Journal of Law and Economics

Volume 66 | Number 1 Article 6

2-1-2023

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Did the Dependent Coverage Mandate Reduce Crime?

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Abstract

The Affordable Care Act's dependent coverage mandate (DCM) induced approximately 2 million young adults to join parental employer-sponsored health insurance plans. This study is the first to explore the impact of the DCM on crime, a potentially important externality. Using data from the National Incident-Based Reporting System, we find that the DCM induced a 2–5 percent reduction in property crime incidents involving young adult arrestees ages 22–25 relative to those ages 27–29. This finding is supported by supplemental analysis using data from the Uniform Crime Reports. An examination of the underlying mechanisms suggests that declines in large out-of-pocket expenditures for health care, increased educational attainment, and increases in cohabitation of parents and adult children may explain these declines in crime. Back-of-the-envelope calculations suggest that the DCM generated approximately \$371–\$512 million in annual social benefits from crime reduction among young adults.

1. Introduction

Crime is disproportionately committed by young adults. In 2019, approximately 45 percent of all arrestees were under the age of 30 (Federal Bureau of Investigation 2020), and arrestees ages 22–25 generated social costs of property and violent crimes of \$50.5 billion (in 2020 dollars) (McCollister, French, and Fang

Sabia acknowledges support from the Center for Health Economics and Policy Studies, including grants from the Troesh Family Foundation and the Charles Koch Foundation. The authors thank Karen Conway and Jacob Vogler and participants at the 2019 Denver/Boulder Applied Microeconomics Workshop, the Johns Hopkins Carey Business School, and the 2019 Western Economic Association conference for their comments and suggestions. The views expressed are those of the authors and do not necessarily reflect the official policy or position of the United States Air Force Academy, the Air Force, the Department of Defense, or the US government. Approved for public release: distribution unlimited (PA USAFA-DF-2022-609). All errors or omissions are our own.

[*Journal of Law and Economics,* vol. 66 (February 2023)]

2010).[1] In light of the high costs of crime attributable to young adults, policies that change incentives for youths' criminal behavior can have potentially large social welfare effects.

The dependent coverage mandate (DCM) of the Affordable Care Act (ACA) requires health insurers to allow young adults to remain on their parents' private health insurance plans until age 26. This provision was designed to increase insurance coverage among a relatively healthy population with historically high uninsured rates, which potentially reduces adverse selection in insurance markets. Early estimates show that in the first year following the DCM's adoption, approximately 2 million young adults added parental employer-sponsored health insurance (ESI), which translated to approximately 938,000 fewer uninsured persons (Antwi, Moriya, and Simon 2013). Other research documents that the DCM reduced young adults' out-of-pocket (OOP) health care costs (Busch, Golberstein, and Meara 2014; Chua and Sommers 2014), increased their educational attainment (Colman and Dave 2018; Heim, Lurie, and Simon 2018), increased their coresidency with parents (Chatterji, Liu, and Yörük 2022), and improved their access to mental health services (Kozloff and Sommers 2017; McClellan 2017), each of which may reduce crime. This study is the first to explore whether the DCM impacted criminal behavior.

Recent studies have found that increased access to public health insurance via Medicaid is associated with substantial reductions in crime (Wen, Hockenberry, and Cummings 2017; Vogler 2020; Aslim et al. 2019; Arenberg, Neller, and Stripling 2020; He and Barkowski 2020).[2] While insuring against financial risk and inducing increased use of health care services are shared mechanisms through which the DCM and Medicaid may affect crime, there are a number of reasons why the effect of the DCM on crime may differ.

First, the sociodemographic characteristics of those affected by the DCM and Medicaid expansions differ in ways that could generate differential policy impacts. The DCM targets those ages 19–25, a demographic that accounts for a substantial share of criminal arrests in the United States (Federal Bureau of Investigation 2020). For comparison, the average individual newly insured as a result of the ACA's Medicaid expansion was age 39. A 26-year-old, who has the lowest arrest propensity of those affected by the DCM, is 65 percent more likely to be arrested for a property crime and 84 percent more likely to be arrested for a violent crime than a 39-year-old (Courtemanche et al. 2017; Federal Bureau of Investigation 2020). In addition, the DCM was substantially more likely to impact males than most previous Medicaid expansions given that Medicaid recipients are disproportionately female (Kaiser Family Foundation 2019). In 2019, females

[1] These figures are calculated using Federal Bureau of Investigation (2020, table 38 [https://ucr.fbi.gov/crime-in-the-u.s/2019/crime-in-the-u.s.-2019/topic-pages/tables/table-38]; table 1 [https://ucr.fbi.gov/crime-in-the-u.s/2019/crime-in-the-u.s.-2019/tables/table-1]), supplemented with 2019 National Incident-Based Reporting System (NIBRS) data (Kaplan 2021).

[2] Also relevant is Jácome (2022), which shows that loss of Medicaid increased criminal behavior.

accounted for only about one-quarter of all arrestees (Federal Bureau of Investigation 2020). Thus, because the marginal person impacted by the DCM has a higher propensity to commit crime than the marginal person impacted by Medicaid expansions, one might expect larger effects on crime from the former policy change.

On the other hand, the ACA's Medicaid expansion targeted individuals living in poverty and in near-poverty households (household income up to 138 percent of the federal poverty line), while those affected by the DCM were generally from moderate- to higher-income households. Because family socioeconomic status is a potentially important correlate of young adults' crime, especially violent crime, the DCM's effect on some offenses may be smaller.[3]

Second, the DCM impacted access to private health insurance plans, which could have a very different effect on crime relative to expansions of public plans. This may be due to differences in the breadth of medical services provided (in services covered and breadth of the network of providers) and differences in the generosity of coverage. For example, the DCM decreased the total OOP spending of young adults (Busch, Golberstein, and Meara 2014) and, in particular, decreased the percentage of OOP costs for young adults with behavioral health conditions (Ali et al. 2016).[4] These effects may be due in part to increases in the quality of coverage under the DCM: we estimate a reduction of 954,000 22–25-year-olds without any form of coverage but an increase of 2.03 million young adults with dependent coverage, which implies that 1.07 million young adults transitioned to dependent coverage from another source of coverage. This large compositional shift in the type of coverage was not as substantial for the ACA's Medicaid expan-

[3] There is a widely accepted law and economics literature showing that criminal behavior is responsive to changes in local economic conditions (that is, wages and employment); for examples, see Gould, Weinberg, and Mustard (2002), Machin and Meghir (2004), Lin (2008), and Draca and Machin (2015). However, results in the literature on the cross-sectional relationship between household income and crime are more mixed than one might suppose, with several studies finding little evidence of an association (Wikström and Butterworth 2006; Dunaway et al. 2000; Wright et al. 1999). Further, while a young adult had to have access to private, parental insurance to benefit from the federal dependent coverage mandate (DCM), evidence suggests that not only the highest-income young adults gained coverage under the provision (McMorrow et al. 2015). In fact, absolute declines in the uninsured rate were similar among moderate-income (139–400 percent of the federal poverty level) and high-income young adults (more than 400 percent of the federal poverty line), which is likely because high-income young adults were much less likely to be uninsured before the DCM was implemented (McMorrow et al. 2015). On the basis of our analysis of waves 1 and 3 of the National Longitudinal Study of Adolescent to Adult Health, youth covered by private insurance (who could be affected by the DCM) report lower propensities to commit violent crimes but higher rates of property crime than youth covered by Medicaid (in wave 1; no statistically significant differences are found in wave 3). While these calculations are purely descriptive, they suggest that the population targeted by the DCM is not necessarily less prone to property crime than the population affected by Medicaid expansion under the Affordable Care Act (ACA). See Online Appendix Tables OA1 and OA2 for these calculations.

[4] Access to mental and behavioral health care has been shown to decrease local crime (Wen, Hockenberry, and Cummings 2017; Bondurant, Lindo, and Swensen 2018; Deza, Maclean, and Solomon 2020).

sions (Courtemanche et al. 2017; Clemens, McNichols, and Sabia 2020) and may have important and unique effects on crime.

In addition, there is strong evidence that the DCM incentivized greater investments in human capital (that is, educational attainment) among young adults (Colman and Dave 2018; Heim, Lurie, and Simon 2018), an added channel through which the DCM could reduce crime. Moreover, unlike Medicaid expansions, the DCM created incentives for closer residential and financial arrangements between adult children and their parents. For example, the DCM increased the rate of cohabitation of adult children and their parents (Chatterji, Liu, and Yörük 2022), which may have induced additional monitoring of the former and increased the costs of their engaging in surreptitious criminal activity. Such changes in living arrangements may also have affected parents' finances and incentives by others in the household (that is, siblings, parents, other residents) to commit crimes, which generated spillover effects on those not directly impacted by the DCM.

Finally, the marginal cost of insurance coverage for an uninsured adult differs substantially between the ACA's Medicaid expansions (Wolfe, Rennie, and Truffer 2017) and the DCM (Depew and Bailey 2015), and there are also differences in who bears the costs of these policies. This leads to different per-beneficiary social gains or losses and a different societal distribution of the net benefits and costs.[5]

This study is the first to provide estimates of the impact of the DCM on crime. First, using a panel of law-enforcement-agency–months from the 2008–13 National Incident-Based Reporting System (NIBRS)—and a difference-in-differences approach that relies on age eligibility as the primary source of identifying variation—we find that the DCM is associated with a 2–5 percent decline in property crime arrests among eligible individuals ages 22–25 relative to those ages 27–29. Our preferred estimate of the implementation effect translates to about 47,000–65,000 crimes averted by the DCM and an implied elasticity of crime with respect to dependent health insurance coverage of −.06. We find no evidence that the DCM affects violent crime. Supplemental estimates generated from the Uniform Crime Reports (UCR)—which rely on an alternate source of identifying variation, pre-DCM young adults' county-level uninsured rates, to capture a heterogeneous sample of the national program's shock across jurisdictions—largely confirm our NIBRS-based findings and further suggest little evidence of within-household spillover effects on older individuals. Back-of-the-envelope calculations suggest that the DCM generated approximately \$371–\$512 million in annual social benefits from reductions in crime among young adults.

[5] There are also identification-related advantages to studying the DCM. The age-based (or birth-cohort-based) nature of the DCM permits within-city, cross-cohort comparisons of crime, which better permit one to isolate the effects of DCM-induced private health insurance expansions from contemporaneous local economic and policy shocks.

2. Background

2.1. The Premandate Landscape and the Mandate's Impact on Insurance Coverage

Prior to March 2010, approximately one in three young adults ages 19–25 was uninsured (Antwi, Moriya, and Simon 2013). While many states passed dependent coverage laws before the federal implementation of the DCM, they tended to be weaker along several dimensions. In particular, most state DCMs had additional eligibility criteria (other than age), such as requiring that young adults be financially dependent on their parents, unmarried, childless, uninsured, or enrolled in school (Cantor et al. 2012a). Further, according to the Employee Retirement Income Security Act, firms that self-insure are not required to follow state-level insurance mandates (Pierron and Fronstin 2008).[6] Studies generally find that state-level DCMs result in modest increases in dependent coverage of about 1–2 percentage points (Levine, McKnight, and Heep 2011; Monheit et al. 2011; Depew 2015), with some results suggesting that the increases are largely offset by reductions in own-name coverage (Monheit et al. 2011; for a review, see Trudeau and Conway 2018).

Many studies document a reduction in the uninsured rate and an increase in private coverage among young adults after implementation of the federal DCM (Sommers and Kronick 2012; Cantor et al. 2012b; Antwi, Moriya, and Simon 2013; Sommers et al. 2013; O'Hara and Brault 2013; Kotagal et al. 2014; Chua and Sommers 2014; Shane and Ayyagari 2014; Barbaresco, Courtemanche, and Qi 2015; Jhamb, Dave, and Colman 2015; Scott et al. 2015a, 2015b; Wallace and Sommers 2015). Estimates of the increase in health insurance coverage generally range between 3 and 7 percentage points (or approximately 4 and 10 percent).[7] In addition to the increase in any source of health insurance coverage, many young adults transitioned to a parent's insurance plan from another form of health insurance (Antwi, Moriya, and Simon 2013).

2.2. Labor Market and Financial Mechanisms

The federal DCM may impact crime through a number of channels. Some of the pathways are shared with expansions in public health insurance (that is, a reduction in negative income effects from adverse health shocks and increased access to some health care services), while others are unique to the DCM (incentives for greater financial and residential ties between adult children and parents, increased college enrollment, and access to a greater breadth and quality of services with private coverage). We detail these below, beginning with economic and financial channels.

[6] In 2009, self-insured firms represented 59 percent of firms providing employer-sponsored insurance (Claxton et al. 2010).

[7] See Breslau et al. (2018b) for a review of this literature and the literature on the DCM and health care utilization and outcomes.

The theoretical connection between economic well-being and the propensity to commit crime is well established (Becker 1968). Theory suggests that noncriminal alternatives are a major component of the opportunity cost of engaging in criminal behavior to generate income. By this logic, any improvements in noncriminal options such as higher income, a better employment match, or additional educational choices will increase the opportunity cost of criminal behavior and reduce crime (see Gould, Weinberg, and Mustard 2002; Machin and Meghir 2004; Lin 2008; Draca and Machin 2015).

The DCM may have affected the trade-off between engagement in legal and illegal activities in several ways. First, the DCM has been shown to have an impact on employment and income, though research on this topic comes to mixed conclusions. There is evidence that the DCM decreased employment and wages among young adults (Antwi, Moriya, and Simon 2013; Heim, Lurie, and Simon 2018), decreased hours and the likelihood of working full-time (Antwi, Moriya, and Simon 2013), and increased job search activities (Colman and Dave 2018). However, there is a lack of consensus as to whether these reductions in employment represent an increase in job mobility, a decreased need to work full-time because of reduced reliance on employer-provided benefits, or worsening employment outcomes (Bailey and Chorniy 2016; Bailey 2017; Heim, Lurie, and Simon 2018).

Second, there is emerging evidence that increasing access to private health insurance increases educational attainment among dependents. Heim, Lurie, and Simon (2018) find a 2.3 percent increase in full-time college enrollment and a 4.3 percent increase in graduate student enrollment due to the DCM. Colman and Dave (2018) indicate that the DCM is associated with a 15–20 percent increase in young adults' time spent on educational activities. And while not specifically studying the effects of federal implementation of the DCM, Dillender (2014) finds that state-level DCMs increased educational attainment among young men.

Finally, the DCM has been shown to decrease OOP spending on health care services (Busch, Golberstein, and Meara 2014; Chua and Sommers 2014; Ali et al. 2016) and improve overall financial stability (Blascak and Mikhed 2018), which can be viewed as a positive shock to an individual's expected disposable income.[8] Moreover, as the DCM increases the likelihood that adult children live with their parents (Chatterji, Liu, and Yörük 2022), changes in cohabitation may also contribute to improvements in financial status through reductions in the costs of living (for example, rent and groceries). This cohabitation may have the additional effect of changing economic conditions for others in the household, which could create spillover effects on criminal behavior for parents or siblings not directly targeted by the DCM.

Taken together, the impacts of the DCM on crime via economic channels is theoretically ambiguous. While increasing educational attainment, reducing job lock, and softening financial strain should decrease the incentives for crime, neg-

[8] Chen, Bustamante, and Tom (2015) find no effect of the DCM on out-of-pocket (OOP) expenditures but exclude high-cost outliers from their sample.

ative employment effects could instead increase them (Gould, Weinberg, and Mustard 2002; Lochner 2004; Lochner and Moretti 2004; Machin and Meghir 2004; Lin 2008; Anderson 2014; Draca and Machin 2015).

2.3. Health Mechanisms

Health care access and utilization may affect criminal behavior by improving physical and mental health, though the magnitude and direction of the effects may depend on the type of care consumed. With regard to general health services, research suggests that the DCM increased the likelihood that young adults had a usual source of care (Kotagal et al. 2014; Wallace and Sommers 2015), increased the number of visits to physicians per year (Jhamb, Dave, and Colman 2015), and improved self-reported measures of physical and mental health (Carlson et al. 2014; Chua and Sommers 2014; Barbaresco, Courtemanche, and Qi 2015; Wallace and Sommers 2015; Burns and Wolfe 2016). These results do not theoretically imply growth or reduction in criminal activities, as better health and health care access in general may be beneficial in terms of both legal and illegal activities.

One form of health care particularly relevant to criminal activity is treatment for substance abuse disorder (SUD) and other related mental health services. Substance abuse has several connections to crime, with individuals abusing substances theoretically being more likely to commit crimes and more likely to be victimized (Goldstein 1985; Dobkin and Nicosia 2009; Dave, Deza, and Horn 2018). Several studies explore the relationship between the DCM, mental health services, and SUD treatment, and on balance this research suggests that the mandate increased access to treatment among young adults.[9,10] Antwi, Moriya, and Simon (2015) and Golberstein et al. (2015) find increases in psychiatric hospital admissions due to the DCM, with the latter study finding the largest increase due to SUD treatment. Fronstin (2013) finds that young adults newly covered by the DCM were more likely to access mental health services and SUD treatment, and Saloner and Cook (2014) find that the DCM led to a 17 percent increase in utilization of mental health treatment (but no impact on SUD treatment). Finally, Saloner et al. (2018) find reductions in inpatient SUD treatment because of the DCM. The authors point out that their result may not be reflective of a reduction in SUD treatment overall but a substitution toward other venues for care such as specialty rehabilitation and detoxification in outpatient settings.[11]

While increased access to SUD treatment is likely to reduce substance use and abuse, gaining health insurance coverage may increase access to prescription

[9] Wettstein (2019) finds that each additional percentage point of insurance coverage induced by the DCM reduced opioid-related deaths by 19.8 percent among young adults. This result may be driven by access to substance use disorder (SUD) treatment but could also be attributed to other factors such as lower barriers to access for overdose-reversal drugs such as naloxone.

[10] Bondurant, Lindo, and Swensen (2018) use openings and closings of SUD treatment centers to show that centers are associated with reductions in both violent and property crime.

[11] Saloner et al. (2018) also cannot rule out that reductions in substance use or improved general health make young adults less likely to need severe interventions.

drugs and therefore increase the potential for abuse of legal substances (Goldstein 1985). Health insurance may also increase the likelihood of substance abuse via ex ante moral hazard. Barbaresco, Courtemanche, and Qi (2015) find that the DCM increased risky drinking among young adults, a behavior that has been tied to increased crime (Carpenter 2007; Carpenter and Dobkin 2015; Anderson, Crost, and Rees 2018). In summary, these health-related mechanisms suggest a theoretically ambiguous impact of the DCM on crime, as the newly insured are more likely to be able to obtain SUD treatment but are also more likely to engage in crime-related risky behaviors.

2.4. Social Mechanisms

One final way that the DCM may have influenced crime is by changing household structure or through peer influences. Chatterji, Liu, and Yörük (2022) find that the DCM is associated with a 6-percentage-point (17.5 percent) increase in young adults living with their parents.[12] This change in living arrangements could provide a deterrent to crime through parental monitoring or risk of loss of inexpensive housing if parents are unwilling to support children engaged in criminal activities.[13] In addition, if the DCM increases the likelihood that young adults attend school, positive (potentially crime-reducing) social effects could be generated via peer influences (Gaviria and Raphael 2001).

2.5. Prior Literature on Health Insurance and Crime

The existing literature on health insurance and crime concludes that Medicaid expansions led to reductions in crime. Wen, Hockenberry, and Cummings (2017) examine expansions in Medicaid well before the enactment of the ACA via health insurance flexibility and accountability (HIFA) waivers. The authors demonstrate a direct connection between access to SUD treatment and reductions in crime using HIFA waiver timing as an instrument. More recent studies use variation from the ACA Medicaid expansions and demonstrate sizable reductions in crime (Vogler 2020; He and Barkowski 2020). These studies estimate cost savings from reduced crime due to Medicaid expansions of between $4 billion and $10.5 billion a year. Along the dimension of violent criminals' recidivism, Aslim et al. (2019) find that the ACA Medicaid expansions reduced the likelihood that a multiple reoffender returned to prison within 1 year of release by 30 percent.[14]

[12] Chatterji, Liu, and Yörük (2022) hypothesize that young adults may be more likely to live at home after enrolling in their parents' health insurance because of the geographic boundaries of an insurance provider's network. A young adult living far from his or her parents may not have access to the provider network, and therefore being on parental health insurance would provide little if any benefit in terms of access to health care and lower OOP spending.

[13] As noted above, cohabitation with parents could also have an income effect (through lower living expenses) that might reduce the propensity for economically motivated crime.

[14] Also relevant is Arenberg, Neller, and Stripling (2020), which demonstrates a link between childhood Medicaid eligibility and a decrease in adult incarceration, and Jácome (2022), which documents increases in crime after loss of Medicaid.

3. Data

Our primary source of data is the Federal Bureau of Investigation's NIBRS, which compiles incident reports from law enforcement agencies. Each incident report provides information about the nature of the crime and the demographics of up to three arrestees. We aggregate the 2008–13 NIBRS data into law-enforcement-agency–month–age counts (hereafter, agency-month-age counts) of criminal incidents leading to an arrest for part I property crimes (larceny, motor vehicle theft, burglary, or arson), part I violent crimes (aggravated assault, robbery, rape, or murder), and all crimes (property and violent crime) for those ages 22–25 and 27–29.[15] This differentiation by an individual's age at arrest is important for our first empirical strategy, which relies on eligibility for the DCM (persons become ineligible at age 26) as a key source of identifying variation.[16]

We supplement our main analysis with data from the 2008–13 UCR, which has far wider geographic coverage than the NIBRS. While 37 states and the District of Columbia participated in the NIBRS (as of 2014), representing coverage of roughly 93 million US residents (Federal Bureau of Investigation 2015b), the UCR data cover roughly 98 percent of the US population in all 50 states and the District of Columbia (Federal Bureau of Investigation 2015a) and, when weighted, are representative of the US population. This additional geographic coverage allows us to employ a second identification strategy, which relies on cross-county variation in pre-DCM uninsured rates of young adults to capture a heterogeneous sample of the national policy shock across jurisdictions; to test for spillover effects of the DCM on older individuals, who could be affected by changes in household composition; and to examine the effects of the DCM on 19–21-year-olds, who (as discussed below) have very different criminal propensities relative to older individuals. The key drawback of the UCR is that data on the exact ages of arrestees older than 24 are not available (the data are recorded in 5-year age bins, that is, 25–29, 30–34, and so on); therefore, we rely on data for individuals who are 19–21, 22–24, and 25 and older. Note that the final age bin is partially treated, as 25-year-olds may be affected by the DCM. We also investigate potential mechanisms using the 2008–13 Survey of Income and Program Participation (SIPP), the Medical Expenditure Panel Survey (MEPS), the National Health Interview Survey (NHIS), and the Treatment Episode Data Set (TEDS). These data sources are described in more detail in Section 6.3.

Table 1 presents population-weighted means of incident counts for the main analysis samples of 22–25-year-olds (treatment group) and 27–29-year-olds

[15] We end our sample with 2013 to avoid contaminating our data with the ACA Medicaid expansions and rollout of the health insurance marketplaces.

[16] For each agency, we also calculate an estimated age-specific population by single year of age. We generate an estimate of the age-specific agency population by multiplying the agency population from the NIBRS by the share of the population that each age represents in the county in which the reporting agency is located. These county-specific shares are calculated using data from the National Cancer Institute's Surveillance, Epidemiology, and End Results Program.

Table 1
Summary Statistics

	Ages 22–25			Ages 27–29		
	Mean	SD	*N*	Mean	SD	*N*
Full sample:						
Property crime	1.924	3.569	950,488	1.530	2.840	712,866
Violent crime	.831	1.911	733,748	.647	1.464	550,311
Preenactment:						
Property crime	1.808	3.456	330,052	1.405	2.693	247,539
Violent crime	.831	1.851	259,828	.664	1.474	194,871
Enactment:						
Property crime	1.933	3.510	78,108	1.519	2.823	58,581
Violent crime	.907	1.987	59,976	.724	1.635	44,982
Implementation:						
Property crime	1.992	3.642	542,328	1.609	2.928	406,746
Violent crime	.820	1.935	413,944	.626	1.431	310,458

Note. Results are weighted means of counts of criminal incidents leading to an arrest from the National Incident-Based Reporting System for 2008–13. The unit of observation is an agency-age-month. The dependent coverage mandate became law March 23, 2010.

(control group).[17] The preenactment period is January 2008–March 2010, the enactment period is April–September 2010, and the postimplementation period is October 2010–December 2013. Arrests for property crimes are roughly two to three times as common as arrests for violent crimes.

Trends in mean counts of property and violent crimes are plotted in Figure 1.[18] In both panels the arrest rate largely follows a common trend before the DCM for the two age groups. There is a more noticeable trend break at the time of the DCM for those ages 22–25, particularly for property crime.

4. Methods

4.1. Primary Estimation Strategy

We begin by pooling 2008–13 data from the NIBRS on agency-age counts of criminal incidents that involve arrestees ages 22–25 and 27–29. Because these are count data, we estimate the following Poisson regression (for a description, see

[17] Unweighted means are in Online Appendix Table OA3.

[18] In Figure 1, average monthly crime arrest rates use data from the NIBRS. Arrest rate trends span January 2008–March 2010 (pre-DCM) and April 2010–December 2013 (post-DCM); the ACA was signed into law on March 23, 2010. The dashed lines represent the predicted values from simple linear regressions of the arrest rate on a linear-time-trend variable. The vertical lines differentiate the periods: pre-DCM (event time < 0), enactment (event time $= 0$), and postimplementation (event time > 0).

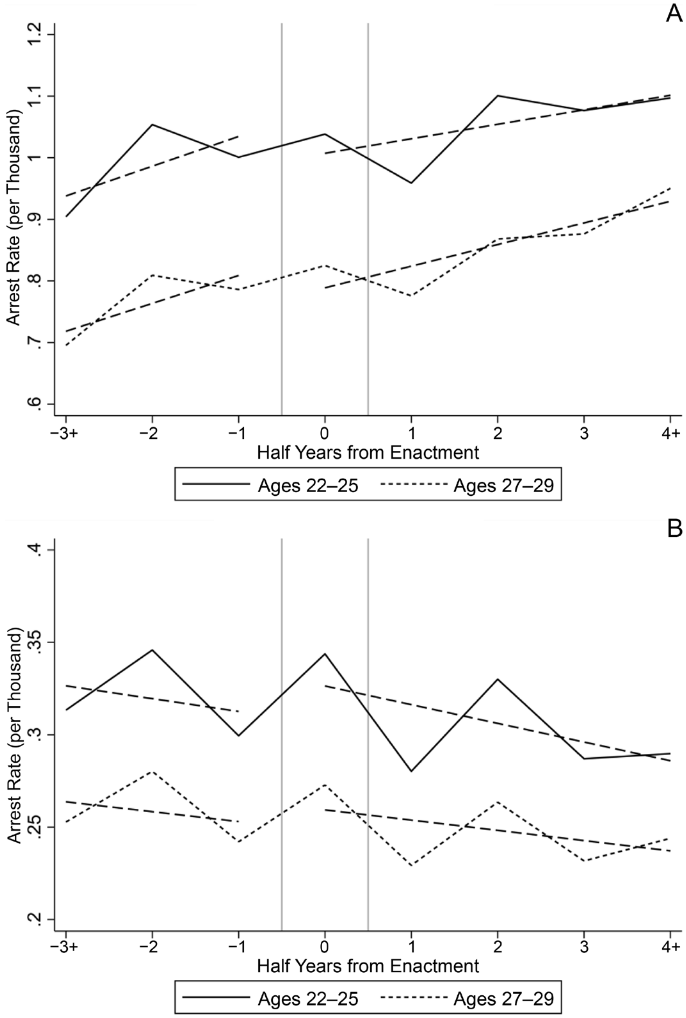

Figure 1. Trends in arrest rates, 2008–13. *A,* Property crime; *B,* violent crime

Rees et al. 2019; Cameron and Trivedi 1986; Grootendorst 2002) via a maximum-likelihood method:

$$\begin{aligned} Y_{iast} = E_{iast}\exp[\beta_0 &+ \beta_1(\text{Treat}_i \times \text{Enact}_t) + \beta_2(\text{Treat}_i \times \text{Implement}_t) \\ &+ \beta_3\text{State DCM}_{ist} + \text{Unemployment}_{iast}\beta_4 + \theta_i + \tau_t \\ &+ \delta_a \times \text{Year}_t + \varepsilon_{iast}], \end{aligned} \tag{1}$$

where Y_{iast} denotes the number of criminal incidents involving an arrestee of age i reported by law enforcement agency a in state s during month-year t. The exposure parameter in the Poisson model is E_{iast}, for which we use the estimated age-specific population served by the reporting agency.[19] The binary variable Treat_i equals one for agency-age cells for 22–25-year-olds and zero for 27–29-year-olds.

While the DCM affected young adults ages 19–25, we focus on a narrower treatment group—those ages 22–25—for several important reasons. First, previous research documents that criminal activity increases until 18 and then declines with age (Lee and McCrary 2017); moreover, the propensity to commit violent crimes is higher among minors and younger adults than among older adults (Perkins 1997).[20] Thus, comparing those ages 19–21 with those ages 27–29 could be problematic given substantial differences in underlying propensities to commit crimes. In contrast, comparing those who are closer in age (those 22–25, who are directly affected by the DCM, and those 27–29, who are not) ensures that the treatment and control groups have similar age-specific propensities. Second, the minimum legal drinking age of 21 is associated with increases in crime among young adults (Carpenter and Dobkin 2015; Callaghan et al. 2016). To disentangle the effects of the DCM from changes in arrests caused by the drinking age, we ensure that individuals in our treatment and control groups are over age 21. Finally, we focus on a narrower treatment group so that we have similar-sized (in terms of ages) treatment and control groups.

In equation (1) Enact_t is a binary variable that equals one during the DCM enactment period (April–September 2010) and zero otherwise, and Implement_t is a binary variable that equals one during the implementation period (October 2010–December 2013) and zero otherwise. We omit 26-year-olds because of the ambiguity in their treatment status, as variation in birthdays relative to insurance plan start dates creates unobserved variation in who is covered as a dependent during their 26th year (Antwi, Moriya, and Simon 2013).[21] We split the post-DCM period into enactment and implementation windows because of the possibility of anticipatory changes in insurance enrollment, an analytical strategy consistent with Antwi, Moriya, and Simon (2013). The variable State DCM_{ist}

[19] Despite the advantages of the Poisson model, one limitation is the implicit assumption that the mean of the error term is equal to the variance of the outcome variable. To ensure that the estimated treatment effects are not sensitive to this assumption, we also estimate a (less saturated) negative binomial regression (that includes state-time fixed effects to permit convergence of the likelihood function). This estimation strategy produces estimated DCM effects that are qualitatively similar to those from Poisson regressions.

[20] Another potential comparison would be to use 19–20-year-olds as the treatment group and 16–17-year-olds as the control group. These groups face very different home and drinking environments, and comparisons of the groups fail the common-pretreatment-trends assumption (results available from the authors on request).

[21] Including 26-year-olds as part of the control group leads to results that are similar to those found when omitting them.

is a control for age-specific pre-ACA state dependent coverage mandates,[22] and $\text{Unemployment}_{iast}$ is a vector of covariates for macroeconomic controls: the county-month unemployment rate, its interaction with the treatment group, and the unemployment rate by state-year–age-bin (22–25 and 27–29; hereafter state-year-bin). Finally, θ_i is an arrestee age effect, τ_t is a month-year effect, and $\delta_a \times \text{Year}_t$ is a vector of agency-year fixed effects. An important advantage of the Poisson regression model is that the inclusion of fixed effects does not lead to an incidental-parameters problem (Card and Dahl 2011).[23] We also experiment with controls with state-specific age effects, which allow crime propensity levels to differ by age across states. Regressions are weighted by the estimated age-specific agency population,[24] and standard errors corrected for clustering are at the state level.

In alternate specifications, we use birth year rather than age cohort to define treatment (that is, 27-year-olds in 2013 would have been covered by the DCM when they were younger and could, theoretically, experience lagged arrest effects from prior coverage). We also explore the relationship between years of exposure to the DCM and crime.

4.2. Tests of Identification Assumptions

Estimates of our primary coefficients of interest, β_1 and β_2, identify the reduced-form impact of the DCM on crime so long as the common-pretreatment-trends assumption is satisfied and no other shock differentially impacted the treatment and control groups at the same time.[25] We utilize a number of approaches to add credibility to a causal interpretation of our estimates. First, as noted above, the specification includes a fully interacted set of agency-year fixed effects. This helps to ensure that no state or local policy shocks or economic conditions common to 22–25-year-olds and 27–29-year-olds contaminate the estimates. Moreover, as noted above, we include interactions of the local unemployment rate with the treatment group, age-group-specific unemployment rates, and state–treatment-group fixed effects. These additional controls net out any differential trends across ages that are due to age-specific economic conditions.

Second, while common trends in the posttreatment period are untestable, we

[22] Between 1995 and 2010, 35 states implemented a DCM. The ages covered and coverage requirements varied by state. Most states required that young adults be unmarried, eight states required full-time student status, and four states required that young adults not have their own dependents. See Dillender (2014) for a description of the laws by state.

[23] As discussed below, we also experiment with agency-month-year fixed effects, which, while more computationally intensive, more flexibly control for agency-specific time trends.

[24] Our primary estimates are weighted to address potentially heteroskedastic errors (because of larger variance in reported crimes from smaller law enforcement agencies) and generate estimates that are representative of the average person living in the United States in the NIBRS sample (see Solon, Haider, and Wooldridge 2015). However, estimates are not sensitive to the choice of weighting versus not weighting.

[25] It is important to note that the estimates of β_1 and β_2 represent the net effect of all channels at play and do not identify the mechanisms individually.

do explore whether trends in crime for treatment and control groups evolved similarly in the pretreatment (pre-DCM) period by estimating an event-study equation of the following form:

$$Y_{iast} = E_{iast}\exp\left[\alpha_0 + \sum_{j=-3,\, j\neq-1}^{4} \gamma_j(\text{Treat}_i \times D_t^j) + \eta\text{State DCM}_{ist} \right. \\ \left. + \text{Unemployment}_{iast}\rho + \theta_i + \tau_t + \delta_a \times \text{Year}_t + \varepsilon_{iast}\right], \quad (2)$$

where D_t^j is a set of binary variables for each 6-month period, indexed relative to the date of passage of the ACA (and the DCM) into law (the omitted period is the 6-month period immediately preceding the enactment of the DCM). Each γ_j coefficient tests the impact in the given 6-month period (relative to the omitted period) for the treatment group relative to the control group. Estimates that are statistically indistinguishable from 0 in the pre-DCM period are indicative of a lack of differential trend between treatment and control groups.[26]

Third, given that the DCM was implemented in the wake of the great recession and an escalating national opioid epidemic, we take a number of steps in additional specifications to ensure that the findings are not contaminated by those events. With regard to the great recession, we control for interactions of the treatment group with measures of exposure to the great recession (state housing price indices and unemployment). This is done to directly control for the localized impact of the recession. We also show the robustness of the results to omitting the recession period. With regard to the opioid epidemic, we control directly for state-level measures of opioid exposure for age groups unaffected by the treatment (those 30 and older) interacted with the treatment group. Alternatively, we omit the states with the largest exposure to the opioid epidemic, measured using the Centers for Disease Control and Prevention's Multiple Cause of Death data files.[27] Note that this approach does not preclude the possibility that the ACA could have affected crime through the channel of opioid abuse among those affected by the treatment.

4.3. *Exploiting Geographic Heterogeneity*

One concern with the identification strategy based on age is that estimates could be contaminated if age-specific crime rates were on different trends following the DCM for reasons unrelated to the change in policy. Thus, we next draw data from the UCR and explore an alternative identification strategy with

[26] This exercise is of particular importance because of findings in Slusky (2017) that many DCM analyses fail tests of the parallel-trends assumption for certain age groups.

[27] We rank the states in our NIBRS analysis sample by their rates of opioid-related mortality for individuals 30 and older and then produce estimates from a sample that excludes the top five states in opioid mortality.

county-level data on pre-DCM uninsured rates among young adults (ages 18–24) from the 2009 American Community Survey summary files. We estimate

$$\begin{aligned} Y_{iast} = E_{iast}\exp[\beta_0 &+ \beta_1(\text{Uninsured Pre-DCM}_{a,2009} \times \text{Enact}_t) \\ &+ \beta_2(\text{Uninsured Pre-DCM}_{a,2009} \times \text{Implement}_t) \\ &+ \boldsymbol{Z}_{iast}\beta_3 + \theta_i + \tau_t + \delta_a + \varepsilon_{iast}], \end{aligned} \tag{3}$$

where $\boldsymbol{Z}_{iast}$ is a vector of controls including pre-DCM state mandates, the county unemployment rate, the state-year-bin unemployment rate, and a control for agency-month fixed effects to control for heterogeneous reporting of arrests during the year.

The key treatment measures are the interaction of the posttreatment period (Enact_t and Implement_t) and Uninsured $\text{Pre-DCM}_{a,2009}$, the county-level uninsured rate for 18–24-year-olds in 2009 associated with agency *a*. We expect that the DCM will have a larger effect in counties with higher rates of uninsured young adults before the DCM, consistent with the literature on Medicaid expansion (Courtemanche et al. 2017; Clemens, McNichols, and Sabia 2020). The main difference with this estimation approach is that identification no longer leverages variation in treatment based on age groups but instead uses variation based on geography.[28]

An additional advantage of this approach is that we are able to test for spillover effects of the DCM on other age groups. This is done by varying the age group for which the outcome variable Y_{ast} is measured. If larger rates of uninsured young adults before the DCM predict changes in arrests for individuals not directly targeted by the DCM, this would be evidence in support of spillover effects.

5. Results

Our main findings are presented in Table 2. In all models, standard errors are clustered at the state level (Bertrand et al. 2004).

5.1. The Dependent Care Mandate and Crime

Table 2 presents difference-in-differences estimates from equation (1) of the effect of the DCM on criminal incidents leading to an arrest for property and violent crimes. When controlling for age, month-year, and agency fixed effects, we find that the DCM is associated with a statistically insignificant 1.9 percent [exp(−.019) − 1] decline in property crime arrests involving 22–25-year-olds during the enactment period and a statistically significant 5.0 percent decline in property crime in the postimplementation period (column 1). Estimates are similar with a control for the monthly county unemployment rate interacted with treatment status (5.1 percent implementation effect, column 2), state-bin unemployment rates (5.8 percent implementation effect, column 3), and treatment

[28] Equation (3) also includes interactions of Uninsured $\text{Pre-DCM}_{a,2009}$ with State DCM_{ist} and Unemployment_{at}.

Table 2

Poisson Estimates

	(1)	(2)	(3)	(4)	(5)
Property crime:					
$\text{Treat}_i \times \text{Enact}_t$	−.019	−.008	−.018	−.026	−.037+
	(.016)	(.030)	(.023)	(.019)	(.021)
$\text{Treat}_i \times \text{Implement}_t$	−.051*	−.052*	−.060*	−.047*	−.046*
	(.025)	(.025)	(.026)	(.021)	(.023)
N	1,663,354	1,663,354	1,663,354	1,663,354	1,157,688
Violent crime:					
$\text{Treat}_i \times \text{Enact}_t$	−.033	−.036	−.035	−.038	−.037
	(.042)	(.044)	(.043)	(.043)	(.042)
$\text{Treat}_i \times \text{Implement}_t$	.019	.018	.019	.019	.023
	(.017)	(.017)	(.017)	(.015)	(.015)
N	1,284,059	1,284,059	1,284,059	1,284,059	959,196
County Unemployment Rate × Treat	No	Yes	Yes	Yes	Yes
Age-specific state unemployment rate	No	No	Yes	Yes	Yes
State-treatment fixed effects	No	No	No	Yes	Yes
Balanced panel	No	No	No	No	Yes

Note. The dependent variable is the count of criminal incidents leading to an arrest in an agency-age-month. Models are weighted using the estimated age-specific agency population. All models include age, month-year, and agency-year fixed effects and a control for state dependent coverage mandates. The interaction of county-month unemployment rate and treatment group status includes the main effect. Age-specific state employment rate is a control for the state-year-bin (22–25 and 27–29) unemployment rate. Standard errors are clustered at the state level.

+ Statistically significant at the 10% level.

* Statistically significant at the 5% level.

status with state fixed effects (4.6 percent implementation effect, column 4) and when the sample is restricted to a balanced panel (4.5 percent implementation effect, column 5). All implementation effects for property crimes are statistically significant at the 5 percent level.[29] These findings are consistent with the hypothesis that expansions in private health insurance are effective at reducing young adults' property crime. They suggest approximately 65,000 fewer property crimes as a result of the DCM. Compared with the gain in coverage for nearly 2.03 million young adults, this corresponds to an implied crime elasticity with respect to dependent coverage of $-.06$, an estimate that is about a quarter of the size of previously estimated elasticities of crime with respect to public health insurance (Vogler 2020).[30,31]

In contrast to the findings for property crime, none of the estimates for violent offenses are statistically significant at conventional levels, and the estimates for implementation are much smaller in magnitude (between one-half and one-third the size) than the estimates for property crime. This result could suggest that the DCM generates more economic incentives for crime.[32]

5.2. Event-Study Analyses

To ensure that the effects in Table 2 are not being driven by differential pretreatment trends in crime, we turn to the event-study analysis and equation (2). Figure 2 provides support for the hypothesis that the common-trends assumption is satisfied.[33] Pretreatment differentials in crime between the treatment and control groups for both property (Figure 2*A*) and violent (Figure 2*B*) crime suggest no evidence of different pretreatment trends. Following the enactment and implementation of the DCM, there is clear evidence of a modest decline in property crime but little evidence of a decline in violent crime. In terms of postimplementation effects, declines in property crime were somewhat larger in magnitude 2 or more years after implementation (4 or more half years) and followed a consistent pattern. The sizes of the effects were similar in each additional postimplementation period and became statistically significant beginning 1.5 years after

[29] Table OA4 includes agency-month-year fixed effects. While computationally more intensive, the results are quantitatively similar to those in column 3 of Table 2.

[30] Note that this implied elasticity is an upper bound given that there are many indirect channels (other than the direct effects of health insurance) through which the DCM may affect crime (see Sections 6.1–6.3).

[31] The magnitude of the property crime effect (5.8 percent; Table 2, column 3) and the dependent health insurance coverage effect (97.5 percent; Table 7, column 2) imply an elasticity of property crime with respect to dependent coverage of $-.058/.975$, or $-.06$.

[32] Table OA5 presents coefficient estimates on the unemployment rate. The empirical results suggest a positive but statistically insignificant relationship between the county unemployment rate and property crime. The effect on property crime is positive for both the treated (23–25) and control (27–29) age groups, but the sign on the interaction suggests that the positive effect of the county unemployment rate is less positive (though not significantly so) for the treatment group than the control group.

[33] Figure 2 presents event-study coefficients from the regression in equation (2) using data from the NIBRS. The dependent variable is the count of criminal incidents leading to an arrest in an agency-age-month. Errors bars are 95 percent confidence intervals. The vertical lines differentiate the periods: pre-DCM (event time < 0), enactment (event time $= 0$), and postimplementation (event time > 0)

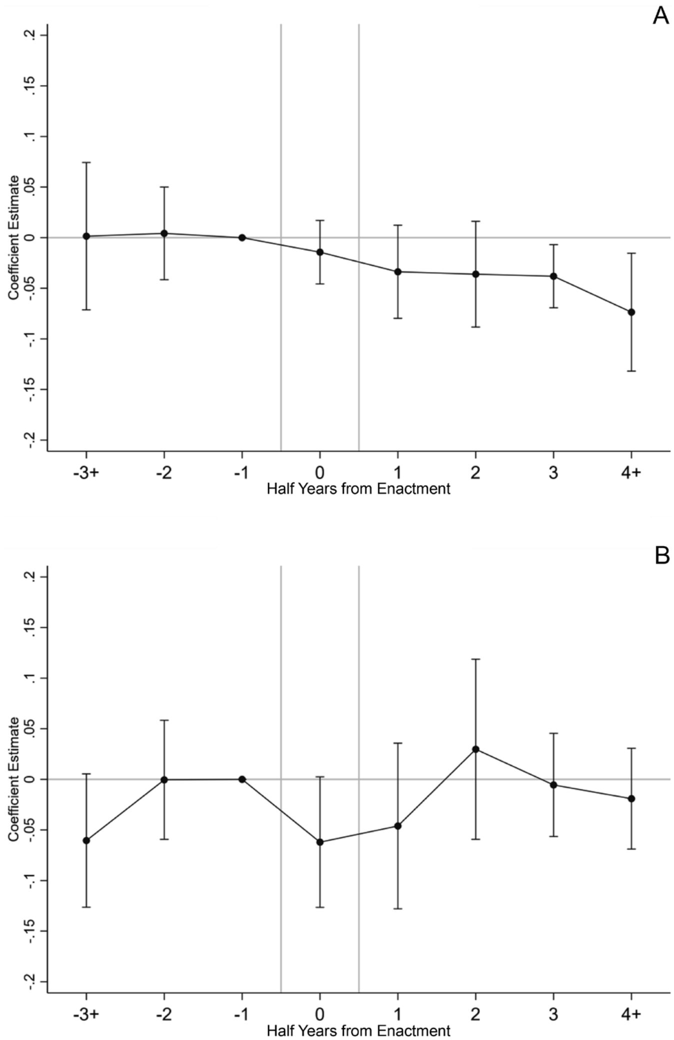

Figure 2. Event-study analysis of the mandate and arrests. *A,* Property crime; *B,* violent crime.

implementation. There is little evidence of declines in violent crime in the post-implementation period.

Figure 3 explores heterogeneity in the treatment effect by gender and race.[34]

[34] The dependent variable in Figure 3 is the count of criminal incidents leading to an arrest in an agency-age-month. Models are estimated via Poisson regression and are weighted using the estimated age-race-gender population. All models include age, month-year, and agency-year fixed effects and controls for states' dependent coverage mandates, county unemployment rates (main effect and interaction with treatment group), and state-year-bin unemployment rates. Standard errors are clustered at the state level. Errors bars are 95 percent confidence intervals.

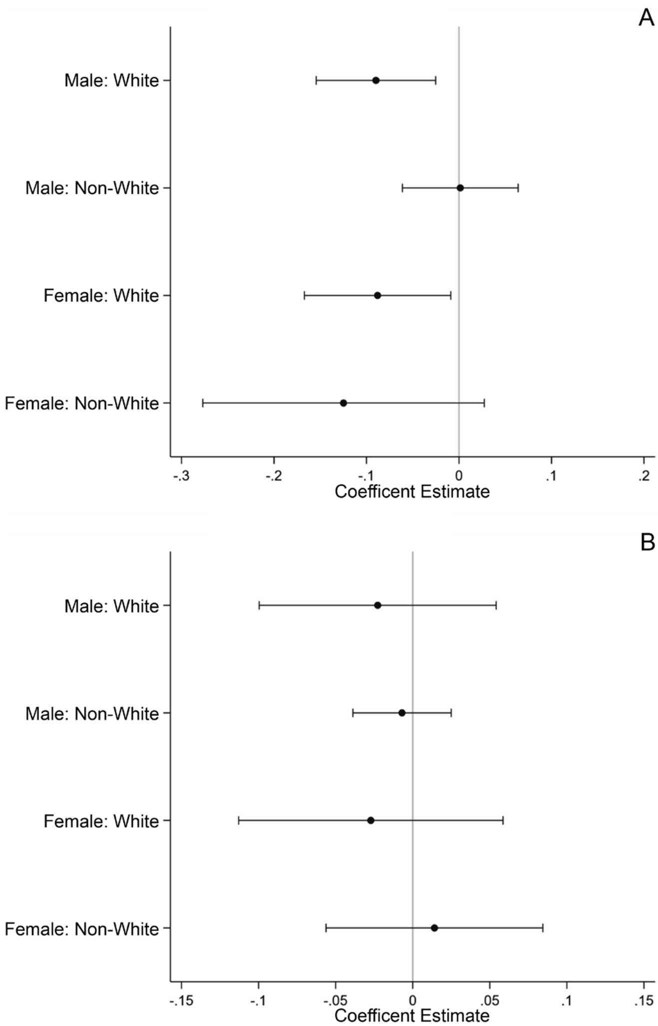

Figure 3. Estimated effects of the mandate's implementation on crime by race and gender. *A,* Property crime; *B,* violent crime.

For property crime, there are larger effects among White males than among non-White males, for whom there is no significant effect. Declines in property crime are of a similar magnitude among White females and non-White females, but only the former is statistically significant at conventional levels. These results are consistent with previous findings that the DCM increased insurance coverage for White young adults more than it did for Black or Hispanic young adults (Antwi, Moriya, and Simon 2013; O'Hara and Brault 2013; Breslau et al. 2018a).[35] Con-

[35] Other studies find a more equal distribution of insurance gains across ethnic groups due to the DCM (Sommers et al. 2013; Kotagal et al. 2014; Shane and Ayyagari 2014).

sistent with the overall results, there are no significant implementation effects for violent crime arrests among any subgroup.

Figure 4 shows the results of event-study analyses of the effect of the DCM on individuals' part I offenses for property crimes and violent crimes.[36] The findings suggest that the declines in property crime are driven largely by larcenies, motor vehicle thefts, and burglaries. Postimplementation estimates for violent crimes are, in the main, not statistically significant. The pattern of results from the event-study analyses is consistent with a DCM-driven reduction in property crime arrests.[37]

5.3. *Sensitivity Tests*

Table 3 explores the robustness of the findings to changing sampling and modeling decisions. We reestimate the main specification without using weights, limit agencies to those who serve populations of at least 20,000, omit states that had already adopted a major health insurance reform or Medicaid expansion, and limit or expand the posttreatment period. The estimates are qualitatively similar to those reported above.[38]

To further guard against concerns that the results reflect differential responses to the great recession (beyond the extensive unemployment controls included in the main specification), we collect data on the state-quarter house price index from the Federal Housing Finance Agency and interact those values with the indicator for the treatment group. As shown in column 7, the main findings hold. In addition, the omission of the recession period from the sample (see Online Appendix Table OA8) produces a qualitatively similar pattern of results.

We also attempt to disentangle the effects of the DCM from the opioid epidemic, which may have differential spillover effects on the treatment and control groups. We use data on state-level opioid overdose deaths to measure the extent of what the Centers for Disease Control and Prevention called "the worst drug overdose epidemic" in US history (Ahmed 2013).[39] When we interact the state-

[36] Figure 4 presents event-study coefficients from the regression in equation (2) using data from the NIBRS. The dependent variable is the count of criminal incidents leading to an arrest in an agency-age-month. The estimated treatment effect (ATT) is for 22–25-year-olds during the implementation period. Errors bars are 95 percent confidence intervals. The vertical lines differentiate the periods: pre-DCM (event time < 0), enactment (event time $= 0$), and postimplementation (event time > 0). Statistical significance is indicated at the 10 percent (plus) and 1 percent (asterisks) levels.

[37] Table OA6 explores the robustness of our findings to the use of 30–32-year-olds and 33–35-year-olds as alternate control groups to minimize the possibility that the control group of 27–29-year-olds includes some who were treated when they were younger. The results show a qualitatively similar pattern to those obtained with the preferred control group.

[38] The results in Table OA7 show no evidence that the main findings change if the control group is 26-year-olds. Moreover, there is no evidence that the DCM affected arrests among 27–29-year-olds relative to 26-year-olds.

[39] From the Centers for Disease Control and Prevention's Multiple Cause of Death data set, we gather data on opioid-related overdose deaths over the 2008–13 period. Overdose deaths involving opioids are defined as drug overdose deaths with the following International Classification of Disease codes: T40.0 (opium), T40.1 (heroin), T40.2 (other opioids), T40.3 (methadone), T40.4 (other synthetic narcotics), and T40.6 (other or unspecified narcotics).

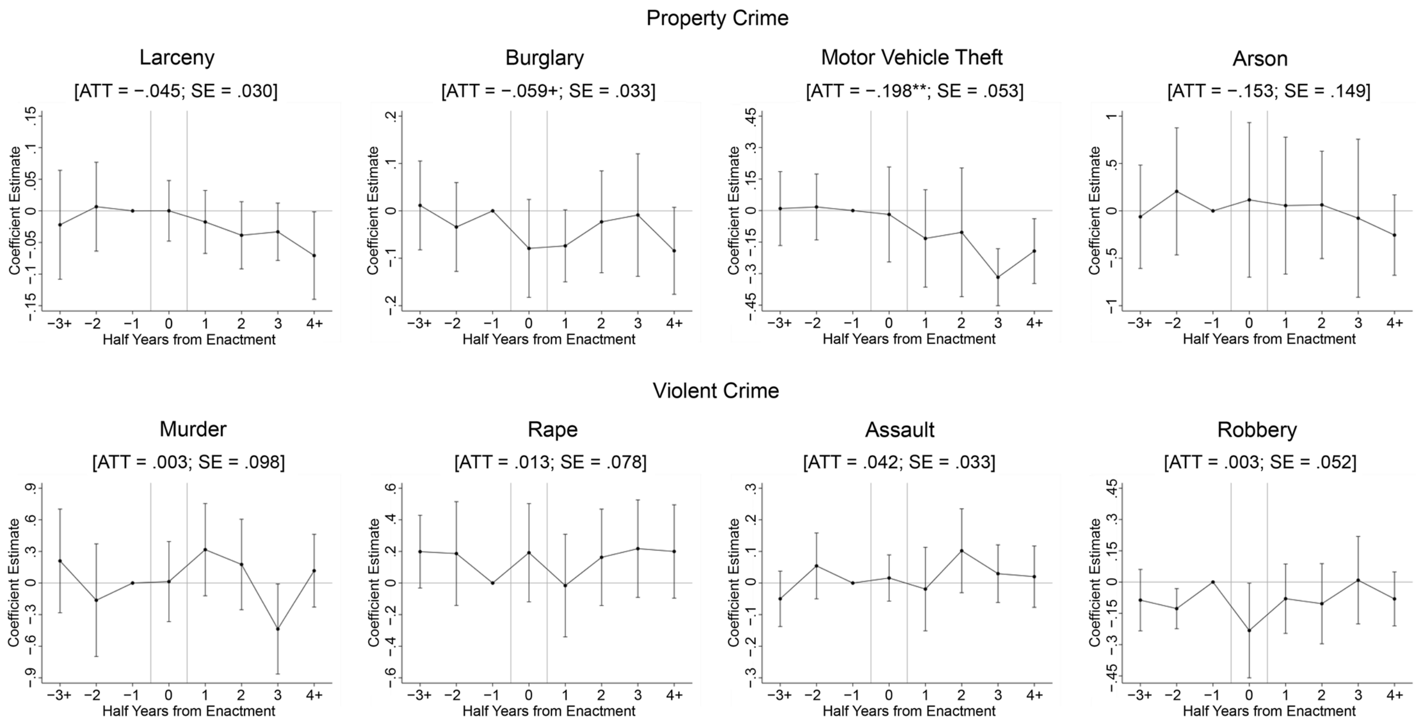

Figure 4. Event-study analysis of the mandate and arrests by crime. *A*, Property crime; *B*, violent crime

Table 3

Robustness Checks

	Main (1)	Unweighted (2)	Agency Population ≥ 20,000 (3)	Exclude States (4)	2008–12 (5)	2008–15 (6)	HPI Control (7)	Opioid Mortality Control (8)	Exclude High Opioid Mortality States (9)
Property crime:									
$Treat_i \times Enact_t$	−.026	−.031*	−.027	−.029	−.026	−.036+	−.033	−.024	−.024
	(.019)	(.015)	(.020)	(.020)	(.019)	(.019)	(.022)	(.018)	(.020)
$Treat_i \times Implement_t$	−.047*	−.092**	−.046*	−.044*	−.038+	−.065**	−.053**	−.040+	−.044*
	(.021)	(.015)	(.021)	(.022)	(.019)	(.019)	(.021)	(.024)	(.022)
N	1,663,354	1,663,354	488,159	1,467,186	1,363,922	2,259,656	1,663,354	1,656,977	1,441,741
Violent crime:									
$Treat_i \times Enact_t$	−.038	.011	−.039	−.045	−.045	−.043	−.038	−.036	−.039
	(.043)	(.020)	(.043)	(.047)	(.043)	(.043)	(.038)	(.043)	(.044)
$Treat_i \times Implement_t$	.019	−.031*	.021	.019	.013	.015	.020	.026	.021
	(.015)	(.013)	(.015)	(.015)	(.012)	(.017)	(.016)	(.017)	(.015)
N	1,284,059	1,284,059	458,339	1,122,233	1,054,669	1,751,736	1,284,059	1,278,921	1,136,989

Note. The dependent variable is the count of criminal incidents leading to an arrest in an agency-age-month. Models are estimated via Poisson regression and are weighted using the estimated age-specific agency population. All models include age, month-year, agency-year, and state-treatment fixed effects and controls for state dependent coverage mandates, county unemployment rates (main effect and interaction with treatment group), and state-year-bin unemployment rates. Standard errors are clustered at the state level. In column 4, states that expanded Medicaid early (Connecticut and Washington) are omitted, as is Massachusetts because of its early health care reform. Column 7 includes a control for the percentage change in the state-quarter housing price index (HPI) interacted with the treatment group (main effect included). Column 8 includes a control for the state-year opioid-related overdose mortality rate for those 30 and older interacted with the treatment group. Column 9 excludes the five states with the highest opioid-related overdose mortality rate for those 30 and older.

+ Statistically significant at the 10% level.

* Statistically significant at the 5% level.

** Statistically significant at the 1% level.

year opioid-involved mortality rate for individuals who are 30 and older with an indicator for the treatment group (column 8) or drop the five states with the highest mortality rates among those 30 and older (column 9), the findings are largely unchanged from those of the preferred model in Table 2. These results are consistent with recent evidence by Coupet et al. (2020) showing that the DCM had no impact on young adults' emergency room visits for prescription opioid overdoses or opioid-related mortality.

6. Extensions

6.1. Treatment by Birth Year

Table 4 reports results with treatments based on birth cohort and include birth-year fixed effects as controls. In this model, an individual would be treated (exposed to the DCM) if he or she was born in May 1984 or later.[40] One advantage of this approach is that it accounts for the possibility that crime among those who were previously treated but aged out of the 27–29-year-old control group (that is, those who were 27 years old in 2013) were affected with a lag. In the presence of such a lagged crime effect, our prior estimates would be biased toward 0. On the other hand, unless currently treated and prior treated individuals are permitted their own treatment effects, the overall treatment effect gives equal weight to previously and concurrently treated individuals.

Column 1 of Table 4 shows that ever being exposed to the DCM is associated with a 9.6 percent reduction in property crime, and there is a statistically insignificant 4.5 percent reduction in violent crime in column 4. This result is consistent with the hypothesis that the age-specific definition of treatment exposure results in a downward bias (in absolute magnitude) in the crime-reducing effects of the DCM.[41]

The remaining columns of Table 4 explore whether the effects of the DCM on crime differ by length and timing of exposure to the DCM. It is possible that longer eligibility under the DCM could have a different impact on crime. For example, longer coverage by health insurance may make finishing educational investments more likely (Dillender 2014; Colman and Dave 2018; Heim, Lurie, and Simon 2018), which would make criminal activity less likely (Lochner 2004; Lochner and Moretti 2004; Anderson 2014). To the extent that any investments in noncriminal income relative to criminal income persist, individuals who are exposed to the DCM but age out could potentially have lasting reductions in the propensity to commit crimes.

[40] Since our data do not include exact dates of birth, we impute birth year by assuming that the date of arrest is the arrestee's birthday. We calculate the arrestee's year of birth by subtracting age in years from the year of arrest and assume that the arrestee's month of birth is the same as the month of arrest (for example, the birth cohort for 22-year-olds arrested in April 2010 is April 1988). A person born in May 1984 would be 25 years and 11 months old in April 2010 (the start of the DCM enactment period).

[41] The results are similar if the time period of the analysis is extended through 2019 (available from the authors on request).

Table 4

Sensitivity of Estimates to Use of Birth Cohort

	Property Crime			Violent Crime		
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{Exposed}_b \times \text{Enact}_t$	−.036	−.048		−.044	−.048	
	(.034)	(.032)		(.040)	(.041)	
$\text{Currently Exposed}_{ib} \times \text{Enact}_t$			−.044			−.096+
			(.027)			(.050)
$\text{Previously Exposed}_{ib} \times \text{Enact}_t$			−.053			.025
			(.070)			(.055)
$\text{Exposed}_b \times \text{Implement}_t$	−.101**			−.046		
	(.021)			(.030)		
$\text{Exposed}_b \times \text{Implement}_t \times$ 0–1 Year		−.100**			−.039	
		(.023)			(.034)	
$\text{Exposed}_b \times \text{Implement}_t \times$ 1–2 Years		−.155**			−.080*	
		(.020)			(.033)	
$\text{Exposed}_b \times \text{Implement}_t \times$ 2 Years and Older		−.233**			−.085+	
		(.024)			(.047)	
$\text{Currently Exposed}_{ib} \times \text{Implement}_t \times$ 0–1 Year			−.122**			−.041
			(.022)			(.037)
$\text{Currently Exposed}_{ib} \times \text{Implement}_t \times$ 1–2 Years			−.173**			−.065
			(.026)			(.051)
$\text{Currently Exposed}_{ib} \times \text{Implement}_t \times$ 2 Years and Older			−.232**			−.072
			(.026)			(.047)
$\text{Previously Exposed}_{ib} \times \text{Implement}_t \times$ 0–1 Year			−.070+			−.018
			(.038)			(.041)
$\text{Previously Exposed}_{ib} \times \text{Implement}_t \times$ 1–2 Years			−.119**			−.075**
			(.040)			(.025)
$\text{Previously Exposed}_{ib} \times \text{Implement}_t \times$ 2 Years and Older			−.194*			−.110
			(.077)			(.070)

Note. The dependent variable is the count of criminal incidents leading to an arrest in an agency-age-month. Models are estimated via Poisson regression and are weighted using the estimated age-specific agency population. All models include birth cohort, month-year, and agency-year fixed effects and controls for state dependent coverage mandates, county unemployment rates (main effect and interaction with the exposed group), and state-year-bin unemployment rates. Standard errors are clustered at the state level. $N = 1{,}663{,}354$ for property crimes; $N = 1{,}284{,}059$ for violent crimes.

+ Statistically significant at the 10% level.

* Statistically significant at the 5% level.

** Statistically significant at the 1% level.

To explore these possibilities, we interact the treatment exposure variable with binary variables for the length of time that a birth cohort has been covered by the DCM (0–1 years, 1–2 years, or 2 years or more) at a particular point in time in the posttreatment period.[42] For both types of crime, the size of the reduction in criminal arrests increases with the length of coverage by the DCM.

We also separate treatment exposure in the posttreatment period into current (22–25-year-olds) and previous (27–29-year-olds from birth cohorts that were previously covered by the DCM) exposure variables, which are then interacted with the length of time that the birth cohort has been covered by the DCM. Being currently covered by the DCM after having been covered by it for longer periods has larger effects on crime reduction. However, there is some evidence that previous exposure to the DCM has a (muted) impact, decreasing criminal activity even after individuals age out of eligibility for coverage, especially when coverage occurred for a longer duration.

Finally, in Table OA9, we reestimate the specifications in Table 4 but include age-specific linear time trends, month-year fixed effects, and birth cohort fixed effects as controls (with all of the other controls mentioned above). While the identifying variation is much more limited in this specification, there is evidence that the DCM is significantly negatively related to property crime, with larger estimated marginal effects for those currently exposed than for those previously exposed.

6.2. Geographic Variation

Next we explore whether declines in crime are greater in counties with larger anticipated impacts from the DCM. To do this, we leverage cross-county variation in (pretreatment) 2009 uninsured rates among young adults ages 18–24 using data from the American Community Survey summary files.[43] We first interact this continuous measure in the context of the NIBRS-based difference-in-differences model. The results in Table 5 show that counties with larger pre-DCM uninsured rates indeed have larger reductions in property crimes among 22–25-year-olds relative to 27–29-year-olds, although the effect is less precisely estimated. The estimate implies that a 10-percentage-point higher pre-DCM county-level uninsured rate for young adults is associated with a 4.2 percent [exp $(-.429 \times .1) - 1$] decline in property crime arrests, slightly smaller than the estimates from the preferred model in Table 2.

One concern with this identification strategy is that despite being close in age,

[42] Birth cohorts with 1–12 months of exposure are included in the 0–1-year bin, those with 13–24 months of exposure are in the 1–2-years bin, and those with 25 months or more of exposure are in the 2 years or more bin. As exposure length is time varying, it is not collinear with the birth cohort fixed effects.

[43] These data are the most granular, publicly available source of insurance information about an age group (18–24-year-olds) very similar to our treatment group (22–25-year-olds). However, these data do not contain all counties in the United States. In results available from the authors on request, we find that using our preferred specification (equation [1]) for this restricted sample produces quantitatively similar estimates to those found in Table 2.

Table 5

Poisson Difference-in-Difference-in-Differences Estimates

	Property Crime	Violent Crime
$\text{Treat}_i \times \text{Enact}_t$	.000	−.002
	(.065)	(.140)
$\text{Treat}_i \times \text{Implement}_t$	.064	.048
	(.066)	(.046)
$\text{Treat}_i \times \text{Enact}_t \times$ Uninsured Pre-DCM	−.044	−.118
	(.225)	(.493)
$\text{Treat}_i \times \text{Implement}_t \times$ Uninsured Pre-DCM	−.429+	−.096
	(.233)	(.129)
Uninsured Pre-DCM mean	.255	.256
N	824,845	661,437

Note. The dependent variable is the count of criminal incidents leading to an arrest in an agency-age-month. Models are weighted using the estimated age-specific agency population. All models include age, month-year, and agency-year fixed effects and controls for state dependent coverage mandates, county unemployment rates (main effect and interaction with treatment group), and state-year-bin unemployment rates. The remaining two-way interactions are omitted for brevity. Standard errors are clustered at the state level. The 2009 county-level uninsured rate is for those ages 18–24 from the American Community Survey summary files.

+ Statistically significant at the 10% level.

22–25-year-olds and 27–29-year-olds have insufficiently similar propensities for criminal behavior for the latter to be a credible control group for the former. Moreover, if the DCM affects living arrangements and household resources, there could be spillover effects of the DCM on older individuals who might live in the household with directly impacted individuals. To explore the sensitivity of our findings for young adults to our identification strategy, test for spillover effects on older individuals, and allow for greater geographic external validity, we turn to the UCR data and the alternative identification strategy based on equation (3).

Table 6 reports these results. First, there is no evidence that pre-DCM county-level uninsured rates (which capture a jurisdiction-level sample) are associated with significant changes in arrest rates among all individuals. This suggests that if there are spillover effects on older (or younger) individuals, they are likely small. Evidence of statistically significant effects of the DCM on property crime arrests is only among 22–24-year-olds. This finding, based on a very different identification strategy than our main approach, nonetheless confirms the NIBRS-based results and adds substantial credibility to our finding about the policy.[44] The estimate implies that a 10-percentage-point higher pre-DCM county-level uninsured rate for young adults is associated with a 3.6 percent [exp $(-.369 \times .1) - 1$] decline

[44] Figure OA1 shows the results of an event-study analysis that decomposes the treatment effect over time (using the full distribution of county unemployment rates, following Schmidheiny and Siegloch [2019]). This exercise generally supports the property-crime-reducing effect for those ages 22–24. For violent offenses, there is stronger evidence of a pretreatment trend, which provides little evidence of a violent-crime-reducing effect of the DCM.

Table 6
Testing for Spillover Effects

	All Ages	Ages 19–21	Ages 22–24	Ages 25 and Older
Property crime:				
Enact$_t$ × Uninsured Pre-DCM	.065	.068	−.137	−.002
	(.202)	(.144)	(.262)	(.224)
Implement$_t$ × Uninsured Pre-DCM	−.190	−.015	−.369*	−.244
	(.200)	(.129)	(.160)	(.206)
Uninsured Pre-DCM mean	.290	.284	.291	.289
N	8,794,386	921,292	870,435	3,368,530
Violent crime:				
Enact$_t$ × Uninsured Pre-DCM	−.140	−.167	−.211	−.437+
	(.194)	(.300)	(.361)	(.255)
Implement$_t$ × Uninsured Pre-DCM	−.172	−.317+	−.620+	−.432
	(.241)	(.191)	(.324)	(.275)
Uninsured Pre-DCM mean	.291	.289	.296	.290
N	7,673,894	622,591	596,141	2,861,327

Note. The dependent variable is the count of criminal arrests in an agency-bin-month. Models are estimated via Poisson regression and are weighted using the estimated age-specific agency population. All models include age, month-year, and agency-month fixed effects and controls for state dependent coverage mandates, county unemployment rates, and state-year-bin unemployment rates (and interactions with the 2009 county-level uninsured rate for young adults). The uninsured rate for 18–24-year-olds in 2009 is from American Community Survey summary files. Arrest counts by year are available for ages 15–24 in the Uniform Crime Reports; thereafter, arrest counts are available only in 5-year age bins (25–29, 30–34, and so on, to 60–64 and then 65 and older). Hence, the 22–25-year-old treatment group cannot be isolated. Standard errors are clustered at the state level.

+ Statistically significant at the 10% level.

* Statistically significant at the 5% level.

in property crime arrests among 22–24-year-olds, similar to the triple-differences finding in Table 5. There is little evidence that the DCM affected property crime arrests among those 25 and older, which suggests that spillover effects are likely small. There is also no evidence of property crime effects for 19–21-year-olds, though the policy conclusion for this group is mixed (see column 4 of Online Appendix Table OA6).

While the results on violent crime arrests suggest some evidence of a DCM-induced decline for those ages 22–24, the effect for those 25 and older is of comparable magnitude (which is at least somewhat unlikely). The event-study analyses suggest that the result for property crime arrests is much more likely to be causal in nature (see Online Appendix Figure OA1). Thus, we conservatively conclude that the DCM is likely most effective at curbing young adults' property crime.

6.3. Plausible Mechanisms

Table 7 presents estimates of the effects on several mechanisms through which the DCM may have plausibly reduced arrests. We first present estimates of the effect of the DCM on the probability of a 22–25-year-old having any source of health insurance coverage and having dependent coverage.[45] Consistent with previous work (Sommers and Kronick 2012; Cantor et al. 2012b; Antwi, Moriya, and Simon 2013; Sommers et al. 2013; O'Hara and Brault 2013; Kotagal et al. 2014; Chua and Sommers 2014; Shane and Ayyagari 2014; Barbaresco, Courtemanche, and Qi 2015; Jhamb, Dave, and Colman 2015; Scott et al. 2015a, 2015b; Wallace and Sommers 2015), we find that the DCM increased insurance coverage by 1.3 percentage points (2 percent) during the enactment period and 5.6 percentage points (8.6 percent) during the postimplementation period (column 1). The DCM-induced increases in dependent coverage are, as expected, larger in both the enactment and postimplementation periods, at 17.2 percent and 97.5 percent, respectively.[46] These findings can be viewed as a necessary first stage for the DCM to impact crime, but we caution against the simple interpretation that increases in coverage directly affect criminal activity. As shown in the following analyses, many indirect channels that likely stem from this base change in insurance are active, which makes the net effect of the DCM on crime a conglomeration of multiple intermediate impacts.

Table 7 also examines the impact of the DCM on OOP medical expenditures using MEPS data.[47] Because MEPS data are not available at the subyear level, we omit 2010 and focus on comparisons in the pre-2010 and post-2010 periods.[48] The DCM is associated with a reduction in average OOP health-care costs of about a $118 per year, driven by a 60 percent reduction in the probability of OOP expenditures greater than $2,000. Preventing large negative income shocks may be a mechanism through which the DCM reduces property crime (Cortés, Santamaría, and Vargas 2016; Bignon, Caroli, and Galbiati 2017; Watson, Guettabi, and Reimer 2020).

Columns 5 and 6 use NHIS monthly data to study the impact of the DCM on

[45] The variable Health Insurance equals one if the respondent has employer-provided health insurance (in his or her own name or under someone else's plan), individually purchased coverage (in his or her own name and as a dependent), insurance through the Department of Veterans Affairs or the Department of Defense, Medicaid, Medicare, or other private coverage and zero otherwise. Following Antwi, Moriya, and Simon (2013), Dependent Health Insurance equals one if the respondent has employer-provided health insurance through someone other than a spouse and zero otherwise. Further following Antwi, Moriya, and Simon (2013), we restrict the sample to the fourth reference month in the Survey of Income and Program Participation (SIPP) to reduce recall bias. However, using all months of the SIPP produces a similar pattern of results.

[46] We also explore pretreatment trends and postpolicy effects for health insurance coverage using the event-study framework of equation (2). Results for any coverage and dependent coverage are shown in Figure OA2. Reassuringly, there are no significant pretreatment trends for any coverage or dependent coverage. Following enactment, there is a positive and significant increase in having any kind of coverage. Dependent coverage follows a similar postenactment pattern.

[47] The variable OOP in the Medical Expenditure Panel Survey (MEPS) is calculated as the sum of direct payments made by the person or the person's family for the individual's health care during the year. The indicator OOP $>$ $2,000 equals one if the respondent had OOP health care spending greater than $2,000 (in 2009 dollars) during the calendar year and zero otherwise.

[48] The MEPS employs an overlapping panel design, with each panel interviewed five times over a roughly 2-year period, which yields annual data for 2 calendar years.

Table 7

Exploration of Mechanisms through which the Mandate Reduced Crime

	Health Insurance (1)	Dependent Health Insurance (2)	OOP (3)	OOP >$2,000 (4)	Visit Health Professional (5)	Cannot Afford Prescription (6)	K6 Screening (7)	Live with Parents (8)	SNAP Benefits (9)	Full-Time Student (10)	Employed (11)	Employed Full Time (12)	Substance Use Treatment (13)
$Treat_i \times Enact_t$	.013	.021**	N.A.	N.A.	.008	−.001	.009	.033**	−.011	.034**	.014	−.001	N.A.
	(.010)	(.005)			(.012)	(.022)	(.011)	(.008)	(.007)	(.009)	(.017)	(.013)	
$Treat_i \times Implement_t$	.056**	.119**	−118.016**	−.029**	.020**	−.014	−.004	.065**	−.013+	.033**	−.014	−.028**	−.748*
	(.012)	(.007)	(45.020)	(.009)	(.007)	(.012)	(.006)	(.010)	(.008)	(.008)	(.012)	(.010)	(.332)
Pretreatment mean	.648	.122	405.314	.048	.105	.119	.027	.393	.115	.214	.713	.477	8.016
N	109,248	109,248	15,868	15,868	52,274	21,598	21,373	109,248	109,248	109,248	109,248	109,248	494
Data source	SIPP	SIPP	MEPS	MEPS	NHIS	NHIS	NHIS	SIPP	SIPP	SIPP	SIPP	SIPP	TEDS

Note. Models are estimated via ordinary least squares using data for 2008–13; all models include controls for gender, race or ethnicity, and marital status. Data from the Survey of Income and Program Participation (SIPP) are weighted by SIPP person weights, cluster standard errors at the state level and are restricted to the fourth reference month of each wave (following Antwi, Moriya, and Simon 2013). The SIPP models also control for state, age, and month-year fixed effects; state-specific linear time trends; state-year-bin unemployment rate; share 25 and older with a college degree (state-year); and (excluding column 10) student status. Models using the Medical Expenditure Panel Survey (MEPS) omit 2010 and include age and census-region–year fixed effects. Enactment windows for the MEPS data cannot be separately identified. Models using the National Health Interview Survey (NHIS) data include age, month-year, and census-region–year fixed effects. Standard errors for the MEPS and the NHIS are calculated using Taylor-linearized variance estimation, which accounts for the complex survey design. The treatment group for models using the SIPP, MEPS, and NHIS data is individuals ages 22–25; the control group is those ages 27–29. The MEPS and NHIS models use survey weights. The model using the Treatment Episode Data Set (TEDS) is weighted by state-year-bin population, clusters standard errors at the state level, and omits 2010 because the enactment window cannot be isolated. The treatment group is individuals ages 21–24; the control group is those ages 30–34. Additional controls include state, age bin, and year fixed effects; state-specific linear time trends; and state-level demographics for age distribution, high school diploma or greater, rural population, and state-year-bin unemployment rate. N.A. = not applicable.

+ Statistically significant at the 10% level.

* Statistically significant at the 5% level.

** Statistically significant at the 1% level.

access to medical care and prescription drugs.[49] The results show that implementation of the DCM increased the probability that a 22–25-year-old visited a health professional in the prior 2 weeks by 2.0 percentage points (19 percent). The effect was 2.5 times greater in the postimplementation period than in the enactment period, consistent with health insurance effects observed in the SIPP. While there is some evidence that the DCM was negatively related to the probability that the respondent needed but could not afford prescription medication, this effect is not statistically distinguishable from 0. Moreover, there is little evidence that the DCM had a significant impact on psychological well-being, as measured by the Kessler 6 scale (column 7).[50]

Next we provide evidence that the DCM changed household living arrangements. Using data from the SIPP, we find strong evidence that the DCM was associated with an increase of 3.3–6.5 percentage points (8.4–16.5 percent) in the likelihood that a 22–25-year-old lived with his or her parents.[51] This finding is consistent with Chatterji, Liu, and Yörük (2022). Moreover according to SIPP data, implementation of the DCM was associated with a 1.3-percentage-point (11.3 percent) reduction in the likelihood that a 22–25-year-old received benefits from the Supplemental Nutrition Assistance Program, consistent with increased access to family resources via changes in living arrangements.[52] This pattern of results suggests that change in household composition that led to increased parental monitoring and greater financial security may be important channels through which the DCM reduced crime.

Table 7 shows that the DCM is associated with a 3.3-percentage-point (15.4 percent) increase in the likelihood that a 22–25-year-old is a full-time student.[53] Incapacitation and human-capital-related channels could also be important in explaining DCM-induced reductions in crime (Lochner and Moretti 2004; Anderson 2014).[54]

However, in contrast to the results for education, the employment effects of the DCM may increase the likelihood of crime. Table 7 shows that the implemen-

[49] The binary variable Visit Health Professional indicates whether the respondent had an office visit to a health professional in the previous 2 weeks. The binary variable Cannot Afford Prescription indicates whether the respondent needed but could not afford prescription medicines in the previous 12 months.

[50] The Kessler 6 scale is composed of six questions that assess mental health over the previous 30 days. Respondents are asked to answer on a Likert scale from 0 to 4 how often they felt certain ways (sad, nervous, restless or fidgety, hopeless, everything an effort, and worthless), with 0 meaning none of the time and 4 meaning all of the time. The scores to each question are summed, and the variable K6 Screening equals one if this sum is greater than 12 and zero otherwise.

[51] The variable Live with Parents is created using the household roster. It equals one if the respondent lived with at least one parent in the current month and zero otherwise.

[52] The variable SNAP Benefits equals one if the respondent received benefits from the Supplemental Nutrition Assistance Program in the current month and zero otherwise.

[53] The variable Full-Time Student is an indicator that equals one if the respondent was enrolled full-time in school during any of the months of the 4-month sample wave.

[54] Table OA10 provides evidence of larger DCM-induced increases in the probabilities of having dependent health insurance coverage, living with parents, and full-time student status for Whites compared with non-Whites, which is consistent with the larger impacts for property crime arrests for these groups in Figure 2.

tation of the DCM is associated with a statistically insignificant 1.4-percentage-point (2 percent) decline in any employment and a statistically significant 2.8-percentage-point (5.9 percent) decline in full-time employment.[55] These findings are generally consistent with studies using data from the SIPP and the Current Population Survey (Antwi, Moriya, and Simon 2013; Hahn and Yang 2016) but contrast with a recent analysis using tax records (Heim, Lurie, and Simon 2018). Finally, we use TEDS data to estimate the relationship between the DCM and admissions for SUD treatment.[56,57] Implementation of the DCM is associated with a 9.3 percent decline in admissions for SUD treatment (.748 fewer per 1,000 people). This finding, consistent with Saloner et al. (2018), could reflect declines in substance abuse among affected young adults or perhaps substitution toward treatment in nonadmission settings. Together, the findings in Table 7 point to a number of credible channels through which the DCM may have reduced crime among young adults.

6.4. *Comparison of Estimated Effects with Other Literature*

This study provides the first estimates of the impact of private health insurance expansions on crime. How large are the estimated effects and how might they be compared with the existing literature on public health insurance? Our estimates show that the federal DCM increased dependent coverage among young adults by about 11.9 percentage points, which translates to an additional 2.03 million 22–25-year-olds having dependent coverage. The range of our estimated implementation effects on crime (including from the geography-based specification in Section 6.2) translate to about 47,000–65,000 crimes averted by the DCM. Note that these figures likely overstate the number of criminal offenders deterred by the DCM because many arrestees are recidivists.[58]

These estimates can be used to compute an elasticity of crime with respect to dependent coverage, which allows comparisons with existing literature on the effects on crime of the ACA's Medicaid expansion. We caution, however, that many channels beyond direct effects of dependent health insurance coverage can explain our findings. Therefore, both because of indirect channels and our inability to adjust for recidivism in the NIBRS data, our estimated elasticities should

[55] The indicator variable Employed equals one if the young adult had a job for at least 1 week during the reference month.

[56] In the Treatment Episode Data Set (TEDS), each observation is a record of an admission for SUD treatment. We collapse the data to the state-year-bin level to compile state-year counts of admissions for SUD treatment by age group. State-year-bin population data are then merged in to create a SUD admissions rate variable. An important drawback of TEDS is that admissions are available only by age bins (for example, 18–20, 21–24, 25–29, 30–34, and so on). We define 21–24-year-olds as the treatment group and 30–34-year-olds as the control group, although we recognize the limitations of such coding.

[57] Admissions data are collected at the annual level in TEDS. With the DCM being implemented in 2010, we omit that year from the sample.

[58] For example, Durose, Cooper, and Snyder (2014) find that approximately 76 percent of arrestees ages 18–24 are rearrested within 3 years, and 84 percent are rearrested within 5 years.

be considered upper-bound estimates of the effect of dependent health insurance coverage on crime.

With these caveats in mind, we find that our estimates imply an elasticity of property crime with respect to dependent health insurance coverage of −.06. By comparison, using the estimated effect of Medicaid expansion on violent crime of −5.3 percent in Vogler (2020) and the estimated effect of Medicaid expansion on Medicaid enrollment of 24 percent in Kaestner et al. (2017) yields an elasticity of crime with respect to public health insurance coverage of about −.22. Our results suggest that expansions of private dependent insurance coverage among young adults produces smaller reductions in crime than Medicaid expansions to near-poverty households. Our estimated elasticity is also smaller in magnitude than the elasticity of (cost-adjusted) crime to expansions of the police force of −.21 to −.47 (Chalfin and McCrary 2017; Evans and Owens 2007).

7. Conclusion

An important efficiency rationale for enacting the Affordable Care Act's DCM was to increase health insurance coverage among a healthy, often uninsured population to ameliorate social welfare losses due to adverse selection. However, there may have been other important gains in efficiency if increased health insurance coverage among young adults generated positive externalities. This study is the first to explore whether the DCM generated spillover effects on crime. Given the important effects of the DCM on living arrangements, financial resources, and access to health care services—and that the DCM targets an age demographic responsible for a disproportionate share of criminal arrests—the externality effects from crime may be important for a full cost-benefit analysis of the DCM.

Using data from the 2008–13 NIBRS and a difference-in-differences approach, we find that the DCM is associated with a 2–5 percent decline in property crime among 22–25-year-olds. These estimates imply an elasticity that is a quarter to an eighth of the magnitude of the benefits in crime reduction from increasing the size of the police force (Chalfin and McCrary 2017; Evans and Owens 2007), a policy that explicitly targeted crime. These findings suggest that health insurance has the potential to decrease crime (at least for certain populations) as an added benefit to its primary goal, though effects in our context are smaller than for major anticrime interventions.[59]

Our findings suggest that the DCM led to reductions in young adults' arrests for property crimes, but we found less evidence of an effect on violent crime. The nature of the policy may explain these results, as the motivations for perpetrating property and violent crimes likely differ. To the extent that the policy affects more permanent situations (that is, living modality, school enrollment, economic well-being), including financial health, a larger effect of the DCM on property crime might be expected, given that violent crime may be more affected by situ-

[59] Another example of a health policy with this characteristic is the Supplemental Nutrition Assistance Program (Carr and Packham 2019).

ational circumstances such as stress or weather (Chalfin, Danagoulian, and Deza 2019).

Our main analysis estimates the reduction in criminal activity due to the DCM, allowing for straightforward comparisons with the DCM's effects on potential mechanisms. Nonetheless, our main results do not account for substantial heterogeneity in the social costs of different types of crime, which may be desirable in assessing policy relevance. However, the treatment effects obtained from the event-study analyses shown in Figure 3 do allow us to estimate offense-specific social benefits from crime reduction. To account for these differences, we use estimates of the social costs of crime from McCollister, French, and Fang (2010) for burglary, larceny, and motor vehicle theft. Back-of-the-envelope calculations suggest that this reduction corresponds to an annual reduction in social costs of approximately \$371–\$512 million per year (in 2020 dollars).[60] These estimates suggest a more modest reduction in crime from the DCM than from ACA Medicaid expansions. Vogler (2020) estimates that Medicaid expansions were associated with a 5.3 percent reduction in violent crime, saving expanding states roughly \$4 billion per year. Vogler and Barkowski (2020) find larger savings in the cost of crime from ACA-based Medicaid expansions of about \$10.5 billion per year.

While the total social benefits from crime reductions from the DCM are smaller in magnitude, it is important to recognize that the DCM and the ACA's Medicaid expansions targeted different populations and resulted in enrollment effects of vastly different magnitudes. Courtemanche et al. (2017) estimate that the ACA's Medicaid expansions increased insurance coverage by 3.1 percentage points for the nonelderly adult population, an increase of roughly 5.8 million people (.031 $\times$ 185.7 million US residents ages 19–64 in 2013). The DCM was much smaller in scope, reducing the number of uninsured young adults by 938,000 (Antwi, Moriya, and Simon 2013). Thus, using our lower-bound estimates, we calculate that the DCM saved approximately \$396 in the costs of crime per newly insured person per year. The ACA's Medicaid expansions yielded a crime-related cost savings between \$690 and \$1,810 per newly insured person per year, which

[60] We use 2009 data for age-specific arrests from Federal Bureau of Investigation (2020, table 38) and for aggregate crime from Federal Bureau of Investigation (2020, table 1) to estimate total crimes for those 22–25 (the aggregate crime counts for prior years are often updated, which is why we use the aggregate crime entry for 2009 from the latest report). While Federal Bureau of Investigation (2020, table 38) provides data on arrests for 22–24-year-olds and 25–29-year-olds, we use 2009 NIBRS data to estimate the share of arrests among 25–29-year-olds that involved 25-year-olds to calculate the share of arrests for 22–25-year-olds. Taking the aggregate totals in 2009 for larceny, burglary, and motor vehicle theft and then multiplying by the respective shares of arrests for 22–25-year-olds yields 739,739 larcenies, 279,179 burglaries, and 103,880 motor vehicle thefts committed by 22-year-olds. The estimated per-crime costs (in 2020 dollars) are calculated using estimates from McCollister, French, and Fang (2010): \$4,309 for larceny, \$7,884 for burglary, and \$13,142 for motor vehicle theft. We then use the marginal effects obtained in the implementation period, coupled with per-crime costs for each part I crime (McCollister, French, and Fang 2010), to generate estimates of the social benefits of reductions in crime. The \$512 million figure is based on \$140 million in cost savings from larceny, \$126 million from burglary, and \$245 million from motor vehicle theft. Excluding larcenies (significant at the 15 percent level) would yield cost savings of \$371 million.

means that the crime-reduction benefits of the DCM and the ACA were much closer in magnitude.

Moreover, the cost of implementation was not the same for the DCM and Medicaid expansions. The ACA Medicaid expansion operated through an increase in government spending, estimated by Wolfe, Rennie, and Truffer (2017) to be $6,365 per enrollee (in 2015). In contrast, Depew and Bailey (2015) estimate the average additional cost of adding one dependent to a family health insurance plan under the DCM to be $211.41, which gives the DCM an estimated benefit-cost ratio (solely with regard to crime reduction) of approximately 1.87 (396/211.41), compared with .28 (1,810/6,365) using the more generous estimates from Medicaid expansions (He and Barkowski 2020). We conclude that by targeting a younger and healthier population using a policy with relatively lower marginal premiums, the DCM generated important social benefits in crime reduction at a relatively modest cost.

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