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THREE ESSAYS ON THE SOCIETAL IMPACT OF HEALTH POLICIES AND LAWS

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by

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DISSERTATION

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DEDICATION

To my family,

for their patience and support.

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ABSTRACT

This dissertation provides evidence to the two contentious debate over health policies and laws in the US, Medicaid expansion and sex offender registration and notification. In Chapter 2, I explore one key determinant of Medicaid take-up, the benefit of access to care proxied by the Medicaid-to-Medicare primary care physician payment ratio. Using a unique dataset of Medicaid physician reimbursement rates and the American Community Survey of 2010 – 14, I find that a 10-percentage-point increase in the payment ratio of a 30-minute new patient office visit will increase Medicaid enrollment among uninsured adults in poverty by more than 150,000. In Chapter 3, we re-examine the impact of Medicaid on birth outcomes. To mitigate the crowd-out from private insurance to Medicaid, we focus on the population eligible for Medicaid during its implementation period. Using predicted individual-level Medicaid treatment intensity among childbearing age women and state-level variation from Medicaid roll-out, we find that Medicaid provision shifted the labor delivery method from not in a hospital (with a midwife or a physician) to in a hospital and increased birth weight modestly. These impacts were driven by nonwhite mothers and mothers aged 30 – 49. Chapter 4 evaluates sex offenders' crime-risk. Using the single-family residential property data of 2008 – 2018 and sex offender data of April 2019 in Maryland, we apply the spatial difference-in-difference method to estimate the crime risk capitalized into housing markets. The results suggest no negative impact on proximate home values within the 0.1-mile of sex offenders' residences after their arrivals.

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Chapter 1: Introduction to Health Insurance and Sex Offender Policies in the US

The United States is experiencing one of the most significant changes in the health care history, the Affordable Care Act (ACA). The ACA's objective was to expand health insurance coverage to the uninsured through health insurance exchanges, Medicaid expansion, and the individual mandate, among others. Since its implementation, there has been a contentious debate over its impact on access to health care and health, especially over Medicaid expansion. Indeed, evaluating the impact of health insurance coverage on health is complicated. Health is determined by many factors (e.g., income), which also affects insurance coverage status. Even if controlling these factors, the effect of insurance on health may not be evident within a short period. To evaluate the effectiveness of the public health insurance, the first question to ask is whether or not Medicaid reaches the target population, or what proportion of the target population takes-up the insurance.

Chapter 2 aims to evaluate one key determinant of Medicaid take-up, the benefit of access to care. Although Medicaid provides healthcare services almost free of charge to many disadvantaged individuals, take-up rates have long been below 100 percent. This chapter seeks to examine a potential determinant of Medicaid take-up: the reimbursement rate to primary care providers. Traditionally, the Medicaid reimbursement rate for primary care was lower than that of Medicare or private insurance plans. The Affordable Care Act raised the primary care reimbursement rate of Medicaid to match that of Medicare during 2013–14. This chapter evaluates the impact of Medicaid primary care reimbursement rate on Medicaid coverage among low-income non-elderly adults. The analysis is conducted using a generalized difference-indifferences method and a novel dataset of Medicaid-to-Medicare primary care reimbursement ratios combined with the

American Community Survey of 2010–14. The results show that a 10-percentage-point increase in the reimbursement ratio of the 30-minute new patient office visit is associated with a 0.40-percentage-point increase in Medicaid coverage rate among adults whose family income is below 250% of the federal poverty line. This association is most significant among the near-elderly (aged 50–64), non-parents, African-Americans, and those living in urban areas. The reimbursement ratio is also negatively associated with the uninsured rate, but not with the privately insured rate. Overall, these findings suggest that Medicaid physician payment policies are effective in promoting public insurance take-up among potential beneficiaries.

Once Medicaid reaches the target population, the next question to ask is if Medicaid improves access to care and health outcomes among them. Chapter 3 addresses the question based on the population eligible for Medicaid during its initial provision period. Previous literature found mixed results on the impact of Medicaid on birth outcomes. Most studies focused on the Medicaid expansion period, suffering from crowd-out of private insurance into Medicaid. In this chapter, we reevaluate the impact of Medicaid on prenatal care utilization and birth outcomes. To minimize the concern of crowd-out, we focus on the population during the initial Medicaid rollout period, when individuals eligible for Medicaid were unlikely to be covered by private insurance. Also, we exploit individual-level variation by simulating Medicaid treatment intensity in addition to state-by-year variation during Medicaid rollout. Specifically, we predict women's probabilities of being treated by Medicaid using 1963 National Health Interview Survey data and match them to babies in Vital Statistics Natality birth data of 1968 – 1973 based on their mothers' socioeconomic characteristics. We find that

Medicaid provision improved access to care by shifting labor delivery from not in a hospital (with a physician or a midwife) to in a hospital. Further, we find that Medicaid increased the birth weight by 0.7%. Our results suggest that Medicaid provision improved health care utilization among pregnant women and their birth outcomes.

In addition to the debate over health insurance policies and laws, the debate over the sex offender policies and laws continued in the US during the past two decades. In response to several horrific crimes conducted by sex offenders towards children, the US implemented a series of laws targeting sex offenders, including registration, community notification, and residency restriction laws, among others. These laws are based on the following three main premises. First, sex offenders are more likely to re-commit crimes in areas where they live compared to other criminals. Second, sex crimes are committed by strangers who newly moved into neighborhoods rather than someone familiar with. Third, through empowering the public with the knowledge of sex offenders living in the same communities and increasing the penalty of sex offenses, these registration and notification laws will reduce the recidivism rate among sex offenders. In the past two decades, however, scholars struggled to find evidence supporting these premises and found these laws might increase the rate of recidivism (Schram and Milloy 1995, Adkins et al. 2000, Vásquez, Maddan, and Walker 2008, Agan 2011, Prescott and Rockoff 2011). Further, these laws stigmatized sex offenders and their families, preventing them from re-integrating into communities, finding housing, and employment opportunities (Zevitz and Farkas 2000, Tewksbury 2005, Levenson and Cotter 2005).

Also, the landscape of sex offender registration and notification laws has changed a lot during the past two decades. With the establishment of the Protect Act in 2003 and the Adam Walsh Act in 2006, the primary community notification method changed from proactive to a passive notification method, i.e., sex offender registry websites. Because of these backgrounds, it is essential to re-evaluate the crimes risk caused by sex offenders. One important technique to measure the crime risk due to sex offenders is to use the revealed preference approach to evaluate the risk capitalized into housing markets. Chapter 4 evaluates the impact of crime risk from sex offenders' arrivals, perceived through sex offender registry websites, on proximate property values. Using single-family residential house dataset of 2008 – 2018 and registered sex offender dataset of April 2019 in four counties of Maryland, this paper shows no evidence of negative externality created by sex offenders' arrivals on property values in the vicinity. This result contributes to the continuous debate over the sex offender registration and notification laws, suggesting little financial burden created by sex offenders' arrivals on proximate households.

In Chapter 5, the main conclusions the policy implications are revisited. Future work and extensions of this dissertation are also explored.

Chapter 2: Physician Payment and Demand for Health Insurance: Evidence from Medicaid Primary Care Payment Parity

2.1 Introduction

The uninsured rate is highest among non-elderly adults.[1] Although the Medicaid expansion under the Affordable Care Act (ACA) sought to expand health insurance coverage to the uninsured population, low-income adults' uninsured rate is still high. As of 2017, 25.7% adults in poverty, compared to 7.8% of children, remained uninsured (Berchick, Hood, and Barnett 2018). One reason for the insurance coverage gap is low take-up of Medicaid. During 2005–2010, the national average Medicaid take-up rate was estimated to be 62.6 percent among non-elderly adults and 38.3 percent among childless adults, who were eligible for Medicaid and not covered by private insurance (Sommers, Tomasi, et al. 2012). This phenomenon of incomplete take-up of Medicaid, and more broadly welfare programs, among potential beneficiaries, has attracted numerous researchers to examine take-up barriers and to develop policy tools to help mitigate the barriers (see (Currie 2006) for a review). The barriers include a lack of program knowledge, transaction costs during the enrollment process, welfare stigma, and low program benefits (Moffitt 1983, Kubik 1999, Currie and Grogger 2002, Aizer 2007). In the context of Medicaid, little research has been done to understand the barrier of low program benefits.

In this paper, I explore the impact of the Medicaid benefit of access to care on Medicaid coverage among adults. Potential Medicaid beneficiaries face great access

[1] This is based on the author's calculation using the health insurance coverage reports of 2000 – 17 from the Census Bureau.

barrier to primary care. It is because primary care physicians are often reluctant to accept new Medicaid patients (Decker 2012, Rhodes et al. 2014) and, even for existing Medicaid patients, offer low-quality services (Decker 2007, 2009). According to the surveys of physicians, the most-stated reason for nonparticipation is the low reimbursement rate (Long 2013). Primary care is reimbursed at a particularly low rate among all services. In 2012, Medicaid reimbursed 66% of Medicare rate for all services and 59% of that for primary care services (Zuckerman and Goin 2012).

To increase primary care physicians' participation and thus alleviate access barrier, the ACA mandated both fee-for-service (FFS) reimbursement rate and managed care (MC) capitation rate for primary care services furnished in 2013–14 at the Medicare level (hereinafter referred to as the Medicaid primary care payment parity or fee bump).[2] The parity affected each state's Medicaid primary care physician payment differently, depending on the state's payment level before the mandate. On average, this parity increased the amount reimbursed to primary care physicians by approximately 55% from 2012 to 2013, and by 50% from 2012 to 2014.[3] Given that primary care is essential for preventive care and care continuity (Friedberg, Hussey, and Schneider 2010) and the high prevalence of chronic illnesses among potential Medicaid beneficiaries, adults eligible for Medicaid may increase the program take-up once access barrier is reduced.

The objective of this paper is to examine the effect of the Medicaid benefit of access to primary care, proxied by Medicaid-to-Medicare primary care reimbursement

[2] Medicaid primary care payment parity applies to primary care services, including evaluation and management services and vaccine administration, furnished by physicians specialized in family medicine, general internal medicine, or pediatric medicine.

[3] This is based on the author's calculation of the Medicaid reimbursement rate for 30-minute new patient office visit in 2012–14.

ratio, on Medicaid coverage among adults. To achieve this objective, I merged a novel dataset of state-level Medicaid-to-Medicare reimbursement ratios (hereinafter referred to as fee ratios) for adult patient office visits with individual-level data from the American Community Survey (ACS) of 2010 – 14. The sample was restricted to nonpregnant civilian adults aged 27–64, whose family income was below 250% of the federal poverty line (FPL). To utilize the state-by-year variations in the Medicaid-to-Medicare fee ratios, this study used a generalized difference-in-differences (DD) method. The analysis controlled both individual- and state-level characteristics, such as demographics, state financial health status, primary care physician supply, and Medicaid income eligibility thresholds, that could be correlated with the fee ratios and have independent effects on Medicaid coverage. The state-specific linear trends were included to purge further omitted variable bias. State and year fixed effects were used.

This study finds that the Medicaid-to-Medicare fee ratios of patient office visits are positively associated with Medicaid coverage among low-income non-elderly adults. For instance, a 10-percentage-point increase in the fee ratio of a 30 (45)-minute new patient office visit is associated with a 0.40 (0.36)-percentage-point increase in adult Medicaid coverage rate. Also, the same increase in the fee ratio is associated with a 0.40 (0.39) percentage-point decrease in the uninsured rate, while not being associated with a change in the privately insured rate. To compare the estimate of adults with that of children (Hahn 2013), I restricted the sample to be adults in poverty. I find that a 10-percentage point increase in the fee ratio of a 30-minute new patient office visit will increase Medicaid enrollment status by 0.9-percentage-point and reduce uninsured status by 0.7-percentage point among adults in poverty. These estimates are consistent with the previous study. Because more than 22 million adults were in poverty, and 26% of them were uninsured as of 2017, the estimates suggest that more than 150,000 uninsured adults would enroll in Medicaid. Further, stratified analyses find that the positive association between the fee ratios and Medicaid coverage rate is most significant among the near-elderly, non-parents, African-Americans, as well as those living inside central cities of metropolitan areas.

Numerous robustness checks were implemented. First, I added additional state-level controls, including MC penetration rate, welfare caseload, and the federal medical assistance percentage (FMAP). Second, alternative criteria were used to determine potential Medicaid beneficiaries, such as educational attainment less than a high school diploma. Third, the Medicaid-to-Medicare fee ratios under both FFS and MC were used to measure the Medicaid physician payment level relative to Medicare. Fourth, the year 2014 was excluded from the sample, mitigating potential bias from the Medicaid demand increase due to eligibility expansions. Fifth, the logistic model was used. The baseline results were robust to all these checks. Finally, the results passed a falsification test using the sample of adults whose family income was above 400% of the FPL.

The contribution of this study is three-fold. Departing from a previous study on children (Hahn 2013), this study, to my knowledge, is the first to examine the effect of Medicaid primary care reimbursement rate on Medicaid coverage among adults.[4] Compared to children, Medicaid participation rate among adults is substantially lower (Sommers and Epstein 2010, Kenney, Lynch, et al. 2012). Once enrolled in Medicaid,

[4] One working paper examined the relationship between the Medicaid reimbursement rate for all specialty services and the Medicaid coverage among the non-elderly using state-level data (Chen, 2014).

adults face a more severe barrier of access to care than children (Bodenheimer and Pham 2010, Rosenbaum 2014). Thus, it is critical to examine the access barrier and Medicaid take-up among adults. Second, the findings suggest that a higher Medicaid primary care reimbursement rate will increase Medicaid participation among uninsured adults eligible for Medicaid. Third, this implication highlights the critical role of the higher reimbursement rate in the context of the ACA's Medicaid expansion. With the increased competition for physicians among newly enrolled and existing Medicaid patients, terminating the payment parity might substantially increase patients' barrier of access to care and thus uninsured rate.

This paper is organized as follows. Section 2 describes the background of Medicaid physician payment rates and the Medicaid primary care payment parity. Section 3 provides a literature review to explain why the relationship between Medicaid primary care reimbursement rates and Medicaid coverage may exist. Section 4 presents the data sources for this study. Section 5 specifies the generalized DD method. Section 6 illustrates the method graphically and reports the baseline results. Section 7 reports the results of robustness checks and placebo tests. Section 8 extends the analyses using subsamples and other insurance coverage rates. Finally, section 9 concludes by summarizing the study's findings and discussing limitations as well as policy implications.

2.2 Background

2.2.1 Medicaid Physician Payment

Medicaid delivers physicians' services through two systems: FFS and MC. Under FFS, state Medicaid programs pay physicians reimbursement rates (or fee) on a per-claim basis and often adjust the reimbursement rates for the site of service and patients' age (children versus adults). More specifically, the payment rates are determined using the following three methods. As of October 2013, 22 states paid physicians using the resource-based relative value scale (RBRVS), which is based on the relative value of each physician procedure and is used for Medicare rate development; 17 states paid a fixed percentage of the Medicare rate; and ten states developed a state-specific rate schedule developed from market value or via an internal process (Medicaid, Chip Payment, and Access Commission 2017). Under MC, states pay Medicaid managed care organizations a capitation payment per member per month for each person enrolled in the organizations' plans. The plans often negotiate with providers to provide services for their enrollees on an FFS basis, either through traditional FFS or through a primary care case management (PCCM) arrangement (Medicaid, Chip Payment, and Access Commission 2011).[5]

The disparity in physician payment between Medicaid and other payers is increasing over time. Despite the general principle that the rate is sufficient to ensure access to care or cover all reasonable and appropriate service costs (Social Security Act 1902(a)(30)(A), Title 42 CFR 438.4), there is very little guidance on physician payment under either FFS or MC. Also, states are not required to monitor the quality of care received by Medicaid patients. Without close oversight at the federal level, states have considerable discretion when it comes to Medicaid reimbursement policy and have historically paid a considerably lower reimbursement rate to Medicaid providers

[5] Under PCCM, primary care providers are paid a monthly fee to coordinate enrollees' care and assure their access.

compared to other payers (e.g., Medicare). Further, states often cut Medicaid reimbursement rates, especially during economic downturns, to balance the budget.

The Medicaid physician payment used in the main analysis of this paper is based on FFS. Despite states' transitions from FFS toward MC, Medicaid FFS reimbursement rate remains a good proxy for Medicaid physician payment for the following reasons. As of July 2014, over 40% of approximately 71 million Medicaid beneficiaries were covered by programs based on the FFS payment method (Government Accountability Office 2016). In the fiscal year 2015, the majority (54%) of national Medicaid spending was for services delivered under FFS (Medicaid, Chip Payment, and Access Commission 2016). Also, the change in FFS payment affects that under MC, because capitation rate often benchmarks Medicaid reimbursement rate (Howell, Palmer, and Adams 2012, Zuckerman and Goin 2012, Medicaid, Chip Payment, and Access Commission 2017).

2.2.2 Medicaid Primary Care Payment Parity

The Medicaid primary care payment parity under the ACA mandated a two-year increase of Medicaid physician payment for primary care services, under both FFS and MC, to Medicare Part B levels in 2013 and 2014 (Public Law 111-152). The parity required the federal government to fully fund the gap in the payment between the state plan level as of July 1, 2009, and the level required in 2013–14.[6] The states that reduced the primary care physician reimbursement rate after July 1, 2009, were required to make up the reduced portion using state funding (Federal Register Vol.77 No. 215). Potentially due to the late publication of Centers for Medicare & Medicaid Services' (CMS) final

[6] The required rate was the higher of the effective Medicare rate in 2013-14, or the applicable rate using 2009 conversion factor.

rule on implementing the parity and operational difficulties in establishing the parity, especially under MC, most states reported making the first enhanced payment in May 2013 or later (Medicaid, Chip Payment, and Access Commission 2015). In these cases, states were required to make supplemental payments to make up the difference between the amount paid and the required rates retroactively (Centers for Medicare and Medicaid Services 2012). While the federal government no longer funded the parity beyond 2014, eighteen states continued the fee bump at least partially in 2015 (Wilk, Evans, and Jones 2018).

Primary care services furnished from Jan 1, 2013, to Dec 31, 2014, including evaluation and management services of the Current Procedural Terminology (CPT) code 99201-99499 and vaccine administration services for children of the CPT code 90460-90461 or 90471-90474, were eligible for the fee bump (Tollen 2015). The eligible evaluation and management services included both new and established patient office visits. New patient office visits usually lasted 30 minutes (CPT code 99203) or 45 minutes (CPT code: 99204), and because they often involved evaluations of undiagnosed signs, symptoms, or health concerns, they typically lasted longer than later office visits. Established patient office visits usually lasted 15 minutes (CPT code 99213) or 25 minutes (CPT code 99214). Also, both physicians and non-physician practitioners with a primary designation of family medicine, general internal medicine, or pediatric medicine were eligible for the fee bump.

2.3 Literature Review

This study builds upon three strands of literature. The first is related to studies on the relationship between Medicaid primary care reimbursement rate and the behavior of healthcare providers. Theoretically, Sloan, Mitchell, and Cromwell (1978) proposed a model of physician behavior in a multi-payer market, which predicts that physicians, who accept both private and Medicaid patients, will provide more service to Medicaid patients and less service to private patients following a reimbursement rate increase; however, physicians, who accept private patients only, may not start accepting Medicaid patients after the rate increase. Later, the model of physician behavior with demand inducement concludes that an increase in Medicaid payment would induce higher demand among Medicaid patients, assuming the substitution effect dominates the income effect from the rate increase (McGuire and Pauly 1991, Gruber and Owings 1996).

Empirically, some literature has found that the rate increase is associated with more private providers accepting (new) Medicaid-covered patients (Decker 2007, 2012, Alexander and Schnell 2018). However, other literature has found that the rate increase was not associated with a higher Medicaid participation rate or Medicaid service volume among primary care physicians (Decker 2018, Mulcahy, Gracner, and Finegold 2018). Overall, the current literature has found a non-negative effect of Medicaid reimbursement rate on physicians' labor supply to Medicaid patients. Extending from the direct supply-side effect from the provider payment incentive, this study focuses on the demand-side effect among potential Medicaid beneficiaries who were the ultimate target of the Medicaid primary care payment parity.

The second area of literature upon which this study builds is related to studies on the relationship between Medicaid primary care reimbursement rate and access to care. The current literature has found that an increase in the reimbursement rate is associated with a higher likelihood of having a usual source of care, and an increased volume of office (outpatient, emergency department) visits and prescription filling among Medicaid beneficiaries (Callison and Nguyen 2018, Alexander and Schnell 2018). It was also associated with higher appointment availability, shorter wait-to-appointment (in days), and longer visit durations among Medicaid patients (Decker 2007, Polsky et al. 2015, Sharma et al. 2018). On the other hand, a reimbursement rate cut was found to reduce Medicaid beneficiaries' visit volume and shifted their site of care from physician offices to hospital emergency and outpatient departments (Decker 2009). Given the better access to care following the reimbursement rate increase, this study proposes a hypothesis that the increase in the reimbursement rate is likely to increase the Medicaid take-up among potential beneficiaries, who perceive the quality enhancement of Medicaid relative to other insurances.

Third, this study relates to a line of literature on the relationship between Medicaid reimbursement rate and patient outcomes. An increase in Medicaid primary care reimbursement rate increase was associated with improved self-reported general health among all beneficiaries and with school attendance among children beneficiaries (Alexander and Schnell 2018). It was also correlated with less substance use disorders treatment or tobacco product use and better mental health among beneficiaries (Maclean et al. 2018). A Medicaid reimbursement rate increase for other services also improved beneficiaries' health outcomes. An increase in the Medicaid payment to obstetrical services was associated with higher birth weight (Sonchak 2015), lower risk of low and very low birth weight (Gray 2001), and lower infant mortality (Currie, Gruber, and Fischer 1995). Higher Medicaid payments for dental services was correlated with more frequent dental visits (Buchmueller, Orzol, and Shore-Sheppard 2015, Decker and Lipton

2015), which may improve dental health. The reasons for the improvement in patient outcomes could be that higher reimbursement rate incentivizes physicians to see patients more often. Additionally, this study proposes another possible channel for this improvement to the extent that the reimbursement rate increase might encourage Medicaid take-up among potential beneficiaries, leading to more widespread, regular health care utilization and consequently better health outcomes. On the other hand, the improvement in patient outcomes could further enhance the quality of Medicaid, promoting more participation in Medicaid.

Although the literature has found various benefits of the reimbursement rate increase on both supply and demand sides, there are three impediments, however, to increasing Medicaid take-up through the provider payment incentive. First, other reasons besides reimbursement rate (e.g., the transaction cost of receiving reimbursement rates) may reduce primary care physicians' willingness to accept new Medicaid patients. Second, physicians' willingness to accept new Medicaid patients may be hindered by residential segregation. Office-based primary care physicians take the majority of their patients from areas immediately surrounding their practice locations, whereas the Medicaid population is concentrated in depressed inner-city areas underserved by the physicians (Fossett et al. 1992, Rosenbaum 2014). Third, potential beneficiaries may not seek primary care if they have been receiving charity care in federally qualified health centers or hospitals. Therefore, the relationship between Medicaid primary care reimbursement rate and Medicaid coverage remains uncertain.

2.4 Data and Sample

2.4.1 Medicaid-to-Medicare Reimbursement Ratio

For this analysis, I assembled a state-level dataset of the non-facility Medicaid-to-Medicare reimbursement ratios (or fee ratios) of adult patient office visits during 2010–14. The non-facility fee ratios reflect the relative Medicaid reimbursement rate level among office-based physicians. I focused on office-based physicians for two reasons. First, Medicaid patients are less likely to be seen in physician offices than in hospital outpatient or emergency departments (Baker and Royalty 2000, Decker 2009). Second, physicians practicing in office settings have more discretion over participating in Medicaid (Perloff, Kletke, and Fossett 1995).

To construct the fee ratios, first, I collected the Medicaid reimbursement rates as of July 1st from the websites of states' Medicaid agencies.[7] In cases where historical fee schedules were not available, I contacted Medicaid agencies directly via email or phone, or through submitting public record requests. After collection, 40 (78%) states have complete data of Medicaid reimbursement rates for the full sample period 2010–14, 48 (94%) states have complete data in the pre-parity period 2010–2012, four states have four years' data[8], and four other states have three years' data[9]. I excluded Tennessee since it does not use Medicaid FFS payment method. I also excluded Hawaii and South Dakota due to the inability to obtain data.

[7] Since states were required to make retroactive payments for the difference between the amount paid and the required rates in 2013–14, the required rates were used in these two years.

[8] The Medicaid physician reimbursement rate is missing in Delaware and Minnesota of 2013, and in Alaska and Indiana of 2014.

[9] The Medicaid physician reimbursement rate is missing in New Mexico, North Carolina, Utah, and West Virginia of 2013–14.

I used the Medicaid reimbursement rates for the following adult patient office visits, 30- and 45-minute new patient office visits as well as 15- and 25-minute established patient office visits. Alexander and Schnell (2018) used the reimbursement rate of 30-minute new patient office visits only, missing a critical component of access to care among potential Medicaid beneficiaries—care continuity. The services of 30- and 45-minute new patient office visits each accounted for 41% and 33% of the total Medicaid FFS spending on new patient office visits in 2009. The services of 15- and 25-minute established patient office visits each accounted for 56% and 30% of the total Medicaid FFS spending on established patient office visits in 2009.[10]

Second, I collected the Medicare reimbursement rates of July for the same services from the national physician fee schedule relative value files.[11] These files contain the measures of procedure complexity, such as relative value units, practice expense, and malpractice expense, as well as geographic practice cost indexes and conversion factors. I applied the formula of Medicare physician reimbursement rates and calculated the reimbursement rates per state and year. For states within which geographic practice cost indexes vary, I estimated population-weighted average reimbursement rates.[12] Third, the Medicaid-to-Medicare fee ratio was calculated for each service. The fee ratio of Delaware was coded as one in 2013, based on the communication with the state Medicaid

[10] The calculation was ignored the service of 99201, which was not used in (Government Accountability Office, 2014).

[11] CMS releases and revises the files in January, April, July, and October each year. The results are not sensitive to using files released in other months.

[12] The average population of each region during 2010–2016 was used for the calculation. The data of total cities and towns came from U.S. Census Bureau.

agency. In the final dataset of Medicaid-to-Medicare fee ratios, forty-one (80%) states have complete data of the whole sample period.

Figure 2.1 plots the change of Medicaid-to-Medicare reimbursement rate ratios for 30minute new patient office visit across states from 2010 to 2014.[13] Twenty-nine states did not change their Medicaid reimbursement rate between 2010 and 2012, and the rest states changed their rate by less than 10% in the absolute term during this period except Connecticut and Minnesota.[14] Similarly, all states (except Maryland, South Dakota, and Wyoming) changed their Medicare reimbursement rate by less than 5% in the absolute term in these three years.[15] The reimbursement rate ratio ranged from 0.21 to 1.39, and its median value stayed at 0.7 throughout the three years. While the median Medicare reimbursement rate for 30-minute new patient office visit increased by 1.5% only from $104.47 in 2012 to $106.06 in 2014, the median Medicaid reimbursement rate for the same service increased by 45% from $74.38 in 2012 to $107.77 in 2014, moving the reimbursement rate ratio into the top quintile across all states in 2014.[16]

2.4.2 American Community Survey

The primary dataset used for the analysis came from the American Community Survey (ACS), an annual national mandatory survey with over 3.5 million households conducted by the U.S. Census Bureau. The Medicaid-to-Medicare fee ratios were

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[13] The variations of the rates for other services are similar during this period.

[14] Connecticut had a 14.5% fee bump from 2011 to 2012 and Minnesota had 124% rate increase from 2010 to 2011.

[15] The Medicare reimbursement rate for 30-minute new patient office visit increased by 5.69% in Maryland, by 5.3% in South Dakota, and by 7.85% in Wyoming.

[16] Although Alaska and North Dakota Medicaid programs reimbursed physicians above the level of Medicare before the parity, their reimbursement rates increased from 2012 to 2014. For instance, the Medicaid rate in North Dakota increased from $140.07 (compared to the Medicare rate of $104.58) in 2012 to $152.62 (compared to the Medicare rate of $106.02) in 2014.

matched to the individuals in the ACS data based on state identifiers. Since the fee ratios are not available in Hawaii, South Dakota, and Tennessee, I excluded these states from ACS data.

I restricted the sample to civilian adults aged 27 to 64, whose family income is below 250% of the FPL, and who did not give birth to a child within one year before the survey. With these sample restrictions, the sample contains 2,169,335 non-elderly non-pregnant civilian adults, 26% of whom were covered by Medicaid (Table 2.1). The average age of the adults was 46, 54% of them were female, and 43% were married. Regards to racial distribution, 74% were white, 17% were black, 3% each were Hispanic and Asian. Less than half (47%) of them had a high school or equivalent degrees, a quarter had some college education, and 14% had college degrees. On average, their family income was at 135% of the FPL.

2.5 Empirical Approach

I used a generalized difference-in-differences (DD) method to examine the impact of Medicaid benefit of access to care, proxied by Medicaid-to-Medicare primary care fee ratios, on Medicaid coverage among non-elderly adults. The generalized DD method uses a continuous variable to proxy for the treatment intensities of multiple groups (e.g., states) in numerous periods (e.g., years). Formally, I estimated the following linear probability model (LPM).

$$Medicaid_{ist} = \beta_0 + \beta_1 FeeRatio_{st} + \beta_2 X_{ist} + \beta_3 Z_{st} + State_s + Year_t + Trend_s + \varepsilon_{ist} \quad (1)$$

In this equation, *Medicaid* is an indicator of being covered by Medicaid; $FeeRatio$ stands for Medicaid-to-Medicare primary care fee ratios; *X* is a vector of individual-level characteristics including age, gender, marriage status, race, income and levels of education attainment, such as having a high school degree (or other equivalent ones), some college education, and a college degree. The average treatment effect of the Medicaid-to-Medicare primary care fee ratios on adults' Medicaid coverage is captured by β_1. Its validity relies on the assumption that the fee ratio is exogenous to the system of demand for Medicaid. This assumption would fail if the change in fee ratio was a tool to increase physician participation, thus Medicaid take-up, or was correlated with state policies and conditions affecting the demand for Medicaid. To address this concern of endogeneity, I included a vector Z containing the following state-level covariates in the baseline specification. I also tested baseline results' sensitivity to the endogeneity of the fee ratio by restricting the sample period to 2012–14 in robustness checks (see Section 7: Robustness Checks and Placebo Test).

First, I included two measures of macroeconomic condition and state financial health status. During economic downturns, states may reduce Medicaid physician reimbursement rate to balance budget. In the meantime, more individuals may become impoverished and qualified for Medicaid. Failing to control these would lead to a spurious relationship between Medicaid physician payment and Medicaid coverage. So, I included tax revenue per capita[17] and the unemployment rate[18].

Second, I included a measure of primary care physician supply. States may increase primary care reimbursement rate to encourage primary care physicians'

[17] The total tax revenue data comes from the Annual Survey of State Government Tax Collections conducted by the U.S. Census Bureau. Since the survey does not contain the tax revenue data of District of Columbia in 2010-2012, I supplemented the data using the DC Tax Facts. The state population data is from the U.S. Census Bureau.

[18] The unemployment data is from the Bureau of labor statistics, U.S. Department of Labor.

participation in Medicaid when there is a shortage of primary care physicians. An increase in the number of primary care physicians may increase the overall acceptance of Medicaid patients, improving their access to care, thus increasing the Medicaid take-up among potential beneficiaries. The omission of primary care physician supply would result in underestimation of β_1. Thus, I included an active primary care physician to population ratio per state and year.[19]

Third, I included Medicaid income eligibility thresholds. Medicaid income eligibility expansion increases the demand for Medicaid and thus increases the total number of enrollees. In the meantime, the Medicaid income eligibility thresholds were negatively associated with the reimbursement rates over the sample period; more liberal states had higher income eligibility limits but lower reimbursement rates.20 Without the inclusion of the Medicaid income eligibility limits would lead to an underestimation. To avoid this bias, I included Medicaid income eligibility thresholds of both parents and childless adults.[21]

In addition to the state-level controls, Trend is a vector of the state-specific linear trend to control for any other confounding time-varying factors unique to each state.

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[19] The active primary care physician to population ratio is from bi-annual State Physician Workforce Data Reports, produced by the Association of American Medical Colleges. The report is available in the years 2009, 2011, 2013, 2015. I interpolated the ratio of other years using the active primary care physician and population in the closest years.

[20] The data is available upon request.

[21] Both eligibility thresholds were from the Kaiser Family Foundation, taking into account earning disregards and the Medicaid expansion status in 2014. The parents' income eligibility thresholds were based on a family of three. The childless adults' income eligibility thresholds were based on the coverages providing full Medicaid benefits. The childless adults' income eligibility thresholds for waiver programs providing more limited benefits or for fully state-funded programs were not included. Since the survey does not cover the childless adults' threshold of 2010, I collected them from state websites. Only Arizona, Connecticut, Delaware, District of Columbia, Hawaii, New York, Vermont provided full Medicaid benefits for childless adults in 2011. I interpolated the benefits threshold of 2010 based on the states' websites.

These factors could include the hassle of Medicaid enrollment process (e.g., Face-to-face interview requirement when applying), other benefits and costs of Medicaid (e.g., annual cost sharing), and state contextual factors (e.g., presumptive eligibility, asset test for eligibility determination, asset limit, et al.) (Sommers, Buchmueller, et al. 2012, Hahn 2013). Finally, State is a vector of state fixed effects, Year is a vector of year fixed effects, and ε is a random error. The variation for identification comes from the change of the primary care fee ratio within states over the years. Robust standard errors were clustered at the state-level to allow for any within-state serial correlation. All estimates used ACS sampling weights.

2.6 Results

2.6.1 Graphical Evidence

Figure 2.2 plots the national average Medicaid coverage rate among non-pregnant civilian adults aged 27–64, whose income was below 250% of the FPL. It shows that the average Medicaid coverage rate increased from 2010 to 2012, before decreasing somewhat from 2012 to 2013, and increased substantially from 2013 to 2014. Part of the reason for the substantial increase was the Medicaid expansion started in 2014, targeting adults with income less than 138% of the FPL.

The DD method requires a treatment and control group to follow the same pre-trend. As an internal validity check of the DD method, I separated the states into two groups according to the Medicaid-to-Medicare fee ratios in 2012. Specifically, the states with the fee ratios below the national median in 2012 had a more substantial fee ratio increase from 2012 to 2013, thus were treated as a treatment group (dashed line). The other states had a smaller fee ratio increase during the period and were treated as a control group (solid line). A similar trend of the average Medicaid coverage rate among the treatment and control groups before 2013 would enhance the internal validity of the DD method. Figure 2.3 presents that the treatment and control group follow the same trend of adult Medicaid coverage rate through 2012.

The DD method compares the pre-post difference in the average Medicaid coverage rate of the treatment group with that of the control group. The result of the comparison is the DD estimator, β_1, reflecting the average effect of the reimbursement rate on adult Medicaid coverage rate. Figure 2.3 shows that the Medicaid coverage rate of the control group dropped by approximately 0.5-percentage-point, while that of treatment group stayed almost the same when the primary care payment parity started in 2013. The Medicaid coverage rates of both groups followed a very similar trend from 2013 to 2014, when the fee ratios stayed almost unchanged. This graph shows that the parity was associated with an approximately 0.5-percentage-point increase in the Medicaid coverage rate among adults.

2.6.2 Medicaid Coverage Rate

Table 2.2 presents the baseline regression results of the DD method. Since all the fee ratios are in decimal points, their coefficients multiplied by ten are interpreted as the effect of a 10percentage-point increase in the fee ratios. Results show that the increases in the fee ratios for patient office visits are associated with higher Medicaid coverage probability among adults. Column 1 & 2 show that a 10-percentage-point increase in the $FeeRatio$ of 30- (45) minute new patient office visit is associated with a 0.40 (0.36)-percentage-point increase in adult Medicaid coverage probability (reflecting 1.5 (1.4) percent increase relative to the mean). This Medicaid coverage increase could happen through two channels. A higher payment to a new patient office visit incentivizes physicians to accept new Medicaid patients, who would otherwise not have access to primary care or specialty care assuming no charity care. Further, physicians may directly enroll uninsured new patients into Medicaid.[22] Potential Medicaid beneficiaries may increase Medicaid take-up, perceiving the benefit of receiving healthcare under Medicaid.

Column 3 & 4 show that the reimbursement rate increases of established patient office visits have a smaller and insignificant positive effect on adult Medicaid coverage. Specifically, a 10-percentage-point increase of the $FeeRatio$ of 15- (25-) minute established patient office visit is associated with a 0.18 (0.15)-percentage-point increase in adult Medicaid coverage probability (reflecting 0.7 (0.6) percent increase relative to the mean). A higher payment to an established patient office visit encourages physicians to see established Medicaid patients more often, who would otherwise wait long before their next appointments. Potential beneficiaries may perceive the benefit of better care continuity under Medicaid and increase their participation rate. The difference between the estimates of new and the established patient office visit is likely due to the difference in perception of access to care. Previous literature found that self-rated access to care is mainly determined by having health insurance, rather than having continuity of care (Stewart et al. 1997), suggesting that the perception of access to care enhancement from shorter wait-to-appointment in days is likely to be less salient, compared to that from the change from not being accepted to being accepted by physicians.

[22] Considering Medicaid allows benefits to be covered retroactively for up to 3 months before the month of application, physicians could accept new uninsured patients and enroll them into Medicaid during or after the new patient visit.

Regarding the individual- and state-level controls, their coefficients have expected signs and very similar magnitudes and significance across all columns. Specifically, being female or unmarried was more likely to be covered by Medicaid. Being African-Americans was positively correlated with Medicaid coverage while being White or Asian has inverse correlations. Medicaid coverage probability decreased at an increasing rate as educational attainment increased. The primary care physician supply was strongly negatively associated with Medicaid coverage. Additionally, both types of Medicaid income eligibility limits were positively associated with Medicaid coverage, and the association was more extensive and more significant with childless adults' limit.

2.7 Robustness Checks and Placebo Test

2.7.1 Additional State-level Controls

To begin with, I included three other state-level controls to the baseline model. First, the diffusion of Medicaid MC delivery system across states may affect the physician reimbursement rate and Medicaid coverage simultaneously. The failure to exclude the partial effect of MC diffusion on Medicaid coverage might lead to biased estimates of $FeeRatio$. I added the MC penetration rate (column 2 of Table 2.3).[23] Second, states with higher lift in reimbursement rates may also become more generous in other welfare or assistance programs. These programs may raise the familiarity of Medicaid among their recipients and raise Medicaid take-up probability. I included the monthly average number of welfare recipients (column 3 of Table 2.3).[24] Third, poorer

[23] The managed care penetration rate was obtained from the annual Medicaid Managed Care Enrollment Reports, produced by the CMS.

[24] The data came from the Office of Family Assistance, Administration of Children & Families, Department of Health & Human Services.

states have higher Federal Medical Assistance Percentages (FMAP) and more residents eligible for Medicaid. Their reimbursement rate may be higher due to the higher federal matching rate. I included FMAP (column 4 of Table 2.3).[25] The column 5 included all three additional controls. The results show very similar estimates and its significance of the $FeeRatio$ across specifications.

2.7.2 Various Income Cutoffs and Placebo Test

Next, I used various low-income cutoffs to select potential Medicaid beneficiaries and run a placebo test. As a sensitivity test of the low-income threshold of the baseline model (below 250% of the FPL), I varied the threshold from below 100% of the FPL (column 2) to 200% of the FPL (column 4) at a step of 50% of the FPL in Table 2.4. The estimates are highly significant across all patient office visit services and the largest using the lowest income threshold. The magnitudes of the estimates decrease as the income threshold increases and stabilize as it reaches 250% of the FPL. For instance, a 10-percentage point increase of the $FeeRatio$ of 30- (45-) minute new patients' office visit and 15- (25-) minute established patient office visit are associated with an additional 0.87 (0.80) and 0.70 (0.61)-percentage-point of Medicaid coverage probability among those with family income below 100% of the FPL. Also, I did a placebo test using the high-income cutoff of above 400% of the FPL. It is expected that these individuals will not increase their Medicaid coverage since they're too rich to qualify for it (column 5). The placebo results show a miniature and negative association, if any, between the Medicaid primary care physician payment and their Medicaid coverage.

[25] FMAP was collected from Federal Register.

2.7.3 Low Educational Attainment

The third robustness check used low-educational attainment instead of low-income to select individuals eligible for Medicaid, since they may reduce labor supply and income to qualify for Medicaid. To avoid potential selection bias, I used a sample of adults without a high school degree (column 1 of Table 2.5).[26] The status of not having a high school degree is highly correlated with that of income being less than 250% of the FPL, with a correlation coefficient of 0.34. Also, both criteria are highly correlated with Medicaid coverage probability, with correlation coefficients being 0.22 (low-income) and 0.23 (low education) respectively. The results show larger estimates of the $FeeRatio$ across all services than the baseline results. For instance, a 10-percentage-point increase in the rate of 30 minutes' new patient office visit is correlated with a 0.49-percentage-point higher Medicaid coverage probability (reflecting a 1.5 percent increase relative to the mean 0.32).

2.7.4 Expected Reimbursement Ratio Under FFS & MC

The validity of the baseline results relies on the assumption that Medicaid reimbursement rate under FFS is still a good proxy for Medicaid physician payment. To verify this assumption, I reran the baseline model using the fee ratio under FFS & MC. If there is no substantial difference across the payment variation under the two methods, the estimates on the $FeeRatio$ won't change much. But, considering the impact of MC payment on the access to care among adults is limited,[27] I expect the estimate under both payment methods to be smaller and less significant.

[26] I also tried using the adults without any college education, which, however, is not highly correlated with either low-income or Medicaid coverage status.

[27] Please refer to the Section of Subsample Analyses for more explanation.

To construct an expected fee ratio under both FFS & MC, I first followed Alexander and Schnell (2018) to create an expected enrollment-weighted Medicaid fee across both FFS and MC for each service.[28] Second, I divided this expected Medicaid fee by the corresponding Medicare fee to obtain an expected Medicaid-to-Medicare fee ratio under both methods. The results show a consistent pattern of results with baseline results, though coefficients are smaller and less significant (column 2 of Table 2.5). A 10-percentage point increase in the average fee ratio of 30- (45-) minute new patient office visit is associated with 0.25 (0.23)-percentage-point increase of Medicaid coverage probability.

2.7.5 Post-2012 Sample

As another robustness check, I tested the sensitivity of results using the post-2012 sample. Fee ratio could be endogenous if it was internally determined by the system of demand for Medicaid. The baseline estimates could suffer from omitted variable bias if the fee ratio was correlated with state policies happen simultaneously. While there is no direct test for the randomness of fee ratio, I tested the sensitivity of baseline results using exogenous fee ratio. Specifically, I ran the baseline model using the sample of 2012–14, when the fee ratio was entirely determined by the primary care payment parity. Similar results from using the entire sample period (baseline results) and that using 2012–14 would strengthen the confidence in the reliability of the baseline estimates. After the

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[28] This expected Medicaid fee is calculated as follows.

$$\widetilde{Fee_{ist}} = (1 - P_{st}^{MC}) \times Fee_{ist}^{FFS} + P_{st}^{MC} \times Fee_{ist}^{FFS} \times (\frac{Fee^{MC}}{Fee^{FFS}})_{s,2010} \tag{2}$$

where Fee_{ist}^{FFS} is the Medicaid fee under FFS for service *i* in state *s* and year *t*, P_{st}^{MC} is the percentage of Medicaid beneficiaries enrolled in comprehensive managed care per state and year, $(\frac{Fee^{MC}}{Fee^{FFS}})_{s,2010}$ is MC to FFS fee ratio for office visits based on 20 states in 2009 and 2010 (mainly) from (Government Accountability Office 2014) (the median ratio of available states was used for missing states).

sample restriction, the estimates are larger and less significant, possibly due to weaker statistical power (column 3 of Table 2.5). However, there is no substantial difference from the baseline estimates.

2.7.6 States with Reimbursement Ratio Available in All Years

Besides, I examined the sensitivity of the baseline results to data incompleteness. In the final dataset of Medicaid-to-Medicare fee ratio, three states have four years' data (missing the data of 2013 or 2014), four states have three years' data (missing the data of 2013–14), and three states (Tennessee, Hawaii, and South Dakota) have no data. If these states changed the Medicaid reimbursement rate in a way systematically different from the states with complete data in the data missing years, missing data could bias the estimate of interest. Addressing this, I reran the baseline results using the 41 states with complete fee ratio data. The estimates are very similar in magnitudes and significance to the baseline results (column 4 of Table 2.5).

2.7.7 Logit Model

Additionally, I used a logistic model instead of LPM to test the results' sensitivity to the distribution assumption of random error. The error was assumed to follow a normal distribution in LPM and was assumed to follow a logistic distribution in the logistic model. Column 5 of Table 2.5 presents the estimates of marginal effects from the logistic model on the sample of non-elderly adults with family income below 250% of the FPL. The results show similar estimates on the $FeeRatio$. A 10-percentage-point increase in the fee ratio of 30- (45-) minute new patient office visit is associated with a 0.42 (0.39)-percentage-point increase in the Medicaid coverage probability (reflecting 1.6 (1.5) percent increase relative to the mean).

2.8 Extension

2.8.1 Subsample Analyses

First, as states gradually moving lower-risk Medicaid population into risk-based MC, the aged, disabled and children with special health care needs are most likely to be served through FFS arrangements (Government Accountability Office 2016). Different from FFS reimbursement rate, MC capitation rate discourages physicians from increasing service volume (Robinson 2001) and has limited impact on access to care and health care utilization for adults (Shen and Zuckerman 2005). So, I expect the fee ratio to have a stronger effect on the high-risk population. While the estimates on young adults (aged below 50) are barely significant (column 1 of Table 2.6), the estimates on the near-elderly (aged 50–64) are highly significant and much bigger (column 2). Specifically, a 10-percentage-point increase in the fee ratio for 30- (45-) minute new patient office visit is associated with a 0.52-percentage-point increase in Medicaid coverage rate among the near-elderly. Similarly, the same increase of 15- (25-) minute established patient office visit is associated with a 0.46 (0.44)-percentage-point increase in Medicaid coverage rate. These findings suggest that near-elderly, who are more likely to have marginal health status than younger adults, is also more likely to be covered by FFS arrangements.

Second, the effect of the fee ratio on Medicaid coverage may differ across previously-eligible and newly-eligible adults. The Medicaid expansion under the ACA mostly affects the eligibility status of adults without dependent children (childless adults), who had categorical restrictions and did not qualify for Medicaid before the expansion (Kenney, Zuckerman, et al. 2012). Even though the full expansion did not start until 2014, some states adopted the early expansion option to extend Medicaid coverage to childless adults with income up to 200% of the FPL after 2010 (Sommers, Kenney, and Epstein 2014). To explore the heterogeneous effect across adults who are newly eligible for Medicaid and those who are not, I present the results using adults with dependent children living in the same household (column 3) and those without (column 4) in Table 2.6. The results show a larger and more significant effect of fee ratios on coverage rate among childless adults. Specifically, a 10-percentage-point increase in the payment generosity of 30- (45-) minute new patient office visit is associated with a 0.45 (0.41)-percentage-point increase in the Medicaid coverage rate among childless adults, while with a 0.34 (0.31)-percentage-point among adults living with children. The results using the payment to established patient office visits have a similar pattern. The results imply that the fee ratio impact is larger among newly-eligible adults, who are more likely to delay care because of the cost before becoming eligible for Medicaid.

Third, the effect of the fee ratio on Medicaid coverage may vary across race. The racial disparity exists in the quality of access to care even among individuals covered by the same type of insurances. Compared to the counterpart mainly treating white patients, primary care physicians mostly treating African-American patients were less likely to be board certified and faced greater difficulties in obtaining high-quality subspecialists, diagnostic imaging, and nonemergency admission to hospitals for their patients (Bach et al. 2004). Because of the lower quality of access to care, African-American adults are more likely to respond to the improvement in access to care. Table 2.7 presents the results using white (column 1) and African-American adults (column 2). A 10-percentage-point increase in the fee ratio for 30- (45-) minute new patient office visit is associated with an increase in Medicaid coverage rate by a 0.34 (0.32)-percentage-point among white adults and by a 0.86 (0.77)-percentage-point among the African-Americans. The African-Americans' larger effect is likely due to that higher payment attracted physicians, who used to accept white patients and offer a higher quality of care, to treat more African-American patients.

Fourth, the impact of the fee ratio on Medicaid coverage may differ across urban and rural areas. Physicians in urban areas were less likely to accept new Medicaid patients compared to rural areas (Cunningham and May 2006, Decker 2012). This suggests that the barrier of access to care among Medicaid beneficiaries is higher in urban than that in rural areas. Also, the uninsured population is less likely to have a regular source of care in urban than in rural areas (Hartley, Quam, and Lurie 1994). Together, I expect the effect of fee ratio to be concentrated on adults living in urban areas. I separated the sample into three geographical areas: outside of metro areas (rural), in metro areas but outside of central cities (suburb), and in central cities (central city) (the right part of Table 2.7). Results show that a 10-percentage-point increase in the fee ratio for 30- (45-) minute new patients' office visit is associated with an additional 0.39 (0.41)-percentage-point of Medicaid coverage rate among adults living in rural areas and with an extra 0.74-percentage-point of Medicaid coverage rate among those in central cities, while with no significant Medicaid coverage increase among those in suburb areas.

2.8.2 Other Insurance Coverage Rates

The primary analyses imply a positive impact of Medicaid primary care reimbursement rate on Medicaid coverage among adult beneficiaries. This increase in Medicaid coverage could come from uninsured taking up Medicaid or from privately insured replacing private plans with Medicaid. To explore this, I ran the baseline model with the dependent variable being uninsured and being privately insured. Table 2.8 shows that the fee ratio is negatively associated with being uninsured (column 1), while it is not associated with being privately insured (column 2). Specifically, a 10-percentage-point increase in the fee ratio of 30(45-) minute new patient office visit is associated with a 0.40 and 0.39-percentage-point reduction in the probability of being uninsured (reflecting 1.4 percent decrease relative to the mean 0.28). Also, a 10-percentage-point increase in the fee ratio of 15- (25-) minute established patient office visit is associated with a 0.32 (0.31)-percentage-point reduction in the probability of being uninsured (reflecting 1.1 percent decrease relative to the mean). In contrast, the estimates in column 2 are close to zero and insignificant, suggesting the rate increase is not associated with crowd-out from private insurance.

2.9 Discussion and Conclusion

Despite the insurance expansion under the ACA, the insurance coverage gap among low-income adults is still large. The purpose of this paper is to evaluate the impact of raising program benefits on closing the coverage gap. Specifically, we study the effect of improving access to care, through increasing Medicaid primary care physician payments, on Medicaid coverage rate among adults. Medicaid primary care reimbursement rates have been consistently lower than that of Medicare and private insurance. Recent effort to increase the rates to Medicare level in 2013–14 aimed to address this payment gap and to improve access to care among Medicaid patients. Using Medicaid-to-Medicare fee ratios across states in 2010–2014, I showed that the Medicaid primary care payment parity substantially increased Medicaid rate from 2012 to 2013 across all states. Combined with the ACS data, this study found that the fee ratio increase of patient office visits was associated with a higher probability of Medicaid coverage and lower probability of being uninsured among low-income non-elderly adults. However, no relationship between the fee ratio increase and the propensity of being privately insured was found. For instance, a 10-percentage-point increase in the fee ratio of 30- (45-) minute new patient office visit was associated with a 0.40 (0.36)-percentage-point boost in their Medicaid coverage rate, and with a 0.40 (0.39)-percentage-point cut in their uninsured rate. These estimates among adults are almost 1-percentage-point less than that among children based on Hahn (2013). This smaller magnitude could be attributable to several factors, including higher information barrier, the higher opportunity cost of applying for Medicaid (e.g., the time (earning) lost for application), and higher welfare stigma among potential adult beneficiaries.

Also, the effect of the fee ratio increase on Medicaid participation varies across subpopulations. First, it is concentrated on near-elderly adults rather than young adults. Since near-elderly are more likely to enroll in FFS programs, this result implies that physicians' financial incentive to increase service volume is stronger with FFS than with MC programs. Second, the impact is greater among childless adults than adults living with dependent children, suggesting that the enhancement of access to care has a stronger effect on newly-eligible than previously-eligible adults. Third, the effect among African-American adults is at least twice as high as that among white counterparts. This evidence is consistent with the fact that the quality of care received by African-American patients is lower than that received by white patients with the same type of insurances. Fourth, the impact is stronger among adults living in urban areas than those in rural areas. This is consistent with the fact that Medicaid patients in urban areas face a more substantial barrier of access to care than those in rural areas.

This paper has limitations. The main results of this paper are based on Medicaid reimbursement rate under FFS, which might not be a good proxy for the accessibility to care Medicaid MC enrollees. Despite that, FFS beneficiaries are more sensitive to better access to primary care. They have more complex conditions, requiring primary care physicians to provide better continuing care and to obtain their access to high-quality specialists. I expect the effect of fee ratio increase to be more salient among them. Also, according to the reasons discussed above and the results from the robustness check using Medicaid-to-Medicare fee ratio under both FFS and MC, Medicaid reimbursement rate under FFS is still a good proxy for the overall Medicaid payment. Also, the Medicaid-to-Medicare fee ratio might suffer from endogeneity issues, which could bias the estimates of interest. I did an indirect test for endogeneity by gradually removing individual-level and state-level controls from the baseline specification. The resulting estimates are smaller but still significant. Besides, the robustness check results show little sensitivity to the inclusion of additional state-level controls. Despite being imperfect, these results all support that there is little concern of endogeneity using Medicaid-to-Medicare fee ratio.

The effectiveness of Medicaid expansions depends on whether the target population take-up public insurance or not. Several factors might affect Medicaid participation, including information barrier, administrative burden, program benefits, and welfare stigma. This paper explores the program benefits in terms of access to care in determining Medicaid take-up. The results of this paper provide clear evidence that the increase in Medicaid physician reimbursement rate is effective in promoting the

Medicaid participation rate among low-income non-elderly adults. Further, this paper implies that raising physician payment under Medicaid, in addition to providing Medicaid, would reduce the barrier of access to care among potential Medicaid beneficiaries. Compared to traditional expansions in income eligibility thresholds, physician reimbursement rate increase is advantageous since it might not be associated with crowd-out from private insurance to Medicaid.

Figure 2.1: Medicaid-to-Medicare Fee Ratio of 30-minute New Patient Office Visit

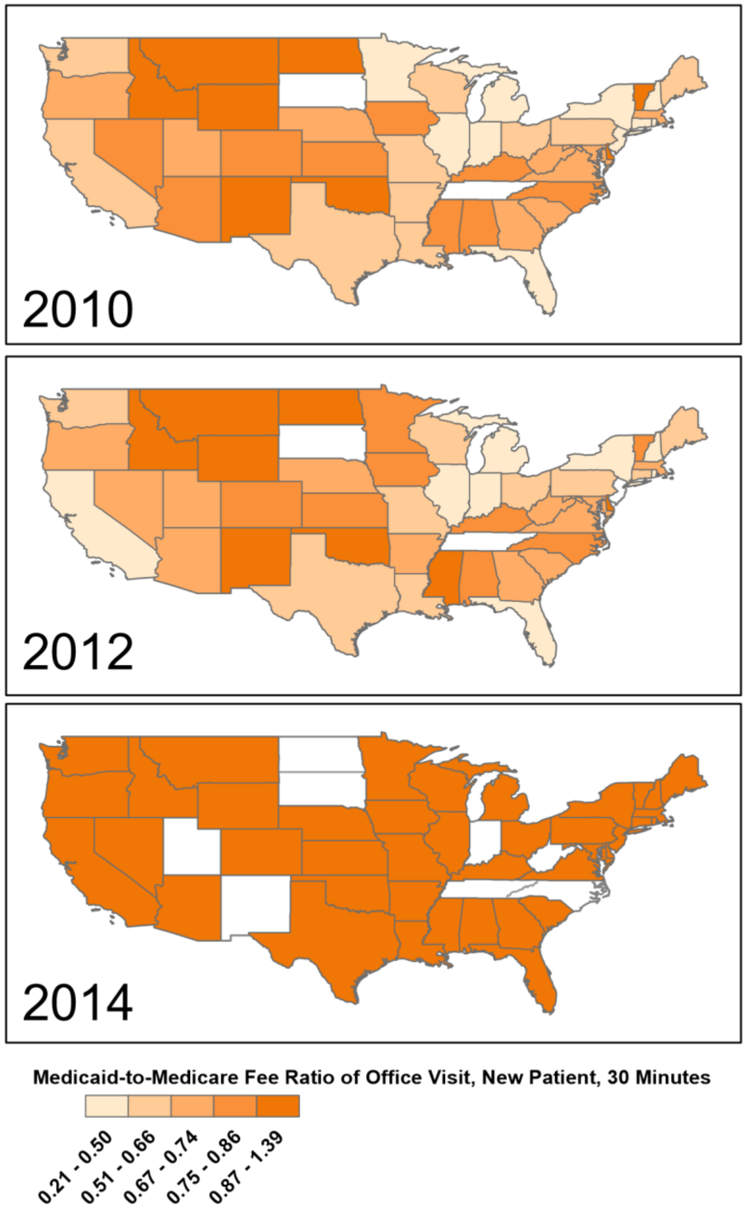
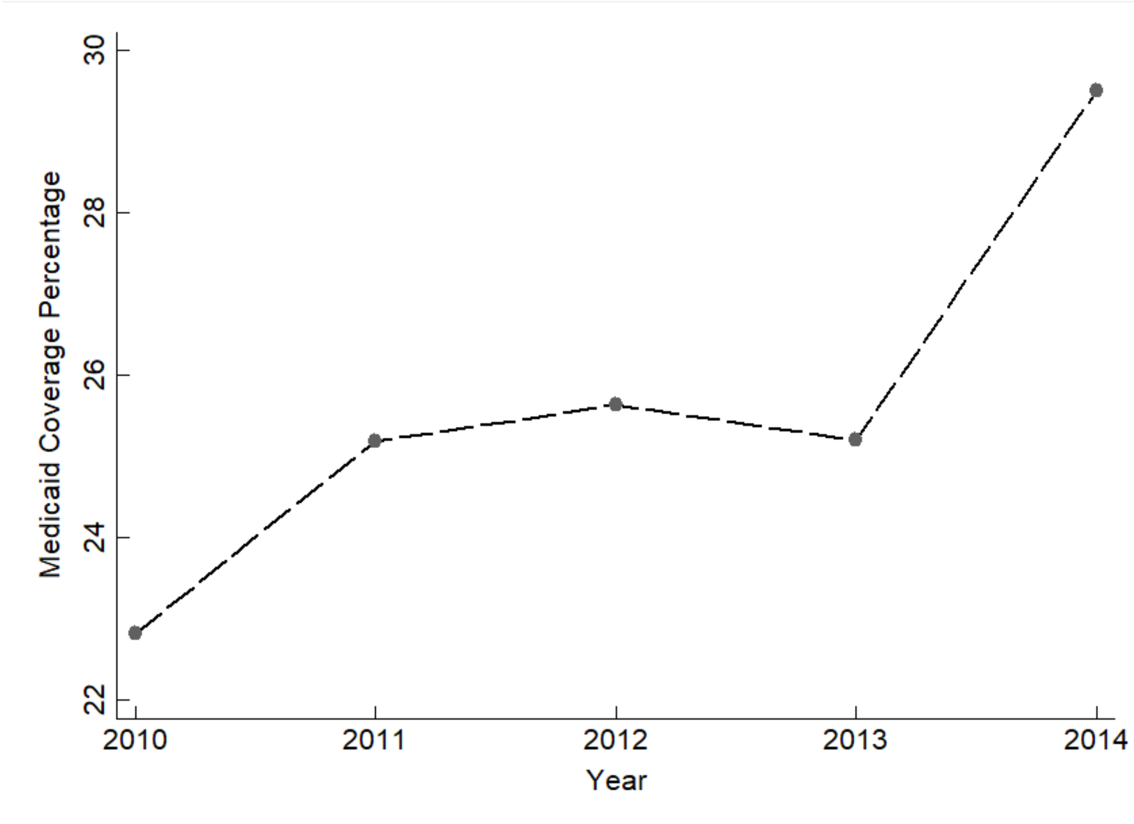


Figure 2.2: Medicaid-to-Medicare Fee Ratio of 30-minute New Patient Office Visit

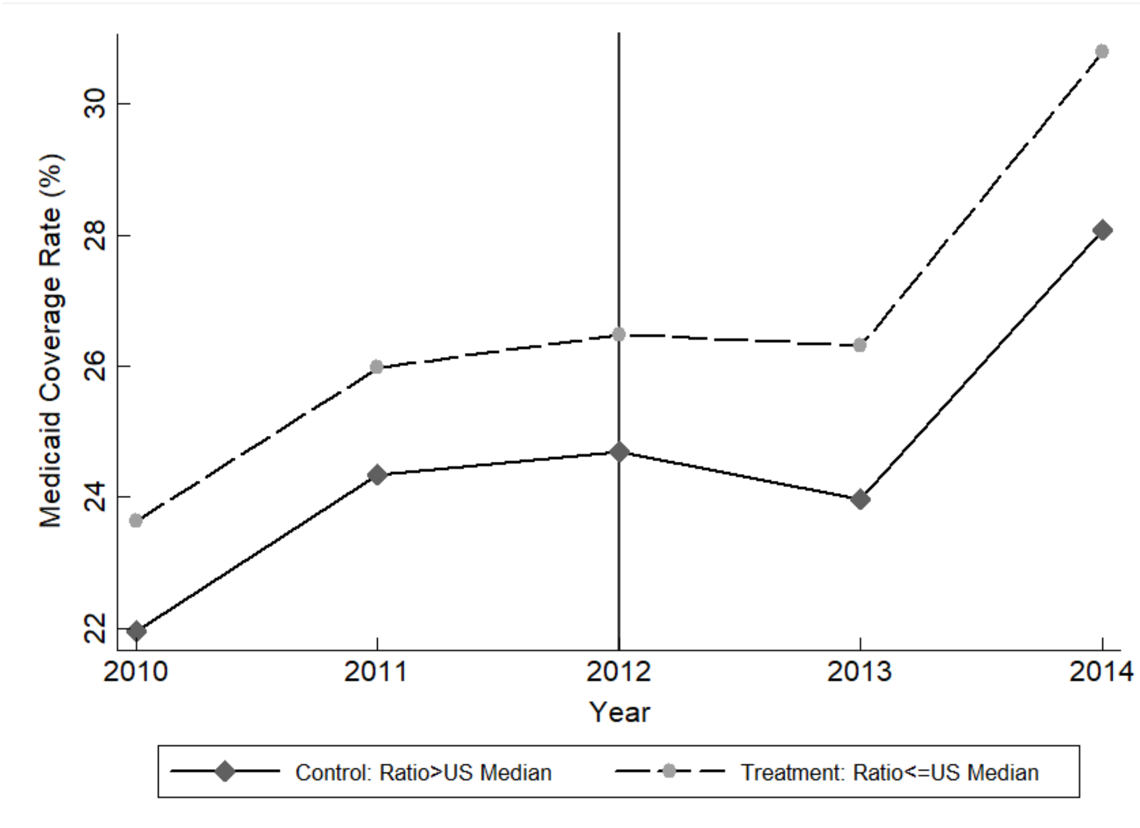
Notes: The map shows how the Medicaid-to-Medicare fee ratios for 30-minute new patient office visit (CPT: 99203) changed over the years 2010, 2012, and 2014. The fee ratios were calculated by the author using Medicaid and Medicare reimbursement rates, collected from state Medicaid agencies and the CMS. Blank stands for states whose fee ratios are not available. The fee ratios of 2010 are not available in Hawaii, South Dakota, and Tennessee. The fee ratios of 2012 are not available in Hawaii, South Dakota, and Tennessee. The fee ratios of 2014 are not available in Alaska, Hawaii, Indiana, New Mexico, North Carolina, South Dakota, Tennessee, Utah, and West Virginia.

Figure 2.3: Medicaid Coverage Rate among Low-income Adults



Notes: The figure shows that the Medicaid coverage rate among non-pregnant civilian adults aged 27-64, whose income is below 250% of the FPL, increased from 2010 to 2012, reduced somewhat from 2012 to 2013, and increased substantially from 2013 to 2014. Data is from ACS 2010-2014.

Figure 2.4: Medicaid Coverage Rate among Low-income Adults Grouped by Fee Ratio in 2012



Notes: The figure shows that the Medicaid coverage rate among non-pregnant civilian adults aged 27-64, whose income is below 250% of the FPL. The Medicaid coverage rate in the states with below median fee ratios for 30-minute new patient office visit in 2012 (dashed line) and the counterpart in those with above median fee ratios (solid line) had the same trend prior to the Medicaid primary care payment parity. The former stayed relatively constant from 2012 to 2013, while the latter dropped by a larger amount during this period. After the payment bump, both increased at the same rate from 2013 to 2014. Medicaid coverage data is from ACS 2010-2014. The fee ratios were calculated by the author using Medicaid and Medicare reimbursement rates, collected from state Medicaid agencies and the CMS.

Table 2.1: Summary Statistics

	N	Mean	SD	Min	Max
Outcome					
Medicaid Coverage	2169335	0.26	0.44	0	1
Individual Level Control					
Age	2169335	45.85	10.99	27	64
Female	2169335	0.54	0.5	0	1
Married	2169335	0.43	0.5	0	1
White	2169335	0.74	0.44	0	1
Black	2169335	0.17	0.38	0	1
Hispanic	2169335	0.03	0.18	0	1
Asian	2169335	0.03	0.18	0	1
High School Degree	2169335	0.85	0.36	0	1
Some College Education	2169335	0.25	0.43	0	1
College Degree	2169335	0.14	0.34	0	1
Income (% of FPL)	2169335	134.81	71.89	1	250

Notes: The table presents the summary statistics of non-pregnant civilian adults aged 27-64, whose income is below 250% of the FPL. The data is from ACS 2010-2014.

Table 2.2: Medicaid Coverage Rates

	Office Visit of New Patient		Office Visit of Established Patient	
	(1)	(2)	(3)	(4)
	30 Mins	45 Mins	15 Mins	25 Mins
FeeRatio	0.0400***	0.0363***	0.0180	0.0153
	(0.0145)	(0.0128)	(0.0124)	(0.0132)
Age	-0.000245	-0.000245	-0.000245	-0.000245
	(0.000362)	(0.000362)	(0.000363)	(0.000363)
Female	0.0488***	0.0488***	0.0488***	0.0488***
	(0.00346)	(0.00346)	(0.00346)	(0.00346)
Married	-0.0367***	-0.0367***	-0.0367***	-0.0367***
	(0.00425)	(0.00425)	(0.00425)	(0.00425)
White	-0.0346***	-0.0346***	-0.0346***	-0.0346***
	(0.00935)	(0.00935)	(0.00935)	(0.00935)
Black	0.0302***	0.0302***	0.0302***	0.0302***
	(0.00935)	(0.00935)	(0.00936)	(0.00936)
Hispanic	-0.0269	-0.0269	-0.0269	-0.0269
	(0.0221)	(0.0221)	(0.0221)	(0.0221)
Asian	-0.0304**	-0.0304**	-0.0304**	-0.0303**
	(0.0135)	(0.0135)	(0.0135)	(0.0135)
High School Degree	-0.110***	-0.110***	-0.110***	-0.110***
	(0.00560)	(0.00560)	(0.00560)	(0.00560)
Some College Education	-0.0396***	-0.0396***	-0.0396***	-0.0396***
	(0.00174)	(0.00174)	(0.00174)	(0.00174)
College Degree	-0.115***	-0.115***	-0.115***	-0.115***
	(0.00694)	(0.00694)	(0.00694)	(0.00694)
Income	-0.00141***	-0.00141***	-0.00141***	-0.00141***
	(0.0000862)	(0.0000862)	(0.0000862)	(0.0000862)
Unemployment Rate	-0.00315	-0.00298	-0.00308	-0.00316
	(0.00309)	(0.00319)	(0.00308)	(0.00303)
Tax Per Capita	0.00200	0.000962	0.00331	0.00385
	(0.00830)	(0.00788)	(0.00807)	(0.00820)
Active PCP /100,000	-0.00753***	-0.00738***	-0.00678**	-0.00668**
	(0.00250)	(0.00252)	(0.00258)	(0.00257)
Parents' Income Limit	0.000248**	0.000247**	0.000245**	0.000247**
	(0.000113)	(0.000114)	(0.000116)	(0.000116)

Table 2.2 Cont'd: Medicaid Coverage Rates

	Office Visit of New Patient		Office Visit of Established Patient	
	(1)	(2)	(3)	(4)
Childless Adults' Income Limit	0.000379∗∗∗	0.000380∗∗∗	0.000379∗∗∗	0.000378∗∗∗
	(0.0000475)	(0.0000480)	(0.0000470)	(0.0000467)
N	2095408	2095408	2095408	2095408
R-squared	0.136	0.136	0.136	0.136

Notes: The table presents the results of the linear probability models of Medicaid-to-Medicare fee ratios of patient office visits on Medicaid coverage among non-pregnant civilian adults aged 27-64, whose income is below 250% of the FPL. Column 1 uses the fee ratio of 30 minutes' new patient office visits, column 2 uses that of 45 minutes' new patient office visits, column 3 uses that of 15 minutes' established patient office visits, and column 4 uses that of 25 minutes' established patient office visits. All regressions use state and year fixed effects and state-specific linear yearly trend. The fee ratios were calculated by the author using Medicaid and Medicare reimbursement rates, collected from state Medicaid agencies and the CMS. Main data is from ACS 2010-2014. Robust standard errors are clustered at the state level. ∗∗∗ $p<0.01$, ∗∗ $p<0.05$, ∗ $p<0.1$, + $p<0.15$.

Table 2.3: Additional State-level Controls

	Office Visit, New Patient, 30 Minutes				
	(1) Baseline	(2) MC Rate	(3) Welfare Caseloads	(4) FMAP	(5) All Included
FeeRatio	0.0400***	0.0383**	0.0403**	0.0402***	0.0392**
	(0.0145)	(0.0150)	(0.0152)	(0.0142)	(0.0152)
	Office Visit, New Patient, 45 Minutes				
FeeRatio	0.0363***	0.0346**	0.0365***	0.0365***	0.0353**
	(0.0128)	(0.0133)	(0.0133)	(0.0124)	(0.0134)
	Office Visit, Established Patient, 15 Minutes				
FeeRatio	0.0180	0.0160	0.0177	0.0180	0.0159
	(0.0124)	(0.0131)	(0.0130)	(0.0123)	(0.0136)
	Office Visit, Established Patient, 25 Minutes				
FeeRatio	0.0153	0.0131	0.0150	0.0153	0.0130
	(0.0132)	(0.0140)	(0.0138)	(0.0131)	(0.0146)
N	2095408	2095408	2095408	2095408	2095408
R-squared	0.136	0.136	0.136	0.136	0.136

Notes: The table presents the results of the linear probability models of Medicaid-to-Medicare fee ratios of patient office visits on Medicaid coverage among non-pregnant civilian adults aged 27-64, whose income is below 250% of the FPL. Only the coefficients on the fee ratios are included. The first part uses the fee ratio of 30 minutes' new patient office visits; the second part uses that of 45 minutes' new patient office visits; the third part uses that of 15 minutes' established patient office visits, and the fourth part uses that of 25 minutes' established patient office visits. Column 1 repeats the baseline specification, column 2 adds Medicaid MC penetration rate, column 3 adds an average monthly number of welfare recipients (1,000s), column 4 adds FMAP, and column 5 adds all three controls. All regressions use state and year fixed effects and state-specific linear yearly trend. The fee ratios were calculated by the author using Medicaid and Medicare reimbursement rates, collected from state Medicaid agencies and the CMS. Main data is from ACS 2010-2014. The MC penetration rate is from the annual Medicaid MC Enrollment Reports of the CMS. The monthly number of welfare recipients comes from the Office of Family Assistance, Administration of Children & Families. FMAP is from Federal Register. Robust standard errors are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1, + p<0.15.

Table 2.4: Various Income Cutoffs and Placebo Test

	(1) Baseline ≤250% FPL	(2) ≤100% FPL	(3) ≤150% FPL	(4) ≤200% FPL	(5) Placebo ≥400% FPL
	Office Visit, New Patient, 30 Minutes				
FeeRatio	0.0400***	0.0869***	0.0562***	0.0438***	-0.00215
	(0.0145)	(0.0238)	(0.0179)	(0.0162)	(0.00246)
	Office Visit, New Patient, 45 Minutes				
FeeRatio	0.0363***	0.0804***	0.0503***	0.0399***	-0.00250
	(0.0128)	(0.0227)	(0.0165)	(0.0147)	(0.00230)
	Office Visit, Established Patient, 15 Minutes				
FeeRatio	0.0180	0.0696***	0.0285*	0.0213	-0.00342
	(0.0124)	(0.0211)	(0.0167)	(0.0148)	(0.00224)
	Office Visit, Established Patient, 25 Minutes				
FeeRatio	0.0153	0.0605***	0.0251	0.0176	-0.00330
	(0.0132)	(0.0222)	(0.0175)	(0.0157)	(0.00237)
N	2095408	706935	1142270	1608285	2983710
R-squared	0.136	0.111	0.0969	0.116	0.0245

Notes: The table presents the results of the linear probability models of Medicaid-to-Medicare fee ratios of patient office visits on Medicaid coverage among non-pregnant civilian adults aged 27-64. Only the coefficients on the fee ratios are included. The first part uses the fee ratio of 30 minutes' new patient office visits; the second part uses that of 45 minutes' new patient office visits; the third part uses that of 15 minutes' established patient office visits; and the fourth part uses that of 25 minutes' established patient office visits. Column 1 uses the subsample of adults with income below 250% of the FPL, column 2 uses the subsample of adults with income below 100% of the FPL, column 3 uses the subsample of adults with income below 150% of the FPL, column 4 uses the subsample of adults with income below 200% of the FPL, and column 5 uses the subsample of adults with income above 400% of the FPL as a placebo test. All regressions use state and year fixed effects and state-specific linear yearly trend. The fee ratios were calculated by the author using Medicaid and Medicare reimbursement rates, collected from state Medicaid agencies and the CMS. Main data is from ACS 2010-2014. Robust standard errors are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1, + p<0.15.

Table 2.5: Further Robustness Checks

	(1) No HighSch Degree	(2) FFS&MC Payment	(3) Post-2012	(4) Full Data States	(5) Logit Model
	Office Visit, New Patient, 30 Minutes				
FeeRatio	0.0485**	0.0250**	0.0523*	0.0406**	0.0423***
	(0.0238)	(0.0117)	(0.0296)	(0.0179)	(0.0145)
	Office Visit, New Patient, 45 Minutes				
FeeRatio	0.0393*	0.0227***	0.0538*	0.0350**	0.0385***
	(0.0224)	(0.0107)	(0.0279)	(0.0154)	(0.0126)
	Office Visit, Established Patient, 15 Minutes				
FeeRatio	0.0304	0.0115	0.0367	0.0128	0.0196+
	(0.0221)	(0.0105)	(0.0276)	(0.0152)	(0.0122)
	Office Visit, Established Patient, 25 Minutes				
FeeRatio	0.0276	0.00864	0.0336	0.00954	0.0171
	(0.0214)	(0.0102)	(0.0282)	(0.0151)	(0.0132)
N	459546	2095408	1235761	1937363	2095408

Notes: The table presents the results of the linear probability models of the Medicaid-to-Medicare fee ratio of patient office visits on Medicaid coverage among non-pregnant civilian adults aged 27-64, with either low-income or low-education attainment. The first part uses the fee ratio of 30 minutes' new patient office visits; the second part uses that of 45 minutes' new patient office visits; the third part uses that of 15 minutes' established patient office visits; and the fourth part uses that of 25 minutes' established patient office visits. Column 1 uses the fee ratios under FFS and the linear probability models (LPM) on the subsample of adults without high school degrees in all states and years of 2010-2014. Column 2 uses the fee ratio under both FFS &MC and the LPM on the subsample of adults with income below 250% of the FPL in all states and years. Column 3 uses the fee ratio under FFS and the LPM on the subsample of adults with income below 250% of the FPL in all states and years of 2010-2013. Column 4 uses the fee ratios under FFS and the LPM on the subsample of adults with income below 250% of the FPL in the 41 states with the fee ratios available in all years. Column 5 uses the fee ratios under FFS and the logistic model on the subsample of adults with income below 250% of the FPL in all states and years. All regressions use state and year fixed effects, and state-specific linear yearly trend. Each column reports the coefficients on the fee ratios, except column 5 reports the marginal effects of the fee ratios. The fee ratios were calculated by the author using Medicaid and Medicare reimbursement rates, collected from state Medicaid agencies and the CMS. Main data is from ACS 2010-2014. Robust standard errors are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1, + p<0.15.

Table 2.6: Subsample Analyses by Age and Parental Status

	(1) <= 50	(2) > 50	(3) Parents	(4) Childless Adults
	Office Visit, New Patient, 30 Minutes			
FeeRatio	0.0320*	0.0522***	0.0338**	0.0448***
	(0.0169)	(0.0193)	(0.0157)	(0.0163)
	Office Visit, New Patient, 45 Minutes			
FeeRatio	0.0271*	0.0515***	0.0308**	0.0410***
	(0.0144)	(0.0183)	(0.0136)	(0.0149)
	Office Visit, Established Patient, 15 Minutes			
FeeRatio	0.00301	0.0462**	0.0105	0.0245*
	(0.0134)	(0.0200)	(0.0151)	(0.0135)
	Office Visit, Established Patient, 25 Minutes			
FeeRatio	0.000453	0.0436**	0.00916	0.0206
	(0.0142)	(0.0191)	(0.0156)	(0.0142)
N	1286676	808732	1004426	1090982
R-squared	0.147	0.131	0.177	0.125

Notes: The table presents the results of the linear probability models of Medicaid-to-Medicare fee ratios of patient office visits on Medicaid coverage among non-pregnant civilian adults aged 27-64, whose income is below 250% of the FPL. Only the coefficients on the fee ratios are included. The first part uses the fee ratio of 30 minutes' new patient office visits; the second part uses that of 45 minutes' new patient office visits; the third part uses that of 15 minutes' established patient office visits; and the fourth part uses that of 25 minutes' established patient office visits. Column 1 uses the subsample of adults aged 27-50, column 2 uses the subsample of adults aged 50-64, column 3 uses the subsample of adults with children living in the same household, and column 4 uses the subsample of childless adults. All regressions use the same individual and state level controls as in Table 2, state and year fixed effects and state-specific linear yearly trend. The fee ratios were calculated by the author using Medicaid and Medicare reimbursement rates, collected from state Medicaid agencies and the CMS. Main data is from ACS 2010-2014. Robust standard errors are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1, + p<0.15.

Table 2.7: Subsample Analyses by Race and Metro Status

	(1) White	(2) African-American	(3) Rural	(4) Suburb	(5) CentralCity
	Office Visit, New Patient, 30 Minutes				
FeeRatio	0.0340**	0.0856***	0.0393**	0.0264	0.0742*
	(0.0152)	(0.0283)	(0.0177)	(0.0251)	(0.0372)
	Office Visit, New Patient, 45 Minutes				
FeeRatio	0.0322**	0.0774***	0.0410**	0.0221	0.0741**
	(0.0139)	(0.0277)	(0.0173)	(0.0221)	(0.0342)
	Office Visit, Established Patient, 15 Minutes				
FeeRatio	0.0170	0.0456	0.0271*	0.00812	0.0334
	(0.0133)	(0.0291)	(0.0161)	(0.0241)	(0.0324)
	Office Visit, Established Patient, 25 Minutes				
FeeRatio	0.0125	0.0518*	0.0188	0.00821	0.0250
	(0.0144)	(0.0286)	(0.0215)	(0.0219)	(0.0362)
N	1550170	364902	382197	440048	263791
R-squared	0.124	0.143	0.129	0.128	0.173

Notes: The table presents the results of the linear probability models of Medicaid-to-Medicare fee ratios of patient office visits on Medicaid coverage among non-pregnant civilian adults aged 27-64, whose income is below 250% of the FPL. Only the coefficients on the fee ratios are included. The first part uses the fee ratio of 30 minutes' new patient office visits; the second part uses that of 45 minutes' new patient office visits; the third part uses that of 15 minutes' established patient office visits; and the fourth part uses that of 25 minutes' established patient office visits. Column 1 uses the subsample of white adults, column 2 uses the subsample of African-American adults, column 3 uses the subsample of adults not living in a metro area, column 4 uses the subsample of adults living in a metropolitan area but outside of its central city, and column 5 uses the subsample of adults living inside the central city. All regressions use state and year fixed effects and state-specific linear yearly trend. The fee ratios were calculated by the author using Medicaid and Medicare reimbursement rates, collected from state Medicaid agencies and the CMS. Main data is from ACS 2010-2014. Robust standard errors are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1, + p<0.15.

Table 2.8: Other Insurance Coverage Rates

	Office Visit, New Patient, 30 Minutes	
	(1)	(2)
	Uninsured	Private
FeeRatio	-0.0404∗∗∗	-0.00254
	(0.0147)	(0.0130)
	Office Visit, New Patient, 45 Minutes	
FeeRatio	-0.0392∗∗∗	-0.00272
	(0.0144)	(0.0124)
	Office Visit, Established Patient, 15 Minutes	
FeeRatio	-0.0323∗∗	0.00430
	(0.0138)	(0.0128)
	Office Visit, Established Patient, 25 Minutes	
FeeRatio	-0.0305∗∗	0.00647
	(0.0142)	(0.0132)
N	2095408	2095408
R-squared	0.0626	0.183

Notes: The table presents the results of the linear probability models of Medicaid-to-Medicare fee ratios of patient office visits on the uninsured and privately insured status among non-pregnant civilian adults aged 27-64, whose income is below 250% of the FPL. Only the coefficients on the fee ratios are included. The first part uses the fee ratio of 30 minutes' new patient office visits; the second part uses that of 45 minutes' new patient office visits; the third part uses that of 15 minutes' established patient office visits; and the fourth part uses that of 25 minutes' established patient office visits. Column 1 uses the dependent variable of being uninsured, and column 2 uses that of being privately insured. All regressions use state and year fixed effects and state-specific linear yearly trend. The fee ratios were calculated by the author using Medicaid and Medicare reimbursement rates, collected from state Medicaid agencies and the CMS. Main data is from ACS 2010-2014. Robust standard errors are clustered at the state level. ∗∗∗ p<0.01, ∗∗ p<0.05, ∗ p<0.1, + p<0.15.

Chapter 3: Public Insurance and Birth Outcomes: Evidence from Medicaid Implementation

3.1 Introduction

Medicaid is one of the most important sources of health insurance for the disadvantaged population in the US. The goal of Medicaid is to provide qualified individuals access to health care services, to buffer negative shocks associated with adverse health events, and to improve their overall health. Many studies have evaluated Medicaid's effectiveness in promoting health. For one specific population – pregnant women (and by implication, newborns) – studies have found mixed results. While Currie and Gruber (1996b) found an increase in Medicaid income eligibility threshold reduces the incidence low birth weight, Dave et al. (2010) showed that Medicaid has a small and statistically insignificant impact on birth outcomes. Both studies investigate the Medicaid expansion period during the 1980s – 1990s. During this period, Medicaid income eligibility threshold for pregnant women and children under one-year-old expands from 100% of the federal poverty line (FPL) in 1986 (Medicaid & Medicare Milestones 1937 – 2015[29]) to 133% of FPL in April 1990 (Currie and Gruber 1996b).

Several reasons may have contributed to the lack of consistent empirical evidence for the causal relationship between Medicaid and birth outcomes. First, the Medicaid expansions in the 1980s caused non-negligible crowd-out of private health insurance. Such crowd-out resulted in little change in the actual uninsured rate, thus birth outcome was not improved during this period (Dave et al. 2010). Second, the identification of

[29] See https://www.cms.gov/About-CMS/Agency-Information/History/Downloads/Medicare-and-Medicaid-Milestones-1937-2015.pdf.

existing studies is based on state-by-year expansions, which may suffer from potential policy endogeneity (Aizer and Grogger 2003). States' expansions of Medicaid are voluntary. Economic thrift enables states to expand Medicaid but also broadly improves coverage and access, which might create a spurious correlation between eligibility expansion and health outcomes (Sommers, Baicker, and Epstein 2012). Because of these two obstacles, the Medicaid provision period provides a better setting to estimate the impact of insurance coverage on health outcome. Surprisingly, there has been very little research focusing on this period of Medicaid. Recently, Goodman-Bacon (2018) found that Medicaid provision reduced mortality among nonwhite infants and children in the 1960s and 1970s.

In this study, we seek to provide new evidence on the impact of Medicaid on birth outcomes by addressing the above concerns in three ways. First, we focused on the population eligible for Medicaid during the initial rollout of Medicaid to mitigate crowd-out. For one reason, private health insurance coverage rate was much lower among people who would be eligible for Medicaid upon its provision than that among those to be eligible through Medicaid expansion. For the other, women eligible for Medicaid during the Medicaid introduction did not reduce their labor supply (Strumpf 2011), one potential mechanism of crowd-out, whereas those eligible for Medicaid through labor expansions in the 1980s – 90s did (Dave et al. 2015). Compared to new enrollees from later expansions, the first group of Medicaid enrollees was the most disadvantaged population, who had less income and were more sensitive to better health care. Second, we purged biases from policy endogeneity by utilizing within-state-level variation. Specifically, we predicted the probability of being uninsured, thus being affected by

Medicaid provision using the 1963 National Health Interview Survey (NHIS) data. Third, the initial rollout of Medicaid took place within a short period, leaving less concern with policy endogeneity. Seventy-three (73%) states rolled out Medicaid from the beginning of 1966 to the end of 1967.

We have three main findings. First, Medicaid increased the probability of labor delivery in a hospital rather than with a physician or a midwife, suggesting better quality care during labor delivery because of Medicaid. Second, Medicaid increased the birth weight on average and potentially reduced the probability of having low birth weight babies. Third, these positive effects of Medicaid were mainly driven by nonwhite and unmarried mothers and those aged 30-49. These results provide an important contribution to the literature on Medicaid's impact on health outcomes.

3.2 Background

3.2.1 Insurance Coverage before Medicaid

Prior to the implementation of Medicaid, the coverage rate of private insurance was very low. Among people with income less than $2,000 in 1959, only 8.9% had insurance that covered doctor visits, and less than one-third had a hospital or surgical insurance (Goodman-Bacon 2018). Also, the percentage of adults who received charity care in 1960 was 8% at most (Morgan et al. 1962). To address the lack of insurance coverage, Social Security Act of 1935 established a public assistance program for those unable to work and thus unable to obtain employer-provided health insurance. Later, this program was extended to the medically needy, the aged, the blind, the disabled, and single women with children. Federal share of the program cost was increased after the

Kerr-Mills Act in 1960.[30] However, only 1% of children received aid on health care by 1963 (Goodman-Bacon 2018).

3.2.2 Introduction of Medicaid Program

Expanding the goal of the Kerr-Mills Act, Medicaid was signed into law by President Lyndon Johnson in 1965. It was designed to replace the earlier federal programs granting states to provide medical care to welfare recipients and the aged. Federal and state jointly fund Medicaid with a significantly more generous matching rate than that under the Kerr-Mills Act. Federal contributions to each state are based on a matching formula, which depends on state per capita income. The higher the state's willingness to finance Medicaid coverage of medical services, the higher the federal contributions will be in that state.

Medicaid targets low-income people. Prior to the 1980s, only very low-income families with dependent children, poor elderly, disabled individuals, and the "medically needy" were eligible for Medicaid. Eligibility rule required that applicants' income and asset or other resources must be within some limits. The major way for non-elderly, mainly low-income families with children, to be eligible for Medicaid before the 1980s was through being the beneficiaries of the Aid to Families with Dependent Children (AFDC) program. Families who were eligible for cash welfare payments under former the AFDC program rules were automatically eligible for Medicaid until 1987. The majority of children (89%) covered by Medicaid in 1976 were eligible for the AFDC (Goodman-Bacon 2018). Being eligible for Medicaid provided both poor and medically-

[30] Both State and Federal governments share the cost of the program.

needy individuals with insurance against medical debt, avoiding personal bankruptcy (Gross and Notowidigdo 2011, Finkelstein et al. 2012).

To qualify for AFDC, individuals must meet not only states' AFDC income and asset requirements, but also the categorical eligibility requirements. Categorical eligibility rule restricts the AFDC to those low-income families with "dependent children" only. Whether "dependent children" includes an unborn child or not was not clearly defined under the Social Security Act of 1935. The Department of Health, Education, and Welfare (DHEW), as the regulation agency of Medicaid at that time, treated the aid to the unborn child to be optional for states participating the AFDC program (1975). As a result, around 20 states did not provide aid to first-time mothers because their programs do not cover unborn child (Davis and Schoen 1978, Grossman and Jacobowitz 1981).

3.3 Literature Review

Most of the previous research in the area of Medicaid's impact on birth outcome focuses on the Medicaid expansion period during the 1980s and 1990s. Currie and Gruber (1996a) studied the period between 1984 and 1992, during which AFDC qualification for children of 0 – 5 and first time pregnant women coverage became mandatory for the first time. They found that the take-up rate among otherwise uninsured children was much lower than expected. Nevertheless, the expansion of Medicaid eligibility significantly increased the utilization of medical care. They also found that the change in eligibility rule reduced the incidence of infant mortality and low birth weight (Currie and Gruber 1996b). One-fifth of the increase in eligibility under overall expansion was correlated with a 7% reduction in infant mortality rate and a 2% reduction in the incidence of low birth weight. In contrast, Dave et al. (2010) found the Medicaid expansion crowded-out private insurance coverage by at least 55%, caused a small reduction (10% - 15%) in the uninsured rate of pregnant women and little impact on prenatal care utilization and birth weight (1% increase). Yazici and Kaestner (2000) re-estimated the magnitude of crowding-out during the same period and found a much smaller magnitude of crowding-out, 18.9%. Based on their estimation, the increase in insurance coverage rate because of Medicaid expansion was 2.5%. Goodman-Bacon (2018) summarized that the estimated impact of the expansion on insurance coverage rate ranged from 0 to 3 percentage points for pregnant women and from a slight decrease to a rise of between 2.4 to 4 percentage points for children.

More recent research investigated the issue of public insurance take-up rate and private insurance crowd-out rate with the State Children's Health Insurance Program (SCHIP). Lo Sasso and Buchmueller (2004) found that around 9% of eligible children enrolled in this program, with a take-up rate of 5.4% and a crowd-out estimate of 10%. Bansak and Raphael (2007) found that the take-up rate of SCHIP varied a lot depending on states' policy designs, and the overall take-up rate was around 10%. They did not find evidence of the states' policy design affecting the degree of crowd-out of private health insurance coverage. Koch (2013) applied regression discontinuity method to series of income eligibility thresholds of SCHIP across states and found 17% average crowd-out rate among the group of children, 60% of whom were covered by private plans. This crowd-out rate decreased as the income threshold of a marginally eligible child rose. Strumpf (2011) explored the potential mechanism of crowd-out through pregnant women's labor supply during Medicaid introduction and did not find any impact. Based on Dave et al. (2015)'s estimation, a 20-percentage-point increase in Medicaid eligibility among unmarried women without high school education and who gave birth in the past year was associated with a 13 – 16% reduction in the probability of being employed during the late 1980s and the early 1990s.

In general, the relationship between Medicaid coverage and health care utilization, as well as health outcomes, has been discussed for many years. Literature has found some evidence of a positive relationship between the two (Kaestner, Joyce, and Racine 2001, Buchmueller et al. 2005, Dafny and Gruber 2005). In contrast to those aged 7-9, children aged 2-6 benefited from public insurance coverage; Medicaid expansion reduced the incidence of ambulatory care sensitive hospitalizations among the young children from near poor areas (Kaestner, Joyce, and Racine 2001). Dafny and Gruber (2005) analyzed the impact of Medicaid expansion during the late 1980s and early 1990s on the child hospitalizations and found a ten percentage-point rise in Medicaid eligibility led to 8.1% increase in the unavoidable hospitalization rate. Buchmueller et al. (2005) found that insurance coverage increased outpatient (associated with preventive care) and inpatient utilization for both children and adults.

3.4 Data and Sample

This study mainly uses two datasets. One is the National Health Interview Survey (NHIS). Conducted by the National Center for Health Statistics (NCHS) since 1960, the NHIS is a nationally representative survey that collects information on the distribution of health on the noninstitutionalized civilian population in the U.S. We used person-level file from NHIS of 1963, which provides information before any states implemented Medicaid. It contains respondents' private health insurance coverage status, including doctor visit insurance, hospital insurance, and surgical insurance. It also contains detailed information on an individual's demographics, educational attainment, and income.

The other dataset is the Vital Statistics Natality birth data from NCHS, which collects information from birth certificates filed in Vital Statistics Offices across U.S. Public available data is available for each calendar year after 1968. We collected the birth data of babies born between 1968 and 1973 in the U.S., which covers the Medicaid implementation dates in most states (from 1966 to 1972).[31] We generated sampling weights based on the reporting status of each state in each year.[32] The data contains information on both mothers and babies, including mother's age, race, marital status, length of gestation, the month when prenatal visits begin,[33] birth attendant, and baby's date of birth, gender, race, and birth weight among others. The conception month was calculated by subtracting the length of gestation[34] from the date of birth of a mother. The month of conception ranges from December 1966 to July 1973. Third, we merged the Medicaid effective date of each state from Boudreaux, Golberstein, and McAlpine (2016) (Table 3.3) to the birth dataset based on months of conception.[35]

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[31] All the states including District of Columbia implemented Medicaid through July 1972, except Arizona, which didn't provide Medicaid until October 1982.

[32] Prior to 1972, all states reported 50% of birth certificates to NCHS. After 1972, states participating in the Vital Statistics Cooperative Program (VSCP) reported 100% of birth records, while the rest still reported 50%.

[33] Data of prenatal care utilization is available after 1969.

[34] We used national average gestation weeks of the year when mothers' gestation weeks were missing if the mothers were living in states that didn't report last menstrual periods in that year. We also replaced the gestation weeks of mothers, who didn't state their last menstrual period, using state average of the corresponding year. Results with and without missing gestation weeks' replacement are not significantly different from each other.

[35] If conception happens in or after the month of Medicaid implementation, variable *Medicaid* was coded as one, otherwise, zero. Twenty-five states implemented Medicaid during the sample period, including New Mexico providing Medicaid in December 1966.

We focused on two sets of outcomes. One is health care utilization during pregnancy and delivery, measured by the timing of prenatal care initiation and the methods of labor delivery. The other is the birth outcomes of mothers and their newborns, measured by gestation length and birth weight. Specifically, we categorized birthweight in several ways, including BIRTHWEIGHT in grams, a binary variable LOWBIRTHWEGIHT, indicating if birth weight is less than 2,500 grams, a binary variable VLOWBIRTHWEIGHT, indicating if birth weight is less than 1,500 grams, and a binary variable VVLOWBIRTHWEIGHT, indicating birth weight is less than 1,000 grams. Fixed effects for age, quarter, year, and state are used for all regressions.[36] Standard errors are clustered by state.

Table 3.1 provides descriptive statistics of the two datasets. The left half of Table 3.1 shows the summary statistics of the females from 10 to 49 years' old in the NHIS data of 1963. Their average age was 28, 89% of them were white, and 53% were married. The right half presents the statistics of the Vital Statistics Natality birth data of 1968 – 73.[37] Compared to those in NHIS data, the females in birth data was approximately younger by three years, three percentage points less likely to be white, and 37 percentage points more likely to be married. Also, Medicaid was provided at or before the month of pregnancy for 88% of the women. Among them, 67% were living in metropolitan counties. Regarding the timing of prenatal care initiation, 70% of pregnant women started their prenatal care visits in the first trimester, 93% started prenatal visits before the third trimester, and 2% did not have any prenatal visits during pregnancy. Regarding the

[36] Cell fixed effects based on each cell created using white dummy, marriage status dummy, and age groups, were tried. Results with cell fixed effects and without them are not substantially different from each other.

[37] Records with the predicted probabilities of private insurance coverage being 0 or 1 (6%) were dropped.

methods of labor delivery, 99% of the babies were delivered in hospitals while the rest were delivered by a physician or midwife. Regarding birth outcomes, the mothers' gestation period length was 39.5 weeks on average, and 9% of them had pre-term birth. Babies weighed 3,289 grams at birth on average, but 8% of them weighed less than 2,500 grams, 1% weighed less than 1,500 grams, and 0.5% weighed less than 1,000 grams.

3.5 Empirical Methodology

The empirical objective of this study is to examine the effects of Medicaid on the use of prenatal care and birth outcomes. There are several difficulties to establish causality. First, the health conditions of mothers with and without insurance can be different. Medicaid covers low-income single mother, who has children, and who has lower nutritional intake in a worse home environment during pregnancy. Without accounting for the difference in mothers eligible for Medicaid and not would lead to a downward bias in the estimates of Medicaid's impact. Second, bias could come from reverse causality of health insurance coverage. Lower health status could lead to higher cost of medical care, thus lower income, making the family eligible for Medicaid. Third, eligible mothers could choose to apply for Medicaid or not, depending on their preference for the health of themselves and their babies. This unobserved preference could affect the degree to which they take care of themselves during pregnancy and thus birth outcomes.

To causally identify the effect of Medicaid on birth outcomes, we adopted a difference-in-differences (DID) strategy exploiting two sources of variation. One is state-level variation in Medicaid rollout, and the other is individual-level variation in the probability of being treated by Medicaid. The idea of DID is to compare the pre- and post-Medicaid difference between the birth outcomes of mothers who were treated by

Medicaid and those of mothers who were not. Also, we simulated the probability of enrolling in Medicaid, rather than using the actual coverage Medicaid status, due to the unobserved preference. Those who would otherwise be uninsured are expected more likely to be affected by Medicaid.

First, we generated the likelihood of insurance coverage before Medicaid using the NHIS data of 1963. We used doctor visit insurance as the proxy for private insurance to predict the propensity of being eligible for private insurance for each woman aged between 10 – 49. We adopted the following specification.

$$\begin{aligned}
INSURED_i = \alpha_0 &+ \alpha_1 MARRIED_i + \alpha_2 WHITE_i + \alpha_3 AGE_i \\
&+ \alpha_4 REGION_i + \alpha_5 MARRIED_i \times WHITE_i \\
&+ \alpha_6 MARRIED_i \times AGE_i \\
&+ \alpha_7 MARRIED_i \times REGION_i \\
&+ \alpha_8 WHITE_i \times AGE_i + \alpha_9 WHITE_i \times REGION_i \\
&+ \alpha_{10} AGE_i \times REGION_i \\
&+ \alpha_{11} MARRIED_i \times WHITE_i \times AGE_i \\
&+ \alpha_{12} MARRIED_i \times WHITE_i \times REGION_i \\
&+ \alpha_{13} MARRIED_i \times AGE_i \times REGION_i \\
&+ \alpha_{14} WHITE_i \times AGE_i \times REGION_i \\
&+ \alpha_{15} MARRIED_i \times WHITE_i \times AGE_i \times REGION_i \\
&+ \varepsilon_i
\end{aligned} \tag{1}$$

In this equation, i indexes for an individual woman. INSURED, MARRIED, and WHITE are binary variables, indicating if the woman was covered by doctor visit insurance, married and white. AGE and REGION are categorical variables of the women's age and census region. We predicted the propensity of being eligible for private insurance for each group of women based on the value taken by each control variable from equation (1).[38] This propensity was matched to each baby in the birth data based on their mothers' socioeconomic backgrounds.

Second, we assumed that private insurance is the only type of insurance available before Medicaid. Based on this assumption, the probability of being uninsured (UNINSURED) and being treated by Medicaid is,

$$UNIN\widehat{SUR}ED_j = UNIN\widehat{SUR}ED_i = 1 - IN\widehat{SUR}ED_i \tag{2}$$

Third, we estimate the DID model using the simulated intensity of Medicaid treatment. Formally, we estimated the following model.

$$\begin{aligned} y_{jsm} = \beta_0 + \beta_1 UNIN\widehat{SUR}ED_J + \beta_2 MEDICAID_{jsm} \\ + \beta_3 UNIN\widehat{SUR}ED_J * MEDICAID_{jsm} + \gamma X_{jsm} + \eta_a \\ + \delta_s + \theta_q + \lambda_t + \varepsilon_{jsm} \end{aligned} \tag{3}$$

In this equation, j indexes the baby, s the state of birth, m the conception month, a the age of his (her) mother, q the conception quarter, and t the conception year. Policy variable $\mathrm{MEDICAID_{jsm}}$ is a binary variable, indicating Medicaid availability at the month of conception.[39] From equation (2), $\mathrm{UNIN\widehat{SUR}ED_J}$ is a continuous variable, indicating the probability of being uninsured before Medicaid provision and the intensity of being treated by Medicaid once provided. The coefficient of interest is β_3, which captures the impact of the Medicaid provision. In terms of other relevant aspects of our model, X_{ism}

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[38] Probit model was also used to predict the propensity of private insurance coverage. The results are not significantly different from those using the linear probability model.

[39] Since Medicaid stays once it is implemented, babies are exposed to Medicaid treatment after the state's implementation month.

represents individual level characteristics, including binary variables indicating if the baby was male (Male), if he (she) was born in a metropolitan county (METROPOLITAN), and if the mother was white (WHITE). The model also includes different levels of fixed effects, including the mother's age (η_a), state of residency (δ_s), a birth quarter (θ_q), and birth year (λ_t).[40]

3.6 Results

3.6.1 Predicted Uninsured Rate

Based on equation (1), we used the marital status, race, age, as well as census region from NHIS 1963 to make the prediction and matched to the main dataset birth data of 1968 – 1973.[41] Specifically, we predicted the probability of doctor visit insurance coverage for four distinct groups in four census regions: white and married women, white and unmarried women, nonwhite and married women, as well as nonwhite and unmarried women (Figure 3.1). Figure 3.1 shows that the mothers' private insurance coverage probabilities were mostly below 25% in all regions except West. This figure shows results consistent with the fact that during this period, married women were more inclined to have health insurance than unmarried women (Verbrugge 1979).

The predicted likelihood of doctor visit insurance coverage was then converted to the probability of being uninsured without Medicaid, thus the likelihood of being treated by Medicaid. In Table 3.2, we showed the actual uninsured rate of females 10 – 49 in 1963 NHIS data and predicted uninsured rate in Vital Statistics Natality Birth data of

[40] Previous literature finds high correlation between birth quarter and birth outcome (Lam and Miron 1994, Pitt and Sigle 1998, Doblhammer and Vaupel 2001, Bobak and Gjonca 2001).

[41] For states that don't include legitimacy (marriage status) information on birth certificates, or girls less than 17-year-old, we use race, age, census region to make a prediction. These observations account for 3.4% of the sample.

1968 – 1973. In 1963, 83% of these females were not covered by doctor visit insurance. Compared to the actual uninsured rate, the predicted uninsured rate is very similar, and its variance is much smaller among the study population.

3.6.2 The Effect of Medicaid on Health Care Utilization and Birth Outcomes

3.6.2.1 Health Care Utilization

We began with full sample analyses of health care utilization. Table 3.4, columns (1) – (3) present the results of prenatal care starting before the end of the first trimester, second trimester, and third trimester. The results show no significant impact of Medicaid on these outcomes. Column (4) presents the results of birth in a hospital (versus with a physician or midwife). The first row shows that uninsured pregnant women had lower probabilities of delivering babies in hospitals before Medicaid provision. Specifically, a 10-percentage-point increase in the probability of being uninsured was associated with a 0.44-percentage-point reduction in the probability of delivering babies in hospitals before Medicaid. The second row shows the average likelihood of labor delivery in hospitals among all women decreased by 2.48% after Medicaid provision. The third row shows the impact of Medicaid implementation on prenatal care utilization among those who were more likely to be treated by Medicaid. With a 10-percentage-point increase in the probability of being uninsured prior to Medicaid, Medicaid implementation increased the probability of delivering babies in a hospital by 0.41-percentage-point (0.4% increase relative to the mean). Regarding control variables, mothers who were white or living in a metropolitan county were more likely to deliver babies in a hospital rather than with a physician or a midwife.

3.6.2.2 Birth Outcomes

The previous section shows that Medicaid improved the quality of care during labor delivery among those who were more likely to be treated. This section examines if Medicaid improved birth outcomes. Medicaid had no statistically significant impact on gestational length (column (1) of Table 3.5) or the probability of preterm labor (column (2)). These results are consistent with the findings in Table 3.4 that Medicaid had no impact on prenatal care utilization. Column (3) – (6) presents birth weight outcomes. Uninsured pregnant women were more likely to have babies with low birth weight before Medicaid provision (row (1) of column (3) – (6)). A 10-percentage-point increase in the probability of being uninsured before Medicaid was associated with a 51.65 grams' reduction in birth weight, a 0.16-percentage-point increase in the probability of babies having birth weight below 2,500 grams, a 0.05-percentage-point increase in the probability of babies having birth weight below 1,500 grams, and a 0.03-percentage-point increase in the probability of babies having birth weight below 1,000 grams. Row (2) shows that the average birth weight reduced by 25.77 grams after Medicaid provision. Also, with a 10-percentage-point increase in the probability of being uninsured prior to Medicaid, thus being treated by Medicaid, Medicaid provision increased the birth weight by 24.28 grams (reflecting approximately 0.7% increase relative to the mean) but had little impact on the probability of giving birth to babies with birth weight less than 2,500 grams (row (3)). Compared to female birth, male birth weight was 123 grams higher and 1.31% lower probability of having low birth weight (row (4)). White mothers' babies had 201.8 grams' higher birth weight and were 5.59% less likely to have low birth weight than nonwhite mothers (row (5)). Mothers living in metropolitan counties had babies whose birth weight were 30.19 grams' lower than those not living in metropolitan counties (row(6)).

3.6.3 Robustness Checks using Birth Order

We tested the main results using subsample analyses separated by birth order. Since approximately 20 states did not provide benefit to first-time mothers, we expected a stronger effect of Medicaid on later-born babies than on first-born babies. Table 3.6 presents the results of the timing to initiate prenatal care on the left and those of labor delivery on the right using the coefficients on the interaction terms only in equation (3). The estimates on the prenatal care initiations in first and second trimesters among later born babies are larger and positive while those among first-born babies are smaller and mixed. This suggests that Medicaid had relatively stronger effects on prenatal care utilization among later-born babies than first-born ones. There is not much difference in the Medicaid effect on labor delivery between first and later born babies.

Table 3.7 presents the birth outcomes using the coefficients of the interaction terms only in equation (3). Consistent with the pattern of prenatal care utilization, the results show Medicaid 's benefit on gestational age measured in weeks and premature labor is stronger among later born babies. Further, Medicaid reduced the probability of women having low birth weight babies among later born babies only. With a 10-percentage-point increase in the probability of being uninsured before Medicaid, Medicaid provision decreased the probability of giving birth with weight less than 2,500 grams by 0.12-percentage-point among later-born babies. Overall, the findings of this robustness check support our main results.

3.6.4 Extension

Findings in the main analysis suggest that Medicaid provision led to a better quality of care during labor delivery among pregnant women and higher birth weight among their babies who were more likely to be treated by Medicaid. These effects mainly work through later born babies. In this section, we explore the heterogeneous effects across various groups of sub-population separated by race, marital status, and age.

Table 3.8 presents the subsample analyses' results of labor delivery on the left and those of birth weight on the right. The labor delivery quality improvement mainly came from nonwhite instead of white mothers. With a 10-percentage-point increase in the probability of being uninsured, Medicaid provision increased the probability of delivering babies in hospitals by 0.4-percentage-point among nonwhite mothers while had no impact on white mothers. The Medicaid's effect on labor delivery quality increase was larger in magnitude among unmarried mothers than married ones. This effect existed across all age groups and was largest among teenage mothers, whose probability of delivering babies in hospitals increased by 0.89-percentage-point after Medicaid provision.

The subanalyses of birth weight show a similar pattern of heterogeneity. The birth weight increase was concentrated on nonwhite mothers. With a 10-percentage-point increase in the probability of being uninsured, Medicaid provision increased the birth weight by 41.78 grams among nonwhite mothers while had no impact on white mothers. The Medicaid had a larger effect in magnitude on unmarried mothers' birth weight than married mothers'. Across age groups, however, Medicaid improved birth weight among mothers aged 30-49 only. With a 10-percentage-point increase in the probability of being uninsured, Medicaid provision increased the birth weight by 50.79 grams among these mothers. Overall, the subsample analyses show that the effects of Medicaid on health care utilization and birth outcomes are stronger among nonwhite and unmarried mothers.

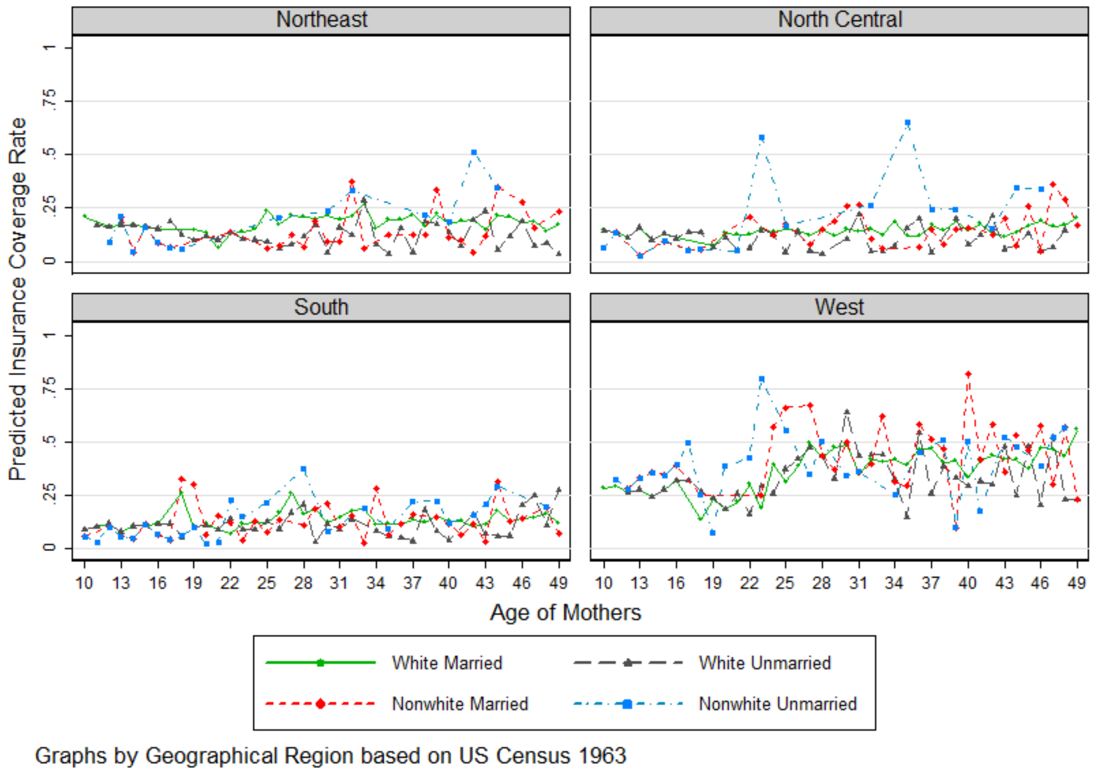
3.7 Discussion and Conclusion

This study examines the impact of Medicaid provision on prenatal care and birth outcomes using both NHIS and Vital Statistics Natality birth data from NCHS. By focusing on the population during the initial rollout period of Medicaid, we mitigated the crowd-out from private insurance to Medicaid and improved the accuracy of the estimates on Medicaid's impact. We also exploited individual-level variation, using simulated propensity of being treated by Medicaid, to overcome unobserved preference towards public insurance enrollment. This study contributes to the current literature by providing the following evidence. First, Medicaid provision caused a higher probability of labor delivery in a hospital rather than with a physician or midwife. Second, Medicaid increased the birth weight on average. With a 10-percentage-point increase in the probability of being treated by Medicaid, Medicaid implementation increased the likelihood of delivering babies in a hospital by 0.41-percentage-point and increased the birth weight by 24.28 grams on average. These effects mainly came from later born rather than first-born babies. Third, the positive effects of Medicaid provision on prenatal care utilization and health outcomes were concentrated among nonwhite, unmarried mothers, and those aged 30-49.

Overall, the findings in this study made several contributions to the literature on the impact of Medicaid on health outcomes. First, Medicaid provision improved health care utilization among pregnant women, improving the quality of care at labor delivery by changing the way of delivery from with a physician or a midwife to in a hospital.

Second, the improvements in the quality of labor delivery might increase birth weight and potentially reduced low birth weight incidence. Third, these benefits of Medicaid provision were mainly obtained by nonwhite and unmarried mothers and those aged 30-49.

Figure 3.1: Predicted Doctor Visit Insurance Coverage Rate



Notes: The figure shows the predicted doctor visit insurance coverage rate among mothers aged between 10 and 49 from 1968 – 73 birth records.

Table 3.1: Summary Statistics

Variable	Description	NHIS			Birth Data		
		N	Mean	SD	N	Mean	SD
AGE	Age of female/mother	36,059	27.75	12.29	10,118,378	24.82	5.52
WHITE	Dummy, 1 if female is white, 0 otherwise	36,059	0.89	0.32	10,118,378	0.86	0.35
MARRIED	Dummy, 1 if female is married	36,059	0.53	0.50	6,729,317	0.90	0.30
MEDICAID	Dummy, 1 if Medicaid is available at the month of pregnancy				10,118,378	0.88	0.33
MALE	Dummy, 1 if the child is male				10,118,378	0.51	0.50
METROPOLITAN	Dummy, 1 if living in the metropolitan county				10,118,378	0.67	0.47
FIRSTTRIMESTER	Dummy, 1 if prenatal care begins in the 1st trimester				6,737,972	0.70	0.46
SECONDTRIMESTER	Dummy, 1 if prenatal care begins in the 1st or 2nd trimester				6,737,972	0.93	0.25
THIRDTRIMESTER	Dummy, 1 if prenatal care begins in 1st, 2nd, or 3rd trimester				6,737,972	0.98	0.12
BIRTHINHOSPITAL	Dummy, 1 if labor is in hospital				10,118,378	0.99	0.09
GESTATIONWEEK	Gestation period length in weeks				6,468,345	39.50	2.76
PREMATURE	Dummy, 1 if gestation period is < 37 weeks				6,468,357	0.09	0.29
BIRTHWEIGHT	Birth weight in grams				10,080,296	3289	587
LOWBIRTHWEGIHT	Dummy, 1 if birth weight is < 2,500 grams				10,080,296	0.08	0.27
VLOWBIRTHWEIGHT	Dummy, 1 if birth weight is < 1,500 grams				10,080,296	0.01	0.11
VVLOWBIRTHWEIGHT	Dummy, 1 if birth weight is < 1,000 grams				10,080,296	0.005	0.07

Notes: Sample contains women aged 10-49 from 1963 NHIS and 1968 - 1973 Birth Data. Medicaid dummy is predicted using gestation weeks, whose missing values are replaced by state or national average (see main text for detailed explanation).

Table 3.2: Likelihood of Insurance Coverage

Variable	Description	N	Mean	SD
Based on NHIS 1963 Data				
UNINSURED	Dummy, 1 if female does not have doctor visit insurance coverage	36,059	0.83	0.38
Predicted and Matched to Birth Data 1968 - 1973				
UNINSURED	Dummy, 1 if female does not have doctor visit insurance coverage based on prediction	10,118,378	0.83	0.10

Notes: Doctor visit insurance coverage was obtained from 1963 NHIS. The sample is restricted to women between 10 and 49 years' old. Predicted insurance coverage was matched to 1968 - 1973 Vital Statistics Natality Birth Data.

Table 3.3: Medicaid Implementation Dates of States

State Name	Implementation Date	State Name	Implementation Date
Alabama	1/1/1970	Montana	7/1/1967
Alaska	7/1/1972	Nebraska	7/1/1966
Arizona	10/1/1982	Nevada	7/1/1967
Arkansas	10/1/1970	New Hampshire	7/1/1967
California	3/1/1966	New Jersey	1/1/1970
Colorado	1/1/1969	New Mexico	12/1/1966
Connecticut	7/1/1966	New York	5/1/1966
Delaware	10/1/1966	North Carolina	1/1/1970
District of Columbia	7/1/1968	North Dakota	1/1/1966
Florida	1/1/1970	Ohio	7/1/1966
Georgia	10/1/1967	Oklahoma	1/1/1966
Hawaii	1/1/1966	Oregon	7/1/1967
Idaho	7/1/1966	Pennsylvania	1/1/1966
Illinois	1/1/1966	Rhode Island	7/1/1966
Indiana	1/1/1970	South Carolina	7/1/1968
Iowa	7/1/1967	South Dakota	10/1/1967
Kansas	6/1/1967	Tennessee	1/1/1969
Kentucky	7/1/1966	Texas	9/1/1967
Louisiana	7/1/1966	Utah	7/1/1966
Maine	7/1/1966	Vermont	7/1/1966
Maryland	7/1/1966	Virginia	7/1/1969
Massachusetts	9/1/1966	Washington	7/1/1966
Michigan	10/1/1966	West Virginia	7/1/1966
Minnesota	1/1/1966	Wisconsin	7/1/1966
Mississippi	1/1/1970	Wyoming	7/1/1967
Missouri	10/1/1967		

Notes: The data comes from (Boudreaux, Golberstein, and McAlpine 2016).

Table 3.4: Healthcare Utilization

VARIABLES	(1) First Trimester	(2) Second Trimester	(3) Third Trimester	(4) Birth in Hospital
UNINSURED	-0.129	-0.0501	0.00993	-0.0441***
	(0.0816)	(0.0660)	(0.0232)	(0.0132)
MEDICAID	-0.00780	0.00568	0.0193	-0.0248**
	(0.0687)	(0.0541)	(0.0184)	(0.00960)
UNINSURED* MEDICAID	0.00562	-0.00499	-0.0244	0.0414***
	(0.0811)	(0.0648)	(0.0224)	(0.0140)
BOY	-0.00286***	0.000806***	0.000218**	0.000369***
	(0.000340)	(0.000183)	(9.17e-05)	(0.000127)
WHITE	0.203***	0.0681***	0.0232***	0.0268***
	(0.0185)	(0.00864)	(0.00363)	(0.00812)
METROPOLITAN	0.0472***	0.00778*	-0.000357	0.0102***
	(0.00794)	(0.00393)	(0.000944)	(0.00266)
N	6,737,972	6,737,972	6,737,972	10,118,378
R-squared	0.082	0.034	0.015	0.032

Notes: 1968 - 1973 Vital Statistics Natality Birth Data. All estimates are weighted using sampling weights, and standard errors are clustered on the state of birth. All regressions use age, quarter, year, state fixed effects. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.5: Birth Outcomes

VARIABLES	(1) Gestation Week	(2) Premature Labor	(3) Birth Weight	(4) Weight < 2,500	(5) Weight < 1,500	(6) Weight < 1,000
UNINSURED	-0.0576	-0.00123	-51.65***	0.0161***	0.00494***	0.00309***
	(0.0699)	(0.0110)	(11.86)	(0.00416)	(0.00170)	(0.000906)
MEDICAID	-0.0606	-0.00283	-25.77**	0.00400	0.00167	0.00109
	(0.0619)	(0.00859)	(10.51)	(0.00372)	(0.00161)	(0.000801)
UNINSURED* MEDICAID	0.00169	0.0143	24.28*	-0.00336	-0.00152	-0.00111
	(0.0773)	(0.0123)	(12.81)	(0.00441)	(0.00195)	(0.000988)
BOY	-0.131***	0.00875***	123.0***	-0.0131***	0.000285***	9.16e-05**
	(0.00367)	(0.000257)	(0.758)	(0.000334)	(7.46e-05)	(3.83e-05)
WHITE	0.784***	-0.0738***	201.8***	-0.0559***	-0.0110***	-0.00547***
	(0.0401)	(0.00451)	(4.877)	(0.00148)	(0.000490)	(0.000293)
METROPOLITAN	-0.0564***	-0.000884	-30.19***	0.00401***	0.00114***	0.000805***
	(0.0152)	(0.000796)	(3.263)	(0.000471)	(0.000208)	(0.000156)
N	6,468,345	6,468,357	10,080,296	10,080,296	10,080,296	10,080,296
R-squared	0.023	0.018	0.040	0.010	0.003	0.001

Notes: 1968 - 1973 Vital Statistics Natality Birth Data. All estimates are weighted using sampling weights. All regressions use age, quarter, year, state fixed effects. Standard errors are clustered on the state of birth. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.6: Subsample Analysis of Prenatal Care Utilization by Birth Order

Prenatal Care Begins in Trimester				Labor Delivery	
Subgroup	First	Second	Third	Subgroup	In Hospital
Birth Order				*Birth Order*	
First Born	0.0248	-0.00538	-0.0227	First Born	0.0555***
[2,560,227]	(0.105)	(0.0667)	(0.0217)	[3,803,793]	(0.0198)
Later Born	0.0347	0.0254	-0.00701	Later Born	0.0518***
[4,177,745]	(0.0735)	(0.0586)	(0.0162)	[6,314,585]	(0.0159)

Notes: 1968 - 1973 Vital Statistics Natality Birth Data. Each estimate is the coefficient of interaction term based on a corresponding subsample regression. Observation number of each regression is reported in square brackets. These regressions use the same sampling weights, controls, and fixed effects as full sample analysis. Standard errors, reported in parenthesis, are clustered by states. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.7: Subsample Analysis of Birth Outcomes by Birth Order

Subgroup	Gestation Weeks	Premature Labor		Birth Weight	< 2,500	< 1,500	< 1,000
Birth Order			*Birth Order*				
First Born	-0.0989	0.0148	First Born	20.51	0.0107	0.000731	-0.00129
[2,448,683]	(0.0911)	(0.0124)	[3,791,026]	(13.87)	(0.00702)	(0.00240)	(0.00120)
Later Born	0.0674	0.00755	Later Born	13.01	-0.0117**	-0.00340	-0.00154
[4,019,662]	(0.107)	(0.0147)	[6,289,270]	(11.62)	(0.00489)	(0.00240)	(0.00123)

Notes: 1968 - 1973 Vital Statistics Natality Birth Data. Each estimate is the coefficient of interaction term based on a corresponding subsample regression. Observation number of each regression is reported in square brackets. These regressions use the same sampling weights, controls, and fixed effects as full sample analysis. Standard errors, reported in parenthesis, are clustered by states. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.8: Subsample Analyses by Race/ethnicity, Marriage Status, and Age Groups

Subgroup	Labor Delivery in Hospital	Subgroup	Birth weight in Grams
Race/ethnicity		*Race/ethnicity*	
White	-0.00228	White	-1.270
[8,669,322]	(0.00269)	[8,640,028]	(21.02)
Non-White	0.0403***	Non-White	41.78**
[1,449,056]	(0.0132)	[1,440,268]	(17.41)
Marriage Status		*Marriage Status*	
Married	0.00735	Married	20.23
[6,034,228]	(0.00639)	[6,018,077]	(16.52)
Unmarried	0.0110	Unmarried	36.61
[695,089]	(0.0234)	[692,522]	(29.85)
Age Groups		*Age Groups*	
Teenage	0.0889***	Teenage	14.90
[1,613,180]	(0.0232)	[1,606,489]	(14.98)
20s	0.0335***	20s	15.88
[6,627,693]	(0.0115)	[6,604,059]	(17.43)
30-49	0.0472***	30-49	50.79**
[1,877,505]	(0.0157)	[1,869,748]	(21.75)

Notes: 1968 - 1973 Vital Statistics Natality Birth Data. Each estimate is the coefficient of interaction term based on a corresponding subsample regression. Observation number of each regression is reported in square brackets. These regressions use the same sampling weights, controls, and fixed effects as full sample analysis. Standard errors, reported in parenthesis, are clustered by states. *** p<0.01, ** p<0.05, * p<0.1.

Chapter 4: Does the Arrival of a Registered Sex Offender Hurt Property Values? Evidence from Maryland 2008 – 2018

4.1 Introduction

In response to some horrendous sex crimes towards children, two federal laws established the requirement of sex offender registration and community notification requirements in the US. The Jacob Wetterling Act in 1994 (P.L. 103 – 106) required states to adhere to minimum standards of registration and to establish registries of sex offenders against children. The Act was amended with Megan's Law (P.L. 104 – 145) two years later (1996), requiring states to release the information on sex offenders to the public.

One of the many potential economic consequences of these laws was a possible property value reduction in the vicinity of a registered offender. In an important and highly-cited early research, Linden and Rockoff (2008) studied property sales data in Mecklenburg County, North Carolina, from 1994 to 2004 and found that properties within 0.1-mile of an offender sold for approximately four percent lower than comparable homes further away upon the offender's arrival. In another highly-cited paper, Pope (2008) used property sales data from 1996 to 2006 from Hillsborough County, Florida, and found average housing values in close proximity to an offender decreased by two percent after arrival but rebounded after the offender moved out.

Overall, it is believed that the estimates of Linden and Rockoff (2008) and Pope (2008) provided compelling evidence that sex offenders moving into an area decrease the area's property values and that these estimates primarily reflected households' perception of crime risk. This study uses more recent data (2008 – 2018) from Maryland to provide updated estimates for the impact of sex offenders on local housing prices. Importantly, during the past two decades, the laws on sex offender registration and notification methods have changed significantly. Notably, in earlier years (1994 – 2006), notification methods were more direct (proactive) and commonly included media releases, print-outs, mailings, door-to-door contact, and community meetings (Matson and Lieb 1996). However, after 2006 information directed to local residents became more passive, with the primary policy tool to inform residents changed to sex offender registry websites (Levenson, D'Amora, and Hern 2007, Levenson et al. 2007, Tewksbury and Jennings 2010).

The objective of this paper is to apply the hedonic pricing method (HPM), using more recent data from Maryland, to estimate the impact of crime risk from sex offenders on proximate property values. Specifically, we use housing values in four counties of Maryland, including Baltimore City, Baltimore County, Prince George County, and Montgomery County from 2008 to 2018. This data allows us to evaluate the impact of sex offenders moving into a neighborhood on property values when the passive notification method is used.

Following the approach in Linden and Rockoff (2008), we obtained data on registered sex offenders in these four counties as of April 2019 and combined it with the housing dataset based on spatial locations and arrival dates relative to residential housing sales dates. Different from the previous evidence on registered sex offenders' crime risk, our econometric results show no negative impacts on proximate home values from sex offenders' arrivals. This is consistent across all four counties in Maryland and is also robust to model specifications using different spatial fixed effects with standard errors clustered at different levels.

The contributions of this paper are of three-fold. First, different from previous research using a single county or city as study areas, we use the latest data on registered sex offenders from four counties of Maryland. Second, this paper provides new evidence of the impact of crime risk from sex offenders' arrivals on proximate property values. Results show no negative impact on the values of single-family properties within 0.1-mile of registered sex offenders upon their arrivals. Third, the new evidence from this paper implies that sex offenders do not cause the financial cost to households living in the vicinity to them. This result has broader implications to the debate over sex offenders' laws to be discussed in later sections.

4.2 Background

4.2.1 The Hedonic Pricing Method and Applications to Sex Offenders' Crime Risk

HPM is a revealed preference approach to evaluate (dis)-amenities capitalized into housing markets. HPM has been applied to evaluate the crime risk from sex offenders moving into neighborhoods on proximate property values. Using single-family housing data and registered sex offender dataset of 2000 from Montgomery County, Ohio, Larsen, Lowrey, and Coleman (2003) found the houses within 0.1-mile of an offender who was subject to proactive notification (e.g., notification of adjacent houses' owners), sold for 17 percent less compared to houses located further away. Houses within 0.1-mile of an offender who was subject to passive notification (e.g., viewable on sheriff's office website), sold for 7.5 percent less compared to houses located further away.

Linden and Rockoff (2008) utilized a difference-in-differences (DD) strategy to address potential cross-sectional and temporal biases. To explain in detail, Linden and Rockoff (2008) use registered sex offender dataset as of January 2005 and single-family housing data of 1994 – 2004 in Mecklenburg County, North Carolina and find that home prices within 0.1-mile of an offender were sold for four percent less upon an offender's arrival. Similarly, Pope (2008) collected single-family housing transactions between October 1996 and April 2006 and the archived information of all the sex offenders registered from November 1997 through April 2006 in Hillsborough County, Florida. Using the same identification strategy, he found that the average housing value within 0.1-mile of an offender decreased by 2.3% after the offender's arrival but rebounded after the offender moved out.

More recently, Wentland, Waller, and Brastow (2014) examined the impact of sex offenders on housing liquidity in addition to the transaction price. Based on a Multiple Listing Service dataset and an archived sex offender dataset in central Virginia (Lynchburg and surrounding areas) between July 1999 and June 2009, they showed that the presence of sex offenders reduced the property values of homes located within 0.1 miles by 7% and this negative effect dampened until 1-mile from an offender. Also, offenders' presence lengthened a property's time on the market by as much as 80%.

To differentiate the risk between transient and non-transient sex offenders, Yeh (2015) applied HPM to high-risk sex offenders listed in Lincoln, Nebraska between 2000 and 2006. She found that the announcement of a nearby high-risk offender, who remained in residence for more than six months, depressed house prices within 0.1 miles by four percentage points while the announcement of all offenders had no impact on nearby house prices. Moreover, offenders tend to stay in better neighborhoods for transient periods while tend to maintain more permanent residence in worse neighborhoods. This pattern may suggest some correlation between the quality of neighborhoods and offenders' negative externality on nearby housing values.

Improving upon Larsen, Lowrey, and Coleman (2003), Caudill, Affuso, and Yang (2015) implemented a cross-sectional HPM analysis accounting for the spatial and temporal autocorrelation between properties. Using sex offender data from Memphis and property data from the Shelby County, they composed a sample of houses sold after sex offenders' arrivals within 1-mile from 2008 to 2012. Using a spatial error model, they found a 10% increase in the distance from the nearest offender lifted house prices by approximately 0.17%, and an additional offender within a one-mile radius lowered property values by around 2%.

The negative impact of sex offenders' crime risk on property values has also been found outside of the US. Using the nationwide administrative data in South Korea, Kim and Lee (2018) found that house prices fell by 5.5% (3% for multifamily homes) within a 150-meter and a 150-300-meter radius of the nearest sex offender in 1-month after the offender moved-in. However, this negative effect disappeared after 1-month of arrival. The effect was more significant and prolonged among homes located in areas wherein the population density was low.

4.2.2 The Change in Sex Offender Registration and Notification Methods

Over the last two decades, two laws have largely contributed to the change of sex offender registration and notification methods. First, in 2003 the Protect Act (P.L. 103) required states to maintain a website containing registry information and the Department of Justice to maintain a website with links to each state website.[42] Second, in 2006 the Adam Walsh Act (P.L. 109 – 248) created a national sex offender registry website and instructed states to apply identical criteria for posting offender data on the internet, including offenders' name, physical description, and employment information among others.

As of February 2001, only 29 states and the District of Columbia had publicly accessible internet sites containing searchable information of sex offenders (Adams 2002). As of March 2018, all the states and the District of Columbia have sex offender registration website.[43] However, the advent and dependence on passive notification methods may have reduced the public's awareness of sex offenders in nearby neighborhoods. Based on two mailing survey samples in Hamilton County, Ohio, and Jefferson County, Kentucky, in 2002 – 2003, Beck and Travis Iii (2006) found that 77 percent of eligible residents living adjacent to sex offenders subject to proactive notification were aware of the presence of the offenders. In contrast, only 26 percent of eligible residents living near offenders under passive notification were aware of the presence of the offenders. Also, based on a telephone survey sample in Washington State in 2002, people who were notified of a sex offender's arrival in their community were more likely to fear personal victimization and to take precautionary behavior to protect themselves and others from victimization than those who were not notified (Beck and Travis Iii 2004).

[42] https://www.smart.gov/legislation.htm.

[43] This is based on the Department of Justice (DOJ) website https://www.justice.gov/criminal-ceos/sex-offender-registration-and-notification-act-sorna (accessed on May 23, 2019).

4.2.3 Sex Offender Registration Laws in Maryland

Maryland adopted a sex offender registration law and established its first version of the Sex Offender Registry on Oct 1, 1995. Anyone who committed crimes on or after Oct 1, 1995, against a victim younger than 14 years old is required to register with local law enforcement annually for ten years. The sex offense victims' automated notification system, which allows victims to access offenders' status any time, was required from then on (Maryland Sexual Offender Advisory Board 2014 Report to the Maryland General Assembly).

Under the current law of Maryland, a sex offender must register with a supervising authority (e.g., the Department of Public Safety and Correctional Services) within three days of release from the court or before the release from a correctional facility. The supervising authority is required to send the registration files to the Sex Offender Registry Unit within five days of registration. Moreover, if offenders move, they must submit written notification to the registry unit within seven days. Offenders failing to register an address or provide notice of the change of address may be subject to imprisonment for up to three years or a fine of up to $5,000 or both.

The Maryland Sex Offender Registry is under the state's Department of Public Safety and Correctional Services. It maintains the Maryland's Comprehensive Registered Sex Offender Website, which reveals each sex offender's address information (e.g., current address, the move in date to the current address, employment address, school address), conviction information (e.g., date, location, charges), custody information (e.g., supervising agency), registration information (e.g., status, tier, date), demographic information (e.g., gender, date of birth, age, height, weight, race). It also ensures that all information on sex offenders in Maryland is up to date and accurate.

4.3 Data Source

4.3.1 Sex Offender Dataset

Sex offender data was collected from Maryland Registered Sex Offender Website. To our knowledge, Maryland is the only state reporting the move-in dates and the current addresses of registered sex offenders online. Other states report initial registration dates or most recent registration dates, which are updated every several months (e.g., three months for Tier III offenders in Maryland). Neither can be used as an approximation for the date upon which offenders move into their current location.

Maryland has a high registration compliance rate among sex offenders. As of April 17, 2019, Maryland Registered Sex Offender Website reported a total of 6,056 individuals required to register. Among them, there were 5,690 (94%) compliant offenders, 192 (3.17%) absconders, and 174 (2.87%) non-compliant offenders.

Information was obtained for all registered sex offenders that were compliant and resided in the four counties with the most registered sex offenders in Maryland (Baltimore City, Baltimore County, Prince George County, and Montgomery County). The sex offender dataset includes 1,208 offenders in Baltimore City, 758 offenders in Baltimore County, 713 offenders in Prince George County, and 399 offenders in Montgomery County (Figure 1). After removing homeless sex offenders and those living in apartments, we geocoded 860 sex offenders in Baltimore City, 623 sex offenders in Baltimore County, 552 sex offenders in Prince George County, and 305 sex offenders in

Montgomery County using Google Open Street Map.[44] After removing sex offenders whose addresses were geocoded imprecisely and those who have lived in their current addresses for less than a year, we kept 447 sex offenders in Baltimore City, 349 sex offenders in Baltimore County, 330 sex offenders in Prince George County, and 173 sex offenders in Montgomery County.[45]

Table 4.1 presents the summary statistics for sex offenders. The average age of sex offenders is 47 years old. The average height and weight of offenders are very similar across four counties. On average offenders have lived at their current address for three and a half years. The percentages of sex offenders being employed and driving vehicles are highest in Montgomery County and lowest in Baltimore City. Approximately a quarter of offenders are under community supervision. Regarding race, 55% of offenders are African Americans, and 42% of them are White. Maryland categorizes sex offenders into three levels based on their degree of riskiness. The majority of offenders are in Tier III, especially in Baltimore City.

4.3.2 Housing Dataset

Housing sale price data used in this study were obtained from the Maryland Open Data Portal. Specifically, data for single-family residential houses were collected for Baltimore County, Baltimore City, Montgomery County, Prince George County for the years 2008 to 2018. Each observation represented a real property transaction record, and prices were inflation adjusted to represent 2018 US dollars. The housing data contains detailed structural characteristics, including lot size, structure size, the year a property

[44] Results with the sex offenders living in apartments are very similar to those without.
[45] The sex offenders' addresses geocoded approximately (interpolated, geometric center, approximate) were removed.

was built, dwelling grade (above average, average, and below average), exterior construction material (e.g., Brick, Frame, Siding, etc.), the number of stories, and whether or not a basement exists.

Table 4.2 presents summary statistics for the parcel characteristics for each of the four counties. On average, Baltimore City has the smallest land size, and structure area, and had the highest percentage of lowest quality properties. Also, Baltimore City contains the oldest and least expensive properties among the four counties. The average age of the parcels in Baltimore City is 77-year-old. The average house price is highest in Montgomery County ($755,000), which is almost twice as high the average house price in Baltimore County ($398,000) and Prince George County ($386,000) and is more than twice as much as that in Baltimore City ($301,000).

More than eighty percent of parcels in these counties have basements, and less than half of them have one or one and a half stories. The most popular dwelling exterior material is siding in Baltimore City or Baltimore County, while is a frame in the rest two counties. The parcel quality is highest in Montgomery County, where more than 56% of all parcels' quality is above average. Baltimore County has the second-best quality properties, with 50% of them has average quality, and 24% of them has above average quality. Prince George County has 57% low-quality properties but 9% high-quality properties only. Similarly, Baltimore City has 62% of low-quality properties but 23% high-quality properties.

Table 4.3 presents the summary statistics for the pooled sample, combining all four counties. The three columns show the summary of (1) all parcels, (2) parcels in the control area (between 0.1 and 0.3 miles to an offender), and (3) parcels in the treated area (within 0.1 miles of an offender). The table shows that parcels inside 0.1 miles of an offender are quite similar to parcels within the 0.3 miles cut-off but are very different from those outside 0.3 miles range. Specifically, on average parcels within 0.1 miles of an offender sold for approximately $6,000 less, were 0.02 acres smaller and two years younger than parcels between 0.1 and 0.3 miles to an offender. In contrast, the parcels within 0.1 miles of an offender sold for approximately $150,000 less, were 0.23 acres smaller and ten years older, compared to all parcels. Also, the houses within the 0.3-mile range have fewer stories, approximately 33% of them have one story, 23% has 1.5 stories, and 44% has 2 or more stories, compared to all parcels.

4.4 Empirical Approach

4.4.1 Cross-sectional Difference Specification

Before testing the impact of sex offenders arriving in a local area, we check the pre-arrival difference between the area offenders will arrive (within 0.1-mile of an offender) and that adjacent to it (between 0.1- and 0.3-mile from the offender). If these two areas are similar in parcel characteristics, the adjacent area can be served as the counterfactual for the within 0.1-mile area when offenders arrive.

The cross-sectional difference specification takes the following form:

$$log(P_{ijt}) = \rho_0 D_{ijt}^{0.1} + \delta_t + \varepsilon_{ijt} \tag{1}$$

The semi-log of the inflation-adjusted sale price of the house is a function of a binary variable, $D_{ijt}^{0.1}$, indicating a parcel sale is inside the treated area (within 0.1 miles of an offender's residence), δ_t, a vector of year specific fixed effects, and a random error term clustered by offender area. We also replace the sale price with other parcel characteristics as dependent variables to examine the difference in others.

4.4.2 Spatial Difference-in-differences Identification Strategy

To estimate the impact of the sex offenders on property values, we use a spatial difference-in-difference (DD) method. The benefit of spatial DD models, compared with cross-sectional models, is that spatial DD models can potentially mitigate endogeneity caused by sex offenders moving into neighborhoods with systematically different property values (Pope 2008).

In addition to the cross-sectional difference specification, the spatial DD specification adds an indicator variable for houses sold within 0.3-mile of an offender's location ($D_{ijt}^{0.3}$) and an interaction of this indicator with another indicator, which equals one if the sale took place after the offender's arrival ($Arrival_{it}$). Also included in the spatial DD model are Census block group-by-year fixed effects (δ_{jt}) and observable parcel level characteristics (X_i). Formally, to evaluate the impact of the sex offenders' arrival on nearby property values, we use the following empirical specification

$$log(P_{ijt}) = (\rho_0 D_{ijt}^{0.1} + \varphi_0 D_{ijt}^{0.3}) + (\rho_1 D_{ijt}^{0.1} + \varphi_1 D_{ijt}^{0.3}) \times Arrival_{it} + \beta X_i + \delta_{jt} + \varepsilon_{ijt} \quad (2)$$

Using this specification, parameters ρ_0 and φ_0 will capture the pre-existing locational difference in property values and ρ_1 and φ_1 will capture the effect of a sex offender's arrival on property values in the treated area and control area.

We first estimate the Equation (2) using the pooled sample. This model assumes that the relationships between housing prices and characteristics are similar within and outside offender areas, the areas within the 0.3-mile of offenders. To relax this assumption, Equation (2) is re-estimated using the sales two years before and after offenders' arrivals in offender areas. For these estimations, we use offender area-by-year fixed effects instead of Census block group-by-year fixed effects and cluster standard errors at the offender area level instead of the Census block group level.

4.5 Results

4.5.1 Graphical Evidence

Figure 4.2 plots pooled sample housing price gradients for the distance from sex offenders' locations for the year before the sex offender moved in and the year after the arrival of an offender. Visually inspecting this figure, there is little observed impact of the arrival of a sex offender on housing prices. In Figure 4.2, housing price gradients stay almost the same within the 0.1-mile of offenders during the year before and after an offender's arrival. The housing price gradients more than 0.1-mile from offenders change substantially, which is likely due to general temporal change in the housing market.

The housing price gradients in each county are different (Figure 4.3). In Baltimore City, the gradients within 0.1-mile of offenders' locations decreased by approximately $50,000 after an offenders' arrival, while other property values did not change much. In contrast, in Montgomery County, the gradients within 0.1-mile of offenders increase by approximately $50,000 post offenders' arrivals. Besides, the gradients in Baltimore County and Prince George County stay almost the same within 0.3-mile of offenders.

In addition to the price gradient, Figure 4.4 and 4.5 plot the price trends over the two years pre- and post-offender arrival in both treated and control areas. Figure 4.4 shows that housing values in the treated area are lower than those in the control area in the year before offenders' arrivals, and both follow the same downward trend. Following the offenders' arrivals, the values in treated area jump by a larger magnitude than those in control area initially, then quickly decrease to levels below those in control area and flatten out after one-year post arrivals. Combined with the separate graphs of the four counties (Figure 4.5), these figures of prices trends show almost no effect on housing values in the treated area following sex offenders' arrivals.

4.5.2 Empirical Results

4.5.2.1 Pre-existing Differences in Housing Characteristics between Treated and Control Areas

Recall that our estimation strategy relies on the relative similarity of houses sold within 0.1-mile of an offender to those sold between 0.1- and 0.3-mile of an offender. To test any pre-existing differences in parcel characteristics between these two areas, a cross-sectional specification, Equation (1), is estimated. In this estimation, we limit the sample to the sales that took place in the two years before the offenders' arrival and test log of price, land size, structure area size, property age, an indicator of dwelling blow average, and an indicator of having a basement. Results are presented in Table 4.4. Overall, we find little evidence of any pre-existing differences, except that the land size inside 0.1-mile is 0.03 acres smaller than that between 0.1- and 0.3-mile from offenders.

4.5.2.2 The Impact of Sex Offenders' Arrivals

Next, we present the impact of sex offenders' arrival using the pooled sample of all four counties in Maryland (Table 4.5). Column 1 presents the estimates from Equation (1). The estimate of ρ_0 from this specification shows that the average price of the houses within 0.1-mile of an offender's future location is approximately 35 percent lower than that of other houses sold in the same year. However, this difference disappears once we include Census block group-by-year fixed effects and housing characteristics in the regression (column 2 of Table 4.3). This demonstrates that the control variables capture almost all the differences between areas in which offenders move in and the rest of the counties.

The estimates from a simple pre-post comparison without the indicator variables for parcels selling between 0.1 and 0.3 miles from the offenders) show sex offenders' arrivals had no impact on the prices of the homes located in the treated area (column 3 of Table 4.5). Based on the estimates from Equation (2), spatial DD, we still find an insignificant estimate of the impact of sex offender's arrival (column 4). We are 95 percent confident that the impact estimate is between – 2.5 percent and 1.2 percent. The estimated change in value for houses located between 0.1 and 0.3 miles of an offender's location when the offender arrives is positive (1.2 percent) and significant at the 10 percent level. This suggests that homeowners living just slightly farther away from the offender (between 0.1- and 0.3-mile) experienced an increase in property values on average.

Next, we re-estimate Equation (2) using the sample of sales two years before and after offenders' arrivals from offender areas (column 5 of Table 4.5). The estimates from this column show that the impact of sex offenders' arrivals on housing values within 0.1-mile of an offender is insignificant. However, the estimated change in value for houses located between 0.1- and 0.3-mile of an offender's location when the offender arrives is positive (3.0 percent).

Note Figure 4.4 and 4.5 show no change in property values within the 0.1-mile of an offender after the offender's arrival, regardless of the distance to the offender. To check this, we add an interaction using a binary variable indicating a sale within 0.1-mile of an offender after the offender has moved in, and the distance from the offender.[46] The results are consistent with the figures. Parcels a tenth of a mile away experienced no change in value as those directly adjacent to the offenders' locations (column 6 of Table 4.5).

Table 4.6 presents the results from our primary model spatial DD for each of the four counties in Maryland. In Baltimore City, homes located in the treated area sold for 0.98 percent more, on average, than surrounding homes before an offender's arrival, and 2.19 percent more after the offender's arrival (column 1 of Table 4.6). This 1.21 percent increase is insignificant. The 95 percent confidence interval of this estimate is between –4.1 percent and 8.4 percent. In Baltimore County, homes located in the treated area sold for 0.13 percent less, on average, than surrounding homes before an offender's arrival, and 1.41 percent less after the arrival (column 2 of Table 4.6). This difference, 1.28 percent, is still insignificant. The 95 percent confidence interval of this estimate is between -4.3 percent and 1.4 percent.

In Prince George County, homes located within 0.1-mile of an offender were 3.46 percent more expensive, on average, than surrounding homes before the offender's arrival, but 1.44 percent less after the arrival (column 3 of Table 4.6). This 4.9 percent drop is still insignificant. Its 95 percent confidence interval is between -5.2 percent and 2.3 percent. Finally, in Montgomery County, homes located in the treated area were 0.9 percent less expensive than surrounding homes prior to the offender's arrival, but just 0.06 percent less after the arrival (column 4 of Table 4.6). Again, this 0.84 percent

[46] We rescale the distance so that a value of 1 represents 0.1-mile from an offender for the ease of interpretation.

increase is insignificant. Its 95 percent confidence interval is between – 2.7 percent and 2.6 percent.

In summary, the results suggest a missing negative externality from sex offenders' crime risk on proximate property values, which is distinctly different from the findings of the previous literature (Linden and Rockoff 2008, Pope 2008). We find no evidence of pre-existing differences in housing price trends between houses in the offender areas and those in the surrounding areas.

4.5.2.3 Robustness Checks

Table 4.7 presents the results of a falsification test using false arrival dates equal to two years and three years prior to offenders' actual arrival dates. Results suggest home values within 0.1 miles of a future sex offender were not changed relative to those between 0.1 and 0.3 miles of the offender in the two years or three years prior to the offender's arrival.

Table 4.8 presents the results of models estimated with alternate fixed-effect specifications. In column 1, Census block group-by-year fixed effects were used, in column 2 Census block-by-year fixed effects were used, and in column 3 Census tract-by-year fixed effects were used. Together, these robustness checks confirm insignificant negative effects of crime risk from sex offenders' arrivals.

4.6 Discussion and Conclusion

This paper evaluates the impact of the perceived crime risk from registered sex offenders' arrivals, notified through sex offender registry websites, on proximate property values. The analysis makes use of a unique data set from 2008 – 2018 for four counties in Maryland, including Baltimore City, Baltimore County, Prince George County, and

Montgomery County. The econometric results provide no evidence that the arrival of a sex offender in a neighborhood causes a reduction in property values. These results differ considerably from the prior literature that found that sex offenders reduced property values.

Both Linden and Rockoff (2008) and Pope (2008) found a significant reduction in property values upon sex offenders' arrivals in the neighborhood. However, their results were based on the housing data around 1995 – 2005 when proactive community notification methods were often used. Proactive notifications are more likely to increase households fears over potential abductions or attacks (Beck and Travis Iii 2004). Our study is based on housing data after 2008 when passive notification methods were used. Our results suggest that passive notification methods (e.g., listing sex offenders on websites) have a smaller impact on property values. This is potentially due in part to awareness (Beck and Travis Iii 2006) and fear over personal victimization (Beck and Travis Iii 2004).

Sex offender's registration and notification laws have generated a contentious debate since their conception. First, there is doubt over the laws' validity since sex offenders' recidivism rates are relatively low among criminals. For instance, the U.S. Department of Justice found 5.3% of sex offender were rearrested for another sex crime among almost 9,700 offenders within three years of their 1994 state prison release (Langan, Smith, and Durose 2003). Also, a meta-analysis of approximately one hundred studies showed that the reported sexual recidivism rate was approximately 14% over a 4 – 6 follow-up years among nearly 30,000 sex offenders in North America and United Kingdom (Hanson, Morton-Bourgon, and Safety 2004, Hanson and Morton-Bourgon

2005). While this could still be considered high, it is much lower than other forms of recidivism.

Second, literature has found mixed evidence on the laws' effectiveness in reducing sex crimes. The purpose of the laws is to provide better information to local law enforcement agencies to apprehend offenders and to empower the public with better knowledge of offenders to self-protect themselves and prevent sex offenses from happening (Bedarf 1995). Earlier studies found little evidence that registration and notification laws had a meaningful effect on sex offenders' recidivism rates or overall sex offense rates (Schram and Milloy 1995, Adkins et al. 2000, Vásquez, Maddan, and Walker 2008, Agan 2011). However, Prescott and Rockoff (2011) found that sex offender registration was associated with a significant decrease in the frequency of reported sex offenses. They also found that the notification law was associated with a reduction in the frequency of sex offenses committed by first-time offenders.

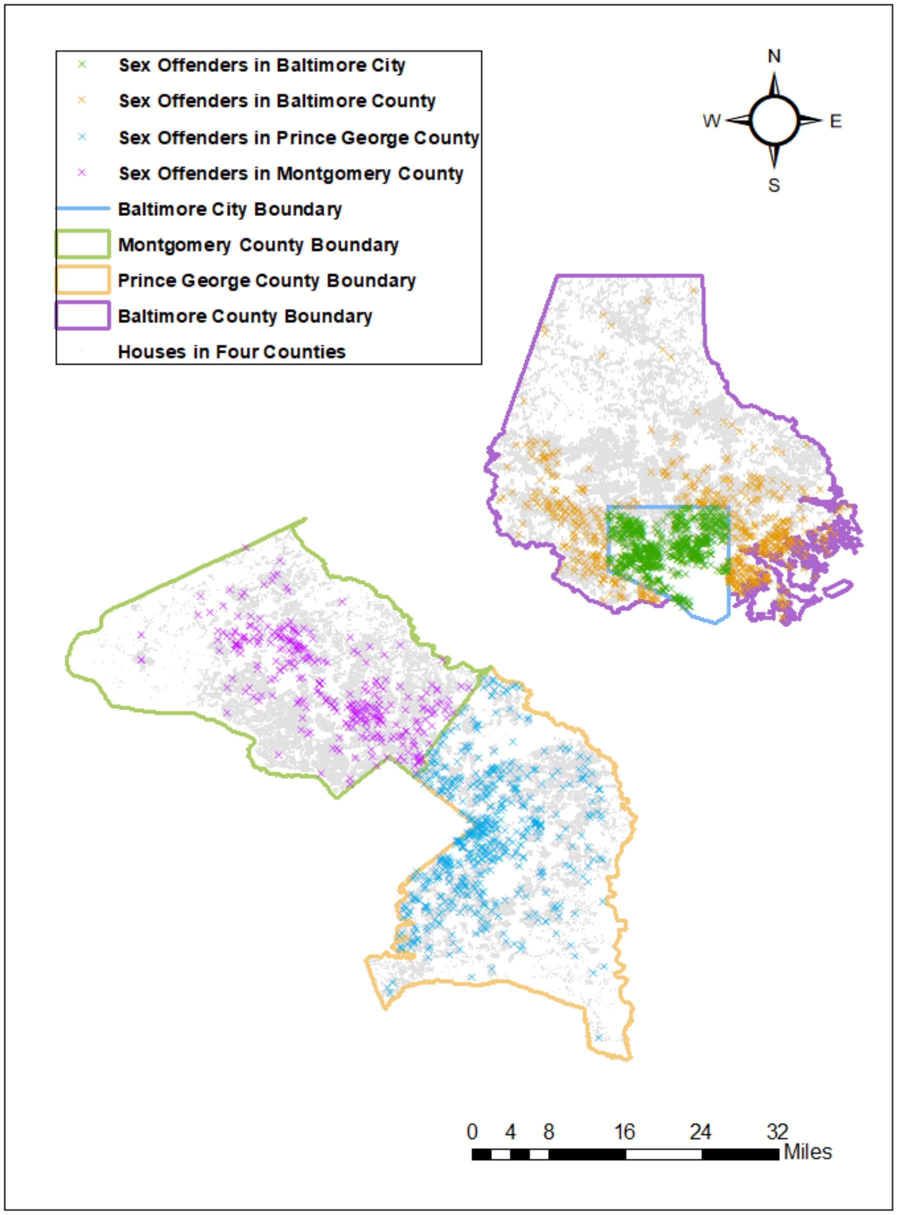
Third, critics also argue that the sex offender laws removed the anonymity of sex offenders, which stigmatizes their families and creates a significant barrier for their re-integration into the community and to find housing as well as employment opportunities, which might increase recidivism (Zevitz and Farkas 2000, Tewksbury 2005, Levenson and Cotter 2005).

This paper suggests that sex offenders' arrivals might not bring financial cost to nearby households. At least two reasons might explain this phenomenon. One, the frequency of registered sex offenders re-committing sex crimes might be low in Maryland during the past decade, which would be consistent with the finding in the literature that sex offenders have a low recidivism rate. The other, the passive notification method might be effective in dissuading crimes committed by sex offenders, particularly first-time offenders, because of the penalty of distributing sex offenders' information through the internet.

An important caveat to our conclusions is that we study the sex offenders who live in their current addresses as of April 2019. Studies based on registry data at one point in time might suffer from bias since move-in dates exist for the current address only. Also, old move-in dates capture offenders who lived in the current addresses for long, which might not be representative to all the offenders moved in during the similar dates but moved out later. Based on the records in Lincoln, Nebraska, most sex offenders tend to live in an address for less than six months, and the neighborhoods offender live temporarily were on average better than those where they maintain more permanent residences (Yeh 2015).

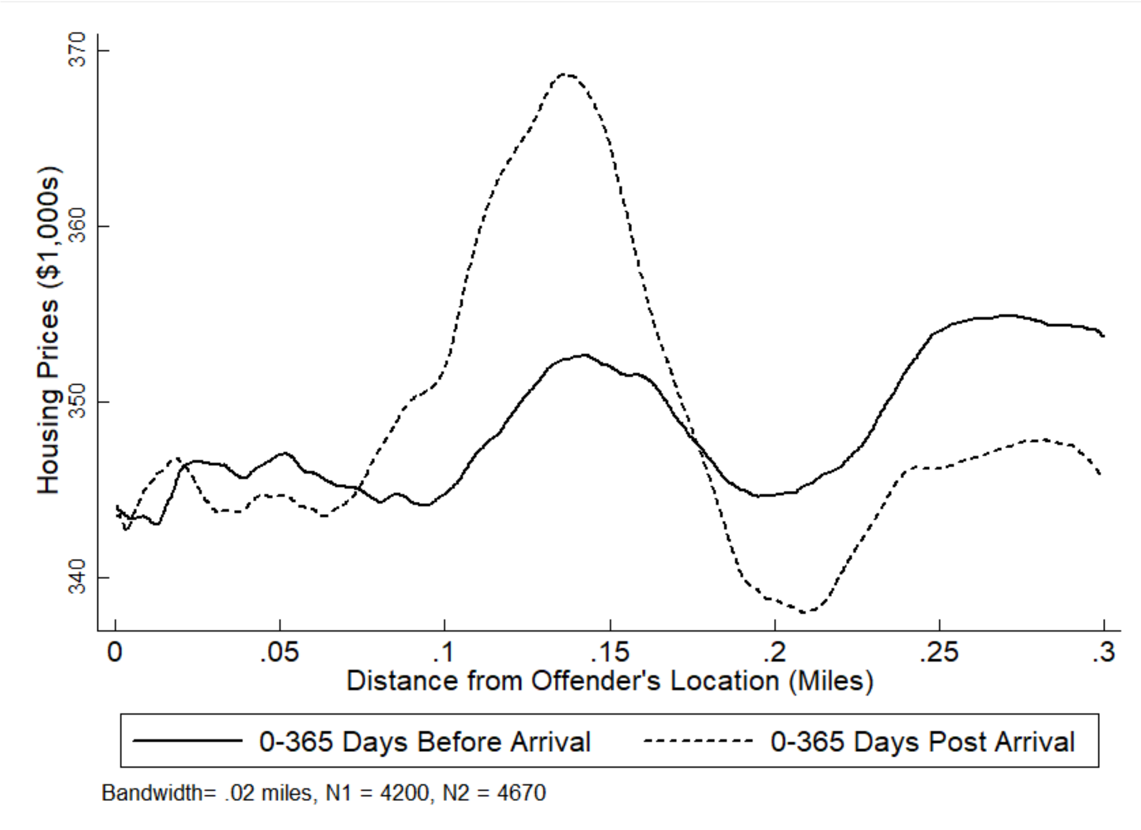
Overall, this analysis provides an additional piece of evidence for the debate over the sex offender registration and notification policies. The results of this paper reveal no reduction in property values proximate to sex offenders' residences following their arrivals in the four counties in Maryland (2018-2019). This evidence implies that households may not face financial loss due to sex offenders' arrivals in the same neighborhoods. However, future research in this area is needed to evaluate the impact of more recent sex offenders' laws on the communities.

Figure 4.1: Sex Offenders and Houses in Four Maryland Counties



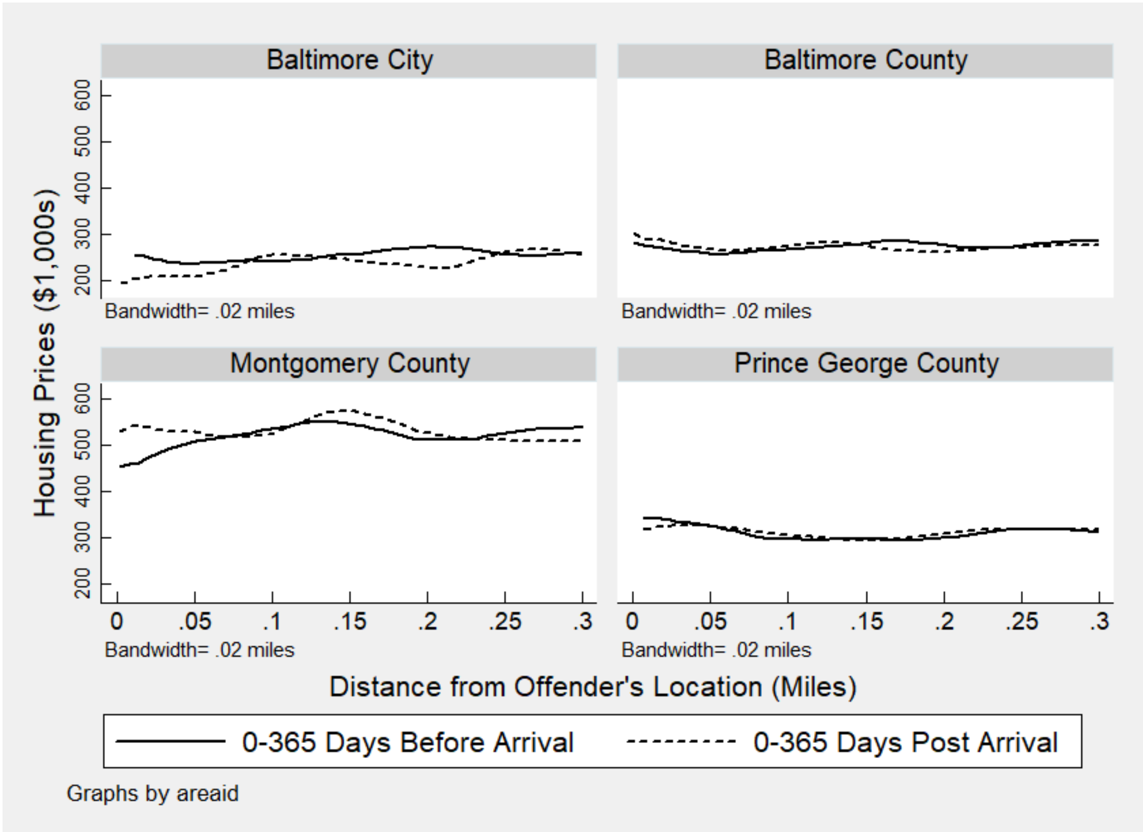
Notes: Sex offender dataset was collected from Maryland Registered Sex Offender Website as of April 2019.

Figure 4.2: Price Gradient of Distance from Offenders in the Four Counties of Maryland



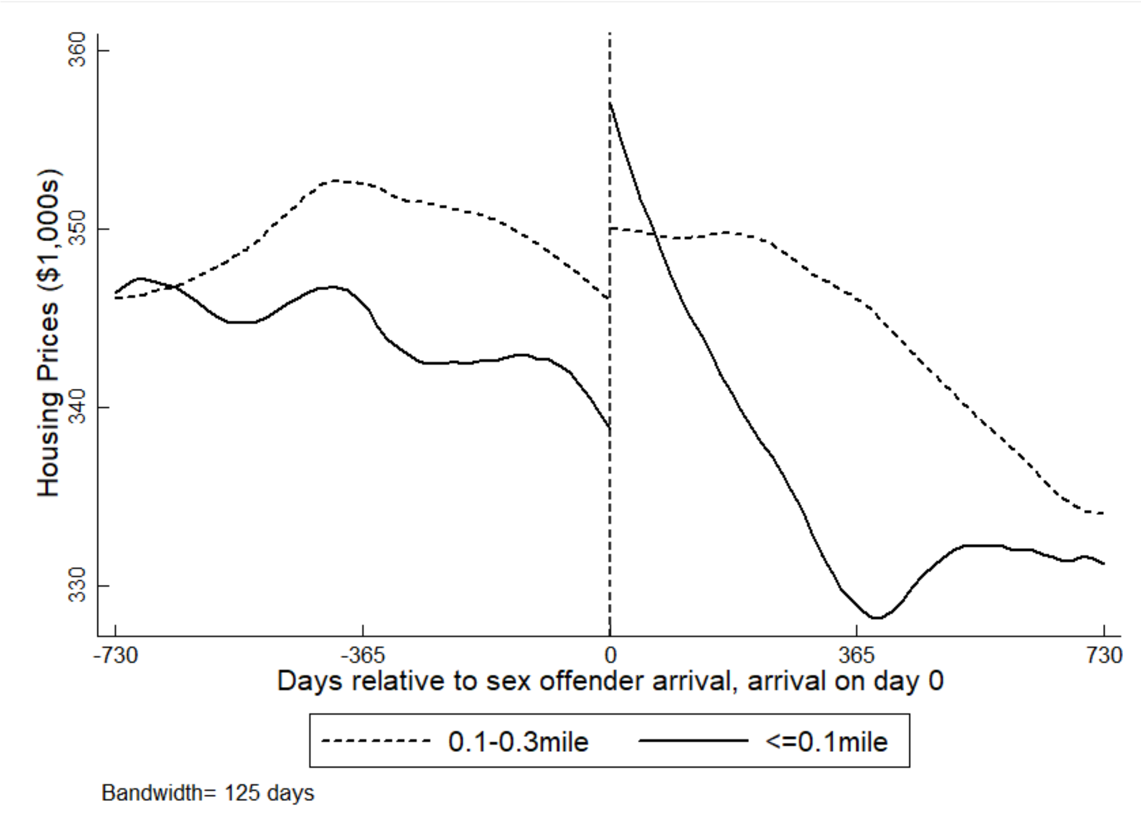
Notes: Results from local polynomial regression of housing price on distance from sex offenders, with epanechnikov function and degree 0. Single-family residential sales data was obtained from the Maryland Open Data Portal, 2008 – 2018.

Figure 4.3: Price Gradient of Distance from Offenders in Maryland Separated by Counties



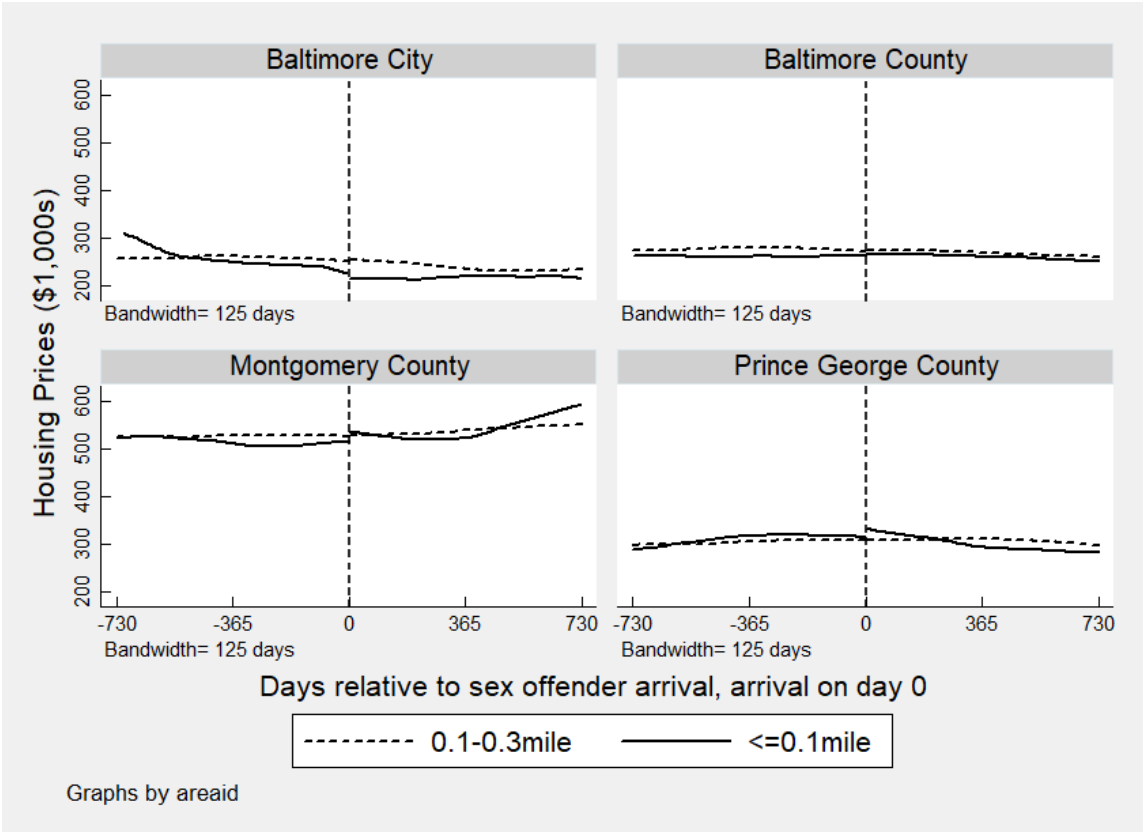
Notes: Results from local polynomial regression of housing price on distance from sex offenders, with epanechnikov function and degree 0. Single-family residential sales data was obtained from the Maryland Open Data Portal, 2008 – 2018.

Figure 4.4: Price Trends before and after Offenders' Arrivals in Maryland



Notes: Results from local polynomial regression of housing price on distance from sex offenders, with epanechnikov function and degree 0. Single-family residential sales data was obtained from the Maryland Open Data Portal, 2008 – 2018.

Figure 4.5: Price Trends before and after Offenders' Arrivals in Maryland Separated by Counties



Notes: Results from local polynomial regression of housing price on distance from sex offenders, with epanechnikov function and degree 0. Single-family residential sales data was obtained from the Maryland Open Data Portal, 2008 – 2018.

Table 4.1: Registered Sex Offender Characteristics in Four Maryland Counties as of April 2018

	Four Counties	Baltimore City	Baltimore County	Prince George County	Montgomery County
Age	47.109	47.018	46.688	46.945	48.503
(years)	(13.22)	(12.68)	(13.69)	(12.78)	(14.43)
Height	69.794	69.819	69.954	69.903	69.202
(inches)	(3.23)	(3.22)	(3.15)	(3.32)	(3.21)
Weight	194.396	189.438	199.352	195.597	194.913
(lbs.)	(41.28)	(39.12)	(43.04)	(40.55)	(43.35)
Length of living	1281.493	1124.132	1331.510	1345.303	1465.468
(days)	(868.12)	(824.02)	(856.32)	(844.47)	(983.68)
Employed	53.4%	41.4%	53.3%	60.3%	71.7%
Vehicle	46.3%	18.6%	66.2%	47.3%	75.7%
Under Community Supervision	24.6%	20.6%	31.2%	22.7%	24.9%
Race					
White	42.0%	22.1%	74.8%	22.1%	64.7%
African American	54.5%	76.5%	21.5%	75.2%	24.9%
Other	3.5%	1.3%	3.7%	2.7%	10.4%
Skin Color					
Dark	11.1%	16.1%	3.4%	15.5%	5.2%
Medium	52.8%	62.4%	30.4%	58.5%	62.4%
Light	32.3%	19.0%	62.5%	22.1%	24.9%
Other	1.2%	0.9%	1.1%	1.8%	1.2%
Eye Color					
Dark	82.9%	91.3%	65.9%	90.6%	80.9%
Light	17.1%	8.7%	34.1%	9.4%	19.1%
Hair Color					
Dark	82.4%	82.6%	76.8%	89.4%	79.8%
Light	12.5%	10.7%	18.1%	6.1%	17.9%
Other	5.2%	6.7%	5.2%	4.5%	2.3%
Risky tier					
I	14.5%	8.3%	14.6%	16.7%	26.0%
II	15.4%	8.5%	32.4%	6.7%	15.6%
III	70.1%	83.2%	53.0%	76.7%	58.4%
Number of Offenders	1,299	447	349	330	173

Notes: Sex offender dataset was collected from Maryland Registered Sex Offender Website as of April 2019. Mean and standard deviations (in parentheses) are reported.

Table 4.2: Parcel Characteristics in Each of the Four Maryland County, 2008 - 2018

	Baltimore City	Baltimore County	Prince George County	Montgomery County
	Mean (SD)	Mean (SD)	Mean (SD)	Mean (SD)
Sale price	304.135	381.894	356.368	742.927
(2018 $1,000)	(198.50)	(220.14)	(161.78)	(406.17)
Land size	0.215	0.649	0.416	0.462
(acres)	(0.15)	(1.65)	(1.55)	(1.04)
Structure area	1.840	2.028	2.188	2.413
(1,000 square feet)	(0.83)	(1.00)	(1.12)	(1.17)
age	81.263	46.372	36.568	41.615
(years)	(22.84)	(29.98)	(29.40)	(26.18)
	Percentage	Percentage	Percentage	Percentage
Basement	97.6%	89.3%	80.6%	93.2%
Story height				
1 story	10.4%	28.4%	31.3%	24.0%
1.5 stories	28.4%	22.6%	13.4%	10.0%
2 or more stories or split	61.2%	49.0%	55.3%	66.0%
Dwelling exterior material				
Frame	13.9%	10.8%	79.1%	39.9%
Brick	19.3%	18.9%	19.7%	28.7%
Siding	33.6%	56.6%	-	21.8%
1/2 Brick 1/2 frame	-	1.7%	-	6.8%
Asbestos shingle	11.7%	5.0%	-	-
Stucco	6.6%	2.7%	-	-
Other	14.9%	4.4%	1.3%	2.8%
Quality tier				
Average	16.4%	50.9%	32.0%	41.4%
Above average	24.6%	23.6%	11.8%	56.9%
Below average	59.0%	25.3%	56.1%	1.4%
Sample size	7,104	35,521	37,088	49,227

Notes: Single-family residential sales data was obtained from the Maryland Open Data Portal, 2008 – 2018.

Table 4.3: Parcel Characteristics in Combined Four Maryland Counties, 2008 - 2018

	All parcels	Parcels between 0.1 and 0.3 mile of an offender	Parcels within 0.1 mile of an offender
	Mean (SD)	Mean (SD)	Mean (SD)
Sale price	508.104	351.328	344.999
(2018 $1,000)	(346.97)	(206.80)	(193.39)
Land size	0.487	0.246	0.225
(acres)	(1.37)	(0.33)	(0.30)
Structure area	2.211	1.717	1.688
(1,000 square feet)	(1.11)	(0.81)	(0.75)
age	43.658	54.301	52.402
(years)	(29.71)	(28.19)	(28.18)
	Percentage	Percentage	Percentage
Basement	88.7%	86.2%	84.2%
Story height			
1 story	26.6%	32.8%	32.2%
1.5 stories	15.4%	23.4%	23.8%
2 or more stories or split	58.0%	43.9%	43.9%
Dwelling exterior material			
Frame	41.7%	37.6%	38.6%
Brick	22.9%	24.0%	23.7%
Siding	25.8%	28.0%	28.4%
1/2 Brick 1/2 frame	3.1%	1.5%	1.4%
Asbestos shingle	2.0%	3.7%	3.4%
Stucco	1.1%	1.6%	1.3%
Other	3.5%	3.7%	3.3%
Quality tier			
Average	39.9%	40.8%	35.7%
Above average	33.0%	14.0%	15.2%
Below average	26.9%	45.0%	49.1%
Sample size	128,940	38,848	6,067

Notes: Single-family residential sales data was obtained from the Maryland Open Data Portal, 2008 – 2018.

Table 4.4: Pre-arrival Differences in Average Characteristics of Homes Sold within 0.3 Mile of Offenders' Locations

Pre-arrival differences in sales	Log price	Land size in acres	Structure Area in 1,000 Square feet	Age in years	Below average dwelling	Basement
Within 0.1 mile of offender	-0.0116	-0.0304***	0.0104	-4.612	0.0102	-0.0133
	(0.0274)	(0.0108)	(0.0771)	(2.817)	(0.0321)	(0.0172)
Constant	12.63***	0.253***	1.748***	53.63***	0.449***	0.860***
	(0.0226)	(0.00712)	(0.0390)	(1.349)	(0.0228)	(0.0102)
Sample Size	8,105	8,105	8,105	8,105	8,105	8,105
R-squared	0.015	0.006	0.016	0.017	0.005	0.002

Notes: Pre-arrival refers to the two-year period before the date upon which the offender moved to a current address. Robust standard errors (in parentheses) are clustered by offender areas. *** p<0.01, ** p<0.05, * p<0.1

Table 4.5: Impact of Sex Offenders' Arrival on Property Value using Combined Four Maryland Counties

	Log (sale price) pre-arrival		Log (sale price), pre- and post-arrival			
	(1)	(2)	(3)	(4)	(5)	(6)
Within 0.1 mile of offender	-0.348***	0.00145	0.00172	0.00911	-0.0142	-0.0143
	(0.0267)	(0.00578)	(0.00839)	(0.00804)	(0.0134)	(0.0134)
Within 0.1 mile * post-arrival			-0.000602	-0.00634	0.00917	0.0175
			(0.00937)	(0.00936)	(0.0163)	(0.0184)
Dist<=0.1mile * post-arrival (0.1 Mile = 1)						0.00590
						(0.00583)
Within 0.3 mile of offender				-0.0220***		
				(0.00702)		
Within 0.3 mile * post-arrival				0.0123*	0.0299**	0.0174
				(0.00684)	(0.0126)	(0.0180)
H0: within 0.1 mile * post-arrival = 0			p-value = 0.949	p-value = 0.498	p-value = 0.575	p-value = 0.342
Housing Characteristics		√	√	√	√	√
Year fixed effects	√					
Census block group-year fixed effects		√	√	√		
Offender area-year fixed effects					√	√
Restricted to Offender Areas and 2 years pre- and post-arrival					√	√
Standard errors clustered by...	Census Block Group	Census Block Group	Census Block Group	Census Block Group	Offender Area	Offender Area
Sample size	128,923	128,923	128,923	128,923	17,376	17,376
R-squared	0.022	0.846	0.846	0.846	0.816	0.816

Notes: Pre-arrival (post-arrival) refers to the two-year period before (after) the date upon which offenders moved into their current address. *** p<0.01, ** p<0.05, * p<0.1

Table 4.6: Impact of Sex Offenders' Arrival on Property Value in Each Four Maryland County

	Baltimore City	Baltimore County	Prince George County	Montgomery County
Within 0.1 mile of offender	0.00980	-0.00132	0.0346**	-0.00922
	(0.0214)	(0.0123)	(0.0173)	(0.0122)
Within 0.1 mile * post-arrival	0.0219	-0.0141	-0.0144	-0.000558
	(0.0319)	(0.0145)	(0.0190)	(0.0137)
Within 0.3 mile of offender	-0.0228	-0.0177*	-0.0274	-0.0195***
	(0.0248)	(0.00906)	(0.0184)	(0.00548)
Within 0.3 mile * post-arrival	-0.00106	0.0132	0.0239	0.00397
	(0.0171)	(0.0121)	(0.0161)	(0.00599)
H0: within 0.1 mile * post-arrival = 0	p-value = 0.492	p-value = 0.331	p-value = 0.449	p-value = 0.967
Housing Characteristics	√	√	√	√
Census block group-year fixed effects	√	√	√	√
Sample size	7,104	35,507	37,085	49,227
R-squared	0.836	0.793	0.578	0.883

Notes: This is based on Column 4 of Table 5. Robust standard errors (in parentheses) are clustered by census block group. *** p<0.01, ** p<0.05, * p<0.1

Table 4.7: Falsification Test on Impact of Sex Offender Location using Combine Four Maryland Counties

VARIABLES	(1) *Baseline estimates*	(2) Two-year prior arrival dates	(3) Three-year prior arrival dates
Within 0.1 mile of offender	0.00911	0.0150	0.0133
	(0.00804)	(0.0105)	(0.0122)
Within 0.1 mile * post-arrival	-0.00634	-0.0138	-0.00992
	(0.00936)	(0.0112)	(0.0125)
Within 0.3 mile of offender	-0.0220***	-0.0237***	-0.0260***
	(0.00702)	(0.00865)	(0.00978)
Within 0.3 mile * post-arrival	0.0123*	0.0113	0.0132
	(0.00684)	(0.00787)	(0.00874)
H0: within 0.1 mile * post-arrival = 0	p-value = 0.498	p-value = 0.219	p-value = 0.426
Sample size	128,923	128,923	128,923
R-squared	0.846	0.846	0.846

Notes: The dependent variable is the log of the house price. All regressions contain census block group-year fixed effects and housing characteristics (see text for a list of characteristics included). Baseline results are taken from Column 4 of Table 3. Robust standard errors (in parentheses) are clustered by census block group. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4.8: Impact of Sex Offenders' Arrival using Alternative Fixed Effects and Combined Four Maryland Counties

	(1)	(2)	(3)
Within 0.1 mile of offender	0.00911	0.00935	0.00544
	(0.00804)	(0.0154)	(0.00717)
Within 0.1 mile * post-arrival	-0.00634	-0.0138	-0.00394
	(0.00936)	(0.0214)	(0.00859)
Within 0.3 mile of offender	-0.0220***	-0.0138	-0.0250***
	(0.00702)	(0.0131)	(0.00567)
Within 0.3 mile * post-arrival	0.0123*	0.0126	0.0132**
	(0.00684)	(0.0164)	(0.00624)
H0: within 0.1 mile * post-arrival = 0	p-value = 0.498	p-value = 0.519	p-value = 0.647
Housing Characteristics	√	√	√
Census block group-year fixed effects	√		
Census block-year fixed effects		√	
Census tract-year fixed effects			√
Standard errors clustered by...	Census Block Group	Census Block	Census Tract
Sample size	128,923	128,923	128,923
R-squared	0.846	0.907	0.830

Notes: Results are based on the model in Column 4 of Table 5. *** p<0.01, ** p<0.05, * p<0.1

Chapter 5: The Conclusion to The Health Insurance and Sex Offender Policies and Laws

This dissertation is motivated by two contentious debate over health policies and laws regarding health insurance and sex offenders. There have been numerous studies on the ACA since the implementation in 2010. Despite that, two research questions remain to be answered. Does Medicaid benefit of access to care encourage Medicaid enrollment among non-elderly adults? Does Medicaid improve access to care and birth outcomes? Chapter 2 and 3 address these two questions. On the other hand, the struggling to find evidence supporting the premises of sex offender registration and notification laws has raised the importance to re-evaluate the crime risk caused by sex offenders. Chapter 4 evaluates the risk through housing price valuation following sex offenders' arrivals on the proximate housing market. I summarize the main findings, limitations, and policy implications of each essay in turn.

In Chapter 2, "Physician Payment and Demand for Health Insurance: Evidence from Medicaid Primary Care Payment Parity," I examine the impact of Medicaid benefit of access to care, proxied by Medicaid-to-Medicare primary care physician payment ratio, on Medicaid coverage among non-elderly adults. This study exploits the quasi-experiment of Medicaid primary care payment parity, which mandated Medicaid primary care physician payment under both FFS and MC to be increased to the level of Medicare in 2013 – 14. Using a generalized DD method, reimbursement rate, and ACS data of 2010 – 2014, I find the Medicaid-to-Medicare fee ratios of patient office visits are strongly positively correlated with Medicaid coverage among low-income non-elderly adults. Specifically, a 10-percentage-point increase in the fee ratio of a 30 (45)-minute new patient office visit is associated with a 0.40 (0.36)-percentage-point increase in adult Medicaid coverage rate, and is associated with a 0.40 (0.39) percentage-point decrease in the uninsured rate, but is not associated with any change in the privately insured rate. These estimates suggest that more than 150,000 uninsured adults in poverty would enroll in Medicaid once this amount of payment change occurs. Further, subsample analyses suggest the impact is most significant among the near-elderly, non-parents, African-Americans, as well as those living inside central cities of metropolitan areas. According to the most recent estimate from CMS monthly reports, Medicaid enrollment has grown by 13.6 million (35.9%) between July 2013 and July 2018 in expansion states, and by 1.9 million (10.2%) between July 2013 and November 2017 in non-expansion states. This growth might have created more demand for health care from Medicaid patients, thus increasing the pressure on among primary care providers. However, the Medicaid physician payment is still low, on average at the level of 72% of Medicare payment as of 2016 (Zuckerman, Skopec, and Epstein 2017). Based on the findings of this chapter, raising primary care physician payment is an important policy tool to reduce the barrier of access to care among Medicaid beneficiaries, especially for those enrolled in FFS based programs.

In Chapter 3, "Public Insurance and Birth Outcomes: Evidence from Medicaid Implementation," we re-evaluate the impact of Medicaid on access to care and birth outcomes based on the population eligible for Medicaid during its provision period. We minimize the concern over crowd-out from private insurance to Medicaid and simulate the individual-level variation of Medicaid treatment intensity among childbearing age women. Using both DD and matching methods, this chapter finds that Medicaid provision shifted labor delivery from with a midwife or a physician to in a hospital and increased birth weight. These impacts were concentrated among nonwhite and unmarried mothers and those aged 30-49. This chapter also contributes to the current debate over Medicaid expansion by providing additional evidence of Medicaid's impact on improving access to care and birth outcomes.

In Chapter 4, "Does the Arrival of a Registered Sex Offender Hurt Property Values? Evidence from Maryland 2008 – 2018," we estimate the crime risk from sex offenders on proximate property values based on most recent laws on sex offender registration and notification. Using the data on single-family residential property of 2008 – 2018 and corresponding registered sex offender in four counties (Baltimore City, Baltimore County, Prince George County, and Montgomery County) as of April 2019 in Maryland, we apply a spatial DD strategy and find no negative impact of sex offenders' arrivals on proximate home values within 0.1-mile of their residences. This result is robust to several robustness checks. Note an important drawback of our study is the sex offender dataset, which records the sex offenders living in their current residence at a particular point of time. This dataset may lose many sex offenders' residences where they lived for some time before they moved out. This could create bias potentially due to the difference between sex offenders who move a lot and those who tend to stay in the same residence for a long period. Alternatively, the bias could come from the differential impacts of crime risk between temporal residences and permanent residences, where neighborhoods tend to be worse on average than the former (Yeh 2015). One extension of this chapter could use an archived sex offender dataset, which tracks their residences through time.

Overall, these chapters provide evidence to the debate over health policies and laws in the US. More work is needed to understand these issues further.

Appendices A: Appendices to Chapter 4

Table A.1: Parcel Characteristics in Baltimore City, 2008 - 2018

	All parcels	Parcels between 0.1 and 0.3 mile of an offender	Parcels within 0.1 mile of an offender
	Mean (SD)	Mean (SD)	Mean (SD)
Sale price	304.135	250.976	244.593
(2018 $1,000)	(198.50)	(156.81)	(153.47)
Land size	0.215	0.191	0.192
(acres)	(0.15)	(0.09)	(0.11)
Structure area	1.840	1.688	1.675
(1,000 square feet)	(0.83)	(0.70)	(0.74)
age	81.263	83.085	82.017
(years)	(22.84)	(18.77)	(19.74)
	Percentage	Percentage	Percentage
Basement	97.6%	98.2%	97.9%
Story height			
1 story	10.4%	9.5%	9.5%
1.5 stories	28.4%	33.3%	32.7%
2 or more stories or split	61.2%	57.3%	57.8%
Dwelling exterior material			
Frame	13.9%	14.2%	15.5%
Brick	19.3%	18.4%	14.5%
Siding	33.6%	35.7%	39.2%
1/2 Brick 1/2 frame	-	-	-
Asbestos shingle	11.7%	13.4%	14.3%
Stucco	6.6%	6.2%	4.1%
Other	14.9%	12.2%	12.4%
Quality tier			
Average	16.4%	14.2%	13.3%
Above average	24.6%	12.6%	9.1%
Below average	59.0%	73.2%	77.6%
Sample size	7,104	4,476	581

Notes: Single-family residential sales data was obtained from the Maryland Open Data Portal, 2008 – 2018.

Table A.2: Parcel Characteristics in Baltimore County, 2008 - 2018

	All parcels	Parcels between 0.1 and 0.3 mile of an offender	Parcels within 0.1 mile of an offender
	Mean (SD)	Mean (SD)	Mean (SD)
Sale price	381.894	281.140	264.448
(2018 $1,000)	(220.14)	(134.82)	(114.62)
Land size	0.649	0.258	0.239
(acres)	(1.65)	(0.34)	(0.29)
Structure area	2.028	1.606	1.559
(1,000 square feet)	(1.00)	(0.65)	(0.57)
age	46.372	53.108	52.427
(years)	(29.98)	(27.50)	(25.98)
	Percentage	Percentage	Percentage
Basement	89.3%	86.8%	83.3%
Story height			
1 story	28.4%	31.1%	32.5%
1.5 stories	22.6%	30.9%	33.3%
2 or more stories or split	49.0%	38.0%	34.2%
Dwelling exterior material			
Frame	10.8%	7.4%	7.3%
Brick	18.9%	17.3%	17.0%
Siding	56.6%	60.6%	61.7%
1/2 Brick 1/2 frame	1.7%	1.0%	1.1%
Asbestos shingle	5.0%	6.8%	6.6%
Stucco	2.7%	2.8%	2.8%
Other	4.4%	4.1%	3.4%
Quality tier			
Average	50.9%	49.4%	43.0%
Above average	23.6%	9.0%	6.8%
Below average	25.3%	41.5%	50.1%
Sample size	35,521	12,112	1,828

Notes: Single-family residential sales data was obtained from the Maryland Open Data Portal, 2008 – 2018.

Table A.3: Parcel Characteristics in Prince George County, 2008 - 2018

	All parcels	Parcels between 0.1 and 0.3 mile of an offender	Parcels within 0.1 mile of an offender
	Mean (SD)	Mean (SD)	Mean (SD)
Sale price	356.368	303.800	305.233
(2018 $1,000)	(161.78)	(134.41)	(126.76)
Land size	0.416	0.259	0.232
(acres)	(1.55)	(0.31)	(0.35)
Structure area	2.188	1.756	1.716
(1,000 square feet)	(1.12)	(0.93)	(0.83)
age	36.568	48.410	47.998
(years)	(29.40)	(28.55)	(28.76)
	Percentage	Percentage	Percentage
Basement	80.6%	79.1%	78.2%
Story height			
1 story	31.3%	40.3%	36.2%
1.5 stories	13.4%	19.1%	20.8%
2 or more stories or split	55.3%	40.5%	43.0%
Dwelling exterior material			
Frame	79.1%	73.7%	72.1%
Brick	19.7%	24.9%	26.1%
Siding	-	-	-
1/2 Brick 1/2 frame	-	-	-
Asbestos shingle	-	-	-
Stucco	-	-	-
Other	1.3%	1.4%	1.8%
Quality tier			
Average	32.0%	16.3%	13.1%
Above average	11.8%	7.4%	9.9%
Below average	56.1%	76.2%	76.9%
Sample size	37,088	11,894	2,060

Notes: Single-family residential sales data was obtained from the Maryland Open Data Portal, 2008 – 2018.

Table A.4: Parcel Characteristics in Montgomery County, 2008 - 2018

	All parcels	Parcels between 0.1 and 0.3 mile of an offender	Parcels within 0.1 mile of an offender
	Mean (SD)	Mean (SD)	Mean (SD)
Sale price	742.927	531.202	524.912
(2018 $1,000)	(406.17)	(250.28)	(228.56)
Land size	0.462	0.241	0.214
(acres)	(1.04)	(0.38)	(0.26)
Structure area	2.413	1.815	1.807
(1,000 square feet)	(1.17)	(0.86)	(0.81)
age	41.615	50.027	47.282
(years)	(26.18)	(24.30)	(25.82)
	Percentage	Percentage	Percentage
Basement	93.2%	88.4%	87.9%
Story height			
1 story	24.0%	36.1%	35.0%
1.5 stories	10.0%	15.1%	13.8%
2 or more stories or split	66.0%	48.8%	51.2%
Dwelling exterior material			
Frame	39.9%	41.4%	39.5%
Brick	28.7%	33.3%	31.5%
Siding	21.8%	18.8%	23.2%
1/2 Brick 1/2 frame	6.8%	4.3%	4.1%
Asbestos shingle	-	-	-
Stucco	-	-	-
Other	2.8%	2.2%	1.8%
Quality tier			
Average	41.4%	70.5%	64.5%
Above average	56.9%	28.2%	33.9%
Below average	1.4%	1.2%	1.6%
Sample size	49,227	10,366	1,598

Notes: Single-family residential sales data was obtained from Maryland Open Data Portal, 2008 – 2018.

Table A.5: Impact of Sex Offenders' Arrival on Property Value in Baltimore City

	Log (sale price) pre-arrival		Log (sale price), pre- and post-arrival			
	(1)	(2)	(3)	(4)	(5)	(6)
Within 0.1 mile of offender	-0.185***	0.0211	0.00922	0.00980	-0.00514	-0.00579
	(0.0630)	(0.0172)	(0.0212)	(0.0214)	(0.0391)	(0.0390)
Within 0.1 mile * post-arrival			0.0217	0.0219	0.0214	0.0557
			(0.0317)	(0.0319)	(0.0493)	(0.0558)
Dist<=0.1mile * post-arrival (0.1 Mile = 1)						0.0232
						(0.0180)
Within 0.3 mile of offender				-0.0228		
				(0.0248)		
Within 0.3 mile * post-arrival				-0.00106	0.00190	-0.0478
				(0.0171)	(0.0359)	(0.0532)
H0: within 0.1 mile * post-arrival = 0			p-value = 0.494	p-value = 0.492	p-value = 0.665	p-value = 0.320
Housing Characteristics		√	√	√	√	√
Year fixed effects	√					
Census block group-year fixed effects		√	√	√		
Offender area-year fixed effects					√	√
Restricted to Offender Areas and 2 years pre- and post-arrival					√	√
Standard errors clustered by...	Census Block Group	Census Block Group	Census Block Group	Census Block Group	Offender Area	Offender Area
Sample size	7,104	7,104	7,104	7,104	2,029	2,029
R-squared	0.051	0.836	0.836	0.836	0.775	0.775

Notes: Pre-arrival (post-arrival) refers to the two-year period before (after) the date upon which offenders moved into their current address. *** p<0.01, ** p<0.05, * p<0.1

Table A.6: Impact of Sex Offenders' Arrival on Property Value in Baltimore County

	Log (sale price) pre-arrival		Log (sale price), pre- and post-arrival			
	(1)	(2)	(3)	(4)	(5)	(6)
Within 0.1 mile of offender	-0.331***	-0.0111	-0.00737	-0.00132	-0.0122	-0.0124
	(0.0297)	(0.00961)	(0.0128)	(0.0123)	(0.0225)	(0.0225)
Within 0.1 mile * post-arrival			-0.00825	-0.0141	-0.0188	-0.00424
			(0.0148)	(0.0145)	(0.0306)	(0.0351)
Dist<=0.1mile * post-arrival (0.1 Mile = 1)						0.0102
						(0.0103)
Within 0.3 mile of offender				-0.0177*		
				(0.00906)		
Within 0.3 mile * post-arrival				0.0132	0.0510*	0.0292
				(0.0121)	(0.0266)	(0.0373)
H0: within 0.1 mile * post-arrival = 0			p-value = 0.578	p-value = 0.331	p-value = 0.538	p-value = 0.904
Housing Characteristics		√	√	√	√	√
Year fixed effects	√					
Census block group-year fixed effects		√	√	√		
Offender area-year fixed effects					√	√
Restricted to Offender Areas and 2 years pre- and post-arrival					√	√
Standard errors clustered by...	Census Block Group	Census Block Group	Census Block Group	Census Block Group	Offender Area	Offender Area
Sample size	35,507	35,507	35,507	35,507	5,193	5,193
R-squared	0.031	0.793	0.793	0.793	0.726	0.726

Notes: Pre-arrival (post-arrival) refers to the two-year period before (after) the date upon which offenders moved into their current address. *** p<0.01, ** p<0.05, * p<0.1

Table A.7: Impact of Sex Offenders' Arrival on Property Value in Prince George County

	Log (sale price) pre-arrival		Log (sale price), pre- and post-arrival			
	(1)	(2)	(3)	(4)	(5)	(6)
Within 0.1 mile of offender	-0.137***	0.0228*	0.0243	0.0346**	-0.0202	-0.0201
	(0.0327)	(0.0117)	(0.0182)	(0.0173)	(0.0285)	(0.0285)
Within 0.1 mile * post-arrival			-0.00328	-0.0144	0.0251	0.0193
			(0.0194)	(0.0190)	(0.0317)	(0.0359)
Dist<=0.1mile * post-arrival						-0.00415
(0.1 Mile = 1)						(0.0124)
Within 0.3 mile of offender				-0.0274		
				(0.0184)		
Within 0.3 mile * post-arrival				0.0239	0.0307	0.0395
				(0.0161)	(0.0223)	(0.0343)
H0: within 0.1 mile * post-arrival = 0			p-value = 0.866	p-value = 0.449	p-value = 0.429	p-value = 0.590
Housing Characteristics		√	√	√	√	√
Year fixed effects	√					
Census block group-year fixed effects		√	√	√		
Offender area-year fixed effects					√	√
Restricted to Offender Areas and 2 years pre- and post-arrival					√	√
Standard errors clustered by...	Census Block Group	Census Block Group	Census Block Group	Census Block Group	Offender Area	Offender Area
Sample size	37,085	37,085	37,085	37,085	5,685	5,685
R-squared	0.054	0.578	0.578	0.578	0.646	0.646

Notes: Pre-arrival (post-arrival) refers to the two-year period before (after) the date upon which offenders moved into their current address. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A.8: Impact of Sex Offenders' Arrival on Property Value in Montgomery County

	Log (sale price) pre-arrival		Log (sale price), pre- and post-arrival			
	(1)	(2)	(3)	(4)	(5)	(6)
Within 0.1 mile of offender	-0.315***	-0.0153	-0.0161	-0.00922	-0.0128	-0.0129
	(0.0381)	(0.00945)	(0.0125)	(0.0122)	(0.0124)	(0.0124)
Within 0.1 mile * post-arrival			0.00182	-0.000558	0.0156	0.0267
			(0.0135)	(0.0137)	(0.0157)	(0.0205)
Dist<=0.1mile * post-arrival						0.00780
(0.1 Mile = 1)						(0.00792)
Within 0.3 mile of offender				-0.0195***		
				(0.00548)		
Within 0.3 mile * post-arrival				0.00397	0.0149	-0.00157
				(0.00599)	(0.0173)	(0.0232)
H0: within 0.1 mile * post-arrival = 0			p-value = 0.893	p-value = 0.967	p-value = 0.322	p-value = 0.194
Housing Characteristics		√	√	√	√	√
Year fixed effects	√					
Census block group-year fixed effects		√	√	√		
Offender area-year fixed effects					√	√
Restricted to Offender Areas and 2 years pre- and post-arrival					√	√
Standard errors clustered by...	Census Block Group	Census Block Group	Census Block Group	Census Block Group	Offender Area	Offender Area
Sample size	49,227	49,227	49,227	49,227	4,469	4,469
R-squared	0.022	0.882	0.882	0.883	0.848	0.848

Notes: Pre-arrival (post-arrival) refers to the two-year period before (after) the date upon which offenders moved into their current address. *** p<0.01, ** p<0.05, * p<0.1

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