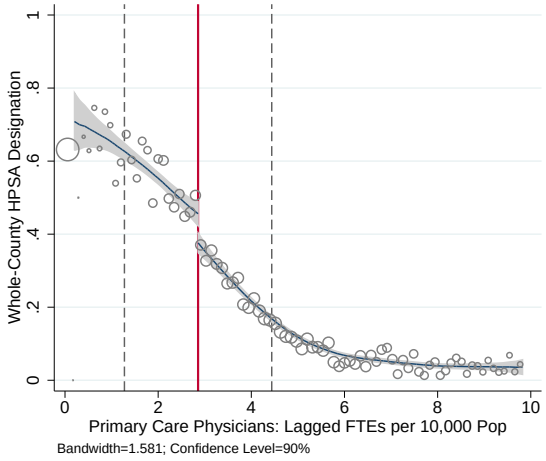
(a) Reduced Form (Outcome: ACSH; IK Bandwidth)



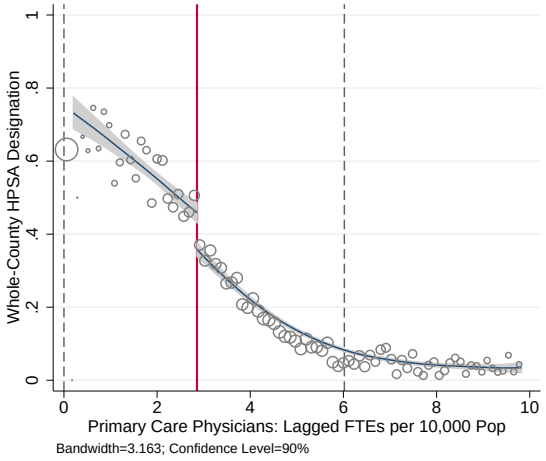
(b) Reduced Form (Outcome: ACSH; 2×IK Bandwidth)



(c) First Stage (Outcome: HPSA Desig.; IK Bandwidth)



(d) First Stage (Outcome: HPSA Desig.; 2×IK Bandwidth)



Solid vertical line indicates eligibility cutoff. Dashed vertical lines indicate bandwidth. Solid gray area is 90% confidence interval.

by the less-needy counties. This would be reflected in bunching to the left of the cutoff, and the results of a McCrary test discussed below find no evidence of such a pattern.

Furthermore, the discontinuity in MUP designations could easily be a result of the HPSA eligibility cutoff in the sense that counties that are ineligible for HPSA designation may be more likely to apply for MUP designation. Counties with whole-county HPSA designation cannot additionally receive population group or sub-county HPSA designations, so sub-county HPSA designation is by construction negatively correlated with whole-county HSPA designation. Since the HPSA and MUA/MUP programs are both administered by HRSA, it is plausible that counties that apply for population group HPSA designation are more likely to apply for MUP designation. If so, the discontinuity in baseline MUP would not reflect manipulation of the physician ratio, but rather contamination of the treatment to counties without HPSA designation – the same challenge posed by sub-county HPSA designations. Fortunately in this case, the principal benefit of MUP designation is eligibility to receive a federally qualified health center, and there is no evidence of a discontinuity in the number of FQHC grantees at the cutoff.

The discontinuities in baseline SSI payments and public assistance medical benefits are more difficult to explain, beyond noting the lack of apparent bunching to the left of the cutoff. It is possible that these discontinuities simply reflect random (unrelated to HPSA designation) variation in average values of the baseline variables on either side of the cutoff. To check this, I re-estimate the fuzzy RD model including these three variables (including MUP designation) as covariates, and the result is qualitatively similar but, at the IK bandwidth, the Wald estimate loses statistical significance.[13] There is a essentially no change in the estimates at the wide (IK $\times 2$) bandwidth. So, the acute ACSH result does seem to be sensitive to controls for these baseline variables.

I also perform a McCrary test for discontinuity in the density of the FTE physician measure at the eligibility cutoff (McCrary, 2008). A discontinuity in the density at the cutoff would suggest that counties might precisely manipulate their physician-to-population ratio to secure HPSA designation, which would violate a key identifying assumption of RD analysis. The McCrary test entails estimating the density of the running variable using an under-smoothed histogram, and then performing local linear regression on either side of the eligibility cutoff to test for a discontinuity in the density.

The results of the McCrary test are shown in Table 2.5. Column (1) contains the results for the entire sample and column (2) contains results for a truncated sample with the top 1% of observations by FTE physicians per capita excluded. The result in column (1) is a statistically significant finding of bunching to

[13]Including the covariates causes the Wald estimate to shrink in magnitude from -2,283 ($p = .087$) to -1,657 ($p = .197$).

Table 2.4: Estimated Discontinuities in Baseline County Characteristics

Variable	Estimate	Std. Err.	p-value
Poverty Rate (%)	0.15	0.168	0.374
Unemployment Rate (%)	0.179	0.11	0.105
Median Household Income ($)	-6.589	368.164	0.986
Total County Population	2946.511	2161.349	0.173
Total County Population Aged 65+	347.869	245.976	0.157
Percent of County Pop Aged 65+	-0.049	0.203	0.811
Total Govt Transfers ($ per capita)	47.16	109.731	0.667
Retirement and Disability Insurance	-14.689	43.461	0.735
Medicare Benefits	12.645	35.208	0.719
Public Assistance Medical Benefits	42.408	24.34	0.081*
SSI Benefits	7.125	3.322	0.032**
Earned Income Tax Credit Payments	-0.53	2.481	0.831
Food Stamp (SNAP) Benefits	4.892	3.832	0.202
Other Income Benefits e.g. TANF, WIC	4.162	4.383	0.342
Unemployment Insurance Payments	6.142	6.471	0.343
County Designated MUA	0.033	0.027	0.224
County Designated MUP	0.016	0.007	0.02**
Sub-County MUA/P	-0.01	0.023	0.67
FQHC Grantees Count	0.057	0.048	0.234

Notes: Estimated discontinuities are at the HPSA eligibility threshold for low-need counties. They are estimated using the IK bandwidth and a triangular kernel.
* $p < .10$; ** $p < .05$; *** $p < .01$

the right of the eligibility threshold. This finding is perplexing, as there is no reason for counties manipulate their physician counts to maintain *ineligibility* for valuable benefits. Column (2) reports the results for the bottom 99% of observations by physician ratio in the sample. The 99th percentile value is 15.8 per 10,000 inhabitants, and the maximum value is 61.2. Omitting the high outliers results in a finding of no discontinuity at the cutoff value of 2.86. As the table shows, the bandwidth is reduced by more than half when the sample is truncated. This is what causes the difference in results, and the reason the bandwidth changes so drastically is that the bandwidth selection procedure used by McCrary entails estimating a fourth-order polynomial on either side of the cutoff. High-order polynomial regressions are highly sensitive to extreme values of a variable (see e.g. Gelman and Imbens (2014)). Because of the sensitivity of the bandwidth selection to values far away from the eligibility cutoff, and because a finding of bunching to the right of the cutoff is counterintuitive, I place greater weight on the results in column (2) suggesting no evidence of bunching.

2.3.3 External Validity: Non-Medicare Hospitalizations in Texas and Georgia

All of the analysis presented to this point has used data on hospital care provided to Medicare beneficiaries. While there are clear advantages to focusing on this population, including its large share of total medical care

Table 2.5: Results of McCrary Test for Manipulation of Running Variable

	(1) Unrestricted Sample	(2) Bottom 99% Sample
Discontinuity Estimate	0.0828**	0.0383
Standard Error	(0.0358)	(0.0521)
p-value	0.0205	0.4619
Bin Size	0.0507	0.0429
Bandwidth	3.2764	1.5960

Notes: Discontinuity estimate is log difference in density. Bin size and bandwidth chosen automatically by procedure described in McCrary (2008). "Bottom 99% sample" excludes top 1% of counties by FTE physicians per capita.
* $p < .10$; ** $p < .05$; *** $p < .01$

provided nationwide and its presence in every county, it might be argued that Medicare beneficiaries are not the population that would be expected to benefit most from HPSA designation. Instead, perhaps having an inadequate number of local physicians disproportionately affects younger and more marginalized populations by exacerbating the barriers to health care access that they already face (such as lack of insurance).

To explore this possibility, I use data from Texas (1999–2008) and Georgia (1999–2011) on ACSH rates among the entire population. As I discuss in Section 2.2.2, I assemble county-level ACSH rates for the overall population and for groups defined by age, race, and sex. I estimate the effect of HPSA designation on ACSH rates in Texas and Georgia by fuzzy regression discontinuity, as above. For Texas, I find a first-stage discontinuity only for non-high-need counties (as with the national data), but for Georgia I find a first-stage discontinuity only for high-need counties. The results presented in Table 2.6 show reduced-form, first-stage, and Wald estimates, estimated at the IK bandwidth with a triangular kernel, for high-need counties only in Georgia and non-high-need only in Texas. The estimated first-stage discontinuities in general are larger in magnitude than in national sample but, at least for Texas, the Wald estimates are even larger relative to the mean ACSH rates than those estimated using national Medicare data. For example, the Wald estimate for the entire population for Texas (-2,442) is equal to 98% of the sample mean ACSH rate (2,502) within the IK bandwidth. It is interesting that the Wald estimates for both states are almost universally negative, though only marginally statistically significant among whites, females, and young adults in Texas. This parallels the results found using the Medicare data, despite the fact that these estimates use a different definition of ACS condition, cover slightly different years, and reflect care provided to the entire population irrespective of age or insurer.

Table 2.6: Regression Discontinuity Results for Georgia and Texas

	Georgia			Texas		
	Reduced Form	First Stage	Wald	Reduced Form	First Stage	Wald
Overall	83.659	-0.173*	-484.080	372.520**	-0.153**	-2441.527
	(295.882)	(0.102)	(1643.134)	(164.496)	(0.065)	(1484.199)
Male	36.871	-0.179*	-206.244	260.719**	-0.157**	-1662.511
	(229.446)	(0.100)	(1251.843)	(122.892)	(0.062)	(1030.532)
Female	121.406	-0.155	-785.529	466.939**	-0.153**	-3057.281*
	(386.950)	(0.106)	(2395.771)	(200.568)	(0.065)	(1818.869)
Age <17	-32.946	-0.152	216.762	107.626	-0.157**	-687.036
	(313.616)	(0.107)	(2091.627)	(84.392)	(0.062)	(607.079)
Age 18 - 44	95.036	-0.168	-565.639	122.669**	-0.165***	-741.813*
	(144.574)	(0.103)	(810.798)	(55.505)	(0.057)	(403.193)
Age 45 - 64	101.832	-0.170*	-598.714	351.365**	-0.148**	-2367.863
	(339.803)	(0.102)	(1913.745)	(167.546)	(0.067)	(1527.104)
Age 65 - 74	525.427	-0.145	-3624.012	837.057*	-0.133*	-6285.024
	(807.193)	(0.109)	(5444.056)	(458.783)	(0.073)	(4837.991)
Age 75 +	2031.812	-0.154	-13160.894	2040.907*	-0.130*	-15667.385
	(1591.021)	(0.106)	(11288.666)	(1223.965)	(0.075)	(12936.497)
White	26.828	-0.175*	-153.391	393.570**	-0.162***	-2431.269*
	(318.197)	(0.101)	(1790.825)	(153.495)	(0.059)	(1275.074)
Black	-24.464	-0.163	150.469	-54.046	-0.166**	325.726
	(361.900)	(0.104)	(2258.163)	(497.109)	(0.067)	(3010.439)
Hispanic				-4.457	-0.136*	32.677
				(158.144)	(0.072)	(1156.521)

Notes: Georgia estimates based on sample of high-need counties only. Texas estimates based on non-high-need counties only. All estimates use local linear regression with triangular kernel and bandwidth selected by IK procedure.
* $p < .10$; ** $p < .05$; *** $p < .01$

2.4 Evidence on the Mechanism

The regression discontinuity results presented in Section 2.3.1 suggest a very large reduction in ACSH for acute conditions resulting from HPSA designation. In this section, I investigate the channels through which HPSA designation might reduce the ACSH rate. Specifically, I test for discontinuities in several "intermediate" outcomes that, one might assume, would respond to HPSA designation if designation indeed reduces the ACSH rate. The results of these tests are presented in Tables 2.7 and 2.8.

The first outcomes I consider are measures of the local supply of physician assistants and nurse practitioners. Sometimes referred to as "mid-level providers" or "physician extenders," these professionals are eligible to participate in the National Health Service Corps and they provide a substantial and growing share of the primary care that is delivered to the underserved (Peterson et al., 2013). As discussed in Section 2.2.3, the data taken from the Area Health Resource File. The reduced-form estimates in Table 2.7 do not suggest that the concentrations of either nurse practitioners or physician assistants are discontinuous at the HPSA eligibility cutoff. In fact, the point estimates of the Wald statistic are negative, though statistically insignificant.

The other outcomes shown in Table 2.7 are Medicare reimbursement rates at the county level. These are taken from two sources: total Part A and Part B reimbursements per capita are reported by CMS based on the universe of Medicare fee-for-service beneficiaries, and reimbursements by type of claimant are published by the Dartmouth Atlas based on a 20% sample of Medicare beneficiaries. A discontinuity in hospital/part A reimbursements would be consistent with the finding of reduced acute ACSH rate in HPSAs, but the discontinuity in expenses could be small since acute ACS conditions account for only a fraction of total hospitalizations. In fact, none of the Medicare reimbursement results reveal a clear discontinuity at the HPSA eligibility threshold, with the exception of outpatient facilities.

Because HPSA designation carries a 10% bonus for physician care under Part B, if designation does not elicit a behavioral response by patient or physicians, one would expect that reimbursements to physicians would increase by 10%. This reflects an institutional aspect of the HPSA program and should hold true whether or not HPSA designation reduces the ACSH rate. In fact, the Wald estimate is negative and statistically insignificant, which (together with the other reimbursement results) seems to suggest that HPSA designation may in fact reduce utilization of health care by Medicare beneficiaries.

Table 2.8 presents further regression discontinuity estimates of the effects of HPSA designation on measures of primary care utilization taken from the Dartmouth Atlas 20% Medicare sample. The first outcome

Table 2.7: Regression Discontinuity Estimates for Mid-Level Health Providers and Medicare Reimbursements

	Reduced Form	Wald Est.	Mean	Bandwidth	Obs.
Mid-Level Health Providers					
Physician Assistants	0.474	-3.548	3.951	1.044	12,751
	(0.621)	(4.685)			
PAs per 10k Pop	0.083	-0.691	1.402	1.585	12,751
	(0.129)	(1.091)			
Nurse Practitioners	0.206	-1.526	5.848	1.076	5,988
	(0.760)	(5.589)			
NPs per 10k Pop	0.229	-2.091	2.051	1.467	5,988
	(0.168)	(1.762)			
Medicare Reimbursements (CMS)					
Part A per capita (65+)	0.949	-10.322	297.7	2.087	27,222
	(2.731)	(29.883)			
Part B per capita (65+)	3.363	-40.571	252.8	1.437	27,222
	(3.434)	(42.950)			
Reimbursements by Type					
Total	104.768	-1042.467	7686.1	3.094	18,436
	(70.026)	(725.195)			
Hospital and SNF	70.506*	-767.040	3820.1	2.623	18,436
	(40.219)	(469.798)			
Physician Services	11.236	-133.370	1857.0	1.989	18,436
	(24.016)	(288.158)			
Outpatient Facilities	52.135**	-654.756*	1116.7	1.612	18,436
	(24.934)	(363.994)			
Home Health Agencies	21.139	-266.398	397.4	1.482	18,436
	(18.895)	(256.436)			
Hospice	-17.142	185.136	204.0	2.663	18,436
	(11.091)	(127.690)			
Durable Medical Equipment	-0.311	3.900	270.6	1.581	18,436
	(4.211)	(52.884)			

Notes: Data on allied health providers taken from Area Health Resource File. Total Medicare reimbursements from CMS. Medicare reimbursements by type are estimates from Dartmouth Atlas based on 20% sample of Medicare beneficiaries. All estimates generated by local linear regression with triangular kernel at HPSA eligibility cutoff for non-high-need counties. First-stage results omitted but statistically significant at 5% level in every case. Mean is average of outcome variable within bandwidth.
* $p < .10$; ** $p < .05$; *** $p < .01$

Table 2.8: Regression Discontinuity Estimates for Measures of Primary Care Utilization and Quality among Medicare Beneficiaries

	Reduced Form	Wald Est.	Mean	Bandwidth	Obs.
Pct with Primary Care Visit	0.527	-6.320	80.9	1.688	18,258
	(0.334)	(4.423)			
ACS Hosp. per 1,000 Enrollees	1.749	-19.927	86.8	2.330	18,337
	(1.482)	(18.110)			
Among Women Aged 67-69					
Pct Mammogram in Past 2 Years	-0.823*	12.656	62.2	2.760	12,451
	(0.460)	(9.251)			
Tests Among Diabetics, 65-69					
Hemoglobin A1c	-0.120	2.031	82.7	2.121	13,314
	(0.335)	(5.801)			
Eye Exam	-0.357	5.611	65.4	2.495	13,309
	(0.396)	(6.839)			
Blood Lipids (LDL-C)	-0.243	4.187	77.4	2.021	13,310
	(0.421)	(7.659)			

Notes: Data from Dartmouth Atlas based on 20% sample of Medicare beneficiaries. All estimates generated by local linear regression with triangular kernel at HPSA eligibility cutoff for non-high-need counties. First-stage results omitted but statistically significant at 5% level in every case. Mean is average of outcome variable within bandwidth.
* $p < .10$; ** $p < .05$; *** $p < .01$

is the percentage of sampled beneficiaries who visited a primary care provider in the previous year, and the corresponding Wald estimate is negative and statistically insignificant. The other outcomes include the fraction of women aged 67-69 who received a mammogram in the previous two years, and the fraction of diabetics aged 65-69 who received each of three diagnostic tests. For none of these outcomes is the Wald estimate statistically significant. The Dartmouth Atlas also reports an ACSH rate derived from their 20% sample of Medicare claims. Table 2.8 includes the fuzzy RD results using this ACSH rate as the outcome. Consistent with the other findings in this study, the Wald estimate of the the effect of HPSA designation on the total ACSH rate is negative but statistically insignificant.[14]

The results presented in this section do not clarify by what mechanism HPSA designation may reduce ACSH rates. ACSH are routinely used as a measure of primary care quality on the premise that an included hospitalization reflects a failure to manage a health problem effectively. The premise of the HPSA program is that people in particular areas are unable to access needed primary care because there is an insufficient number of providers in the area. If HPSA designation reduces ACSH, one would expect to observe an

[14]The Dartmouth ACSH measure does not distinguish between acute and chronic conditions, so this result should be compared to the all-conditions ACSH results and not the acute ACSH results.

increase in the number of primary care providers and/or in the fraction of people who utilize primary care, but neither is observed at the HPSA eligibility cutoff using the Dartmouth Atlas sample data and the AHRF physician assistant and nurse practitioner data. If anything, the point estimates in those specification as well as the Medicare reimbursements specifications suggest that HPSA designation might *reduce* utilization of primary care, or of health care in general.[15]

Certainly, the lack of an observed HPSA effect on these potential mechanisms argues for a very cautious interpretation of the results presented in Section 2.3.1. Yet, by no means are the ACSH results necessarily refuted by the mechanism results. There are few sources of available data on primary care in sparsely populated counties spanning several years. The intermediate outcomes considered in this section, while reasonable, are less than ideally suited to evaluate the specific finding that HPSA designation reduces acute ACSH rates. For example, although the Dartmouth Atlas data include process of care measures related to diabetes and prevention of breast cancer, there are no data specific to care for acute ACS conditions (dehydration, bacterial pneumonia, and urinary tract infection). Furthermore, while the results do not provide evidence of increased primary care utilization on the extensive margin (i.e. the number of people receiving primary care) it is possible that the intensity of care provided to the medically neediest is increased, or that waiting times for patients exhibiting early/ambiguous symptoms of acute conditions are reduced.

2.5 Discussion and Conclusion

In this chapter, I examine to what extent the Health Professional Shortage Area program reduces the rate of ambulatory care sensitive hospitalizations among fee-for-service Medicare beneficiaries. The subsidies attached to HPSA designation are designed to increase the supply of primary care physicians and other health care professionals in order to relieve over-burdened and under-resourced public health providers and allow them to treat a greater number of patients and/or improve the quality of care. In recent years, the resources attached to HPSA designation have increased considerably as policymakers have come to view the HPSA program as an attractive tool to increase health care resources available to the geographically underserved.

Still, it is unknown whether HPSA designation is effective in improving health care access or quality. The physicians likely to be attracted by HPSA-attached programs (which are mostly aimed at physicians completing their professional training) perhaps differ in skill set, approach to care, and retention in the

[15] If HPSA designation resulted in a broad disengagement from the health care system among Medicare beneficiaries (resulting in both a lower ACSH rate and lower utilization of all types of care), it should also increase the mortality rate. In chapter 3, I use mortality data from the National Center for Health Statistics to study this question and I find no effect of HPSA designation on mortality rates.

community from the physicians potentially crowded out by their entry. Ultimately, it is unclear how HPSA designation will affect the delivery of primary care as well as whether any realized gains warrant both the budgetary expense and the effort required to administer the HPSA program and its burdensome application process.

Against this backdrop, I conduct only the second study of the effect of HPSA designation on a population health outcome (after Pathman et al. (2005)), and I am the first to use the rate of ambulatory care sensitive hospitalizations (ACSH) as the outcome and to account for the problem of selection resulting from the HPSA application process. I calculate the ACSH rate using data on every hospital admission of a Medicare fee-for-service beneficiary between 2003 and 2012. I first estimate the effect of designation on ACSH using a fixed effects (and paired event study) specification, but the results indicate that the counties which receive HPSA designation experience relative decreases in ACSH rates in the years preceding designation. This evidence of "positive selection" in HPSA designation motivates my regression discontinuity strategy, which isolates exogenous variation in designation generated by a cutoff in the number of primary care physicians per capita specified in the HPSA eligibility criteria.

I find a statistically significant but small discontinuity in the probability of HPSA designation at the eligibility cutoff. The results also indicate that HPSA designation causes a large and statistically significant reduction in ACSH for acute conditions. Point estimates are consistent with a HPSA effect on chronic ACSH as well, but those estimates are statistically insignificant. I repeat the analysis using all-payer data from Texas and Georgia and find that the pattern of results is the same – point estimates suggest that HPSA designation might reduce the ACSH rate, but the results are mostly statistically insignificant.

These results should be interpreted cautiously. The magnitude of the estimated effect of HPSA designation on acute hospitalizations is on the order of fifty percent of the mean ACSH rate, which seems implausibly large given the modest size of the HPSA program relative to the overall health care system. Furthermore, I was unable to find an effect of HPSA designation any of several health care supply and utilization measures. At the same time, I would expect the Wald estimates to be biased toward zero since many counties without whole-county designations are in fact partially treated with sub-county HPSAs.

The ambiguity of the results is attributable in large part to the small first-stage discontinuity. Since I do not observe physician hours, one reason for the small first stage is measurement error in the full-time-equivalent physician ratio. Another reason is that, for designated HPSAs, the eligibility criteria are only binding during the years in which designation must be renewed. During eight of the nine years included in this study, HPSA designations were not withdrawn from counties that lost eligibility except to be replaced

by a different type of HPSA. To the extent that the eligibility criteria were not strictly enforced in these cases, the criteria could not generate exogenous variation in HPSA exposure, which compromised the power of my empirical analysis.

The institutional details of the HPSA program are complex, and a number of features[16] confound efforts to research the effects of HPSA-tied programs. Yet, the growing reliance on these programs by policymakers has made the need for such research more acute. The analysis performed in this chapter is a first step in that line of inquiry.

[16]Examples include the multitude of types of HPSAs, the reliance on applicant-collected data that are elsewhere unavailable, the additional requirements attached to particular HPSA-tied benefits, the inconsistent enforcement of eligibility rules for designated HPSAs, and the lack of data on J-1 waiver physicians.

Chapter 3

HPSAs, Fertility, Mortality, and Prenatal Care

3.1 Introduction and Background

In this chapter I examine how HPSA designation affects a variety of county-level measures of population health and of health care utilization, including fertility, mortality, prenatal care, and birth outcomes such as birth weight and gestation. I employ the same empirical approach used in chapter 2, namely a fuzzy regression discontinuity design estimated around the eligibility cutoff ratio of primary care physicians to county population. Though the data in this chapter cover a slightly different time span, the results again indicate the presence of a small but robust first-stage discontinuity.

The analysis performed in this chapter complements the ACSH analysis in Chapter 2 for a number of reasons. First, since the ACSH rates were derived from Medicare data, the results only directly pertain to the segment of the population with Medicare coverage. It could easily be the case that the benefits of HPSA designation accrue primarily to more disadvantaged or vulnerable populations, including the uninsured. Such people are included in the vital statistics records, so the results in this chapter reflect the effect of HPSA-tied programs on these populations. Another advantage of the vital statistics data (particularly the birth files) is that they contain a large number of Hispanics, who are identified in the data. For examining the possibility of disparate effects of HPSA designation, it is useful to have this coverage of the Hispanic population who cannot be studied in the Medicare claims files. Thirdly, I observe more years of vital statistics data than I have Medicare claims files. While the regression discontinuity specifications in chapter 2 included data from 2003 through 2012 (excluding 2010), most of the specifications in this chapter use data from 1999 through 2012 (again excluding 2010).

Among the outcomes I consider in this chapter is mortality. It is important to understand the relationship between HPSA-tied interventions and mortality not just because mortality is an important indicator of

population health, but because mortality is the outcome most frequently studied in the relevant literature. This includes the community health center study by Bailey and Goodman-Bacon as well as Pathman et al. (2005), the only other paper to study the effects of HPSA designation on a population health measure. Estimating the effect of HPSA designation on mortality therefore allows direct comparisons with these and similar papers. My ultimate finding of no effect of HPSA designation on mortality corroborates Pathman et al., who use a very different empirical approach, and contrasts with Bailey and Goodman-Bacon, who find substantial reductions in mortality resulting from introduction of community health centers.

I also study fertility, birth weight, and infant mortality. Like mortality, they are all widely used as population health outcomes.[1] However, these measures are particularly important in this context because counties are treated differently under the HPSA rules based on their levels. Specifically, fertility and infant mortality are used to identify high-need counties,[2] and all three are used to assign HPSA scores (Health Resources and Services Administration, 2003). The HPSA score determines the area's priority in receiving National Health Service Corps placements. Since the regulation provides for counties with elevated levels of these variables to receive HPSA-tied benefits more easily than other counties, it is useful to examine whether these variables are in fact particularly sensitive to HPSA designation.

The final variables I consider are indicators of prenatal care utilization. These specifications are particularly important because the prenatal care utilization data from birth certificates are the only source of individual level primary care utilization to which I currently have access. The only other primary care utilization measure I observe is the Dartmouth Atlas measure of the fraction of Medicare fee-for-service beneficiaries in each county who had a primary care visit in the previous year, as detailed in section 2.4. The Dartmouth Atlas measure records care on the extensive margin, but it is possible that HPSA-tied programs improve population health on the intensive margin of care. Observing the number of prenatal visits and the month in which care is initiated allows me to test for effects on both margins.

There is a literature in health economics that focuses on the determinants of prenatal care utilization.[3] Particularly relevant to this research is Evans and Lien (2005) who use a transit strike in Pittsburgh to instrument for prenatal care utilization among black women in the city. Their findings suggest that prenatal care is responsive to non-financial impediments to accessing a provider. However, Currie and Grogger (2002) find that prenatal care did not respond to removal of non-financial barriers to access around the time of

[1] See, e.g., Reichman et al. (2009) for low birth weight and Chay and Greenstone (2003) and Miller and Tucker (2011) for infant mortality.

[2] `http://bhpr.hrsa.gov/shortage/hpsas/designationcriteria/primarycarehpsacriteria.html`

[3] This is often a first-stage relationship in a paper whose ultimate objective is to estimate the effect of prenatal care on health outcomes.

the Clinton welfare reform. They did, however, find that prenatal care increased when there were financial incentives, such as expansions of Medicaid eligibility. Sonchak (2015) also finds that prenatal care responds to expansions in Medicaid eligibility. As the HPSA program exists to alleviate non-financial barriers to accessing primary care, it would seem reasonable that HPSA designation might affect prenatal care.

Of the numerous outcomes I study in this chapter, the only one that appears to respond to HPSA designation is fertility rates among young women, which fall in response to designation. To probe this result, I then use state-reported data from nine states to test whether HPSA designation increases abortion rates, and find no evidence that it does.

3.2 Data

I construct the outcome measures used in this analysis using data furnished by the National Vital Statistics System and originally collected from birth and death certificates. In particular, I use the national Mortality file (1999–2010) to calculate county-level adult mortality rates and the national Natality file (2010–2012) to measure prenatal care and birth outcomes during those years. I also use the period linked birth-infant death file (1999–2009), which contains the birth certificate data reflected in the Natality file in addition to linked death certificate data for infants who die in their first year of life. I use this file to construct the prenatal care, birth, and infant mortality measures in those years. For outcomes that are defined as rates in a population (such as fertility or mortality), I use the intercensal county-level population estimates from the Census Bureau as the denominator. Additionally, I collect data from nine states on the number of abortions (induced terminations of pregnancy, or ITOP) by county and year. The states I include are Georgia, Kansas, Michigan, Minnesota, Missouri, North Carolina, Oklahoma, Texas, and Washington.

I calculate the one-year mortality rate at the county level for the entire population as well as for several sub-populations defined by age, race/ethnicity, and sex. In addition to the all-cause mortality rate, I calculate rates for broad groupings of causes of death as reported in the Mortality file. Specifically, I calculate mortality rates for cancers, cardiovascular disease (to include stroke), diabetes, other diseases, and external causes (e.g. motor vehicle accident, homicide, suicide). The results of regressions using these cause-specific mortality rates appear in Appendix E. Mortality rates are calculated as the number of deaths of county residents older than one year per 1,000 members of the county population.[4] The mortality rates are not age-adjusted, which means that differences in observed mortality between populations reflect differences in both the age profile of the populations and in mortality by age. For this reason, I focus on age-specific mortality rates in this

[4]Infants are excluded from this measure of mortality because a county's "high-need" status (and thus its inclusion in the sample) depend in part on infant mortality.

analysis.

The fertility rate is defined as the number of live births to all members of a female population living in a particular county, per 1,000 members of that female population. For a county as a whole, fertility is defined as the number of live births among women aged between 15 and 44 divided by the number of women (in thousands) in that age band in the county. The abortion rate is calculated analogously. The infant mortality rate is calculated as the number of deaths of people less than one year old, per 1,000 live births in the year that the death occurred. I make two further modifications to the mortality, fertility, infant mortality, and abortion estimates. First, I top-code each of these rates at a value of 1,000, which affects a very small number of observations. This is to minimize the impact of outlier observations that likely reflect errors in measurement. Second, I exclude observations with small denominator populations because rates based on small populations are unlikely to be accurate. Specifically, I exclude mortality rates for which the population is fewer than 40, fertility and abortion rates for which the female population is fewer than 20, and infant mortality rates for which the number of live births is fewer than five.

The birth data record two variables concerning prenatal care utilization: the total number of prenatal care visits, and the month in which prenatal care was initiated. Using these data, Kotelchuck (1994) defines the Adequacy of Prenatal Care Utilization (APNCU) Index, which categorizes prenatal care received as either inadequate, intermediate, adequate, or "adequate-plus" based on both the timing of prenatal care initiation and the number of visits during pregnancy. The number of visits is compared to the clinical recommendations of the American College of Obstetricians and Gynecologists (ACOG) recommended schedule of visits. While the data do not allow the researcher to observe whether the mother followed the ACOG schedule, the Kotelchuck method evaluates whether the number of visits is consistent with the ACOG schedule, given the reported gestation and timing of the first visit. I calculate the APNCU score for each birth record and calculate the fraction of births in each county that are classified as adequate-plus, adequate or higher, and inadequate. In addition to these measures, I also calculate the fraction of births for which prenatal care began in the first trimester (79% nationwide over the sample period) and the fraction that received any prenatal care at all (98% nationwide). I also use the reported month of prenatal care initiation as an outcome (recoding the variable so that women who never receive prenatal care are presumed to have received it beginning in the eleventh month).

Table 3.1 reports the means and standard deviations of variables in the estimation sample, separately for counties with and without whole-county designations. The top panel labeled Full Sample reports summary statistics for the nationwide 1999-2012 sample and the bottom panel pertains only to the nine states for

Table 3.1: Summary Statistics by HPSA Designation Status

	Non-HPSA Counties		*Whole-County HPSAs*	
Full Sample	Mean	Std. Dev.	Mean	Std. Dev.
Lagged PC Physician Ratio	6.155	3.417	3.323	2.142
Poverty Rate (%)	12.604	3.844	13.773	3.919
Female Pop Aged 15-44 (000s)	25.472	83.680	4.109	5.027
Fertility Rate: Aged 15-44	65.005	11.315	66.415	12.135
Fertility Rate: Aged 15-24	78.989	26.865	89.832	26.508
Fertility Rate: Aged 25-34	107.229	21.095	104.563	25.942
Fertility Rate: Aged 35-44	18.839	7.403	16.176	7.611
Total Births in County	1678.003	5764.035	277.704	354.492
Gestation in Weeks	38.597	0.325	38.500	0.399
APNCU Adequate-Plus	0.329	0.105	0.324	0.114
APNCU Adequate or Higher	0.748	0.108	0.731	0.119
APNCU Inadequate	0.132	0.087	0.142	0.094
Mortality: Total	8.631	4.230	9.177	4.296
Mortality: Aged 55-64	8.121	4.229	8.907	4.867
Mortality: Aged 65-74	19.157	9.223	20.144	9.911
Mortality: Aged 75-84	47.237	21.200	48.411	22.317
Mortality: Aged 85+	133.931	58.794	138.540	63.179
Abortion Sample				
Fertility Rate: Aged 15-44	67.891	11.487	68.959	12.360
Abortion Rate: Aged 15-44	7.408	4.819	6.916	5.761
Fertility Rate: Aged 15-24	93.171	29.462	100.876	25.966
Abortion Rate: Aged 15-24	14.551	9.018	13.189	10.591

Notes: Full sample and abortion sample are calculated using all county-year observations included in the respective regressions.

which I observe abortion data. The summary statistics reveal that mortality rates are higher and prenatal care utilization is lower in HPSA counties. Young women in whole-county HPSAs also have higher fertility rates than those in non-HPSA counties. The states for which I observe county-level abortion rates have fertility rates that are comparable to the broader sample.

3.3 Empirical Approach

I estimate the effect of HPSA designation on the outcome measures considered in this chapter by specifying a fuzzy regression discontinuity design around the cutoff ratio of primary care physicians to population on which HPSA eligibility is in part based. The empirical approach is described in detail in section 1.4.2. Still, there is one empirical challenge posed by the specifications in this chapter that is not present when studying ambulatory care sensitive hospitalizations. Two of the outcomes examined in this chapter (fertility and infant mortality) are used in the HPSA program (along with the poverty rate) to identify those counties which are "high-need" and therefore subject to a different eligibility threshold number of primary care physicians.

Specifically, a county is high-need if its fertility rate among women aged 15-44 exceeds 100 per thousand women, if the infant mortality rate exceeds 20 per thousand live births, or if the poverty rate exceeds 20% of the population. As discussed in section 2.3.1, I do not find evidence of a first-stage discontinuity in the probability of HPSA designation among the high-need counties and I therefore exclude them from my estimates.

This sample restriction may be problematic when the outcome variable in the regression is one of the determinants of high-need status, as in the case in the fertility and infant mortality specifications. If all high-need observations are excluded, the dependent variable in the regression will essentially be truncated. However, if the high-need observations are included in the regression, they will bias the discontinuity estimates toward zero and reduce the precision of the Wald estimates. However, in fact this problem does not seem to be as pronounced as might have been feared. First, in the majority of specifications I focus on the fertility or infant mortality rate for a specific subpopulation, whereas it is the overall county-level rates that matter for determining high-need status. Second, it turns out that in most cases high-need status is driven by poverty rather than fertility or infant mortality. Specifically, 5% of high-need counties would be non-high-need with a lower fertility rate only, and 18% would be non-high-need with a lower infant mortality rate only. In light of this, high-need counties are excluded in the majority of specifications reported in this chapter. As a check, in Table 3.6 I report alternative specifications in which those few high-need counties that are excluded based only on fertility rates are included in the fertility regression, and the counties excluded based on infant mortality are included in the infant mortality regression.

3.4 Results

Overall, the pattern of results is quite consistent across the different outcome measures studied in this chapter. Almost without exception, the first-stage discontinuity is estimated to be between 7 and 13 percentage points and is statistically significant at the one percent level. Almost as consistently, the results show no evidence of an effect of HPSA designation on the outcome measure being considered. In all of these tables, "Reduced Form" refers to the estimated discontinuity in the average value of the outcome variable at the HPSA eligibility cutoff, "First Stage" is the estimated discontinuity in the probability of designation, and "Wald Estimate" is the RD estimate of the local average treatment effect of HPSA designation on the outcome measure. Each regression is estimated at the Imbens-Kalyanaraman bandwidth, and the reported mean of the outcome variable refers to the mean of values of the outcome variable included in the estimation sample and lying within one bandwidth of the eligibility cutoff.

Table 3.2: Birth Weight Results

	(1) BW (grams)	(2) Log BW	(3) BW <2500g	(4) BW <1500g
Reduced Form	2.112	-0.000	0.001	0.000
	(3.692)	(0.002)	(0.002)	(0.001)
First Stage	-0.092***	-0.093***	-0.094***	-0.111***
	(0.019)	(0.030)	(0.030)	(0.035)
Wald Estimate	-22.881	0.004	-0.009	-0.004
	(39.634)	(0.020)	(0.019)	(0.007)
Mean of Outcome	3229.114	8.078	0.077	0.013
Observations	13,172	5,192	5,107	3,774
Bandwidth	2.118	0.790	0.778	0.567

Notes: Standard errors in parentheses.
* $p < .10$; ** $p < .05$; *** $p < .01$

3.4.1 Fertility and Birth Outcomes

Table 3.2 reports estimates for four measures of birth weight: raw birth weight in grams, the natural log of birth weight, and fractions of births that are "low birth weight" (< 2500g) and "very low birth weight" (< 1500g). There is no evidence of a reduced form discontinuity, as the reduced form estimates are typically smaller in magnitude than their standard errors.

Table 3.3: Fertility, Infant Mortality, and Gestation Results

	(1) Aged 15-24	(2) Aged 25-34	(3) Aged 35-44	(4) White	(5) Black	(6) Hispanic
Fertility						
Reduced Form	2.491**	-0.137	0.001	0.392*	1.779*	1.637
	(0.971)	(0.916)	(0.215)	(0.208)	(0.994)	(1.115)
First Stage	-0.092***	-0.091***	-0.110***	-0.095***	-0.082***	-0.079***
	(0.019)	(0.019)	(0.016)	(0.018)	(0.024)	(0.020)
Wald Estimate	-27.166**	1.505	-0.010	-4.140*	-21.665	-20.661
	(12.394)	(10.115)	(1.960)	(2.336)	(13.702)	(14.854)
Mean of Outcome	87.8785	104.9870	16.7138	33.2960	33.7219	44.8414
Observations	12,839	12,327	17,585	14,115	7,905	11,580
Bandwidth	2.057	1.963	3.148	2.297	1.721	1.907
Infant Mortality						
Reduced Form	0.317	-0.363	0.440	0.157	1.716	0.428
	(0.472)	(0.377)	(0.920)	(0.318)	(1.641)	(1.152)
First Stage	-0.093***	-0.096***	-0.070***	-0.087***	-0.108***	-0.062**
	(0.020)	(0.019)	(0.022)	(0.021)	(0.029)	(0.030)
Wald Estimate	-3.420	3.784	-6.249	-1.804	-15.899	-6.850
	(5.150)	(4.000)	(13.217)	(3.674)	(15.818)	(18.712)
Mean of Outcome	8.0407	5.8951	7.4476	6.4839	13.7959	5.6910
Observations	11,532	12,617	9,511	10,535	5,822	5,474
Bandwidth	2.168	2.395	2.023	1.946	2.375	1.619
Gestation						
Reduced Form	0.012	-0.010	-0.039	-0.012	-0.128**	0.068*
	(0.020)	(0.018)	(0.036)	(0.015)	(0.057)	(0.040)
First Stage	-0.084***	-0.084***	-0.070***	-0.086***	-0.076**	-0.080***
	(0.021)	(0.021)	(0.023)	(0.021)	(0.032)	(0.025)
Wald Estimate	-0.141	0.119	0.557	0.140	1.673	-0.851
	(0.238)	(0.223)	(0.563)	(0.186)	(1.061)	(0.550)
Mean of Outcome	38.6426	38.5230	38.2135	38.5870	37.9442	38.6235
Observations	10,064	10,249	8,261	9,852	4,697	7,540
Bandwidth	1.584	1.599	1.493	1.543	1.682	1.923

Notes: Standard errors in parentheses.
* $p < .10$; ** $p < .05$; *** $p < .01$

Table 3.4: Fertility Results: Women Aged 15–24

	(1) White 15-24	(2) Black 15-24	(3) Hispanic 15-24	(4) All Race 15-19	(5) All Race 20-24
Reduced Form	1.596*	11.229***	1.280	1.195	2.994*
	(0.941)	(3.359)	(4.288)	(0.779)	(1.632)
First Stage	-0.094***	-0.085**	-0.070**	-0.093***	-0.085***
	(0.018)	(0.035)	(0.028)	(0.018)	(0.021)
Wald Estimate	-16.969	-131.382*	-18.278	-12.851	-35.264
	(10.749)	(69.020)	(61.726)	(8.971)	(21.734)
Mean of Outcome	82.9140	101.4820	124.1187	45.4102	146.5612
Observations	13,784	3,747	6,101	13,451	10,626
Bandwidth	2.228	1.204	1.419	2.166	1.659

Notes: Standard errors in parentheses.
* $p < .10$; ** $p < .05$; *** $p < .01$

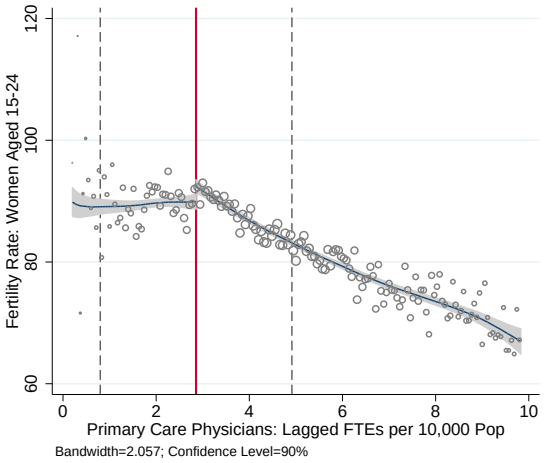
Next, Table 3.3 reports estimates for three variables – fertility, infant mortality, and gestation – by age and race/ethnicity of the mother. Gestation, in the bottom panel, is measured in weeks since last normal menses. There does seem to be a statistically significant reduced form discontinuity in gestation for black mothers, and perhaps another of the opposite sign for Hispanic mothers. The sign of the discontinuity for black mothers is consistent with a positive effect of HPSA-tied programs on gestation for that population. However, the Wald estimates for gestation are not statistically significant. There is also no evidence of an effect of designation on infant mortality. However, the fertility results in the top panel yield evidence of discontinuities. Fertility for women aged 15-24 is estimated to decrease by 27 per 1,000 women, from a baseline rate of 88. This reduction is statistically significant at the five percent level. Looking at fertility by race/ethnicity rather than by age, there is marginally statistically significant evidence that this effect holds for both white non-Hispanic and black non-Hispanic women. The point estimate for Hispanics is also consistent but statistically insignificant.

Table 3.4 as well as Figure 3.1 both pertain to fertility among women aged 15-24. The first three columns of Table 3.4 break down the fertility results by race/ethnicity and show a very large effect for black women, as well as some evidence of a considerably more modest effect for white women. It is important to note that the sample size for black women is considerably smaller than for white or Hispanic women. The final two columns of the table break down the ten-year age range into five-year categories. The results in these columns, while neither yield statistically significant results, suggest that the reduction in fertility in this age range associated with HPSA designation is not concentrated among women older or younger than 20.

In light of the possible finding that HPSA designation reduces fertility among young women, I next

Figure 3.1: RD Results: Fertility, Women Aged 15-24

(a) Reduced Form (Outcome: Fertility)



(b) First Stage (Outcome: HPSA Designation)

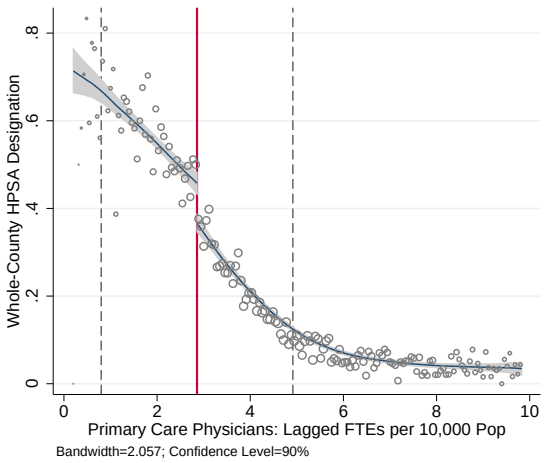


Table 3.5: Fertility and Abortion by Age

	(1) Fertility 15-44	(2) Abortion 15-44	(3) Fertility 15-24	(4) Abortion 15-24
Reduced Form	0.842	0.365	1.456	0.517
	(0.973)	(0.368)	(3.749)	(1.270)
First Stage	-0.074*	-0.072*	-0.141**	-0.155**
	(0.041)	(0.042)	(0.072)	(0.078)
Wald Estimate	-11.322	-5.038	-10.313	-3.330
	(14.889)	(6.078)	(27.413)	(8.569)
Mean of Outcome	68.0926	7.2865	95.325	14.140
Observations	6,703	6,595	2,200	2,216
Bandwidth	1.465	1.375	1.474	1.230
States	GA, KS, MI, MN, MO, NC, OK, TX, WA	GA, KS, MI, MN, MO, NC, OK, TX, WA	GA, TX, WA	GA, TX, WA

Notes: Standard errors in parentheses.
* $p < .10$; ** $p < .05$; *** $p < .01$

Table 3.6: Fertility and Infant Mortality: Women Aged 15-24 by Sample Exclusion Rule

	Fertility		Infant Mortality	
	Baseline	Modified	Baseline	Modified
Reduced Form	2.491**	1.932*	0.317	0.226
	(0.971)	(1.009)	(0.472)	(0.468)
First Stage	-0.092***	-0.092***	-0.093***	-0.088***
	(0.019)	(0.018)	(0.020)	(0.020)
Wald Estimate	-27.166**	-21.080*	-3.420	-2.577
	(12.394)	(12.028)	(5.150)	(5.373)
Mean of Outcome	87.878	88.057	8.041	8.067
Observations	12,839	14,120	11,532	11,967
Bandwidth	2.057	2.250	2.168	2.144

Notes: Standard errors in parentheses. Baseline specification excludes observations for which the county is observed to be high-need. The modified sample exclusion rule only excludes those observations which would be high-need irrespective of the value of the outcome variable. For example, in the second column, observations are excluded if their infant mortality or poverty rates reach the thresholds required to be considered high-need, but observations are not excluded if the fertility rate alone reaches the high-need threshold.
* $p < .10$; ** $p < .05$; *** $p < .01$

consider by what mechanism the programs attached to HPSA designation might have this effect. Although in theory the designation could change individuals' sexual behavior or affect the frequency of fetal death, I believe the more reasonable potential explanations are that HPSA designation either expands access to contraception or to abortion among young women.[5] In Table 3.5, I compare responses in fertility and abortion rates in the states for which I have abortion data. The first two columns report outcomes pertaining to all women aged 15-44 and are calculated using data from nine states. The second two columns report outcomes pertaining only to women aged 15-24 and are calculated using data from three states. With a smaller sample of counties, results are not as precisely estimated as with national data, but the point estimates are consistent with a reduction in fertility but no increase in abortion rates. In fact, point estimates of the effect of HPSA designation on abortion are negative.

Table 3.6 reports the result of the specification check described in section 3.3. Including certain high-need counties in the fertility and infant mortality regressions to avoid truncating the outcome variables does not substantially change the point estimates, suggesting that the truncation is not a critical problem.

3.4.2 Prenatal Care Utilization

[5]An increase in the fetal death rate in response to expanded access to primary care services would be a counter-intuitive result, and in fact the baseline fetal death rate among women in this age range is so low as to make it practically impossible that fetal death could be driving the observed changes in fertility.

Table 3.7: Different Prenatal Care Outcomes by Race of Mother

	(1) Adequate	(2) Adequate-Plus	(3) Inadequate	(4) Any Prenatal	(5) Month Start	(6) Start 1st Tri.
All Races						
Reduced Form	-0.001	-0.001	0.002	-0.002	0.022	-0.003
	(0.006)	(0.007)	(0.004)	(0.003)	(0.028)	(0.005)
First Stage	-0.089***	-0.100***	-0.089***	-0.101***	-0.084***	-0.087***
	(0.026)	(0.032)	(0.027)	(0.033)	(0.022)	(0.025)
Wald Estimate	0.006	0.014	-0.018	0.017	-0.259	0.033
	(0.063)	(0.067)	(0.050)	(0.029)	(0.336)	(0.062)
Mean of Outcome	0.7426	0.3311	0.1386	0.9871	2.8907	0.7843
Observations	6,825	4,551	6,500	4,394	9,344	7,067
Bandwidth	1.056	0.690	1.003	0.664	1.464	1.100
White						
Reduced Form	0.001	0.000	-0.001	0.001	0.021	0.001
	(0.005)	(0.007)	(0.004)	(0.002)	(0.024)	(0.005)
First Stage	-0.089***	-0.101***	-0.091***	-0.102***	-0.086***	-0.091***
	(0.025)	(0.031)	(0.027)	(0.032)	(0.022)	(0.027)
Wald Estimate	-0.009	-0.001	0.009	-0.006	-0.248	-0.010
	(0.057)	(0.067)	(0.043)	(0.016)	(0.284)	(0.059)
Mean of Outcome	0.7689	0.3413	0.1152	0.9911	2.7558	0.8116
Observations	7,101	4,682	6,295	4,511	9,559	6,122
Bandwidth	1.108	0.710	0.975	0.684	1.501	0.947
Black						
Reduced Form	-0.019*	-0.003	0.012	0.005	0.052	-0.019*
	(0.010)	(0.012)	(0.009)	(0.004)	(0.053)	(0.011)
First Stage	-0.068*	-0.044	-0.064*	-0.053	-0.068**	-0.066*
	(0.035)	(0.042)	(0.037)	(0.040)	(0.035)	(0.036)
Wald Estimate	0.280	0.073	-0.187	-0.088	-0.756	0.283
	(0.202)	(0.286)	(0.171)	(0.100)	(0.826)	(0.203)
Mean of Outcome	0.6676	0.3406	0.2132	0.9768	3.2167	0.6950
Observations	3,859	2,670	3,496	2,913	3,871	3,676
Bandwidth	1.396	0.959	1.271	1.054	1.400	1.333

Notes: Standard errors in parentheses.
* $p < .10$; ** $p < .05$; *** $p < .01$

Table 3.7 reports estimation results using indicators of prenatal care utilization as the outcome variables. I observe no reduced-form discontinuities and therefore find no evidence that HPSA designation affects utilization of prenatal care.

3.4.3 Mortality

The results suggest that HPSA designation does not affect mortality. Table 3.8 reports estimation results using age- and race-specific mortality rates as outcomes. Additionally, appendix Tables E.1 and E.2 report results using cause-specific mortality rates as outcomes. In only two of these forty-five specifications, I find possible evidence of a mortality response to HPSA designation: cancer mortality among whites appears to decrease and mortality from external causes among Hispanics appears to increase with HPSA designation. (Both of these results appear in appendix Table E.2.) In the case of cancer mortality among whites, the Wald estimate is not statistically significant, and in the case of external cause mortality among Hispanics, the Wald estimate has a p-value of 0.094. Finding two statistically significant results among 45 specifications aligns with expectations about the incidence of Type I error. Moreover, cancer was the leading cause of death for whites aged 55-64 and for those aged 65-74 during the sample period,[6] but Table 3.8 does not report evidence of a discontinuity in mortality for whites in those age ranges. This further suggests that the cancer mortality finding may be spurious.

3.5 Discussion and Conclusion

In this chapter, I study whether HPSA designation has an effect on a number of outcomes observable in national vital statistics data. The outcomes I consider include mortality, fertility, birth weight, gestation, and prenatal care utilization. The results suggest that shortage area designation reduces fertility among women aged 15-24, but does not affect birth weight, gestation, or prenatal care conditional on live birth. Similarly, I do not find evidence that designation affects mortality. As these measures are derived from vital statistics data, they reflect the health of, and the care delivered to, the population as a whole. For this reason, the analysis performed in this chapter complements the analysis performed using ACSH rates for the Medicare population. While ACSH as a measure are ex ante more likely to respond to variation in primary care access and quality than e.g. mortality,[7] the restriction to an elderly and insured population excludes many of the people that are targeted by the HPSA program.

The reduction in fertility among young women that I observe as an apparent response to HPSA des-

[6] Source: CDC Web-based Injury Statistics Query and Reporting System (WISQARS), available at `http://www.cdc.gov/injury/wisqars/leading_causes_death.html`.

[7] See Section 1.2.3 and references therein. In the literature, the relationship between primary care and ACSH is much more clear than any relationship between primary care and mortality.

Table 3.8: Mortality by Age and Race/Ethnicity

	(1) Overall	(2) Aged 55-64	(3) Aged 65-74	(4) Aged 75-84	(5) Aged 85+
All Races					
Reduced Form	-0.015	0.114	0.387	0.387	-0.312
	(0.160)	(0.198)	(0.427)	(0.869)	(2.212)
First Stage	-0.094***	-0.085***	-0.084***	-0.091***	-0.096***
	(0.018)	(0.021)	(0.021)	(0.019)	(0.017)
Wald Estimate	0.164	-1.342	-4.586	-4.249	3.260
	(1.692)	(2.371)	(5.235)	(9.609)	(23.115)
Mean of Outcome	8.9401	8.7050	20.0222	48.3543	137.4697
Observations	13,916	10,667	10,257	12,470	15,154
Bandwidth	2.256	1.667	1.599	1.992	2.534
White					
Reduced Form	0.038	0.095	0.422	0.440	0.353
	(0.170)	(0.204)	(0.411)	(0.892)	(2.271)
First Stage	-0.095***	-0.084***	-0.086***	-0.090***	-0.095***
	(0.018)	(0.021)	(0.020)	(0.019)	(0.017)
Wald Estimate	-0.395	-1.123	-4.897	-4.873	-3.726
	(1.784)	(2.450)	(4.935)	(9.966)	(23.998)
Mean of Outcome	9.6645	8.5758	19.8850	48.5492	139.4722
Observations	14,378	10,132	11,053	12,144	15,168
Bandwidth	2.351	1.582	1.731	1.934	2.547
Black					
Reduced Form	-0.126	-0.794	0.489	-0.296	-3.052
	(0.344)	(0.727)	(1.182)	(1.830)	(5.644)
First Stage	-0.092***	-0.088***	-0.082***	-0.130***	-0.061
	(0.022)	(0.030)	(0.031)	(0.028)	(0.043)
Wald Estimate	1.369	8.978	-5.935	2.277	50.315
	(3.725)	(8.524)	(14.674)	(14.065)	(98.000)
Mean of Outcome	6.9986	13.4079	27.9445	57.0269	133.2594
Observations	9,040	5,138	4,953	5,747	2,556
Bandwidth	1.791	1.813	2.038	3.078	1.779

Notes: Standard errors in parentheses. "Overall" mortality is the mortality rate for all county residents over age one year.
* $p < .10$; ** $p < .05$; *** $p < .01$

ignation is interesting. Because I do not find evidence of increased abortion rates alongside the fertility reduction, I suspect that the incentives attached to HPSA designation improve access to contraceptives and family planning services. If so, the mechanism is likely working through the interaction between the HPSA program and other areas of health care policy. Specifically, the federal government promotes family planning services by funding certain providers under Title X of the Public Health Service Act.[8] Many community health centers receive funding under Title X, and Wood et al. (2014) report that the overwhelming majority of CHCs offer contraceptives, with oral contraceptives being the most widely available. Gold (2011) discusses the workforce challenges faced by Title X facilities and the importance of the National Health Service Corps in the provision of family planning services. The fertility results in this chapter may offer empirical support to the assertions made by Gold.

I must also note, just as in the conclusion to Chapter 2, that the Wald estimate of the effect of shortage area designation on fertility is very large considering the scale of the HPSA program compared to the broader health care system. The empirical challenges that I outline there hold in this instance as well.

In addition to the fertility findings, the lack of an effect of designation on mortality and on prenatal care are also interesting. I confirm the findings of Pathman et al. (2005), who does not find relative improvements in mortality in rural HPSAs relative to other rural counties. That I do not find evidence of improved access to prenatal care resulting from HPSA designation is surprising. The result seems to align with my finding presented in Section 2.4 that designation does not induce a greater fraction of Medicare beneficiaries to receive primary care. Together with the birth weight, gestation, and infant mortality results, these findings indicate that HPSA designation has different effects for two distinct populations of young women: for those who become pregnant and carry the pregnancy to term, designation appears to have no effect. But, for the population of young women who would become pregnant absent effective birth control, designation may have an effect in reducing unplanned pregnancy. To speak more conclusively requires further research that focuses specifically on contraception, but the apparent pattern in these results is interesting as policymakers consider how the HPSA program interacts with different policy objectives.

[8] http://www.hhs.gov/opa/title-x-family-planning/

Chapter 4

Health IT and Ambulatory Care Quality (coauthored with Carole Roan Gresenz, Amalia Miller, and Catherine Tucker)

4.1 Introduction

The US federal government has devoted considerable resources to promoting the diffusion of information technology (IT) in healthcare. Between January 2011 and April 2015, over 400,000 medical providers received payments totaling over $30 billion through the Medicare and Medicaid incentive programs of the 2009 Health Information Technology for Economic and Clinical Health (HITECH) Act. Underlying these federal investments, and their state-level counterparts, is a belief that creating an electronic rather than a paper interface between patient information and healthcare providers can improve healthcare quality and also save money. However, these federal incentives under HITECH were targeted at hospitals and individual physicians and explicitly omitted ambulatory facilities.[1]

This decision to not give incentives to ambulatory facilities is particularly striking, given the emphasis given in the Patient Protection and Affordable Care Act of 2010 (ACA) on shifting care, where possible, from high cost hospitals towards ambulatory and outpatient centers. This desire to promote care in ambulatory settings reflects work such as Jiang et al. (2009), who estimate that in 2006 alone, over four million hospital

[1]See for example, FAQ 43 in `https://www.cms.gov/Regulations-and-Guidance/Legislation/EHRIncentivePrograms/downloads/faqsremediatedandrevised.pdf` *Will ambulatory surgical centers be eligible for incentive payments under the Medicare and Medicaid Electronic Health Record (EHR) Incentive Program?* Ambulatory surgical centers are not eligible for EHR incentive payments. The following types of institutional providers are eligible for EHR incentive payments under Medicare and/or Medicaid, provided they meet the applicable criteria. Under Medicare, institutional providers eligible for the EHR incentive payments include 'subsection (d) hospitals,' as defined under section 1886(d) of the Social Security Act, and critical access hospitals. Under Medicaid, institutional providers eligible for the EHR incentive payments are acute care hospitals (which include critical access hospitals and cancer hospitals) and children's hospitals.

admissions could have been prevented, with a cost savings of over $30 billion, through effective ambulatory care and adequate patient self-management.

To evaluate whether omitting ambulatory facilities from receiving incentives from the HITECH program is warranted, this paper explicitly evaluates the effect of health care IT adoption by ambulatory facilities on healthcare quality (Buntin et al., 2011). Echoing the federal government's focus on incentivizing only hospitals, empirical evidence on the effects of health IT on health care quality has mostly limited to the hospital setting (Agha, 2014; Lee et al., 2013; Lin et al., 2014; Spetz et al., 2014; Freedman et al., 2014; McCullough et al., 2013; Miller and Tucker, 2014b). This paper aims to fill this gap.

Theoretically, there are reasons to believe that adopting digital health records may be important for patients who receive care outside of hospital admissions. These include improved record-keeping and information flow that can be especially important when patients switch providers and care settings for routine and potentially repetitive proecedures (Coleman and Berenson, 2004; Bodenheimer, 2008). Decision support functions that increase the standardization of care and patient-centered functions that enable better self-care between visits may also be key for patients receiving care in ambulatory settings. In particular, consumer applications of health IT that aim to enhance the ability of patients (and their caregivers) to share responsibility for actively managing their health (Tang and Newcomb, 1998; Eysenbach, 2000) and following treatment regimens will also depend in part on the local adoption of health IT among area providers. Health IT can improve communication and information exchange between patients and providers, but it requires adoption by both consumers as well as ambulatory providers.

This paper measures the effects of health IT on ambulatory care quality in the US using national panel data on local area adoption of ambulatory electronic medical records (EMRs) between 2000 to 2012, that is, years prior to the implementation of HITECH. Our outcome measure is a local area proxy for ambulatory care quality based on rates of hospital admissions for reasons that have been identified by experts as preventable by high quality ambulatory care. We estimate differences-in-differences models that control for location and time fixed effects, as well as observable factors related to healthcare quality and population demographics. Across datasets and models, we find significant reductions in ambulatory care sensitive (ACS) hospitalization rates in an area from increases in ambulatory HIT adoption in the area, suggesting quality improvements. However, these gains are only significant for the population aged 65 and older. Our point estimates suggest possible gains for younger adults, but the findings are not statistically significant. Our results suggest that health IT adoption among ambulatory care providers yields important quality improvements in health care for patients.

This paper has three major contributions.

First, we contribute to the growing literature that attempts to understand in which settings healthcare IT improves health quality outcomes. The theoretical foundation for why healthcare IT may improve the quality of care has been presented in many scholarly and popular articles, such as Brailer (2005) and Hillestad et al. (2005). The potential benefits include lower administrative costs, reduced error rates, especially from drug interactions, and reduced rates of duplicate testing, as well as improved patient monitoring. Although the conceptual notion that computers can thereby improve efficiency and improve quality is compelling, countervailing forces exist that may limit or negate the benefits of health IT. The installation of an IT system may prove unsuccessful if providers and other staff resist changing their work patterns, or if they find that the computerization adds to their administrative burdens, introduces inappropriate redundancy to documentation procedures, or is cumbersome to use. There are also concerns about the security and stability of electronic systems, which may limit the willingness of providers to rely on IT solutions. Empirical evidence is therefore essential to determine if health IT is associated with improved quality in the real world, and to measure the magnitude of the gains.

Large national studies have related hospitals' adoption of electronic medical records (EMRs) and other forms of health IT to higher quality care, measured by process improvements and lower mortality. By using panel data that tracks adoption and outcomes over time, researchers have been able to control for differences between early and late adopting hospitals and geographic areas using difference-in-differences approaches (Miller and Tucker, 2011; Agha, 2014; Lee et al., 2013; Lin et al., 2014; Spetz et al., 2014; Freedman et al., 2014; McCullough et al., 2013). This paper extends this literature by showing that there are also positive effects for quality outcomes in ambulatory centers. Past research on the effects of health IT investment in the ambulatory care setting has primarily consisted of small-scale case studies or surveys, often in single hospitals or large systems (many of which are also early or sophisticated adopters of health IT; see Buntin et al. (2011) and references therein).

Researchers have been constrained in their ability to better assess the impact of ambulatory health IT because of severe data limitations: Data sources capturing hospital IT have had limited or no information on ambulatory care, and sources focused on ambulatory health IT often have had limited information on the nature or timing of the IT implementation or focus exclusively on very recent years. The importance of using data from many locations, as opposed to data from a single hospital, health system or location, is clear from the conflicting results found in different studies of specific implementations of health IT. This conflict occurred in the hospital setting and may be especially important in the ambulatory setting, where

the value of health IT may depend on the specific features and functions of the IT system, as well as the training of providers who use the system and their awareness of its features, and the willingness of staff across the organization to integrate health IT into their usual work-flow (Kaplan et al., 2001). Survey evidence suggests substantial variation across individual physicians and practices in their use and awareness of the various functions of their health IT systems (Hing et al., 2007; Simon et al., 2007; Jha et al., 2006) and points to organizational culture as a factor that supports or inhibits successful health IT deployment.

Second, we contribute to the literature that attempts to understand which patients benefit most from healthcare IT. Work such as Miller and Tucker (2011) and McCullough et al. (2013) has shown that healthcare IT has the greatest benefits for the gravest of cases which benefit the most from the continuity of care. Our research suggests that the largest benefits from healthcare IT in an ambulatory setting are among older patients, who potentially have larger underlying health issues and consequently a more comprehensive medical history to document.

Third, our paper is important from a policy perspective in suggesting that there was no reason in the original HITECH act to exclude ambulatory centers from receiving incentives and focus solely on giving incentives to hospitals or individual physicians or groups. Though ambulatory facilities were specifically excluded from the HITECH institutional incentives (unlike hospitals), there was still an indirect incentive for them to adopt the technology if it would help individual physicians who encountered patients in their premises reach the meaningful use thresholds of Medicare patients that they treated. Even these indirect incentives for 'eligible professionals' (EPs) are substantially muted in the Medicare incentive program because only physicians qualify as EPs.[2]

Furthermore, a new proposed bill in Congress could potentially dilute the remaining indirect incentive, to the extent that it is present, for the case of ambulatory surgery centers. The 'Electronic Health Fairness Act' of 2015 (H.R. 887) proposes that ambulatory surgery centers should be explicitly excluded from counting towards the IT-enabled patient encounter thresholds physicians must meet to avoid Medicare penalties. The rationale behind this act is that exempting encounters with patients in ambulatory surgical centers might give incentives for physicians to shift patients to lower-cost ambulatory centers away from hospitals.[3] However, our estimates suggest that there are real and measurable benefits from IT in ambulatory facilities which speak against a new regulation designed to dull incentives for the use of IT in these settings. Indeed, the estimates in our paper suggest that, at least for the older population which could be reached via the

[2]Nurse practitioners and certified nurse midwives also qualify under the Medicaid incentives.

[3]http://www.outpatientsurgery.net/outpatient-surgery-news-and-trends/general-surgical-news-and-reports/bill-seeks-to-exempt-surgery-centers-from-counting-toward-50-threshold–03-06-15

established system targeting of incentives via Medicare, there are substantial improvements in care quality from the adoption of healthcare IT by ambulatory centers. This result is particularly important in the context of the ACA, which attempts to incentivize the provision of care in ambulatory centers rather than high-cost hospitals and emphasize the need to streamline and speed up the certification process for electronic health record systems in ambulatory centers.

4.2 Data

4.2.1 Data on Ambulatory Health IT

The importance of collecting data over an extended time period relates to the fact that implementations of health IT are not instantaneous, and that there may be a transition period of several years before the full benefits are realized. In order to track how quality of care changes over time as new health IT systems are installed and used, the dataset needs to include several years of observation before installation as well as several years afterward.

We measure local area ambulatory health IT adoption using the 2012 and earlier releases from the Healthcare Information and Management Systems Society (HIMSS) Analytics^{TM} Database (HADB). HIMSS is a non-profit organization "exclusively focused on providing global leadership for the optimal use of information technology (IT) and management systems for the betterment of healthcare."[4] HIMSS Analytics is the organization that collects, analyzes and disseminates "healthcare data relating to IT processes and environments, products, IS department composition, costs and management metrics, healthcare trends and purchasing decisions" (http://www.himssanalytics.org). The HADB is derived from the Dorenfest IHDS+ Database and released annually starting in 2005. From 1998 to 2004, data are available from the Dorenfest Complete IHDS+ Database, and from 1986 to 1995, there is a more limited Dorenfest 3000+ Database.

The HADB has several advantages as a data source for this research. It includes a large set of ambulatory providers from across the country and the availability of information over time allows us to construct the multi-year panel crucial for our difference-in-differences approach. The current version of the database includes data on over 20,000 ambulatory providers nationwide. (While we also have access to earlier versions of the database, those were more focused on hospitals and contained less data on ambulatory providers.) The HADB contains detailed data for several health IT applications, with information about the functions, vendor, implementation status and contract date. The HADB is considered an authoritative source for tracking hospital IT adoption and has previously been used in research on hospital health IT adoption,

[4] `http://www.himss.org/ASP/index.asp`

such as Miller and Tucker (2009), Jones et al. (2010), and Angst et al. (2010). Moreover, The HADB contains detailed data for several health IT applications, with information about the functions, vendor, implementation status and contract date.

Surveys of the status of ambulatory health IT adoption frequently rely on the HADB (see, for example, the recent 2011 report on the "State of Health Information Technology in California" by the California HealthCare Foundation). Other potential data sources for ambulatory health IT are limited in key ways. Some are one-shot surveys of relatively small numbers of providers that cannot be used to generate reliable measures of local adoption or to compute changes in adoption over time. Examples include the National Association of Community Health Centers' 2008 HIT Survey of Health Centers (which had a 37% response rate); the Center for Studying Health System Change 2008 Health Tracking Physician Survey of over 4,700 physicians (62% response rate), and the Harris Interactive 2009 survey of 1,442 primary care physicians and pediatricians (39% response rate). These samples are generally far smaller than the HADB and would be less reliable for computing local information, even at a single point in time.[5]

HADB defines an entity as ambulatory if it offers preventive, diagnostic, therapeutic and rehabilitative services to individuals not classified as inpatients or residents. This includes physician offices and clinics.

Ambulatory care entities are included in the HADB if they are affiliated with a larger health care delivery system, either because they are owned (in whole or in part), leased, or managed by the system. The definition of a system is rather broad in the data, and includes some with only a single hospital. Nevertheless, this selection rule means that our measure of health IT adoption is not available for the full universe of ambulatory providers. It does not capture health IT adoption among free-standing physician offices or ambulatory care providers who are not affiliated with any healthcare system. While the exclusion of such ambulatory providers may limit the generalizability of the results (and may bias us against finding statistically significant effects), no panel data sources exist that provide information on health IT use for the entire set of all ambulatory providers in the United States.

One possible advantage of the HADB inclusion criteria is that they focus our analyses on the set of ambulatory care providers most likely to exchange patient information and benefit from health IT adoption. Miller and Tucker (2014a) report evidence from the AHA special survey of hospitals regarding health IT and information exchange. A key finding of that paper was that hospitals that are part of larger systems are more likely to exchange patient information electronically with other providers (hospitals and ambulatory

[5]In contrast, the SK&A marketing survey contains a larger set of ambulatory providers than the HADB, but their data are too recent for our purposes. The question on EMR adoption only started in 2008, and information on specific types of software and functionalities started even more recently.

Table 4.1: Ambulatory HIT Diffusion over the Sample Period

	Mean	Std. Dev.
2003		
Ambulatory EMR	0.22	0.38
Ambulatory PACS	0.051	0.18
Ambulatory Practice Management	0.41	0.45
2008		
Ambulatory EMR	0.44	0.44
Ambulatory PACS	0.096	0.23
Ambulatory Practice Management	0.55	0.45
2012		
Ambulatory EMR	0.68	0.39
Ambulatory PACS	0.12	0.24
Ambulatory Practice Management	0.63	0.41
Total		
Ambulatory EMR	0.45	0.44
Ambulatory PACS	0.089	0.22
Ambulatory Practice Management	0.53	0.45

Notes: County-level averages of facility-level adoption data, weighted by size (physicians).

care providers), but they are less likely to exchange information outside of their systems. This implies that health IT adoption may be most useful for providers who are able to exchange data with other providers through a shared system. One reason for this may be concerns about privacy and security of private patient health information.

Our outcome measure (see Section 4.2.2) is the ambulatory care sensitive hospitalization rate in a local geographic area (county). Thus, health IT adoption should ideally be measured for providers who serve the patient population in the area. We create a county-level measure of health IT adoption among ambulatory providers as our primary measure, where the county level measure is based on the physical location of providers. While a health IT adoption measure constructed for a larger geographic area may be more likely to include all of the relevant providers serving the patients using inpatient hospital care in that county (who are included in the outcome measure), such a measure would also provide a less precise measure of local health IT for individuals who seek care within county boundaries as well as show less variation at any point in time.[6]

Our main measure of ambulatory health IT is whether an ambulatory EMR system is in place. This is

[6]We aggregate adoption variables across facilities to the county level wighting by practice size (number of physicians). We use AHRQ's modified county definition that groups together cities and counties in Virginia.

the backbone system that stores and manages digital patient records for an ambulatory provider. We use information from contract dates to identify the timing of adoption in order to construct a panel dataset. One limitation is that we assume that contract dates recorded in our data reflect an entity's first (and not just most recent) adoption decision. The data also include information on decisions to replace existing systems. Empirically, we find it is rare for such decisions installed within the past decade, which suggests that our assumption about contract dates indicating first adoption is likely to be valid in most cases.

4.2.2 Data on Quality: Prevention Indicators

We use an indirect measure of ambulatory care quality based on hospital admissions for ambulatory care sensitive (ACS) conditions. ACS conditions are those for which appropriate ambulatory care can prevent or reduce hospitalization. For example, good management of asthma at the first sign of exacerbation can usually alleviate symptoms or keep them from progressing to the point that hospitalization is required. Other examples of ACS conditions include dehydration and hypertension. Hospitalizations for ACS conditions are interpreted as indicating problems in access to ambulatory care or poor-quality outpatient management (Billings et al., 2000). Despite the fact that ACS hospitalizations reflect hospital data, they provide insight into the health care system *outside* the hospital setting (AHRQ, 2007). Indeed, ACS hospitalizations were included in a set of metrics identified by Kern et al. (2009) that were deemed most appropriate for capturing the potential quality effects of ambulatory health IT. More broadly, ACS hospitalizations were one of a set of 20 indicators chosen by the Institute of Medicine (IOM) as critical for understanding and tracking the state of the nation's health system and one of only three key measures the committee pointed to for monitoring the effectiveness of care (IOM, 2009).

Standard, well-validated methods exist for classifying ACS hospitalizations. These methods, which were first established by Billings et al. (2000) and then developed by AHRQ into "Prevention Quality Indicators" (PQIs), are used by AHRQ and several states in monitoring the progress of their health care system. We constructed county-level PQI measures using the AHRQ algorithms applied to hospitalization records from the Medicare Inpatient Limited Data Set (LDS). The Inpatient LDS data include diagnosis and procedure codes along with patient age, sex, and county of residence corresponding to every hospitalization of a Medicare fee-for-service beneficiary between 2003 and 2012 (approximately 13 million records per year).[7]

[7]To address the high rate of missing county data in the 2008 LDS file (86%), we use unique patient identifiers to match individual records from other years and impute location. This lowers the rate of missing geographic information to 18%, which is still somewhat high, but more in line with rates of missing data for other years. Since rates of missing data are unlikely to be related to EMR adoption in 2008, this is unlikely to bias our estimates of its impact.

Table 4.2: Summary Statistics: Control Variables and Medicare Population Outcomes

	Mean	Std. Dev.	Min.	Max.	Obs.
PQI Count	713.1	1638.4	0	46126	25030
PQI Rate	6300.0	2937.6	0	29645.4	25030
Acute PQI Rate (per 100,000)	2929.0	1459.6	0	17180.6	25030
Chronic PQI Rate (per 100,000)	3372.8	1673.6	0	18453.6	25030
Population Age 65+ (00,000s)	0.15	0.39	0.0013	11.4	25030
Short-Term Hosp. Beds (Per 1,000)	3.21	3.89	0	53.2	25030
Primary Care Physicians (Per 10,000)	6.91	4.38	0	94.5	25030
FQHC Grantees (per 100,000 Low Income)	14.0	34.3	0	488.6	25030
Govt Health & Hosp. Spending (000,000s USD)	5100.9	5268.1	95	33773.7	25030
State HMO Penetration Rate	15.8	9.69	0	65.5	25030
State Uninsured Percent	14.5	4.15	3.36	25.5	25030
Median Household Income (000s USD)	42.5	10.9	17.8	119.5	25030
County Poverty Percent	14.5	5.42	2.39	47.8	25030

Notes: County-year unit of observation. PQI count is the number of ACS hospital admissions among adults insured by Medicare. The PQI rate is the ACS hospitalization count per 100,000 population aged 65 and over in the county. Acute and chronic PQI rates are defined similarly and reflect ACS hospitalizations for acute vs. chronic conditions. Hospital beds and primary care physicians are scaled to total population in the county. Federally qualified health centers (FQHCs) are organizations receiving grants under Section 330 of the Public Health Service Act (PHS). FQHCs are scaled to low-income population in the county.

4.2.3 Data on Controls

Control variables include demographic and socioeconomic characteristics of local areas as well as healthcare factors that are likely to affect the ACS rate and that may change differently over time across different areas. We use multiple data sources to construct control variables, including the US Census Bureau County Intercensal Estimates (for county-level population); the Health Resources and Services Administration's Area Health Resource File (AHRF), for primary care physician supply, supply of hospital beds, and number of federally-qualified health center grantees in the county (US Department of Health and Human Services, Health Resources and Services Administration, Bureau of Health Workforce, 2014); the US Census Bureau Small Area Income and Poverty Estimates database, for poverty rates and median household income; Kaiser Family Foundation State Health Facts, for percent uninsured and HMO penetration rate (online at http://kff.org/statedata; based on the data from the Census Bureau's March Supplement to the Current Population Survey and HealthLeaders InterStudy); and the Census of Governments/Annual Survey of State and Local Government, Finances for state and local government health and hospital spending.

We control for health care market and demographic characteristics of the population that are likely to influence the county ACS hospitalization rate, including the supply of primary care, scope of the local health care safety net, availability of hospital beds, pervasiveness of managed care, and insurance status and income

level of the population. A larger supply of primary care providers, richer safety-net, and greater managed care presence are likely to reduce ACS hospitalizations, while greater hospital bed availability, higher rates of uninsurance and lower population income levels are likely to be associated with higher ACS rates.

We measure primary care physician supply using the county-level number (per 10,000 population) of non-federal doctors of medicine (M.D.) and doctors of osteopathy (D.O.) providing direct patient care who practice principally in general internal medicine, general or family practice, pediatrics or obstetrics and gynecology. Our hospital supply measure includes the number short-term general hospital beds per 1,000 in the county. Our safety-net measures include the number of federally qualified health center (FQHCs) grantees per 100,000 low income population in the county and a state-level health and hospital spending variable, which includes state, county, and local expenditures for public health administration, immunization programs, outpatient health clinics, hospital facilities directly administered by the government, and other support for the provision of hospital care (Gresenz et al., 2007). The HMO penetration rate is the percent of the total population in the state enrolled in an HMO and the uninsured rate is the percentage of the total population in the state without health insurance coverage during the year. The county level poverty rate indicates the percentage of the population in the county with income less than the federal poverty line (FPL).

Table 4.2 reports summary statistics on the control variables for our primary sample of 25,030 county-year observations for which we have information on ambulatory EMR adoption and PQIs. This does not include the full set of counties in the US; rather, counties are omitted if there are no data on ambulatory providers in the county in the HADB. Counties in our sample tend to have more hospital beds and primary care physicians per population and more FQHC grantees per low-income population than excluded counties. They also have higher median household income and lower poverty percents. They are in states with higher levels of health spending and HMO penetration, and lower uninsured percentages.

4.3 Empirical Analysis and Results

We use our panel data to estimate difference-in-differences models in order to understand the impact of health IT on ambulatory care quality. We include fixed effects for location and time. This is similar to the approach used in the literature on hospital IT adoption. One difference is that our quality measure is not linked to specific ambulatory care facilities, but is based on the local area. This is similar to Miller and Tucker (2011), who study hospital IT and infant health. That paper also uses hospital privacy laws as a source of exogenous variation in EMR adoption and confirms the qualitative findings from the basic fixed

effects models. We are not able to use those instruments in this paper because of the different setting and time period, but the lack of significant bias in the OLS models in Miller and Tucker (2011), as well as other robustness and falsification checks in the hospital context found in the literature, all provide support for the fixed effect approach in this paper in the ambulatory setting.

4.3.1 Main Results for the Medicare Population

Column 1 of Table 4.3 reports results from regressions that control only for the ambulatory EMR adoption rate, county and year fixed effects, and county population. Standard errors are clustered at the county level to account for serial correlation within counties. The estimate indicates that a 45 percentage point increase in ambulatory EMR adoption in a county (the average increase over our sample period) is associated with a 104 point drop in PQI admission rate (per 100,000 relevant population per year) in that county, or about a 1.6% decline.

Adding controls has a limited effect on our estimate of the effect of health IT on the PQI rate (Column 2), although several controls have statistically significant effects on the PQI rate. ACS admissions are higher when there are more hospital beds per capita, which may reflect availability or demand for hospital care. The negative coefficient for the state percent uninsured variable suggests a positive association between the availability of health insurance and hospital admissions overall as well as hospital emergency visits (Card et al., 2008; Finkelstein et al., 2012). Higher median income level in a county and a higher county-level poverty rate are both associated with higher ACS hospitalization rates. Finally, increasing HMO penetration is associated with a lower ACS admission rate.

The last two columns of Table 4.3 show robustness checks for alternative estimation models. In Column 3, we add state-specific linear time trends to account for the possibility that states with greater ambulatory EMR adoption during the time period happened to be those with decreasing trends in ACS rates. In Column 4, we report estimates from an unweighted version of the model. Both specifications show a reduction in ACS rates from an increase in EMR adoption, though the coefficient estimates are smaller in magnitude.

Table 4.4 considers alternative outcome measures. Column 1 uses the ambulatory care sensitive hospitalization rate for acute health conditions and Column 2 uses the rate for chronic conditions. Columns 1 and 2 show that the overall decline in ACS rates comes from declines in hospitalization rates related to acute conditions, such as urinary tract infections, and chronic conditions, such as hypertension and diabetes. The absolute magnitude of the decline in hospitalizations for chronic conditions is larger, but the effects are more similar in proportion to their average values.

The remaining columns apply different transformations to the dependent variable. In Column 3 we use

Table 4.3: Main Results

	(1) PQI Rate b/se	(2) PQI Rate b/se	(3) PQI Rate b/se	(4) PQI Rate b/se
Ambulatory EMR	-231.3***	-245.1***	-175.6***	-118.4***
	(61.6)	(54.8)	(38.4)	(41.9)
Population Age 65+ (00,000s)	637.3***	414.4	304.6***	3838.3***
	(233.0)	(302.2)	(111.5)	(802.8)
Short-Term Hosp. Beds (Per 1,000)		64.1**	36.7**	31.7
		(26.9)	(18.3)	(20.3)
Primary Care Physicians (Per 10,000)		22.9	64.9***	51.7***
		(19.7)	(17.5)	(13.6)
FQHC Grantees (per 100,000 Low Income)		-2.21	-2.51*	-0.54
		(1.82)	(1.35)	(1.38)
Govt Health & Hosp. Spending (000,000s USD)		0.019	-0.10***	0.017
		(0.017)	(0.030)	(0.015)
Median Household Income (000s USD)		84.5***	46.9***	49.4***
		(7.87)	(5.14)	(5.34)
County Poverty Percent		29.8***	15.2*	13.4*
		(10.3)	(8.10)	(7.19)
County Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	25030	25030	25030	25030
Counties	2503	2503	2503	2503

Notes: Weighted least squares with county population weights in Columns (1) to (3); unweighted least squares in Column (4). Column (3) includes state-specific linear time trends. Robust standard errors clustered at the county level.
* $p < 0.10$, ** $p < 0.05$,*** $p < 0.01$

Table 4.4: Alternative Outcome Measures

	(1) Acute PQI Rate b/se	(2) Chronic PQI Rate b/se	(3) PQI Count b/se	(4) Log PQI Rate b/se
Ambulatory EMR	-93.7***	-151.4***	-196.7**	-0.049***
	(24.9)	(32.3)	(80.9)	(0.017)
Controls	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	25030	25030	25030	25029
Counties	2503	2503	2503	2503

Notes: Weighted least squares with county population weights. Robust standard errors clustered at the county level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4.5: Limiting the Sample to Counties with High Ambulatory EMR Data Coverage

	(1) All Counties b/se	(2) > 10% of Docs b/se	(3) > 25% of Docs b/se	(4) > 50% of Docs b/se
Ambulatory EMR	-245.1***	-298.8***	-216.7***	-128.7**
	(54.8)	(55.0)	(60.2)	(60.8)
Controls	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	25030	22141	18187	12554
Counties	2503	2252	1925	1431

Notes: Weighted least squares with county population weights. Robust standard errors clustered at the county level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

the count of ACS hospitalizations (instead of the rate). Because the population over age 65 in a county is an imperfect measure of the number of Medicare fee-for-service beneficiaries, the ACS rate is potentially subject to measurement error. The ACS count, by contrast, is not. In Column 4, we take the natural logarithm of the ACS rate. Our estimates are robust to using ACS counts instead of rates and when taking the natural logarithm of the PQI rate.

In a final robustness check, we repeat the main regressions using subsamples of our analytic data set that are limited to counties for which we have more complete coverage of physicians providing ambulatory care in the HADB. We calculate the coverage of the HADB data on ambulatory care providers by comparing the physician count in the HADB to the count of primary care physicians in the county during 2012 from the AHRF data. The comparison is imperfect because, for example, the HADB measure is not restricted to primary care physicians, so we should not expect a perfect match. Nevertheless, we should have more confidence in the relevance of our measure of ambulatory EMR adoption in counties with higher ratios of HADB to AHRF data. The first column of Table 4.5 shows the main estimate from Table 4.3 (Column 2 of that table) for the full sample of roughly 2500 counties. Column 2 limits the sample to counties with a ratio of HADB to AHRF physicians greater than 10% (approximately 2200 counties; Column 3 includes counties for which the estimated HADB coverage is 25% or greater (1900 counties) and Column 4 requires an estimated HADB coverage rate of 50% or greater (approximately 1430 counties, or roughly half of the full sample of counties).

The main result is robust to the smaller samples of counties, although the magnitude changes. The effect size is smaller in analyses that use only counties for which estimated HADB coverage is over 20 or 50 percent. These results suggest that a 45 percentage point increase in ambulatory EMR adoption in a county (the

average increase over our sample period) is associated with a 58 point drop in the ACS rate (per 100,000 relevant population per year) in that county, or about a 0.9% decline.

4.3.2 Results from Other Demographic Groups

A natural question to ask is if the benefits associated with ambulatory EMR adoption for the Medicare aged population are also present for younger age groups.

We address the question using inpatient discharge data from the Nationwide Inpatient Sample (NIS), which is part of the Healthcare Cost and Utilization Project (HCUP). Our NIS data span 2000-2010. Unlike the Medicare data, the NIS is based on a sample of hospitals in participating states (AHRQ, 2010). The number of participating states has risen over time from 8 in 1988 to 28 in 2000 and 44 in 2010. The major disadvantage of these data is that the set of hospitals is not stable over time, so the county-year database is a repeated cross-section rather than a true panel, and there may be random noise in the outcome variable that is related to changes in the sample. This variation may make it harder for us to detect effects of ambulatory EMR on outcomes (and is the reason that we focus on the Medicare data for our main analyses.)

Nevertheless, with roughly 8 million hospital stays each year, the NIS is the largest inpatient care database in the United States. It also has the major advantage, especially relative to the Medicare data, that it includes discharge information for patients regardless of payment sources, including private insurance, public insurance, and self-pay. We can therefore use the NIS data as an alternative source to measure the effects for the over 65 population and also to compare that population to other age groups in the general population.

Table 4.6: Differential Effects by Age

	18 to 39		40 to 64		65 to 74		75 Plus	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	PQI Count	PQI Rate	PQI Count	PQI Rate	PQI Count	PQI Rate	PQI Count	PQI Rate
	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se
Ambulatory EMR	-83.2	-3.05	-159.3	-15.9	-167.7**	-130.4*	-383.9***	-400.8**
	(60.7)	(8.42)	(137.9)	(25.9)	(76.3)	(76.7)	(143.9)	(166.3)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4686	4686	4716	4716	4394	4394	4383	4383
Counties	1083	1083	1089	1089	1001	1001	1000	1000

Notes: Weighted least squares with county population weights. Robust standard errors clustered at the county level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Sample restricted to county-year observations with at least 1,000 population in the demographic subgroup. Summary statistics on PQI Rates by subgroup are reported in Table F.2.

Table 4.6 reports estimates using NIS data on 4 age groups: 18 to 39, 40 to 64, 65 to 74 and 75 and older. The regression models, as above, are weighted by county population and include controls for changing county and state characteristics as well as year and county fixed effects. Robust standard errors are clustered at the county level and outcomes are reported for both PQI counts and rates (per 100,000 population). The sample sizes are substantially smaller for the NIS analysis compared to the Medicare analysis in the previous section because of limited coverage of counties in the NIS.

Across the columns of the table, the estimated coefficient for ambulatory EMR coverage is negative and increasing in magnitude for older ages. This indicates that, while there may be benefits from ambulatory EMR for younger patients, the largest gains accrue to older ones. Although older groups have higher baseline PQI rates, as shown in Table F.2, the effect increases in magnitude both in absolute terms and relative to baseline rates. The estimate in column 2 of Table 4.6 is equal to 1 percent of the mean PQI rate for 18 to 39-year-old patients, whereas the estimate in column 8 is equal to 7 percent of the mean PQI rate for patients over 75.

Table 4.7 shows estimated effects of ambulatory EMRs on quality of pediatric care. Because the PQI measures apply only to the adult population, this analysis uses the subset of AHRQ's "Pediatric Quality Indicators" (PDIs) related to preventable hospitalizations (e.g., asthma, diabetes, gastroenteritis, perforated appendix and urinary tract infection admission rates) for children aged 6 to 17. We constructed PDI counts using the NIS and another HCUP database: the Kids' Inpatient Database (KID). The KID is a sample of pediatric discharges - defined as discharges where the patient was age 20 or younger at admission - from community, non-rehabilitation hospitals in states participating in HCUP (AHRQ, 2008). Like the NIS, the number of states participating has risen over time (from 22 to 44 from 1997 to 2009 for the KID); the KID includes discharges for all payers; and data include patient characteristics, hospital characteristics and clinical information (diagnoses, procedures) associated with the inpatient stay. Unlike the NIS, which is available in every year, the KID is available every three years beginning with 1997. Our KID sample uses data from 2000, 2003, 2006, and 2009.

The estimated effects on PDIs are inconsistent in sign across the models and samples and not statistically significant. This lack of an effect for children may be caused by limitations in the samples and available measures or it may reflect the fact that ambulatory EMRs are more important for quality for adults, and especially older adults, rather than children.

Table 4.7: Ambulatory EMR and Pediatric Care

	NIS		KID	
	(1)	(2)	(3)	(4)
	PDI Count	PDI Rate	PDI Count	PDI Rate
	b/se	b/se	b/se	b/se
Ambulatory EMR	-55.1	-8.22	-50.8	3.37
	(42.2)	(6.67)	(39.1)	(5.63)
Local Controls	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	4622	4622	3552	3552
Counties	1084	1084	1058	1058

Notes: PDI count is the number of hospital admissions identified as part of the prevention subcategory of "Pediatric Quality Indicators" by AHRQ. Pediatric admissions data are from the Nationwide Inpatient Sample (NIS) or the Kids' Inpatient Database (KID). Weighted least squares with county population weights. Robust standard errors clustered at the county level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4.4 Implications

As the Office of the National Coordinator for Health Information Technology (ONC) describes it,"The nation has embarked upon an unprecedented effort to transform the flow of information in health care in order to improve the quality and efficiency of care." The goals of federal health IT investments are broad, including increasing the adoption of health IT; improving health care quality and access; supporting the delivery of high value care; and improving clinical and community services and population health (ONC, 2015). However, one area in general where there have been fewer incentives offered and less policy coordination around is ambulatory centers, as the focus of policy has been on hospitals and individual physicians. This is surprising, given the attractiveness of such ambulatory facilities in lowering the costs of routine healthcare relative to hospitals, and has led to proposed regulation such as the 2015 Electronic Health Fairness Act that attempt to further exempt ambulatory facilities from having to adopt healthcare IT.

To address this lack of policy focus on healthcare IT in ambulatory settings, the goal of this paper is to analyze the effects of health IT on ambulatory care quality in the US. We do so using a measure of quality derived from hospitalizations – the ACS rate – but which provides information on the quality of ambulatory care. We find significant reductions in ACS rates in an area following increased ambulatory HIT adoption in the area, suggesting quality improvements among older (65 and over) populations. This holds true across a range of specifications, including for sensitivity analyses that vary the sample of counties and outcome measure used in analysis. Results for other age groups are not statistically significant but our point

estimates for children, young adults (18-39), and those 40-64 suggest an inverse relationship between health IT adoption and ACS rates; however, the standard errors of the estimates are relatively large and thus the estimates lack statistical significance. Taken as a whole, our results suggest that health IT adoption yields improvements in quality, especially for the kind of older adults most likely to be affected by organizational incentives based on Medicare intake.

There are of course limitations to our study. First, our results are imprecise for younger populations and more research is needed to explore whether the benefits for younger populations (less than age 65) may be heterogeneous, and specifically be more pronounced for potentially vulnerable groups of patients such as the uninsured, publicly insured, low income or chronically ill. Second, we focus on basic EHR records and further research is also needed to explore how specific health IT components (e.g. practice management software, or PACS) may contribute to quality improvements in ambulatory care. Third, our findings focus on the subset of ambulatory providers who are affiliated with a larger health system, including by being owned, leased or managed by the system. Therefore, our findings provide an estimate of the effects of health IT on ambulatory care quality for ambulatory providers who may be more likely than others to be exchanging information with hospitals and other providers in their system because of the nexus of affiliation. It is possible that there may be smaller effects if ambulatory providers are less well placed to share information. ONC has focused on interoperability of health IT and information sharing of late, given the possibility that the benefits of health IT may grow exponentially through the added dimension of exchange, but this is not something we are able to study explicitly. Notwithstanding these limitations, our paper represents a useful first step in calibrating the benefits of electronic medical records for an area of practice which has not hitherto been the focus of policy attention.

Bibliography

Agency for Healthcare Research and Quality (AHRQ) (2001, October). *Guide to Prevention Quality Indicators* (3.1 ed.). Rockville, MD.

Agency for Healthcare Research and Quality (AHRQ) (2008). *Introduction to the HCUP Kids' Inpatient Database (KID), 2006*. Rockville, MD.

Agency for Healthcare Research and Quality (AHRQ) (2010). *Introduction to the HCUP Nationwide Inpatient Sample (NIS), 2008*. Rockville, MD.

Agency for Healthcare Research and Quality Quality Indicators (AHRQ QI) (2006, April). Prevention Quality Indicator (PQI) Composite Measure Workgroup Final Report.

Agha, L. (2014). The effects of health information technology on the costs and quality of medical care. *Journal of Health Economics 34*, 19 – 30.

Angst, C. M., R. Agarwal, V. Sambamurthy, and K. Kelley (2010). Social contagion and information technology diffusion: the adoption of electronic medical records in us hospitals. *Management Science 56*(8), 1219–1241.

Arday, S. L., D. R. Arday, S. Monroe, and J. Zhang (2000). HCFA's racial and ethnic data: current accuracy and recent improvements. *Health Care Financing Review 21*(4), 107–116.

Bailey, M. J. and A. Goodman-Bacon (2015). The War on Poverty's experiment in public medicine: Community health centers and the mortality of older Americans. *American Economic Review 105*(3), 1067–1104.

Bärnighausen, T. and D. E. Bloom (2009). Financial incentives for return of service in underserved areas: a systematic review. *BMC Health Services Research 9*(1), 86–102.

Billings, J., N. Parikh, and T. Mijanovich (2000, November). Emergency department use in New York City: a substitute for primary care? *Issue Brief (Commonwealth Fund)* (433), 1–5.

Billings, J., L. Zeitel, J. Lukomnik, T. S. Carey, A. E. Blank, and L. Newman (1993). Impact of socioeconomic status on hospital use in New York City. *Health Affairs 12*(1), 162–173.

Blumenthal, D., E. Mort, and J. Edwards (1995). The efficacy of primary care for vulnerable population groups. *Health Services Research 30*(1 Pt 2), 253–273.

Bodenheimer, T. (2008). Coordinating care – a perilous journey through the health care system. *New England Journal of Medicine 358*(10), 1064–1071.

Bolduc, D., B. Fortin, and M.-A. Fournier (1996). The effect of incentive policies on the practice location of doctors: a multinomial probit analysis. *Journal of Labor Economics 14*(4), 703–732.

Brailer, D. J. (2005). Interoperability: The Key To The Future Health Care System. *Health Affairs* (24), w5.19–21.

Buntin, M. B., M. F. Burke, M. C. Hoaglin, and D. Blumenthal (2011). The benefits of health information technology: a review of the recent literature shows predominantly positive results. *Health Affairs 30*(3), 464–471.

Calonico, S., M. D. Cattaneo, and R. Titiunik (2014). Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica 82*(6), 2295–2326.

Card, D., C. Dobkin, and N. Maestas (2008, December). The impact of nearly universal insurance coverage on health care utilization: Evidence from Medicare. *The American Economic Review 98*(5), 2242–2258.

CDC National Center for Health Statistics (2014a). Mortality – all-county, 1999–2010. Electronic Database.

CDC National Center for Health Statistics (2014b). Natality – all-county, 2010–2012. Electronic Database.

CDC National Center for Health Statistics (2014c). Period Linked Births/Infant Deaths – All-County, 1999–2009. Electronic Database.

Centers for Medicare and Medicaid Services (2014, July). Health professional shortage area (HPSA) physician bonus, HPSA Surgical Incentive Payment, and Primary Care Incentive Payment programs. Technical Report ICN 903196, Centers for Medicare and Medicaid Services.

Chan, L., L. G. Hart, T. C. Ricketts, and S. K. Beaver (2004). An analysis of Medicare's Incentive Payment program for physicians in health professional shortage areas. *The Journal of Rural Health 20*(2), 109–117.

Chang, C.-H., T. A. Stukel, A. B. Flood, and D. C. Goodman (2011). Primary care physician workforce and Medicare beneficiaries' health outcomes. *JAMA 305*(20), 2096–2104.

Chay, K. Y. and M. Greenstone (2003). The impact of air pollution on infant mortality: evidence from geographic variation in pollution shocks induced by a recession. *Quarterly Journal of Economics 118*(3), 1121–1167.

Coleman, E. A. and R. A. Berenson (2004). Lost in transition: challenges and opportunities for improving the quality of transitional care. *Annals of Internal Medicine 141*(7), 533–536.

Crouse, B. J. and R. L. Munson (2006). The effect of the physician J-1 visa waiver on rural Wisconsin. *Wisconsin Medical Journal 105*(7), 16–20.

Currie, J. and J. Grogger (2002). Medicaid expansions and welfare contractions: offsetting effects on prenatal care and infant health? *Journal of Health Economics 21*, 313–335.

Currie, J. and E. Tekin (2015). Is there a link between foreclosure and health? *American Economic Journal: Economic Policy 7*(1), 63–94.

Dafny, L. and J. Gruber (2005). Public insurance and child hospitalizations: access and efficiency effects. *Journal of Public Economics 89*(1), 109–129.

Davies, S. M., K. M. McDonald, E. Schmidt, E. Shultz, J. Geppert, and P. S. Romano (2009, November). Expanding use of the AHRQ prevention quality indicators: Report on the clinical expert review panel.

Eicheldinger, C. and A. Bonito (2008). More accurate racial and ethnic codes for Medicare administrative data. *Health Care Financing Review 29*(3), 27–42.

Evans, W. N. and D. S. Lien (2005). The benefits of prenatal care: evidence from the PAT bus strike. *Journal of Econometrics 125*, 207–239.

Eysenbach, G. (2000, June). Recent advances: Consumer health informatics. *British Medcal Journal 320*(7251), 1713–1716.

Fan, J. and I. Gijbels (1996). *Local polynomial modelling and its applications: monographs on statistics and applied probability*, Volume 66. CRC Press.

Finkelstein, A., S. Taubman, B. Wright, M. Bernstein, J. Gruber, J. P. Newhouse, H. Allen, K. Baicker, et al. (2012). The Oregon health insurance experiment: Evidence from the first year. *The Quarterly Journal of Economics 127*(3), 1057–1106.

Freedman, S., H. Lin, and J. Prince (2014). Information technology and patient health: An expanded analysis of outcomes, populations, and mechanisms. *Available at SSRN 2445431*.

Gelman, A. and G. Imbens (2014). Why high-order polynomials should not be used in regression discontinuity designs. NBER Working Paper 20405.

Georgia Department of Public Health Office of Health Indicators for Planning (2015). Online analytical statistical information system (OASIS). Online database. Version 2.9.4.

Gold, R. B. (2011). The National Health Service Corps: An answer to family planning centers' workforce woes? *Guttmacher Policy Review 14*(1), 11–15.

Goodman, D. C., S. Brownlee, C.-H. Chang, and E. Fisher (2010, September). Regional and racial variation in primary care and the quality of care among Medicare beneficiaries. Technical report, Dartmouth Institute for Health Policy and Clinical Practice. A Report of the Dartmouth Atlas Project.

Government Accountability Office (2006a, November). Data on use of J-1 visa waivers needed to better address physician shortages. Technical Report GAO-07-52.

Government Accountability Office (2006b, October). Health professional shortage areas: Problems remain with primary care shortage area designation system. Technical Report GAO-07-84.

Gresenz, C. R., J. Rogowski, and J. J. Escarce (2007). Health care markets, the safety net, and utilization of care among the uninsured. *Health services research 42*(1p1), 239–264.

Hagopian, A., M. J. Thompson, E. Kaltenbach, and L. G. Hart (2003). Health departments' use of international medical graduates in physician shortage areas. *Health Affairs 22*(5), 241–249.

Hayes, K., K. Bloniarz, and K. Smalley (2013, March). Medicare's health professional shortage areas (HPSA) payment adjustment. PowerPoint Presentation. MedPAC.

Health Resources and Services Administration (2003, May). Criteria for determining priorities among Health Professional Shortage Areas. *Federal Register*, 32531–32533. Notice.

Health Resources and Services Administration (2011, November). Lists of designated primary medical care, mental health, and dental health professional shortage areas. *Federal Register 78*(213), 68198–68199. Notice.

Health Resources and Services Administration (2015). Justifications of estimates for appropriations committees. Technical report.

Health Resources and Services Administration (2016). HRSA data warehouse: Health professional shortage areas (HPSAs). Online database. Accessed April 13, 2016.

Hillestad, R., J. Bigelow, A. Bower, F. Girosi, R. Meili, R. Scoville, and R. Taylor (2005, Sep-Oct). Can electronic medical record systems transform health care? Potential health benefits, savings, and costs. *Health Affairs 24*(5), 1103–17.

Hing, E., C. Burt, and D. Woodwell (2007). Electronic medical record use by office-based physicians and their practices: United States, 2006. *Advance Data from Vital and Health Statistics* (393), 1–6.

Holmes, G. M. (2004). Does the National Health Service Corps improve physician supply in underserved locations? *Eastern Economic Journal 30*(4), 563–581.

Imbens, G. and K. Kalyanaraman (2012, July). Optimal bandwidth choice for the regression discontinuity estimator. *The Review of Economic Studies 79*(3), 933–959.

Institute of Medicine (IOM) (2009). *State of the USA Health Indicators: Letter Report*. Washington, DC: The National Academies Press.

Jha, A. K., T. G. Ferris, K. Donelan, C. DesRoches, A. E. Shields, S. Rosenbaum, and D. Blumenthal (2006). How common are electronic health records in the united states? a summary of the evidence. *Health Affairs 25*(6), w496–w507.

Jiang, H. J., C. A. Russo, and M. L. Barrett (2009). Nationwide frequency and costs of potentially preventable hospitalizations, 2006. HCUP Statistical Brief 72, U.S. Agency for Healthcare Research and Quality, Rockville, MD.

Johnson, K. E., E. Kaltenbach, K. Hoogstra, M. J. Thompson, A. Hagopian, and L. Hart (2003, August). How international medical graduates enter US graduate medical education or employment. *WWAMI Center for Health Workforce Studies* (Working Paper 76).

Jones, S. S., J. L. Adams, E. C. Schneider, J. S. Ringel, and E. A. McGlynn (2010). Electronic health record adoption and quality improvement in US hospitals. *American Journal of Managed Care 16*(12), SP64–SP71.

Kahn, T. R., A. Hagopian, and K. Johnson (2010). Retention of J-1 visa waiver program physicians in Washington state's health professional shortage areas. *Academic Medicine 85*(4), 614–621.

Kaplan, B., P. F. Brennan, A. F. Dowling, C. P. Friedman, and V. Peel (2001). Toward an informatics research agenda: Key people and organizational issues. *Journal of the American Medical Informatics Association 8*(3), 235–241.

Kern, L. M., R. Dhopeshwarkar, Y. Barrón, A. Wilcox, H. Pincus, and R. Kaushal (2009). Measuring the effects of health information technology on quality of care: a novel set of proposed metrics for electronic quality reporting. *Joint Commission Journal on Quality and Patient Safety 35*(7), 359–369.

Kohrs, F. P. and A. Mainous III (1995). The relationship of health professional shortage areas to health status. implications for health manpower policy. *Archives of Family Medicine 4*(8), 681–685.

Kolstad, J. T. and A. E. Kowalski (2012). The impact of health care reform on hospital and preventive care: evidence from Massachusetts. *Journal of Public Economics 96*(11), 909–929.

Konrad, T. R., R. T. Slifkin, C. Stevens, and J. Miller (2000). Using the American Medical Association physician masterfile to measure physician supply in small towns. *The Journal of Rural Health 16*(2), 162–167.

Kotelchuck, M. (1994, September). An evaluation of the Kessner Adequacy of Prenatal Care Index and a proposed Adequacy of Prenatal Care Utilization Index. *American Journal of Public Health 84*(9), 1414–1420.

Krakauer, H., I. Jacoby, M. Millman, and J. Lukomnik (1996). Physician impact on hospital admission and on mortality rates in the Medicare population. *Health Services Research 31*(2), 191.

Laditka, J. N., S. B. Laditka, and J. C. Probst (2005). More may be better: evidence of a negative relationship between physician supply and hospitalization for ambulatory care sensitive conditions. *Health services research 40*(4), 1148–1166.

Lauderdale, D. S. and J. Goldberg (1996). The expanded racial and ethnic codes in the Medicare data files: their completeness of coverage and accuracy. *American Journal of Public Health 86*(5), 712–716.

Lee, D. S. and T. Lemieux (2010). Regression discontinuity designs in economics. *Journal of Economic Literature 48*, 281–355.

Lee, J., Y.-F. Kuo, and J. S. Goodwin (2013). The effect of electronic medical record adoption on outcomes in US hospitals. *BMC Health Services Research 13*(1), 1–7.

Lin, Y.-K., M. Lin, and H. Chen (2014). Beyond adoption: Does meaningful use of EHR improve quality of care? *Available at SSRN 2444054*.

Liu, J. (2007). Health professional shortage and health status and health care access. *Journal of Health Care for the Poor and Underserved 18*(3), 590–598.

Macinko, J., B. Starfield, and L. Shi (2007). Quantifying the health benefits of primary care physician supply in the United States. *International Journal of Health Services 37*(1), 111–126.

McCrary, J. (2008). Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of Econometrics 142*(2), 698–714.

McCullough, J. S., S. Parente, and R. Town (2013, January). Health information technology and patient outcomes: The role of organizational and informational complementarities. Working Paper 18684, National Bureau of Economic Research.

McLafferty, S., V. L. Freeman, R. E. Barrett, L. Luo, and A. Shockley (2012). Spatial error in geocoding physician location data from the AMA Physician Masterfile: implications for spatial accessibility analysis. *Spatial and Spatio-temporal Epidemiology 3*(1), 31–38.

Miller, A. R. and C. Tucker (2009). Privacy protection and technology diffusion: The case of electronic medical records. *Management Science 55*(7), 1077–1093.

Miller, A. R. and C. Tucker (2014a). Health information exchange, system size and information silos. *Journal of Health Economics 33*, 28–42.

Miller, A. R. and C. E. Tucker (2011). Can health care information technology save babies? *Journal of Political Economy 119*(2), 289–324.

Miller, A. R. and C. E. Tucker (2014b). Electronic discovery and the adoption of information technology. *Journal of Law, Economics, and Organization 30*(2), 217–243.

Mueller, K. J. et al. (2002, April). The immediate and future role of the J-1 visa waiver program for physicians: The consequences of change for rural health care service delivery. (P2002-3). Special J-1 Visa Waiver Program Task Force.

National Health Service Corps (2015, March). National Health Service Corps site reference guide. Technical report, Health Resources and Services Administration, Bureau of Health Workforce.

Negotiated Rulemaking Committee on the Designation of Medically Underserved Populations and Health Professional Shortage Areas (2011, October). Final Report to the Secretary. Technical report, Health Resources and Services Administration.

Nichols, A. (2007). Causal inference with observational data. *Stata Journal 7*(4), 507–541.

Nichols, A. (2011). rd 2.0: Revised Stata module for regression discontinuity estimation. Software. http://ideas.repec.org/c/boc/bocode/s456888.html.

Office of the National Coordinator for Health Information Technology (2015, January). Federal Health IT strategic plan 2015-2020: Quick reference factsheet. World Wide Web.

Opoku, S. T., B. A. Apenteng, G. Lin, L.-W. Chen, D. Palm, and T. Rauner (2015). A comparison of the J-1 visa waiver and loan repayment programs in the recruitment and retention of physicians in rural Nebraska. *The Journal of Rural Health*.

Parchman, M. L. and S. D. Culler (1999). Preventable hospitalizations in primary care shortage areas: an analysis of vulnerable Medicare beneficiaries. *Archives of Family Medicine 8*(6), 487–491.

Pathman, D. E., G. E. Fryer, L. A. Green, and R. L. Phillips (2005). Changes in age-adjusted mortality rates and disparities for rural physician shortage areas staffed by the National Health Service Corps: 1984–1998. *The Journal of Rural Health 21*(3), 214–220.

Pathman, D. E. and T. R. Konrad (2012). Growth and changes in the National Health Service Corps (NHSC) workforce with the American Recovery and Reinvestment Act. *The Journal of the American Board of Family Medicine 25*(5), 723–733.

Peterson, L. E., R. L. Phillips, J. C. Puffer, A. Bazemore, and S. Petterson (2013). Most family physicians work routinely with nurse practitioners, physician assistants, or certified nurse midwives. *The Journal of the American Board of Family Medicine 26*(3), 244–245.

Primary Care Office (MD) (2011, October). 2010 primary care needs assessment. Technical report, Office of Health Policy and Planning, Family Health Administration, Department of Health and Mental Hygiene (MD).

Reichman, N., H. Corman, K. Noonan, and D. Dave (2009, July). Infant health production functions: what a difference the data make. *Health Economics 18*(7), 761–782.

Ricketts, T. C., L. J. Goldsmith, G. M. Holmes, M. Randy, R. Lee, D. H. Taylor, and J. Ostermann (2007). Designating places and populations as medically underserved: a proposal for a new approach. *Journal of Health Care for the Poor and Underserved 18*(3), 567–589.

Ricketts, T. C. and G. M. Holmes (2007). Mortality and physician supply: does region hold the key to the paradox? *Health Services Research 42*(6p1), 2233–2251.

Salinsky, E. (2010). Health care shortage designations: HPSA, MUA, and TBD. National Health Policy Forum.

Shi, L. (1992). The relationship between primary care and life chances. *Journal of Health Care for the Poor and Underserved 3*(2), 321–335.

Simon, S. R., R. Kaushal, P. D. Cleary, C. A. Jenter, L. A. Volk, E. J. Orav, E. Burdick, E. G. Poon, and D. W. Bates (2007). Physicians and electronic health records: a statewide survey. *Archives of Internal Medicine 167*(5), 507–512.

Skinner, J. S., D. J. Gottlieb, and D. Carmichael (2011, June). A new series of Medicare expenditure measure by Hospital Referral Region: 2003-2008. Technical report, Dartmouth Institute for Health Policy and Clinical Practice. A Report of the Dartmouth Atlas Project.

Sonchak, L. (2015, December). Medicaid reimbursement, prenatal care and infant health. *Journal of Health Economics 44*, 10–24.

Spetz, J., J. F. Burgess, and C. S. Phibbs (2014). The effect of health information technology implementation in Veterans Health Administration hospitals on patient outcomes. *Healthcare 2*(1), 40–47.

Starfield, B., L. Shi, A. Grover, and J. Macinko (2005). The effects of specialist supply on populations' health: assessing the evidence. *Cancer 103*(20.93), 23–18.

Starfield, B., L. Shi, and J. Macinko (2005). Contribution of primary care to health systems and health. *Milbank Quarterly 83*(3), 457–502.

Tang, P. C. and C. Newcomb (1998). Informing patients: A guide for providing patient health information. *Journal of the American Medical Informatics Association 5*(6), 563–570.

Texas Department of State Health Services (2015). Texas hospital inpatient discharge public use data file, 1999q1 – 2008q4. Online database.

US Department of Health and Human Services, Health Resources and Services Administration, Bureau of Health Workforce (2013-2014). *Area Health Resources Files (AHRF)*. Rockville, MD.

Weissman, J. S., C. Gatsonis, and A. M. Epstein (1992). Rates of avoidable hospitalization by insurance status in Massachusetts and Maryland. *JAMA 268*(17), 2388–2394.

Wood, S., T. Beeson, B. Bruen, D. G. Goldberg, H. Mead, P. Shin, and S. Rosenbaum (2014, February). Scope of family planning services available in Federally Qualified Health Centers. *Contraception 89*(2), 85–90.

Zerehi, M. R. (2008). How is a shortage of primary care physicians affecting the quality and cost of medical care?: A comprehensive evidence review. Technical report, American College of Physicians.

Zhan, C., M. R. Miller, H. Wong, and G. S. Meyer (2004). The effects of HMO penetration on preventable hospitalizations. *Health Services Research 39*(2), 345–361.

Appendix A

Counting FTE Physicians

A.1 HRSA Guidelines

The guidelines developed for HRSA for counting physicians in a potential HPSA are to include doctors of allopathic (MD) and osteopathic (DO) medicine who provide patient care and who practice in one of the following specialties:

1. general practice,
2. family medicine,
3. general internal medicine
4. general pediatrics, and
5. obstetrics/gynecology

Physicians who are either federal employees (e.g. employees of the Indian Health Service or the Public Health Service) are excluded, as are physicians completing service commitments in the National Health Service Corps. Physicians are counted by full-time-equivalents (FTE) based on the number of weekly hours worked in HPSAs. One FTE is considered to be 40 or more hours, so no physician can supply more than one FTE. Interns and residents in primary care specialties are counted as 0.1 FTE.

The county-level physician data in the Area Health Resources File (AHRF) are sufficiently granular to allow the construction of a county-level physician count measure that closely matches the HRSA definition (physicians engaged in patient care who practice in one of the five specialties outlined above). Using de-identified data on National Health Service Corps members including work site, HPSA number, and start and end dates supplied on request by HRSA, I subtract NHSC member physicians from the physician count

calculated for the HPSA in which they work. Because it is unclear as of what date the physician data for a particular year in the AHRF are reported, I count the number of NHSC members in a county as of July 1 of each year.

A.2 Incorporating Osteopathic Physicians

The AHRF includes more complete data on MDs than on osteopathic physicians. First, the MD data are disaggregated by both specialty and professional activity, but the DO data are only presented by specialty, with accompanying data on the total number of DOs who are "active." Second, data on MDs are available for each year between 1995 and 2008, and between 2010 and 2012. By contrast, DO count data are only available for 1998, 2001, 2003, 2004, 2007, and 2010 through 2012.

Specialty refers to e.g. family medicine, internal medicine, or general surgery, and professional activity refers to e.g. patient care, research, or teaching. I count MDs as described above (those engaged in total patient care and in the specialties counted by HRSA). I count DOs as the sum of all physicians identified to be in the five specialties, regardless of (unobserved) professional activity. In the years for which DO counts are unavailable, I linearly interpolate the number using the previous and following year for which data are reported. To correct partially for the inability to restrict the DO count to patient care, I adjust the DO count downward to the total number of "active" DOs where it is less than the reported number of DOs in primary care specialties.

A.3 Estimating Physician FTEs

The ratio of physician FTEs to physician counts is calculated by estimating the joint distribution of physician age and sex in a county in a year and weighting the number of physicians in each age-sex category using and estimate of average physician FTEs taken from the American Community Survey (ACS).

For MDs (but not DOs), the AHRF reports the number of physicians in each specialty broken down by age. The six age categories are: less than 35; 35-44; 45-54; 55-64; 65-74; and 75 or older. Using these data, I assembled a dataset containing the proportions of primary care MDs (defined using the specialties enumerated above) in each of these age categories in each county in each year. Denote these proportions as $f_{ctg} = \{f_{ct1}, f_{ct2}, \ldots, f_{ct6}\}$, where f is the fraction of physicians in county c and year t that are in age group g.

Separately, the AHRF provides the total number of MDs (not restricted to primary care) by gender in each county and year. Denote the fraction of physicians in county c and year t for which sex is equal to s as f_{cts}. To estimate physician FTEs, f_{ctg} and f_{cts} are assumed to be the marginal distributions of physician

sex and age, respectively. These assumptions could be inappropriate if the age distribution of primary-care DOs is very different from the age distribution of MDs in a county, or if the sex distribution of primary care physicians (MDs and DOs) in a county is very different from the sex distribution of all MDs (primary care and otherwise) in the county.

The average number of FTEs per physician as a function of age and sex is calculated from the 2005-2009 5-year American Community Survey (ACS). The ACS contains records for 46,336 people with an occupation code of "Physician or surgeon," each of which contains the age, gender, employment status, and usual hours worked per week. The mean number of hours per week is 49, and in fact 68 percent of employed physicians in the survey work more than 40 hours per week. By HRSA rules, no physician can be counted for more than 1.0 FTE, so individual physician FTE is calculated as follows:

$$fte = \max\left\{\frac{hours}{40}, 1.0\right\}$$

Mean physician FTEs are calculated by sex and by age group, using the same age group definitions used in the AHRF. Denote the mean FTE for physicians in age group g and sex s as ϕ_{sg}. The joint age-sex distribution is also taken from the ACS data. Denote the fraction of physicians in the ACS who are in age group g and sex s as F_{sg}. Overall, mean FTEs were moderately higher for male physicians than for females across age categories, but the fraction of physicians who are female varies considerably by age. In the ACS, women comprise 48 percent of physicians under age 35 but less than 7 percent of those above age 75.

By using the average FTEs calculated from the 2005-2009 ACS, I implicitly assume that primary care physician labor supply conditional on age and sex is relatively constant over my sample period, and that estimates based on the universe of "physicians and surgeons" can be reasonably applied to primary care physicians. To test the this assumption, I compared the average FTEs for physicians working in versus outside of metropolitan areas in the ACS. The results were not materially different, except that the estimates for rural physicians were less precise because approximately 90 percent of the sampled physicians live in urban areas.

As stated above, the procedure for estimating FTEs at the county-year level requires first estimating the county-year-specific joint distribution of physician sex and age. Denote the fraction of physicians in county c in year t who are of sex s and age group g as f_{ctsg}. Using this notation, the county-year estimate of physician FTEs can be expressed as follows, where D_{ct} is the total number of physicians in the county:

$$fte_{ct} = D_{ct} \cdot \sum_{s \in \{m,f\}} \sum_{g=1}^{6} f_{ctsg} \cdot \phi_{sg}$$

The remainder of this appendix describes how the county-level joint age-sex distribution is estimated using the county-level marginal distribution of age and sex, together with the national-level joint distribution from the ACS. When the AHRF data indicate that all MDs in a county are of the same sex, the ACS weights used are the appropriate sex-specific weights, that is, $f_{ctsg} = f_{ctg}$.

When all MDs are not of the same sex, the joint age-sex distribution is approximated using the county-level marginal distributions of age and sex and assuming that their interaction in each county parallels the interaction of physician age and sex observed in the ACS. Specifically, the following sequential procedure is used:

1. For the first 5 of the 6 age groups, the fraction of physicians in age-sex category $\{sg\}$ is calculated as follows: $f_{ctsg} = f_{ctg} \cdot F_{sg} \cdot (F_{sg} + F_{s^-g})^{-1}$, where s^- denotes the opposite sex of s. In words, the fraction is estimated as if county c in year t had a unique age distribution but exhibited the same sex distribution within each age group as the entire ACS sample.

2. In the last age group (age group 6, corresponding to physicians over age 75), the sex distribution is calculated to align the estimated joint age-sex distribution to the observed marginal sex distribution. Using the same notation as above, where f_{cts} is the fraction of county c physicians in year t whose sex is s, we have:
$$f_{cts6} = f_{cts} - \sum_{g=1}^{5} f_{ctsg}$$

3. The preceding two steps solve twelve equations in twelve unknowns. In many cases, it is only possible to match the county-level age distribution, the county-level sex distribution, and the ACS-derived interaction between age and sex if one of the of the age group 6 estimates ($f_{cts6}, s \in \{m, f\}$) is negative. When this occurs, its value is reassigned to zero and the other estimates f_{ctsg} are rescaled so that they sum to one for county c.

The rescaling in the third step, above, results in estimated joint distributions that do not exactly match their observed marginal distributions by age, nor their marginal distributions by sex, nor the ACS-derived interaction between physician age and sex. There are alternative procedures for estimating physician FTEs that could match two of the above. For example, if physician age were assumed to be independent of sex, then the joint distribution would simply be the product of marginal distributions ($f_{ctsg} = f_{cts} \cdot f_{ctg}$) The

estimated joint distribution would therefore align perfectly with the marginal distributions, but it would not match the strong interaction between age and sex observed in the ACS. The approach taken here incorporates all of the information available to estimate the joint distribution and yields a reasonably close approximation to both marginal distributions and to the interaction.

Appendix B

Prevention Quality Indicators

The complete list of Prevention Quality Indicators (PQIs) is presented in Table B.1. All PQIs are calculated for patients aged 18 or older. The acute composite PQI rate (PQI 92) is defined as the sum of PQI 10, PQI 11, and PQI 12 (dehydration, bacterial pneumonia, and urinary tract infection). The chronic composite rate is the sum of PQIs 1, 3, 5, 7, 8, 13, 14, 15, and 16 (all PQIs other than the acute conditions, perforated appendix [2], and low birth weight [9]). The overall composite is the sum of acute and chronic composite rates. The perforated appendix rate and low birth weight rates are excluded from the composites because they are not divided by county population. The perforated appendix rate is defined as the fraction of hospital admissions for appendicitis for which the appendix is ruptured, and the low birth weight rate is defined as live births with birth weight less than 2,500 grams as a fraction of total live births.

Table B.1: List of Prevention Quality Indicators

PQI Number	AHRQ Description
01	Diabetes Short-term Complications
02	Perforated Appendix
03	Diabetes Long-term Complications
05	Chronic Obstructive Pulmonary Disease (COPD) or Asthma in Older Adults
07	Hypertension
08	Heart Failure
09	Low Birth Weight
10	Dehydration
11	Bacterial Pneumonia
12	Urinary Tract Infection
13	Angina Without Procedure
14	Uncontrolled Diabetes
15	Asthma in Younger Adults
16	Lower-Extremity Amputation among Patients with Diabetes
90	Prevention Quality Overall Composite
91	Prevention Quality Acute Composite
92	Prevention Quality Chronic Composite

Source: Agency for Healthcare Research and Quality (AHRQ).

Appendix C

Fixed Effects and Event Study Specifications (ACSH)

C.1 Empirical Approach

I specify a linear regression equation with the ACSH rate on the left-hand side and HPSA designation on the right-hand side, along with county-level controls and year and county fixed effects. The specification is presented in Equation C.1, where h_{it} is a binary indicator of whole-county HPSA designation and the outcome y_{it} is the rate of ACSH per 100,000 members of a particular segment of the Medicare-insured population in county i and year t. The vector $\mathbf{Z}_{it}$ is a set of county-level, time-varying control variables, e_i is the unobserved county fixed effect capturing the combined effects of all time-invariant county characteristics on the ACSH rate, u_t is the year fixed effect capturing all factors that vary over time uniformly among all counties, and ϵ_{it} is an unobserved idiosyncratic error term. This is the familiar fixed effects regression specification. When estimating the model, standard errors are clustered at the county level and observations are weighted by the number of Medicare Part A beneficiaries in the county.

$$y_{it} = \beta_0 + \beta_1 \cdot h_{it} + \mathbf{Z}_{it}\gamma + e_i + u_t + \epsilon_{it} \tag{C.1}$$

The control variables in $\mathbf{Z}_{it}$ include county median household income, unemployment rate, poverty rate, total county population, the share of the county population over age 65, and four measures of government income support to county residents in dollars per capita. The regression also controls for the fraction of the county population that lives in non-whole-county geographic HPSAs and population group HPSAs. These controls are included because the existence of non-whole-county HPSAs implies that counties without whole-county designation are "partially treated" with HPSA designation. Failure to account for this partial treatment will bias the estimated effect of designation toward zero.

I next estimate the relationship between county HPSA designation and the ACS rate by an event study around the year of HPSA designation. The model is closely based on the one used by Bailey and Goodman-Bacon in their 2015 study (hereafter BGB, 2015) of a related question: the effect of community health center introduction on the mortality rate. Like the programs attached to HPSA designation, community health centers focus on delivering primary care to vulnerable populations that might otherwise lack access. The mortality rate, like the ACSH rate, will increase in response to prolonged deprivation of needed health care in a community. In both settings, the effect of the program is likely to be delayed due to setup time and because not everyone will visit a new clinic or physician immediately when it becomes available. The event study employed by BGB, 2015 and adapted here is well suited to the study of programs with delayed effects.

The event study specification entails regressing the outcome y_{it} on a vector of dummy variables indicating the number of years prior to or since HPSA designation, along with county controls and fixed effects. I present the specification in equation C.2. This is modeled on the main specification in BGB, 2015 (Equation 1, p. 1080). The outcome y_{it} is the (one-year) ACSH rate in the county for the segment of Medicare beneficiaries being considered, e_i is the county fixed effect as before, $\delta_{s(i)t}$ is either a year fixed effect or a county-specific linear time trend. The vector $\mathbf{Z}_{it}$ contains the same control variables as in the fixed effects regression. The two summations are identical but omit $j = -1$. D_i is a binary indicator that equals one if county i ever receives a whole-county HPSA designation, and T_i^* is the year in which the county is designated. The two summations serve to include in the regression a vector of dummy variables indicating, for each county-year observation, the number of years until or since whole-county HPSA designation. As with the fixed effects regression, standard errors are clustered at the county level and the regression is weighted by the number of Medicare Part A beneficiaries.

$$y_{it} = e_i + \delta_{s(i)t} + \mathbf{Z}_{it}\beta + \sum_{j=-6}^{-2} \pi_j D_i \mathbb{1}(t - T_i^* = j) + \sum_{j=0}^{6} \tau_j D_i \mathbb{1}(t - T_i^* = j) + \epsilon_{it} \tag{C.2}$$

Following BGB, 2015, the dummy variable indicating that an county-year observation is six years prior to designation is set equal to one for all observations that are six *or more* years prior to designation. Likewise, the dummy variable indicating that a county was designated six years previously is set equal to one for all counties designated at least six years previously. The coefficient on each dummy variable captures the average difference in the ACSH rate between those counties a particular number of years from designation and those counties that are never designated – adjusted for covariates and *relative* to the corresponding difference in ACSH rates for counties the year prior to designation (since $j = -1$ is omitted). The output of

the event study specification therefore reveals the timing of changes in relative ACS admission rates relative to the timing of designation.

Identification is achieved under the same conditions for the fixed effects model as for the event study. Both include an error term consisting of the combined effects of all time-varying county-specific factors that affect the ACSH rate. The effect of HPSA designation (or of time since HPSA designation) is identified if it is uncorrelated with any of the elements of ϵ_{it} conditional on the other covariates in the model. In fact, this is likely to be violated with these specifications because they do not include a control for the number of primary care physicians per capita in the county. This variable is undeniably negatively correlated with whole-county HPSA designation (see the scatter plot in panels (c) and (d) of Figure 2.2), and both the definition of an ambulatory care sensitive hospitalization and the findings of Chang et al. (2011) would suggest that ACSH vary inversely with the number of primary care physicians. In light of this, the reason I do not control for physicians is because the programs attached to HPSA designation are primarily designed to attract physicians and other health professionals. Increased local availability of physicians is presumably a major channel through which HPSA designation affects outcomes. If so, then to control for the number of physicians would bias the effects of designation toward zero.

Even if physicians are controlled for (as they are in unreported specifications to little effect), the identifying assumption could be violated if, for example, the productivity of local public health providers and administrators changes differentially over time and higher productivity results in both fewer ACSH and a higher probability of HPSA designation as existing providers are better able to both deliver improved primary care and bear the burden of applying for HPSA designation. In that case, the estimated effect of HPSA designation would be biased downward (i.e., biased in the direction of finding that designation reduces the ACSH rate when in fact both designation and ACSH are driven by the unobservable productivity of the health care provider/administrator). Alternatively, it could be that counties apply for HPSA designation when confronted with an adverse shock to health care delivery, such as the closure of a clinic in a neighboring county or the departure of non-physician providers. In this scenario, the adverse shock drives both an increase in HPSA designation and an increase in ACSH, biasing the estimated effect of designation in the opposite direction.

As Bailey and Goodman-Bacon discuss, the event study specification allows a check of the plausibility of the identifying assumption. The regression produces estimates of differential changes in the outcome variable between the treated and untreated groups before the treatment is applied. If there are different trends prior to the application of the treatment, that suggests that the two groups differ along unobservable dimensions

in ways that affect the outcome variable, and it would therefore be problematic to ascribe any observed differences following treatment application to the effect of the treatment itself. Certainly, the absence of a pre-treatment trend does not imply that the identifying assumption holds, but the presence of such a trend casts doubt on the assumption.

C.2 Results

The coefficient estimates for the fixed effects specification given in Equation C.1 appear in column (1) of Table C.1. The coefficient on whole-county HPSA designation is positive, and the explanation for this finding is demonstrated in columns (2) and (3) of the same table. The estimate in column 1 is generated by within-county variation over time in whole-county designation status. "Variation" entails either gaining or losing designation. In fact, there is little overlap in the sample between counties that gain designation and those that lose designation between 1995 and 2015. In column (2), the model is re-estimated using only the balanced panel of counties that do not experience a loss of designation (meaning all variation used to estimate the parameters is driven by gains in designation). Column (3) analogously reports the results of estimating the model using only counties that never gain designation, for whom all variation used to estimate the model results from losses in designation. Columns (2) and (3) together clarify that the estimate in column (1) is in fact a pooled average of two opposite-signed effects for distinct subsets of the data. Column (2) suggests that counties that gain designation experience a simultaneous decrease in ACSH rates after controlling for covariates, whereas column (3) suggests that, for counties that lose designation, the ACSH rate falls sharply when the designation is withdrawn (resulting in a positive correlation between HPSA designation and ACSH).

The results of the event study specifications (equation C.2) are presented in Table C.2. Column (1) reports the results of an event study around HPSA designation, and column (3) reports an event study around loss of HPSA designation. The results in columns (1) and (3) are presented in panels (a) and (b) of C.1, respectively.[1] To interpret the estimates, consider the second row of column (1). The estimate of 292.5 indicates that counties that will be designated five years later exhibit an ACSH rate that is 292.5 units higher than counties that will are never designated, *relative* to the difference between these same two groups of counties the year prior to designation, and controlling for the county and year fixed effects and covariates. This difference is statistically significant at the 5% level, and indicates (together with the other estimates in column [1]) that counties that become designated experience relative improvements in ACSH rates prior to designation. The results in column (3) reveal that counties that lose HPSA designation also experience

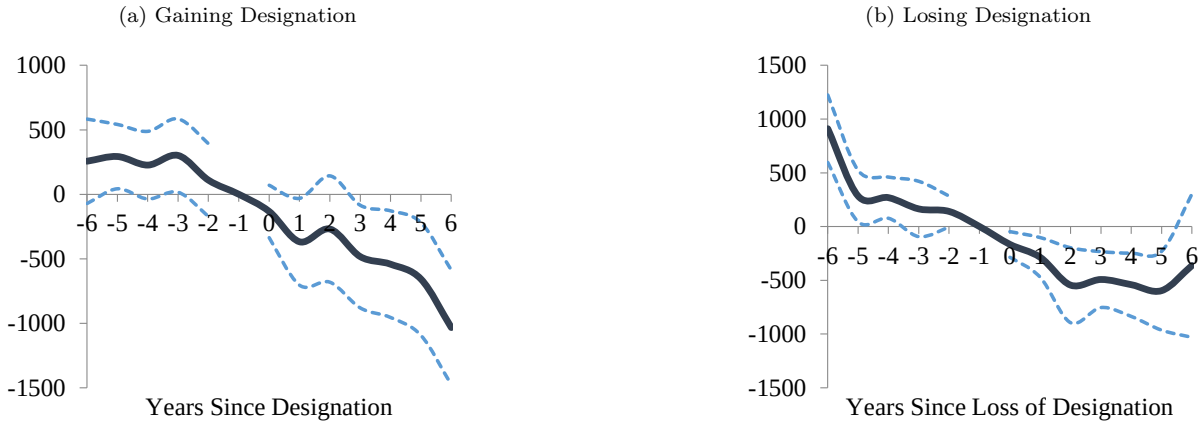
[1]The results in columns (2) and (4) are discussed below.

Table C.1: Fixed Effects Estimates of HPSA Designation on All-Cause ACSH Rate without County-Year Trends

	(1) All Counties	(2) Never Losing Desig.	(3) Never Gaining Desig.
Whole-County HPSA Designated	99.9	-533.6***	584.0***
	(90.7)	(103.0)	(116.4)
Unemployment Rate (%)	49.6***	44.4**	45.1**
	(18.2)	(19.0)	(19.7)
Median Household Income ($)	0.064***	0.064***	0.064***
	(0.011)	(0.011)	(0.011)
Poverty Rate (%)	14.6	12.4	13.6
	(11.8)	(12.8)	(12.9)
Total County Population	-0.00095	-0.0012	-0.0012
	(0.00072)	(0.00074)	(0.00074)
Percent of County Pop Aged 65+	-142.0***	-128.3***	-129.2***
	(42.1)	(44.7)	(44.9)
Retirement and Disability Insurance	0.29**	0.25**	0.27**
	(0.12)	(0.12)	(0.12)
SSI Benefits	-1.85	-1.54	-1.66
	(1.53)	(1.62)	(1.69)
Earned Income Tax Credit Payments	-5.06***	-4.69***	-4.70***
	(1.45)	(1.47)	(1.47)
Food Stamp (SNAP) Benefits	1.38**	1.42**	1.29*
	(0.67)	(0.69)	(0.72)
Other Income Benefits e.g. TANF, WIC	0.15	0.19	0.12
	(0.26)	(0.27)	(0.27)
Fraction of Pop with non-County HPSA	-287.5	-300.8	-272.1
	(288.5)	(311.2)	(303.8)
Fraction of Pop with Population HPSA	-730.5***	-628.2**	-801.0***
	(230.9)	(270.8)	(270.2)
County Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Observations	30681	26833	25465
Counties	3097	2707	2567

Notes: Standard errors in parentheses reflect clustering at county level. Regressions weighted by population of Medicare Part A beneficiaries. Column 2 excludes counties that lose a HPSA designation between 1995 and 2015. Column 3 excludes counties that never gain HPSA designation over that period.
* $p < .10$; ** $p < .05$; *** $p < .01$

Figure C.1: Event Studies Around Gain and Loss of Whole-County Designation: Total ACSH, No County Time Trends



Left panel plots event estimates reported in column (1) of table C.2 and right panel plots estimates from column (3). Dashed lines are 95% confidence intervals. "-6" includes observations 6 or more years prior to designation, and likewise "6" includes observations 6 or more years post-designation. Values in the vertical dimension are differences in expected ACSH rate between the counties gaining/losing HPSA status and those that are never designated, conditional on fixed effects and covariates, and expressed as a function of time since the event.

relative improvements in ACSH rates prior to losing the designation.

The event study estimates indicate that there are pre-trends in ACSH rates that differ between counties that get HPSA designation, those that lose designation, and those that are never designated. A consequence of this is that the fixed effects estimates presented in Table C.1 may be biased. To address this problem, I re-estimated the fixed effects and event study models incorporating county-specific linear time trends (in addition to county and year fixed effects and covariates, as before). I present the fixed effects estimates incorporating the county trends in Table C.3. The results are not statistically significant either for the entire set of counties or either subsample.

The event study results including county trends are presented in columns (2) and (4) of Table C.2. I plot the event study results in Figure C.2, which is identical to Figure C.1 aside from the inclusion of the trends. For counties that gain designation, controlling for the trend results in point estimates that are closer to zero and confidence intervals that are considerably wider. The pattern of point estimates in the years prior to designation is still suggestive of a possible downward pre-trend, and the pattern in the point estimates three or more years following designation is consistent with a small effect of HPSA designation on the ACSH rate. But, since no point estimates are significant, neither of these possibilities can be distinguished from the scenario in which HPSA designation has no effect on the ACSH rate.

Table C.2: Event Study Regressions, by HPSA Status Change and Inclusion of County-Specific Time Trends

	(1) Gain HPSA	(2) Gain HPSA	(3) Lose HPSA	(4) Lose HPSA
Years Since Event: -6	256.2	364.3	910.8***	-819.4*
	(167.1)	(527.7)	(160.2)	(483.0)
Years Since Event: -5	292.5**	247.0	283.8**	-843.4**
	(127.5)	(333.5)	(122.1)	(375.9)
Years Since Event: -4	226.2*	152.3	269.2***	-575.4**
	(133.9)	(279.5)	(97.8)	(288.6)
Years Since Event: -3	300.7**	126.0	164.0	-391.4*
	(145.4)	(169.5)	(131.4)	(221.3)
Years Since Event: -2	109.3	-97.6	140.5*	-166.2
	(144.2)	(137.7)	(73.1)	(110.8)
Years Since Event: 0	-133.0	11.6	-166.1***	187.8*
	(103.8)	(131.5)	(61.0)	(111.5)
Years Since Event: 1	-367.9**	-138.8	-286.7***	407.1*
	(171.7)	(221.1)	(94.3)	(207.9)
Years Since Event: 2	-269.1	-7.92	-545.4***	658.0**
	(210.3)	(249.6)	(177.9)	(327.5)
Years Since Event: 3	-482.8**	-232.0	-492.9***	1051.7***
	(202.9)	(279.5)	(132.8)	(390.6)
Years Since Event: 4	-541.5**	-209.5	-541.4***	1111.5**
	(210.8)	(314.0)	(150.2)	(475.4)
Years Since Event: 5	-656.5***	-166.5	-596.6***	1230.8**
	(222.9)	(361.0)	(187.2)	(524.9)
Years Since Event: 6	-1032.0***	-170.5	-362.5	1465.7**
	(226.2)	(422.6)	(340.7)	(684.5)
County and Year Fixed Effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
County Linear Trends	No	Yes	No	Yes
Observations	30639	30639	30639	30639

Notes: Standard errors reflect clustering at county level. Regressions weighted by population of Medicare Part A beneficiaries. Columns 1 and 3 do not include county-specific linear time trends. Columns 2 and 4 include trends. Columns labeled "Gain HPSA" report results of event studies around designation of new HPSAs, whereas columns labeled "Lose HPSA" report the results of event studies around withdrawals of HPSA designation.

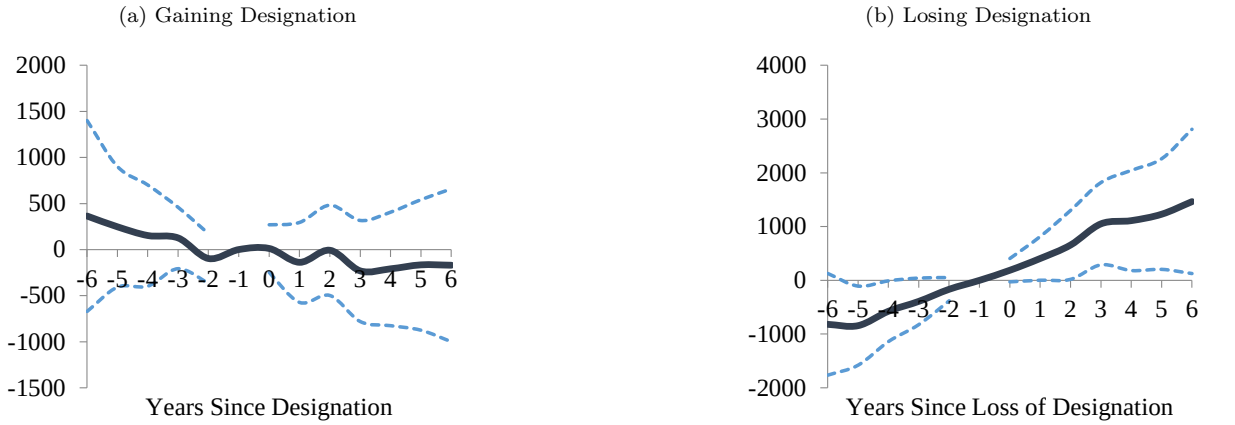
* $p < .10$; ** $p < .05$; *** $p < .01$

Table C.3: Fixed Effects Estimates of HPSA Designation on All-Cause ACSH Rate with County-Year Trends

	(1) All Counties	(2) Never Losing Desig.	(3) Never Gaining Desig.
Whole-County HPSA Designated	-46.6	80.2	-131.0
	(97.9)	(129.7)	(110.9)
Unemployment Rate (%)	-22.9	-24.1	-23.6
	(21.2)	(22.6)	(23.1)
Median Household Income ($)	0.051***	0.051***	0.051***
	(0.0083)	(0.0086)	(0.0086)
Poverty Rate (%)	10.4	13.9	11.8
	(11.1)	(11.9)	(12.0)
Total County Population	-0.0026	-0.0027	-0.0028
	(0.0027)	(0.0027)	(0.0028)
Percent of County Pop Aged 65+	-176.6**	-194.8**	-172.3*
	(80.6)	(90.3)	(88.0)
Retirement and Disability Insurance	0.26	0.24	0.25
	(0.26)	(0.28)	(0.26)
SSI Benefits	0.30	-0.14	0.021
	(1.58)	(1.72)	(1.79)
Earned Income Tax Credit Payments	3.87*	4.00*	3.90*
	(2.10)	(2.21)	(2.12)
Food Stamp (SNAP) Benefits	0.38	0.40	0.19
	(1.10)	(1.14)	(1.18)
Other Income Benefits e.g. TANF, WIC	0.59*	0.60	0.57
	(0.36)	(0.37)	(0.38)
Fraction of Pop with non-County HPSA	216.5	181.3	303.2
	(221.1)	(236.8)	(225.0)
Fraction of Pop with Population HPSA	-18.6	-43.6	6.05
	(247.0)	(289.7)	(286.9)
County Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Observations	30681	26833	25465
Counties	3097	2707	2567

Notes: Standard errors in parentheses reflect clustering at county level. Regressions weighted by population of Medicare Part A beneficiaries. Column 2 excludes counties that lose a HPSA designation between 1995 and 2015. Column 3 excludes counties that never gain HPSA designation over that period.
* $p < .10$; ** $p < .05$; *** $p < .01$

Figure C.2: Event Studies Around Gain and Loss of Whole-County Designation: Total ACSH, Including County Time Trends



Left panel plots event estimates reported in column (1) of table C.2 and right panel plots estimates from column (3). Dashed lines are 95% confidence intervals. "-6" includes observations 6 or more years prior to designation, and likewise "6" includes observations 6 or more years post-designation. Values in the vertical dimension are differences in expected ACSH rate between the counties gaining/losing HPSA status and those that are never designated, conditional on fixed effects, trends, and covariates, and expressed as a function of time since the event.

Counties that lose designation show a clear upward trend in relative ACSH rates both before and after loss of designation after including the county trends. The increase in ACSH rates following loss of designation is potentially consistent with an effect of HPSA designation. But, the existence of the pre-trend suggests that there are still unobserved differences between counties, which not captured by county fixed effects or linear trends and which are correlated with both loss of designation and ACSH rates.

Appendix D

ACSH RD Estimates by Race and Sex

Table D.1 reports fuzzy regression discontinuity estimates of the effects of HPSA designation on ACSH rates calculated for beneficiaries grouped by sex and race. All included beneficiaries are older than 65.

Table D.1: RD Estimates for Sex- and Race-Specific ACS Rates, Patients 65 and Older

All ACS Conditions	(1) Male	(2) Female	(3) Black	(4) Non-Black	(5) Male	(6) Female	(7) Black	(8) Non-Black
Reduced Form	220.211*	284.035*	962.544	247.919*	220.318**	257.039*	868.905	246.100**
	(125.158)	(167.058)	(681.101)	(143.718)	(108.705)	(133.425)	(584.700)	(116.380)
First Stage	-0.092***	-0.082***	-0.093***	-0.084***	-0.141***	-0.110***	-0.141***	-0.115***
	(0.021)	(0.024)	(0.022)	(0.024)	(0.018)	(0.019)	(0.019)	(0.019)
Wald Estimate	-2388.240	-3446.750	-10391.491	-2955.925	-1558.722*	-2339.918*	-6183.911	-2146.211*
	(1498.550)	(2383.724)	(7809.570)	(1980.204)	(807.493)	(1327.902)	(4270.298)	(1105.778)
Mean of Outcome	7424.6	8220.9	9800.9	7802.1	7170.6	7893.4	9296.9	7534.5
Bandwidth	2.662	1.835	2.698	1.968	5.323	3.670	5.395	3.935
Acute ACS Conditions								
Reduced Form	132.383*	217.760**	702.621	173.189**	139.887**	166.733**	570.341	154.313**
	(76.154)	(95.255)	(649.893)	(81.400)	(59.293)	(72.815)	(484.907)	(62.714)
First Stage	-0.080***	-0.080***	-0.080***	-0.080***	-0.104***	-0.100***	-0.101***	-0.102***
	(0.025)	(0.026)	(0.027)	(0.026)	(0.020)	(0.020)	(0.021)	(0.020)
Wald Estimate	-1655.630	-2736.163*	-8757.584	-2177.915*	-1340.670**	-1661.476**	-5637.233	-1516.058**
	(1121.542)	(1582.582)	(8567.078)	(1306.663)	(639.079)	(829.693)	(4923.819)	(709.626)
Mean of Outcome	3350.3	3999.6	4524.5	3702.9	3261.4	3849.3	4124.6	3582.4
Bandwidth	1.677	1.554	1.631	1.598	3.353	3.109	3.263	3.196
Chronic ACS Conditions								
Reduced Form	83.396	100.106	615.989	96.548	83.853	79.936	891.206**	55.837
	(75.619)	(81.239)	(510.403)	(65.333)	(65.968)	(70.726)	(429.408)	(59.781)
First Stage	-0.095***	-0.093***	-0.090***	-0.121***	-0.147***	-0.143***	-0.126***	-0.185***
	(0.021)	(0.021)	(0.024)	(0.019)	(0.018)	(0.018)	(0.020)	(0.017)
Wald Estimate	-876.859	-1074.895	-6833.251	-798.463	-570.320	-557.927	-7052.848*	-302.298
	(828.044)	(932.095)	(6006.909)	(562.865)	(457.387)	(505.960)	(3622.324)	(326.603)
Mean of Outcome	4118.5	4168.5	5861.3	3976.0	3969.5	3988.4	5661.6	3839.1
Bandwidth	2.824	2.716	2.268	4.251	5.648	5.433	4.537	8.502
Relative to IK	IK × 1	IK × 1	IK × 1	IK × 1	IK × 2	IK × 2	IK × 2	IK × 2

Notes: All estimations generated by local linear regression with triangular kernel. IK is the Imbens-Kalyanaraman bandwidth.
* $p < .10$; ** $p < .05$; *** $p < .01$

Appendix E

Cause-Specific Mortality Rates

Table E.1: Mortality by Cause and Sex

	(1) Cancer	(2) Cardiovascular	(3) Diabetes	(4) Other Disease	(5) External Causes
All Residents					
Reduced Form	0.055	0.026	-0.002	-0.001	-0.013
	(0.033)	(0.059)	(0.012)	(0.045)	(0.014)
First Stage	-0.071***	-0.076***	-0.072***	-0.073***	-0.076***
	(0.026)	(0.023)	(0.027)	(0.024)	(0.023)
Wald Estimate	-0.775	-0.338	0.034	0.016	0.167
	(0.560)	(0.791)	(0.162)	(0.621)	(0.193)
Mean of Outcome	2.331	3.861	0.320	3.123	0.763
Observations	6,869	8,584	6,520	7,928	8,484
Bandwidth	1.244	1.553	1.181	1.437	1.535
Males					
Reduced Form	0.056	-0.018	-0.016	-0.031	-0.032
	(0.042)	(0.063)	(0.015)	(0.043)	(0.021)
First Stage	-0.072***	-0.077***	-0.074**	-0.081***	-0.082***
	(0.024)	(0.023)	(0.029)	(0.022)	(0.022)
Wald Estimate	-0.782	0.232	0.219	0.381	0.386
	(0.651)	(0.810)	(0.217)	(0.529)	(0.264)
Mean of Outcome	2.564	3.717	0.297	2.873	1.031
Observations	7,662	8,791	5,491	9,633	9,775
Bandwidth	1.388	1.590	0.982	1.746	1.772
Females					
Reduced Form	0.048	0.068	0.004	0.012	0.013
	(0.035)	(0.068)	(0.014)	(0.058)	(0.017)
First Stage	-0.071***	-0.078***	-0.076***	-0.073***	-0.072***
	(0.025)	(0.023)	(0.023)	(0.024)	(0.026)
Wald Estimate	-0.677	-0.872	-0.058	-0.171	-0.178
	(0.554)	(0.920)	(0.180)	(0.795)	(0.253)
Mean of Outcome	2.101	4.009	0.340	3.377	0.493
Observations	7,310	9,036	8,621	7,877	6,542
Bandwidth	1.321	1.633	1.561	1.427	1.184

Notes: Standard errors in parentheses.
* $p < .10$; ** $p < .05$; *** $p < .01$

Table E.2: Mortality by Cause and Race/Ethnicity

	(1) Cancer	(2) Cardiovascular	(3) Diabetes	(4) Other Disease	(5) External Causes
White					
Reduced Form	0.073**	0.051	-0.006	0.013	-0.015
	(0.035)	(0.062)	(0.012)	(0.047)	(0.016)
First Stage	-0.071***	-0.080***	-0.073***	-0.079***	-0.077***
	(0.025)	(0.022)	(0.027)	(0.022)	(0.023)
Wald Estimate	-1.034	-0.636	0.085	-0.167	0.191
	(0.629)	(0.798)	(0.170)	(0.605)	(0.204)
Mean of Outcome	2.535	4.187	0.319	3.392	0.799
Observations	7,351	9,470	6,347	9,152	8,829
Bandwidth	1.330	1.715	1.145	1.657	1.596
Black					
Reduced Form	-0.138	-0.171	-0.090	0.216	-0.016
	(0.132)	(0.187)	(0.072)	(0.136)	(0.090)
First Stage	-0.103***	-0.090***	-0.086***	-0.115***	-0.086***
	(0.022)	(0.024)	(0.026)	(0.021)	(0.026)
Wald Estimate	1.334	1.903	1.045	-1.888	0.183
	(1.299)	(2.111)	(0.881)	(1.238)	(1.046)
Mean of Outcome	1.735	2.980	0.414	2.338	0.679
Observations	9,699	7,747	6,971	10,961	7,064
Bandwidth	2.308	1.792	1.611	2.709	1.635
Hispanic					
Reduced Form	-0.136	-0.045	-0.052	-0.061	-0.192*
	(0.114)	(0.193)	(0.045)	(0.162)	(0.104)
First Stage	-0.094***	-0.090***	-0.091***	-0.091***	-0.081***
	(0.020)	(0.021)	(0.020)	(0.020)	(0.022)
Wald Estimate	1.446	0.503	0.572	0.672	2.353*
	(1.260)	(2.145)	(0.505)	(1.779)	(1.405)
Mean of Outcome	0.616	1.015	0.136	1.012	0.587
Observations	11,649	10,899	11,145	11,164	9,306
Bandwidth	2.254	2.090	2.145	2.148	1.739

Notes: Standard errors in parentheses. "Hispanic" includes Hispanics of any race, whereas "white" and "black" refer to white non-Hispanic and black non-Hispanic populations, respectively.
* $p < .10$; ** $p < .05$; *** $p < .01$

Appendix F

Additional Ambulatory HIT Results

Table F.1: Representativeness of the Counties in the Merged Panel Database

	Mean	Std. Dev.	Obs.
In Sample			
Short-Term Hosp. Beds (Per 1,000)	3.21	3.89	25030
Primary Care Physicians (Per 10,000)	6.91	4.38	25030
FQHC Grantees (Per 100,000 Low-Income)	14.0	34.3	25030
Health Care Spending (000s USD)	5100.9	5268.1	25030
State HMO Penetration	15.8	9.69	25030
State Uninsured Percent	14.5	4.15	25030
Median Household Income (000s USD)	42.5	10.9	25030
County Poverty Percent	14.5	5.42	25030
Missing EMR Data			
Short-Term Hosp. Beds (Per 1,000)	2.46	4.69	5800
Primary Care Physicians (Per 10,000)	4.01	3.50	5800
FQHC Grantees (Per 100,000 Low-Income)	51.1	124.6	5800
Health Care Spending (000s USD)	4779.6	4460.5	5800
State HMO Penetration	13.5	8.09	5800
State Uninsured Percent	16.2	4.40	5800
Median Household Income (000s USD)	37.3	9.26	5800
County Poverty Percent	17.2	7.07	5800
Observations	30830		

Notes: County-year unit of observation.

Table F.2: Summary Statistics for PQI Rates by Age Group

	Mean	Std. Dev.	Min.	Max.	Obs.
Overall PQI Rate	1280.3	1154.7	0	9563.6	4725
18 to 39	243.0	252.4	0	3670.4	4686
40 to 64	794.6	819.8	0	9582.9	4716
65 to 74	2494.8	2243.8	0	17059.6	4394
75 Plus	5766.3	4481.2	0	30769.2	4383
PDI Rate (NIS)	117.3	182.0	0	4060.0	4622
PDI Rate (KID)	156.3	165.9	0	2198.7	1535

Notes: County-year unit of observation. The PQI rate is for adults age 18 and older per 100,000 population. The PDI rate measures preventable admissions per 100,000 population for children ages 2 to 7. PQIs are calculated using NIS. PDIs are calculated separately using NIS and KID data.