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ABSTRACT

The Dependent Coverage Mandate Took a Bite Out of Crime*

The Affordable Care Act's Dependent Coverage Mandate (DCM) induced approximately two million young adults to join parental employer-sponsored health insurance (ESI) plans. This study is the first to explore the impact of the DCM on criminal arrests, a potentially important externality. Using data from the National Incident-Based Reporting System, we find that the DCM induced an 11 percent reduction in criminal incidents involving arrestees ages 19 to 25, driven by property crime declines. An examination of the underlying mechanisms suggests that declines in large out-of-pocket expenditures for health care, increased educational attainment, and increases in parent-adult child cohabitation may explain these crime declines. Back-of-the-envelope calculations suggest that the DCM generated approximately $3.1 billion in annual social benefits from crime reduction.

JEL Classification: I13, I18, K14

Keywords: Affordable Care Act, Dependent Coverage Mandate, crime, arrests

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1. Introduction

The Affordable Care Act's Dependent Coverage Mandate (DCM) requires health insurers to allow young adult children 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, potentially reducing adverse selection in insurance markets. Approximately two million young adults added parental employer-sponsored health insurance (ESI) in response to the DCM, translating to approximately 938,000 fewer uninsured persons (Antwi et al. 2013). There is evidence that the DCM impacted young adults' out-of-pocket health care costs (Busch et al. 2014; Chua and Sommers 2014), access to mental health services (Kozloff and Sommers 2017; McClellan 2017), labor market outcomes (Antwi et al. 2013; Heim et al. 2018), and living arrangements (Chatterji et al. 2017). This study is the first to explore whether DCM-induced changes to young adults' socioeconomic well-being impacted their criminal behavior. Such spillovers could have important social welfare implications for evaluating the efficacy of the DCM.

Crime is disproportionately committed by young adults. According to Federal Bureau of Investigation (FBI) data, in 2018, approximately 50 percent of all arrests involved arrestees under the age of 30. 19-to-25 year-olds accounted for 23 percent of violent crime arrests and 22 percent of property crime arrests in the United States in 2018, generating social costs of $76.3 billion (in 2019 dollars) (McCollister et al. 2010).[1] In light of the high costs of crime attributable

[1] These figures are calculated using Table 38 (https://ucr.fbi.gov/crime-in-the-u.s/2018/crime-in-the-u.s.-2018/topic-pages/tables/table-38) and Table 1 (https://ucr.fbi.gov/crime-in-the-u.s/2018/crime-in-the-u.s.-2018/topic-pages/tables/table-1) of the 2018 FBI Crime in the United States Report. While Table 38 provides data on arrests for 19-to-24 year-olds and 25-to-29 year-olds, we calculate arrests for 19-to-25 year-olds by using 2016 National Incident-Based Reporting System (NIBRS) data (the most recent available version) to estimate the share of arrests among 25-to-29 year-olds that involved 25-year-olds. The estimated per-crime costs (in 2019 dollars) are calculated using calculations in McCollister et al. (2010), $237,931 for Part I violent crimes and $5,843 for Part I property crimes.

to young adults, even a small impact of DCM on youth criminal behavior has potentially large social welfare implications.

The effect of the DCM on crime is theoretically ambiguous, largely due to competing effects of expanded health insurance coverage. There are a number of channels through which the DCM may have diminished criminal activity. For example, health insurance might reduce crime by allowing greater access to substance use disorder (SUD) treatments (Wen et al. 2017; Bondurant et al. 2018) or psychiatric drugs used to treat mental illness (Marcotte and Markowitz 2011). Expanded coverage options could also reduce out-of-pocket health expenditures (Busch et al. 2014; Chua and Sommers 2014) or ease “job lock” and improve employer-employee matches (Antwi et al. 2013; Dahlen 2015), each of which could increase income and reduce incentives for crime (Draca and Machin 2015). Increased access to health insurance could also lead to additional educational investments (Dillender 2014; Cohodes et al. 2016), which could reduce crime via short-run incapacitation effects (Anderson 2014) and longer-run human capital effects (Lochner and Moretti 2004; Lochner 2004). Finally, if the DCM increased the number of young adults living with their parents (Chatterji et al. 2017), young adult crime may be reduced through enhanced parental monitoring.

On the other hand, the DCM could potentially lead to increases in crime. While the DCM may have increased job mobility through insurance that is not tied to employment, there is some evidence that the DCM reduced employment and wages due to increased reliance on parents for coverage (Antwi et al. 2013; Hahn and Yang 2016; Heim et al. 2018). This may lead to more crime for income-generating purposes. Further, if the DCM lowered out-of-pocket prescription drug costs, substance use and abuse may have increased, leading to more crime; this may have occurred by increasing violent behaviors tied to abuse or by increasing property crime used to fund substance abuse habits.[2] The DCM may have also caused ex-ante moral hazard, such as increasing problem drinking (Barbaresco et al. 2015), a risky behavior linked to increases in crime (Carpenter 2007; Carpenter and Dobkin 2015; Anderson et al. 2018). Given the theoretically ambiguous channels through which DCM-induced expansions in health insurance may influence criminal activity, the DCM's net impact on crime remains an empirical question.

Recent studies have found that Medicaid expansions are associated with a substantial reduction in crime (Wen et al. 2017; Vogler 2018; Aslim et al. 2019; He and Barkowski 2020). While this body of evidence makes an important contribution to our knowledge of the relationship between public health insurance and criminal behavior, understanding the impact of the DCM on crime is uniquely important for several reasons. First, expansions in health insurance for those ages 19-to-25 impacts an age demographic that accounts for a substantial share of criminal arrests in the United States (Department of Justice 2018). Second, the DCM impacted access to *private* health insurance plans, which may have a very different impact on crime relative to increased access to *public* health insurance via the ACA's Medicaid expansions. This may be due to differences in (i) the marginally affected complier[3], (ii) breadth of medical services provided, and (iii) out-of-pocket costs of covered services. For example, the DCM decreased the total out-of-pocket spending of young adults (Busch et al. 2014) and, in particular, decreased the percent of costs paid out-of-pocket for young adults with behavioral health conditions (Ali et al. 2016). In addition, unlike Medicaid expansions, the DCM has the added feature of financially linking adult children to their parents, which may have implications for household bargaining and parental monitoring (Chatterji et al. 2017), each of which could affect

[2] More crime may also result if substance abusers are disproportionately likely to be victimized (Goldstein 1985).

[3] For instance, the DCM targeted a demographic group (i.e., young adults with privately insured parents) that is usually higher income than the demographic group affected by the ACA's Medicaid expansions.

crime. Third, there are identification-related advantages to studying the DCM. Specifically, the age-based nature of the DCM permits within-city, cross-birth cohort crime comparisons, which better permit one to isolate the effects of DCM-induced private health insurance expansions from contemporaneous local economic and policy shocks. Finally, the marginal cost of insurance coverage for an uninsured adult differed substantially between the ACA's Medicaid expansions (Wolfe et al. 2017) and the DCM (Depew and Bailey 2015). This could lead to very different per beneficiary social gains or losses.

To our knowledge, this study is the first to examine the impact of the DCM on crime. Using a panel of law enforcement agency-months from the 2008-2013 National Incident-Based Reporting System (NIBRS), we find that implementation of the DCM is associated with a 10.8 percent decline in criminal incidents involving 19-to-25 relative to 27-to-29 year-old arrestees. Our results are largely driven by property crime: we find that property crime arrests fell by 13.0 percent whereas violent crime arrests fell by 4.0 percent. Event study analyses show that the arrest decline is not driven by pre-treatment trends and that the impact of the policy becomes larger in the longer run. Furthermore, we show that these effects are not driven by differential age-specific responses to the Great Recession, the opioid epidemic, or unmeasured state- or city-specific time shocks. Analyses of potential mechanisms suggest that a decline in large out-of-pocket expenditures for health care, increased educational attainment, increased cohabitation of young adults with parents and, perhaps, reduced need for substance abuse treatment may be important channels through which the DCM contributed to declines in criminal activity.

This paper proceeds as follows. Section 2 provides background on the literature describing potential mechanisms through which the DCM may affect criminal behavior and discusses past studies relating health insurance expansions to crime, Sections 3 and 4 describe our data sources and methods, Section 5 presents our main results, Sections 6 and 7 provide robustness tests and extensions of the main analysis, and Section 8 concludes.

2. Background

2.1. The Pre-DCM Landscape and the Impact of the DCM on Insurance Coverage

Prior to September 2010, approximately one-in-three young adults ages 19-to-25 was uninsured (Antwi et al. 2013). While many states had passed dependent coverage laws before the federal implementation of the DCM, these state laws tended to be weaker along several dimensions. In particular, most state dependent coverage mandates had additional eligibility criteria (other than age), such as requiring that young adults were financially dependent on their parents, unmarried, childless, uninsured, or enrolled in school (Cantor et al 2012a). Further, due to the Employee Retirement Income Security Act (ERISA), firms that self-insure are not required to follow state-level insurance mandates (Pierron and Fronstin 2008).[4] Studies generally find that state-level dependent coverage mandates resulted in modest increases in dependent coverage of about 1-2 percentage-points (Levine et al. 2011; Monheit et al. 2011; Depew 2015), with some results suggesting that these increases were largely offset by reductions in own-name coverage (Monheit et al. 2011).[5]

Many studies have documented a reduction in the uninsured rate and an increase in private coverage among young adults after implementation of the DCM (Sommers and Kronick 2012; Cantor et al. 2012b; Antwi et al. 2013; Sommers et al. 2013; O'Hara and Brault 2013;

[4] In 2009, self-insured firms represented 59 percent of all firms providing ESI (Henry J. Kaiser Family Foundation and Health Research Educational Trust 2010).

[5] See Trudeau and Conway (2018) for a review of this literature.

Kotagal et al 2014; Chua and Sommers 2014; Shane and Ayyagari 2014; Barbaresco et al. 2015; Jhamb et al. 2015; Scott et al 2015a,b; 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).[6]

2.2 The DCM and Crime: Labor Market and Financial Mechanisms

The theoretical connection between economic well-being and the propensity to commit crime is well established (Becker 1968). Theory suggests that legal sector alternatives are a major component of the opportunity cost of engaging in criminal behavior for the purposes of income generation. By this logic, any improvements in legal sector options such as higher income, a better employment match, or additional educational choices will increase the opportunity cost of criminal behavior and lower crime.[7]

The DCM may have affected the trade-off between engagement in the legal and illegal sectors in several ways. First, the DCM has been shown to have an impact on employment and income, though research on this topic has come to mixed conclusions. There is evidence that the DCM decreased employment and wages among young adults (Antwi et al. 2013; Heim et al. 2018), decreased hours and the likelihood of working full time (Antwi et al. 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, decreased need to work full time due to reduced "job lock," or worsening employment outcomes (Bailey and Chorniy 2016; Bailey 2017; Heim et al. 2018).

[6] See Breslau et al. (2018b) for a review of this literature, as well as the literature on the DCM and health care utilization and outcomes.
[7] See also Gould et al. (2002), Machin and Meghir (2004), Lin (2008), and Draca and Machin (2015).

Second, there is emerging evidence that increasing access to private health insurance increased educational attainment among dependents. Heim et al. (2018) show 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) find that the DCM is associated with a 15 to 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 dependent coverage mandates increased educational attainment among young men.

Finally, the DCM has been shown to decrease out-of-pocket spending on health care services (Busch et al. 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, if the DCM increases the likelihood that adult children live with their parents (Chatterji et al. 2017), changes in cohabitation may also contribute to improvements in financial status through reductions in the costs of living (e.g., rent and groceries).

Taken together, the impact 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 (Lochner 2004; Lochner and Moretti 2004; Anderson 2014; Draca and Machin 2015), negative employment effects could instead increase incentives for crime (Draca and Machin 2015).

2.3 Health Mechanisms

[8] Chen et al. (2015) find no effect of the DCM on out-of-pocket expenditures, but also exclude high-cost outliers from their analysis sample.

Health care access and use may affect criminal behavior by improving physical and mental health, though the magnitude and direction of these 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 have a usual source of care (Kotagal et al. 2014, Wallace and Sommers 2015), increased the number of physician visits per year (Jhamb et al. 2015), and improved self-reported measures of physical and mental health (Carlson et al. 2014; Chua and Sommers 2014; Barbaresco et al. 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 to both legal and illegal sector activities.

One form of health care that is 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 those abusing substances theoretically being both more likely to commit crimes and more likely to be victimized (Goldstein 1985; Dobkin and Nicosia 2009; Dave et al. 2018). Several studies have explored 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] Both Antwi et al. (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

[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 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 et al. (2018) use openings and closings of SUD treatment centers to show that these centers are associated with reductions in both violent and property crime.

newly covered by the DCM were more likely to use mental health services and SUD treatment, and Saloner and Lê Cook (2014) found that the DCM led to a 17 percent increase in utilization of mental health treatment (but no impact on SUD treatment use). Finally, Saloner et al. (2018) find reductions in inpatient SUD treatment due to the DCM. The authors point out that their result may not be reflective of a reduction of 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 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 et al. (2015) found that the DCM increased risky drinking among young adults, a behavior which has been tied to increased crime (Carpenter 2007; Carpenter and Dobkin 2015; Anderson and Rees 2015). In summary, these health-related mechanisms again 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 et al. (2017) find that the DCM is associated with a 6.0 percentage-point (17.5 percent) increase in young adults living with their parents due to the

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

DCM.[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

While this study is, to our knowledge, the first to examine the impact of the DCM on crime, it is not the first study to look at health insurance and crime. The literature to date has focused on the effect of Medicaid expansions on crime, concluding that such expansions led to reductions in crime. Wen et al. (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 crime reduction, using HIFA waiver timing as an instrument. More recent studies use variation from the ACA Medicaid expansions and demonstrate sizable reductions in crime (Vogler 2018; He and Barkowski 2020). These studies estimate cost savings from reduced crime due to the Medicaid expansions between \$10.5 billion and \$12.9 billion a year. Along the dimension of violent criminal recidivism, Aslim et al. (2019) find that the ACA Medicaid expansions reduced the likelihood that a multiple re-offender returned to prison within one year of release by 40 percent.

[12] Chatterji et al. (2017) hypothesize that young adults may be more likely to live at home after taking up their parents' health insurance because of the geographic boundaries of an insurance provider's health care network. A young adult living far away from their parents may not have access to the health care provider network, therefore being on parental health insurance provides little if any benefit in terms of access to health care and lower out-of-pocket spending.

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

While the effect of Medicaid expansions on crime is important and policy relevant,[14] the local average treatment effect identified from the DCM may be quite different for a number of reasons. First, the DCM targeted an age demographic with a high propensity for criminal activity. If health insurance reform (or any reform) is to be evaluated on its crime reduction capacity, then an understanding of the relevance of the policy on the most engaged population is of utmost importance. Second, though the DCM is unlikely to be rolled back given its popularity (Hamel, Firth, and Brodie 2014), studying the DCM provides evidence on the secondary effects of private insurance, which allows policymakers to better evaluate alternatives to public insurance provision, as well as the second order implications of changes in the composition of insurers (perhaps due to crowd out). Third, the spillover effects of the DCM on household structure and parent-adult child bargaining, as well as the differences in access to medical care provided by private as compared to public health insurance, make this subject ripe for investigation.

3. Data

Our primary source of data is the FBI's National Incident-Based Reporting System (NIBRS). The NIBRS is a compilation of incident reports from law enforcement agencies. Each incident report provides information on both the nature of the crime and the demographics of up to three arrestees.[15] We aggregate the 2008-2013 NIBRS into agency-by-month-by-age counts of

[14] Republican federal policymakers have attempted to roll back Medicaid expansions as part of the American Health Care Act (H.R. 1628, 2017) and the Better Care Reconciliation Act of 2017 (Senate amendments to H.R. 1628, 2017).

[15] The FBI also releases the Uniform Crime Reporting (UCR) data, which has greater geographic specificity and coverage but reports ages in 5-year age bins (after age 24), which is restrictive given our estimation strategy. We present estimates using the UCR in lieu of the NIBRS in the appendix.

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 total crimes (the sum of property and violent crimes) for those ages 19-to-25 and 27-to-29.[16] In order to be included in the analysis sample, an agency must report data in every month during 2008-2013 (i.e., strongly balanced), though we relax this assumption in a robustness check below. For each agency, we also calculate an estimated age-specific population by single year of age.[17]

Table 1 presents means of incident counts for our main analysis sample for 19to-25 year-olds (treatment group) and 27-to-29 year-olds (control group).[18] Mean counts are reported for the entire sample as well as separately for the pre-enactment (January 2008 to February 2010), enactment (March 2010 to August 2010), and post-implementation (September 2010 to December 2013) periods. Arrests for property crimes are roughly twice to three times as common as arrests for violent crimes.

Quarterly trends in mean counts of crimes for our treatment and control groups are plotted in Figures 1 through 3 for total crime, property crime, and violent crime, respectively. The raw data tell a consistent story in all three figures: the arrest rate largely follows a single trend both before and after the DCM for the 27-to-29-year-old age group. On the other hand, there are much more noticeable trend breaks at the time of the DCM for the 19-to-25 year old age group. The break is the smallest for violent crime.

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[16] We end our analysis sample with 2013 to avoid contaminating our data with ACA Medicaid expansions and insurance exchanges.

[17] 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 Surveillance, Epidemiology, and End Results Program.

[18] Population-weighted means can be found in Appendix Table 1.

4. Methods

Using agency-by-month-by-age criminal incident counts from the 2008 to 2013 NIBRS that involve arrestees ages 19-to-25 and 27-to-29, we estimate the following regression via a Poisson model:

$$Y_{iast} = E_{iast} exp[\beta_0 + \beta_1(Treat_i * Enact_t) + \beta_2(Treat_i * Implement_t) + \beta_3 StateDCM_{ist} + \theta_i + \tau_t + \delta_a * Year_t + \varepsilon_{iast}] \quad (1)$$

where Y_{iast} denotes the number of criminal incidents involving an arrestee of age *i* reported by agency a in state *s* during month-by-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. $Treat_i$ is a binary variable taking on the value of 1 if a criminal incident involves an arrest of an individual ages 19-to-25 and equal to zero if the arrest involves an individual ages 27-to-29; $Enact_t$ is a binary variable taking on the value of 1 during the DCM enactment period (April 2010 to August 2010) and zero otherwise, and $Implement_t$ is a binary variable which takes on the value of 1 during the implementation period (September 2010 to December 2013) and 0 otherwise.[19] We split our post-DCM period into distinct enactment and implementation periods because of the possibility of anticipatory changes in insurance enrollment, an analytical strategy which is consistent with prior work by Antwi et al. (2013). $StateDCM_{ist}$ is a control for age-

[19] To be precise, the enactment period is set equal to 0.290 in March 2010, equal to 1 in April through August 2010, and equal to 0.733 in September 2010. In all other periods, the enactment period is set equal to zero. The implementation period is set equal to 0.267 in September 2010, equal to 1 in October 2010 to the end of our sample, and set equal to zero otherwise.

specific pre-ACA state dependent coverage mandates[20], θ_i is an arrestee age effect, τ_t is a month-by-year effect, and $\delta_a * Year_t$ is a vector of agency-by-year fixed effects.[21] We use Poisson regression due to the count nature of our outcome variable.[22]

Estimates of our primary coefficients of interest, β_1 and β_2, will be unbiased if the common trends assumption is satisfied. We utilize a number of approaches to alleviate concerns that this may not the case. First, as noted above, our specification includes a fully-interacted set of law enforcement agency-by-year fixed effects. This helps to ensure that no state or local policy shocks or economic conditions common to 19-to-25 year-olds and 27-to-29 year-olds contaminate our estimates.

Second, we conduct an event study analysis, where we examine up to 8 or more quarters prior to enactment and 8 or more quarters following implementation:

$$Y_{iast} = E_{iast}exp\begin{bmatrix}\gamma_0 + \sum_{j=-8,j\neq-1}^{8}\gamma_j\left(Treat_i * D_t^j\right) + \eta StateDCM_{ist} + \theta_i + \tau_t \\ +\delta_a * Year_t + \varepsilon_{iast}\end{bmatrix} \quad (2)$$

where D_t^j is a set of binary variables for each quarter in our data, indexed relative to the date of passage of the ACA (and the DCM) into law (the omitted quarter is 2010q1).[23] The coefficients

[20] Between 1995 and 2010, 35 states implemented a dependent coverage mandate. The ages covered and coverage requirements varied by state. Most states required that young adults be unmarried, 8 states required full-time student status, and 4 states required that young adults did not have their own dependents. See Dillender (2014) for a description of the laws by state.

[21] As discussed below, we also experiment with an agency-by-month-by-year fixed effect, which, while more computationally intensive, will more flexibly control for agency-specific time trends.

[22] Negative binomial models produced a quantitatively similar pattern of results.

[23] To be precise, the omitted quarter is set equal to 0.733 in March 2010, equal to 1 in February and January 2010, and equal to = 0.267 in December 2010. In all other periods, the omitted quarter is set equal to 0.

γ_j each test the impact in the given quarter (relative to the omitted quarter) for the treatment group relative to the control group. Estimates that are statistically indistinguishable from zero in the pre-DCM period would be indicative of a lack of differential trend between treatment and control groups.[24]

Third, we test the assumption that the DCM impacted only the treatment and not the control group.[25] To accomplish this, we estimate the following Poisson model:

$$Y_{iast} = E_{iast} exp\left[\gamma_0 + \sum_{j=19, j\neq 26,28}^{29} \gamma_{1j}\left(Age_i^j * Enact_t\right) + \sum_{j=19, j\neq 26,28}^{29} \gamma_j\left(Age_i^j * Implement_t\right) + \eta StateDCM_{ist} + \tau_t + \delta_a * Year_t + \varepsilon_{iast}\right] \quad (3)$$

where a binary variable for each age in our sample Age_i^j is interacted with the binary variables for the enactment and implementation periods. We use age 28 as the omitted age.[26] This analysis allows us to observe directly which age groups are most responsive to the DCM relative to the omitted group. Changes in outcomes for ages making up the control group would constitute a violation of our identifying assumptions, whereas differing changes in outcomes for ages making up the treatment group would be relevant heterogeneity in the impact of the policy.

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[24] This exercise is of particular importance due to Slusky's (2017) findings that many DCM analyses fail tests of the parallel trends assumption for certain age group compositions.

[25]There are a few channels through which the DCM could, in theory, have spillover effects to the control group. One is through social interactions between 19-to-25 year-olds and 27-to-29 year-olds. A second is through some 27-to-29 year-olds having been previously "treated" by the DCM by the end of 2012. We expect these spillover effects to be relatively small and to bias our estimated effects toward zero. Estimating a model utilizing birth cohorts affected by the DCM as the treatment group, we found a qualitatively similar pattern of results.

[26] Versions of this analysis with other omitted ages, which produce a similar pattern of results, can be found in Appendix Figures 1 through 6.

Finally, 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 to ensure that our findings are not contaminated by these events. With regard to the Great Recession, we control for interactions of our treatment group with measures of exposure to the Great Recession (unemployment and state housing price indices). This is done to directly control for the localized impact of the recession. We also show the robustness of our results to omitting the recession years. With regard to the opioid epidemic, we undertake two strategies. First, we control directly for the state-level measures of opioid exposure for age groups unaffected by the treatment (e.g., ages 27-to-29 and ages 30 and older) interacted with our treatment group. Next, we omit the states with the largest exposure to the opioid epidemic, measured using 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 aged such that they were affected by the treatment.

5. Results

Our main findings are presented in Table 2. In all models, standard errors are clustered on the state. However, given that only 33 states appear in our NIBRS-based analysis, we also take the conservative approach of estimating wild cluster bootstrap standard errors (Cameron and Miller 2015). As described in footnote 28 below, our conclusions are unchanged using this approach.

5.1. DCM and Crime

[27] We rank the states in our NIBRS analysis sample by their rates of opioid-related mortality for those ages 30 and older and experiment with dropping the top 5 and top 10 ranked states with regard to opioid mortality.

Table 2 presents difference-in-differences estimates of the effect of the DCM on criminal incidents from equation (1). Controlling for age, month-by-year, and agency fixed effects, we find that the DCM is associated with a 4.7 percent [exp (-0.048) - 1] decline in total criminal incidents involving 19-to-25 year-old arrestees during the enactment period, and an 11.1 percent decline in total crime in the post-implementation period (Panel I, column 1). The addition of a control for state dependent coverage mandates (column 2) and agency-by-year fixed effects (column 3) does not change this pattern of results.[28] When we disaggregate crime by type (Panels II and III), we find that the DCM had a larger crime-reducing effect on property relative to violent crime. Focusing on the post-implementation period in our preferred saturated specification (column 3), we find that the DCM was associated with a 13.0 percent decline in property crime and 4.0 percent decline in violent crime. This pattern of findings is consistent with the hypothesis that the DCM may be reducing crime for economically motivated reasons.[29]

To be sure that the effects we observe in Table 2 are not being driven by differential pre-treatment trends in crime, we turn to the event study analysis described in equation (2).[30] Figure 5 shows little evidence of differential pre-treatment trends in property crime. Following enactment, there was a small decline in crime, while the largest impact was observed more than 7 quarters following enactment of the DCM. For violent crime (Figure 6), the pre-DCM period

[28] In Appendix Table 2, we include agency-by-month-by-year fixed effects. While computationally more intensive, the results obtained using these fixed effects is quantitatively similar to those reported in column (3) of Table 2.

[29] If we utilize the score bootstrap method as proposed by Kline and Santos (2012), the p-value on the estimated effect of the DCM on property crime in the post-implementation period from column (3) is < 0.001 and the p-value on the estimated effect of the DCM on violent crime is < 0.001. The "score bootstrap" adapts the wild bootstrap to maximum likelihood estimators, such as Poisson. For a discussion on implementation of the score bootstrap in Stata using the "boottest" command, see Roodman et al. (2019).

[30] Figure 6 shows the event study for total crime, reflecting a similar pattern of results to property crime, which accounts for 78 percent of all crime during our sample period.

mostly reflects common trends in crime, while post-DCM, the crime decline is much smaller. Together, the pattern of results from our event study analysis is consistent with a DCM-driven reduction in crime.[31,32]

Next, we explore whether our findings could be explained by crime trends for those in our control group, who should be largely unaffected by the DCM. In Figures 7 and 8, we plot 95 percent confidence intervals of estimated DCM effects on criminal incidents involving 19-to-25 year-olds, 27 year-olds, and 29 year-olds, relative to 28 year-olds (see equation 3). We present figures for both the enactment (Figure 7) and implementation periods (Figure 8). For 27 and 29 year-olds, estimated crime effects are centered around zero. In contrast, we find strong evidence of crime declines for those in our treated group, with the absolute magnitude of the crime decline decreasing in age. These findings add to our confidence that we are using an appropriate control group, lending further credibility in interpreting our estimates from Table 3 as causal in nature.[33]

5.2 Robustness of Effects to Great Recession and Opioid Crisis

To guard against concerns that our results are reflecting differential responses to the Great Recession, we collect data on the monthly county unemployment rate, the state-by-quarter housing price index, and the state-by-year-by age unemployment rate and interact each with an

[31] In Appendix Table 3, we explore the robustness of our findings to the use of 30-to-34 year-olds and 35-to-39 year-olds as alternate control groups to minimize the possibility that the 27-to-29 year-old control group includes some who were treated when they were younger. The results show a qualitatively similar pattern as those obtained when using our preferred control group.

[32] In Appendix Table 4, we show results using the Uniform Crime Reports. As noted previously, the UCR has limitations on gathering crime by single-year ages. Hence, we separately estimate models using 19-to-24 and 23-to-24 year-olds as the treatment group and 25-to-29 and 30-to-34 year-olds as the control group. Results from the UCR are quite similar to the results from the NIBRS, giving us confidence that our findings from the NIBRS are not idiosyncratic to the geographic limitations of the data.

[33] The findings shown in Appendix Figures 1 to 6 show that using 25, 27, or 29 year-olds as the reference group produces a qualitatively similar set of findings.

indicator for whether the arrestee was in our treated or control group. [34] Our results in Table 3 present the results from that exercise. Across each of these measures (columns 2 through 5), we find no evidence that controlling for differential responses to the business cycle can explain the DCM-induced decline in crime that we detect in our preferred specification (column 1). The omission of recession years from the analysis sample produces a qualitatively similar pattern of results.[35] The findings in column (5) also suggests that an employment-related channel is an unlikely explanation for the DCM-induced crime reduction we observe.

In Table 4, we attempt to disentangle the effects of the DCM from the opioid epidemic, which may have differential spillover effects in the treatment and control group. We use data on state-level opioid overdose deaths to measure the extent of what the Centers for Disease Control and Prevention has called "the worst drug overdose epidemic" in U.S. history (Ahmed 2013).[36] When we interact the average 2008-2013 state-level opioid-involved mortality rate for 27-to-29 year-olds (column 2) and those ages 30 and older (column 3) with an indicator for our treatment group, the findings are largely unchanged from our preferred model (column 1). Next, we rank states by opioid-related mortality rates for those ages 30 and older and drop the top five (column

[34] We collect the monthly county unemployment rate from the Bureau of Labor Statistics (BLS) Local Area Unemployment Statistics (LAUS), the state-by-quarter housing price index from the Federal Housing Finance Agency (FHFA), and the state-by-year-by-age group unemployment rate from the American Community Survey (ACS). In the case of age specific unemployment rates there is no need to interact with the treatment group as the unemployment rates are already interacted with age, a more granular measure.

[35] Specifically, we first omit the January 2008 to June 2009 period (column 2), finding similar estimates to our preferred specification (column 3). Next, we omit 2008 and 2009 from the sample entirely (column 3), with generally similar findings (though smaller in absolute magnitude) to our preferred specification. However, violent crime appears to be the most sensitive to omitting the recession period from the sample. See Appendix Table 5.

[36] From the Centers for Disease Control and Prevention's Multiple Cause of Death, we gather data on opioid-related overdose deaths over the 2008 to 2013 period. Overdose deaths involving opioids are defined by the CDC as drug overdose deaths with the following International Classification of Disease, 10th revision (ICD-10), multiple-cause-of-death 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).

4) and 10 (column 5) highest ranked opioid mortality states from our sample. Again, we find no evidence that the opioid epidemic is leading to biased estimates of the effect of the DCM on crime. These results are consistent with recent evidence by Coupet et al. (2020) showing that the DCM had no impacts on young adult emergency room visits for prescription opioid overdose or opioid-related mortality.

5.3 Heterogeneity in Crime Effects

We next explore heterogeneity in the crime effects of the DCM based on the type of crime for which the individual was arrested using counts of more granular crime categories. Table 5 reports results for counts of individual Part I property and Part I violent crimes. Across Panel I, we find sizable reductions in several forms of property crime due to the DCM, including larceny (a 13.8 percent reduction post-implementation), burglary (a 10.5 percent reduction post-implementation) and motor vehicle theft (a 14.4 percent reduction post-implementation). We do not observe a change in the count of arsons post-enactment or post-implementation that is statistically significant at conventional levels, which is reassuring if the change in crime is due to financial motivations: larceny, burglary, and motor vehicle theft are all property crimes that economically enrich the criminal by transferring stolen property, whereas arson typically is not. In terms of Part I violent crimes (murder, rape, assault, and robbery), only the estimated effect of the DCM on assaults is statistically distinguishable from zero, with an effect size about 30 to 35 percent of that seen for property crime.

The NIBRS also contains counts of arrests for crimes that are not considered by the FBI definition to be Part I property or violent crimes: drug crimes, stolen property, weapons violations, and vandalism. We find consistent evidence that the DCM was also associated with declines in arrests for these offenses as well.

We next explore heterogeneity in the effect of the DCM on arrests by the demographic characteristics of the individual arrested. In Table 6, we present results from equation (1), estimated separately by gender and race/ethnicity. We find evidence of property crime arrests for both males (column 1) and females (column 2), though effects on violent crimes appear to be somewhat larger in the implementation period for females. We find that the impact of the DCM was concentrated among non-Hispanic whites, with few reductions in crime involving black or Hispanic arrestees that are statistically distinguishable from zero at conventional levels. This finding is consistent with previous findings that the DCM increased insurance coverage for whites more than it did for blacks or Hispanics (Antwi et al. 2013, O'Hara and Brault 2013, Breslau et al. 2018a).[37]

Finally, in Table 7, we explore the robustness of our findings to weighting regressions by the estimated age-specific agency population (column 1), allowing for an unbalanced panel of agency-months (column 2), limiting agencies to those who served populations of at least 20,000 (column 3), omitting states that had adopted a prior major health insurance reform or had adopted an early Medicaid expansion (column 4),[38] and limiting (column 5) or expanding (column 6) our post treatment periods. The findings from these exercises produce estimates that are qualitatively similar to those reported above.

[37] However, we do note that 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).

[38] These states include Connecticut, Massachusetts, and Washington, found in our main NIBRS sample, and California, which was not.

6. Mechanisms

Our findings above provide strong evidence that the DCM reduced arrests among the targeted age group. Table 8 presents estimates of the effects of the DCM on the mechanisms through which we suspect the mandate may have reduced arrests. We first present evidence of the effects of the DCM on health insurance coverage using data from the Survey of Income and Program Participation (SIPP) using data from 2008 through 2013. The first two columns of Table 8 present estimates of the effect of the DCM on the probability of a 19-to-25 year-old having any source of health insurance coverage (column 1) and having dependent coverage (column 2).[39] Consistent with previous work (Sommers and Kronick 2012; Cantor et al. 2012b; Antwi et al. 2013; Sommers et al. 2013; O'Hara and Brault 2013; Kotagal et al 2014; Chua and Sommers 2014; Shane and Ayyagari 2014; Barbaresco et al. 2015; Jhamb et al. 2015; Scott et al 2015a,b; Wallace and Sommers 2015), we find that the DCM increases insurance coverage by 2.4 percentage-points (3.6 percent) during the enactment period and 5.4 percentage-points (8.1 percent) during the post-implementation period (column 1). DCM-induced increases in

[39] *Health Insurance* is set equal to 1 if the respondent has employer-provided health insurance (in own name or by someone else's plan), individually purchased coverage (in own name and as a dependent), insurance through the Department of Veterans Affairs (CHAMPVA) or the Department of Defense (TRICARE), Medicaid, Medicare, or other private coverage; it is set equal to 0 otherwise. Following Antwi et al. (2013), *Dependent Health Insurance* is set equal to 1 if the respondent has employer provided health insurance through someone other than their spouse; it is set equal to 0 otherwise. Further following Antwi et al. (2013), we restrict the sample to the fourth reference month in the SIPP to reduce recall bias. However, using all months of the SIPP produces a similar pattern of results.

dependent coverage are, as expected, larger in both the enactment and implementation periods, at 2.8 (12.0 percent) and 9.6 (41.0 percent) percentage-points, respectively (column 2).[40,41]

In columns (3) and (4), we examine the impact of the DCM on out-of-pocket (OOP) medical expenditures using data from the Medical Expenditure Panel Survey (MEPS) from 2008 through 2013.[42] The MEPS data are not available at the sub-year level, so we omit the year 2010 and focus on comparison in the pre-2010 and post-2010 periods for 19-to-25 year-olds and 27-to-29 year-olds.[43] We find that the DCM is associated with about a $63 per year reduction in average OOP health care costs (column 3), driven by a 56 percent reduction in the probability of OOP expenditures greater than $2000 (2009 dollars) (column 4). Preventing large negative income shocks may be a mechanism through which the DCM reduces property crime (Cortes et al. 2016; Bignon et al. 2017; Watson et al. 2019)

In columns (5) and (6), we use monthly data from the 2008 to 2013 National Health Interview Survey (NHIS) to study the impact of the DCM on access to medical care and

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[40] We also explore pre-trends and post-policy effects for health insurance coverage in an event study framework similar to equation (2). Results for any coverage and dependent coverage are shown in Appendix Figures 7 and 8, respectively. Reassuringly, we do not estimate significant pre-trends for the treatment relative to the control group for any coverage or dependent coverage. Following enactment, we observe a positive and significant increase in any coverage which grows over time, reaching a peak 5 quarters post enactment and remaining consistently above pre-enactment levels through the end of our study period. Dependent coverage follows a similar post-enactment pattern.

[41] In Appendix Table 6, we present estimates of the effect of the DCM mandate on health insurance coverage by race/ethnicity. The results show more robust evidence of increases in any source of health insurance and dependent health insurance coverage for non-Hispanic whites as compared to non-Hispanic blacks and Hispanics, which is consistent with the larger impacts we observe on crime.

[42] The variable *OOP* in the MEPS is calculated as the sum of direct payments made by the person or the person's family for the individual's health care provided during the year. *OOP > $2000* is an indicator set equal to 1 if the respondent had out-of-pocket health care spending that was greater than $2000 (2009 dollars) during the calendar year. It is set equal to 0 otherwise.

[43] The MEPS employs an overlapping panel design, with each panel interviewed five times over a roughly two year period, yielding annual data for two calendar years.

prescription drugs.[44] Our results show that the implementation of the DCM increased the probability that a 19-to-25 year-old visited a health professional in the prior two weeks by 2.2 percentage-points (22 percent). The effect was 3 to 4 times greater in the implementation period than in the enactment period, consistent with health insurance effects observed in the SIPP. While we uncover some evidence that the DCM was negatively related to the probability that the respondent needed, but could not afford prescription medication (column 6), this effect was not statistically distinguishable from zero at conventional levels. Moreover, we find little evidence that the DCM had a significant impact on psychological well-being, as measured using the Kessler 6 Scale (column 7).[45]

Next, we uncover evidence that the DCM changed household living arrangements. Using data from the 2008 through 2013 SIPP, we find strong evidence that the DCM was associated with a 5.1 to 6.6 percentage point (9.7 to 12.6 percent) increase in the likelihood that a 19-to-25 year old lived with his/her parents (column 8).[46] This finding is consistent with Chatterji et al. (2017). Moreover, also using the SIPP, we find that the implementation of the DCM was associated with a 1.2 percentage point (11.1 percent) reduction in the likelihood that a 19-to-25 year old received benefits from the Supplemental Nutrition Assistance Program (column 9),

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[44] *Visit Health Professional is* a binary variable indicating whether the respondent had an office visit to a health professional in the past two weeks. *Needed, Not Afford RX* is a binary variable indicating whether the respondent needed but could not afford prescription medicines in the past 12 months.

[45]The Kessler 6 Scale is comprised of six questions that assess mental health over the past 30 days. Each question asks the respondent to answer on a Likert scale from 0 to 4 how often they experienced certain feelings (sadness, nervousness, restless or fidgety, hopeless, everything an effort, and worthless), with 0 meaning "none of the time" and 4 "all of the time." Summing up the scores to each question, *K6 Screening for SMI* (serious mental illness) is set equal to 1 if this sum is greater than 12 and is set equal to 0 otherwise.

[46] *Live with Parents* is created using the household roster. It is set equal to 1 if the respondent lives in the same household with at least one parent in the current month. It is set equal to 0 otherwise.

consistent with increased access to family resources via changes in living arrangements.[47] This pattern of results suggests that household composition changes that lead to increased parental monitoring and greater financial security may be important channels through which the DCM reduced crime.

In column (10), we explore the schooling effects of the DCM. Using data from the SIPP, we find that the DCM is associated with a 3.6 percentage point (10.6 percent) increase in the likelihood that a 19-to-25 year-old is a full-time student (column 10).[48] Both incapacitation and human capital-related channels could also be important in explaining DCM-induced reductions in crime (Lochner and Moretti 2004; Anderson 2014).

However, in contrast to our education results, the employment effects of the DCM may increase the likelihood of crime. The findings in column (11) show that the implementation of the DCM is associated with a 3.2 percentage-point (4.9 percent) decline in employment.[49] This finding is consistent with prior work using data from the SIPP and Current Population Survey (Antwi et al. 2013; Hahn and Yang 2016), but inconsistent with a recent analysis using tax records (Heim et al. 2018).

Finally, in column (12), we use data from the 2008 to 2013 Treatment Episode Data Set (TEDS) to estimate the relationship between the DCM and admissions to treatment for substance

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[47] *Received SNAP Benefits* is set equal to 1 if the respondent received benefits from the Supplemental Nutrition Assistance Program in the current month; it is set equal to 0 otherwise.

[48] *Full-Time Student* is an indicator set equal to 1 if the respondent was enrolled full-time in school during any of the months of the four-month sample wave.

[49] *Employed* is an indicator set equal to 1 if the young adult had a job for at least one week during the reference month.

use disorder.[50,51] We find that the implementation of the DCM is associated with an 11.3 percent decline in admissions for substance use disorder (SUD) treatment (0.762 fewer admissions per 1,000 population). This finding, consistent with Saloner et al. (2018), could reflect declines in substance abuse among affected young adults or, perhaps, substitution toward treatment in non-admission settings. Together, the findings in Table 8 point to a number of credible channels through which the DCM appears to have reduced crime among young adults.

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 be other important efficiency gains if increased health insurance coverage among 19-to-25 year-olds generates additional positive externalities. This study is the first to explore whether the DCM generated spillovers to crime. Given important effects of the DCM on living arrangements, financial resources, and access to health care services — as well as the fact that the DCM targets an age demographic responsible for the majority of all criminal arrests — the externality effects from crime may be very important for a full cost-benefit analysis of the DCM.

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[50] In the TEDS, each observation is a record of an admission for substance abuse. We collapse the data to the state-by-year-by-age group level to compile state-by-year counts of admissions for SUD by age group. State-by-year-by-age group population data are then merged in to create a SUD admissions rate variable. An important drawback of the TEDS is that admissions are only available in specific age bins (i.e., 18-to-20, 21-to-24, 25-to-29, 30-to-34, etc.). We defined 18-to-20 and 21-to-24 year-olds as the treatment groups and 25-to-29 year-olds as the control group, although we recognize the limitations of such coding. Specifying 21-to-24 year-olds as the treatment group and 30-to-34 year-olds or 25-to-29 year-olds as the control group, as in Saloner et al. (2018), yields a qualitatively similar pattern of results.

[51] Admissions data are collected at the annual level in the TEDS. With the DCM policy change occurring during 2010, we omit that year from the analysis sample.

Our estimates show that the DCM was responsible for a sizable reduction in criminal activity among young adults. We find a decrease of approximately 13 percent in property crimes and 4 percent in violent crimes. Conservative back-of-the-envelope calculations using 2009 pre-treatment arrest estimates for 19-to-25 year-olds, and crime specific arrest reduction estimates from Table 5 (only from estimates that are statistically significant at conventional levels), suggests that this reduction corresponds to an annual social cost reduction of approximately $3.1 billion per year in 2019 dollars based on per crime cost estimates from McCollister et al. (2010).[52]

These estimates suggest a more modest reduction in crime from the DCM as compared to the results found in studies of ACA Medicaid expansions. Volger (2018) estimated that Medicaid expansions were associated with a 3 percent reduction in overall crime, saving expanding states roughly $12.9 billion per year. He and Barkowski (2020) estimate slightly smaller crime reductions from ACA-based Medicaid expansions and calculate cost savings of about $10.5 billion per year.

While the total social benefits from crime reductions of 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 full population, an increase of roughly 9.8 million people (0.031*316.2 million U.S. population in 2013). The DCM was much smaller in scope,

[52] We use arrest data from 2009 FBI crime counts to estimate total arrests to those ages 19 to 25. We then used marginal effects obtained in the implementation period, coupled with per-crime costs for each specific Part I crime (McCollister et al. 2010), to generate estimates of the social benefits of crime reductions. The $3.1 billion figure breaks out into $963 million from larceny, $526 million from burglary, $407 million from motor vehicle theft, and $1.3 billion from assault.

reducing the number of uninsured young adults by 938,000 (Antwi et al. 2013). Thus, we estimate that the DCM saved approximately $3,378 in crime costs per newly insured person per year. In contrast, the ACA's Medicaid expansions yielded a crime-related cost savings between $1,071 and $1,316 per newly insured person per year.

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 et al. (2017) at $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 at $211.41 per additional enrollee, giving the DCM an estimated benefit/cost ratio (solely with regards to crime reduction) of approximately 15.98 (3,378 / 211.41) as compared to 0.21 (1,316 / 6,365) based on the more generous estimates from the Medicaid expansions (Vogler 2018). 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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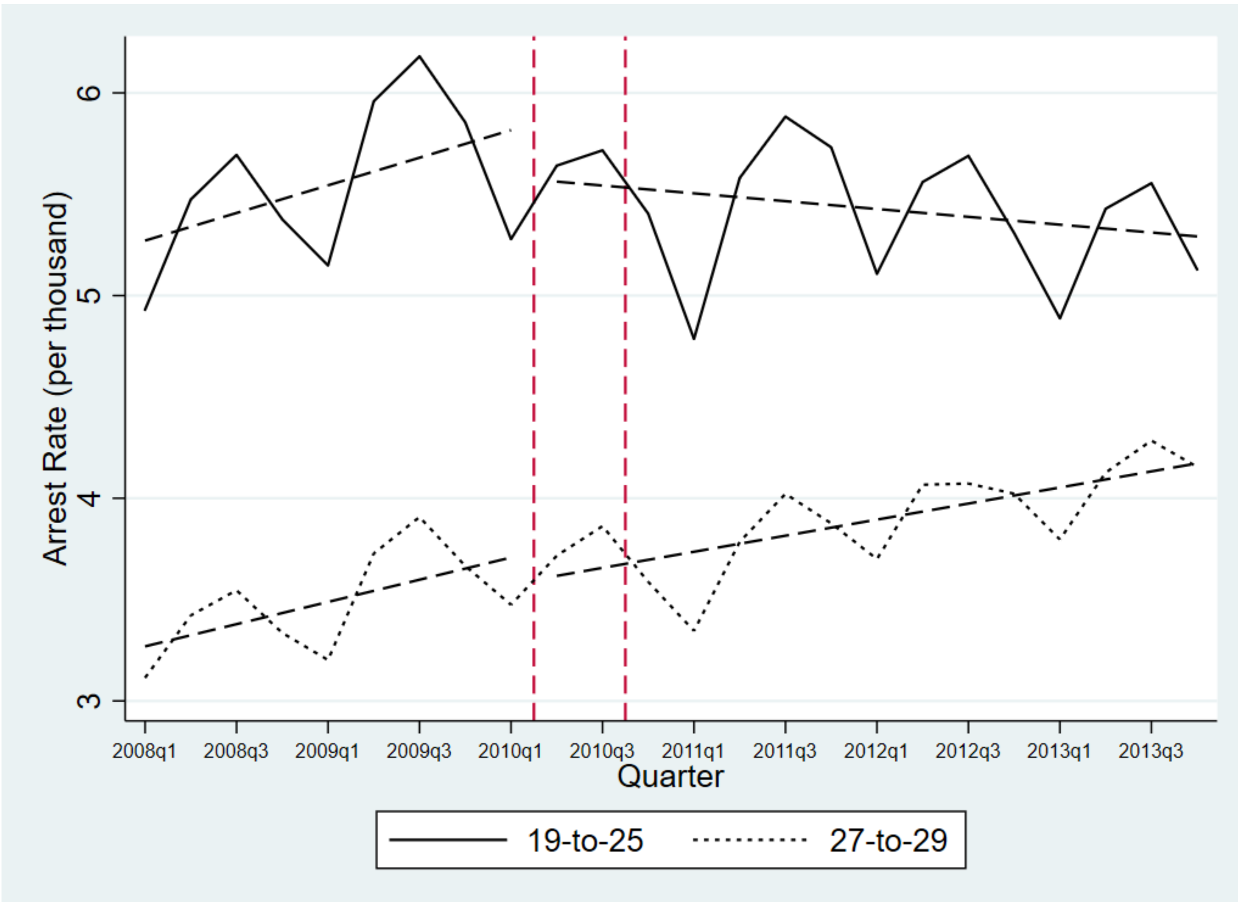
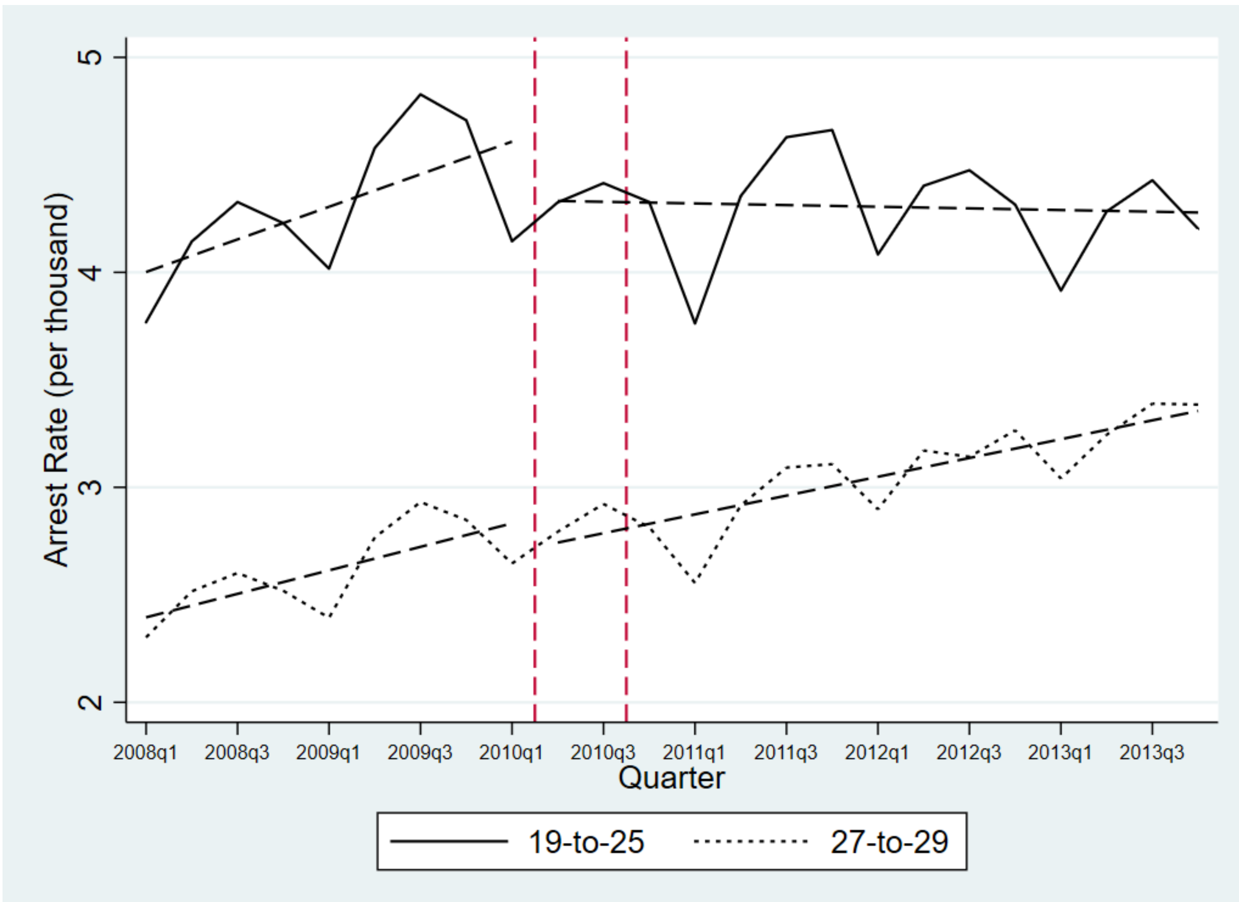
Figure 1. Quarterly Trends in Total Crime Arrest Rates (per thousand), 19-to-25 and 27-to-29 year-olds, NIBRS, 2008 to 2013



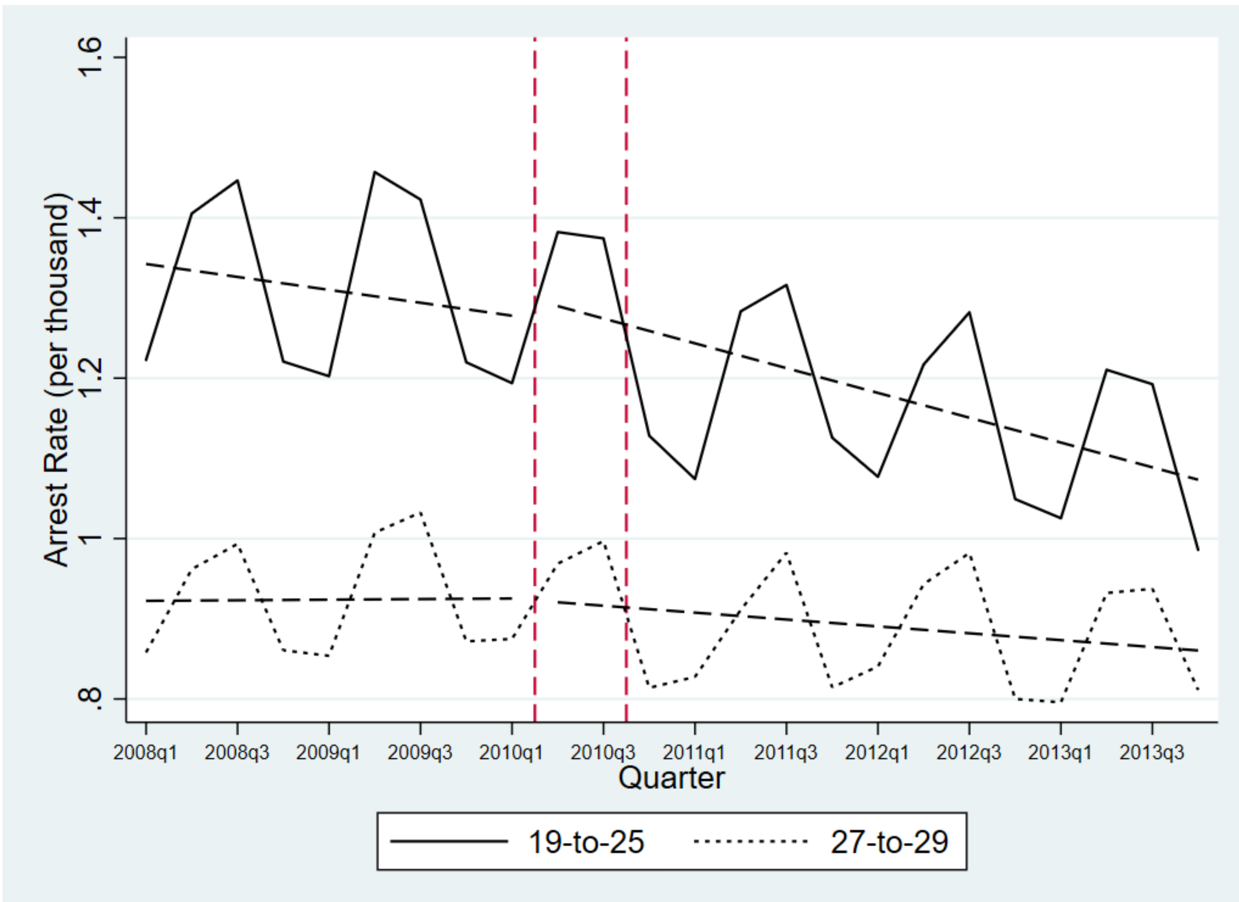
Notes: Aggregate quarterly total crime arrest rates (per thousand population) for 19-to-25 and 27-to-29 year-olds are presented using data from the NIBRS. The sample spans 2008Q1 to 2013Q4. Crime rate trends for each age group are fitted with two dashed lines, spanning the 2008Q1 to 2010Q1 and 2010Q2 to 2013Q4 periods. The dashed lines represent the predicted values from simple linear regressions of the quarterly arrest rate on a linear time trend variable. The 2008Q1 to 2010Q1 period covers the pre-DCM period, with the 2010Q2 to 2013Q4 period covering the post-DCM period (ACA signed into law March 2010). The first dashed vertical line represents the "enactment" period, when the DCM was announced but not fully implemented. The second dashed vertical line represents the "implementation" period, when the DCM was fully implemented.

Figure 2. Quarterly Trends in Property Crime Arrest Rates (per thousand), 19-to-25 and 27-to-29 year-olds, NIBRS, 2008 to 2013



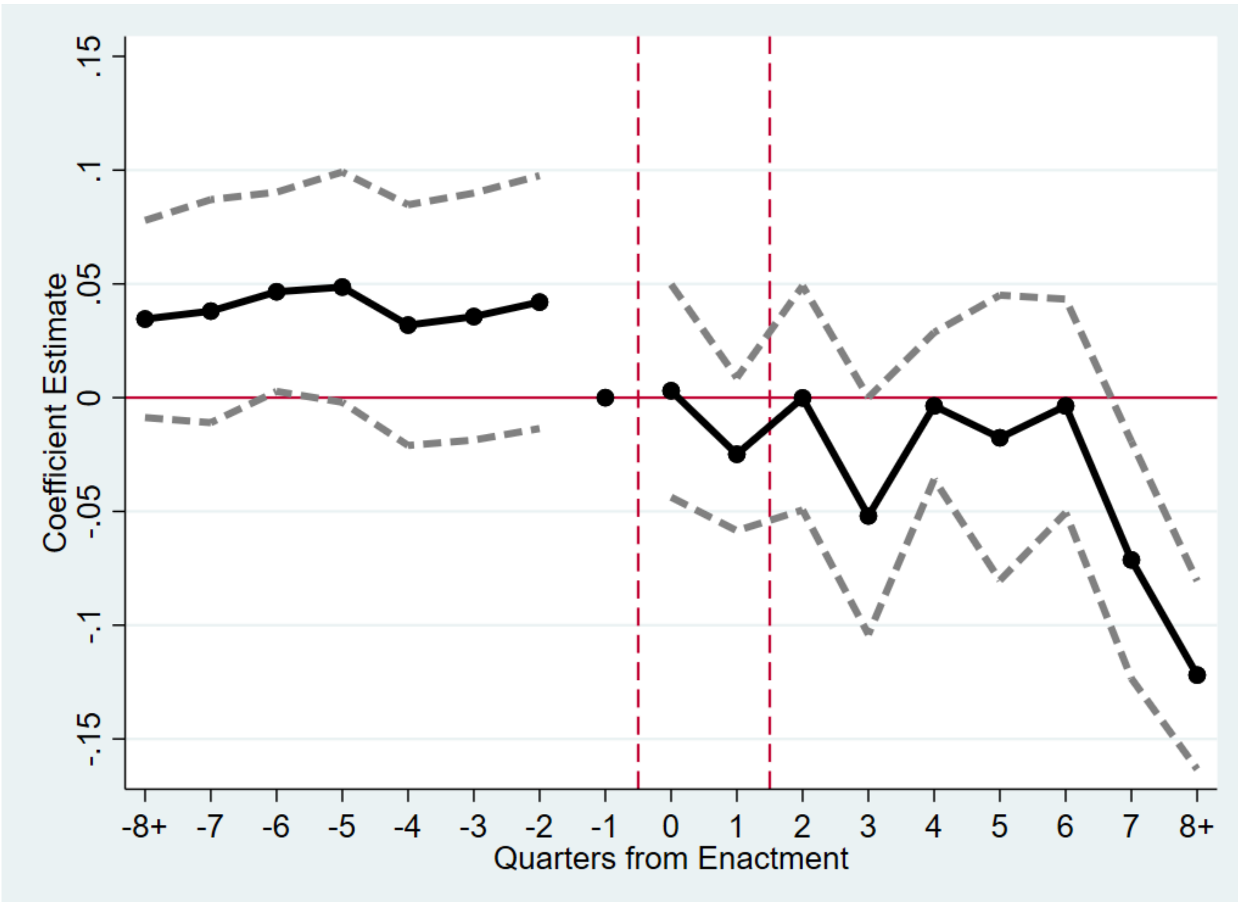
Notes: Aggregate quarterly total crime arrest rates (per thousand population) for 19-to-25 and 27-to-29 year-olds are presented using data from the NIBRS. The sample spans 2008Q1 to 2013Q4. Crime rate trends for each age group are fitted with two dashed lines, spanning the 2008Q1 to 2010Q1 and 2010Q2 to 2013Q4 periods. The dashed lines represent the predicted values from simple linear regressions of the quarterly arrest rate on a linear time trend variable. The 2008Q1 to 2010Q1 period covers the pre-DCM period, with the 2010Q2 to 2013Q4 period covering the post-DCM period (ACA signed into law March 2010). The first dashed vertical line represents the "enactment" period, when the DCM was announced but not fully implemented. The second dashed vertical line represents the "implementation" period, when the DCM was fully implemented.

Figure 3. Quarterly Trends in Violent Crime Arrest Rates (per thousand), 19-to-25 and 27-to-29 year-olds, NIBRS, 2008 to 2013



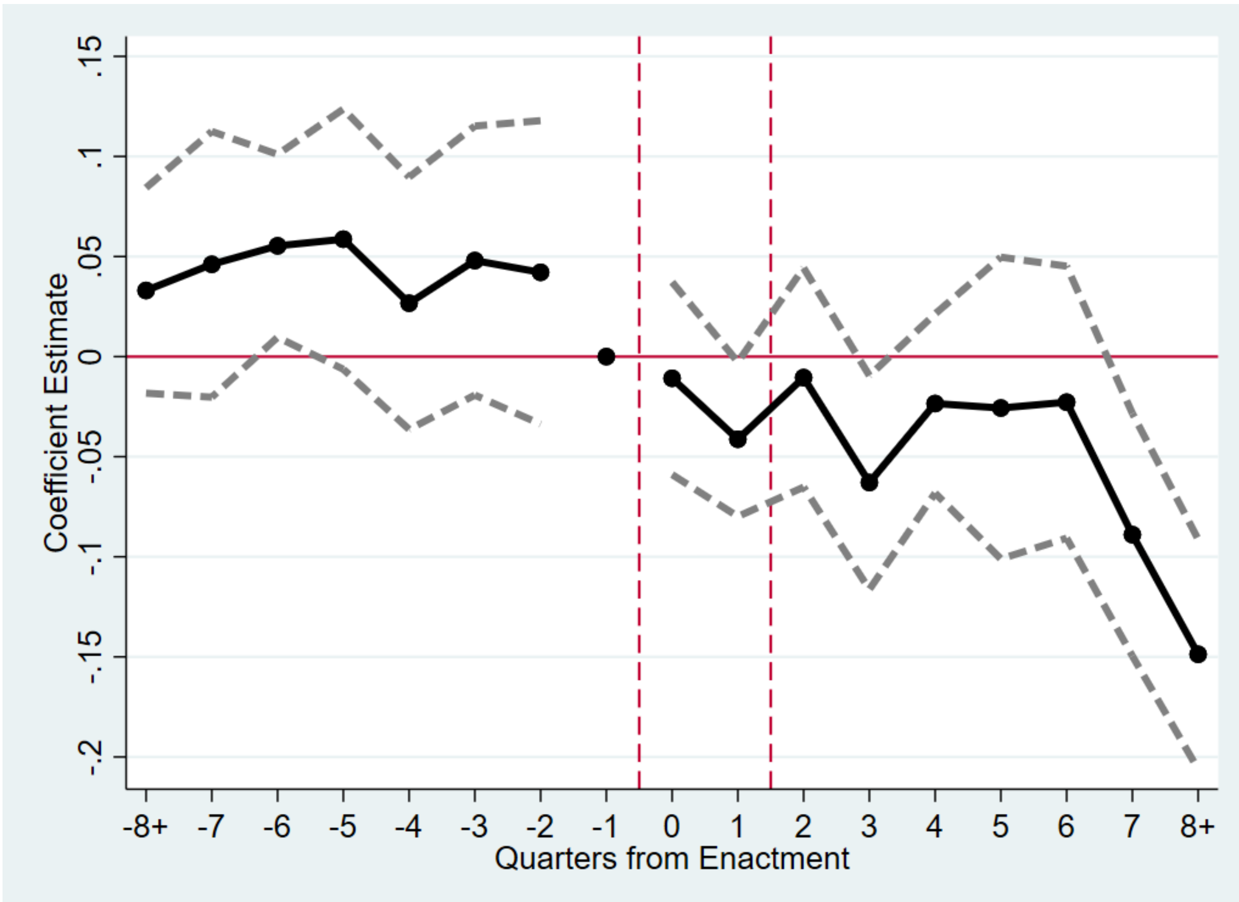
Notes: Aggregate quarterly total crime arrest rates (per thousand population) for 19-to-25 and 27-to-29 year-olds are presented using data from the NIBRS. The sample spans 2008Q1 to 2013Q4. Crime rate trends for each age group are fitted with two dashed lines, spanning the 2008Q1 to 2010Q1 and 2010Q2 to 2013Q4 periods. The dashed lines represent the predicted values from simple linear regressions of the quarterly arrest rate on a linear time trend variable. The 2008Q1 to 2010Q1 period covers the pre-DCM period, with the 2010Q2 to 2013Q4 period covering the post-DCM period (ACA signed into law March 2010). The first dashed vertical line represents the "enactment" period, when the DCM was announced but not fully implemented. The second dashed vertical line represents the "implementation" period, when the DCM was fully implemented.

Figure 4. Event Study Analysis of the Estimated Relationship Between the Dependent Coverage Mandate and Criminal Incidents Leading to an Arrest, Total Crime, 19-to-25 vs. 27-to-29 year-olds, NIBRS, 2008 to 2013



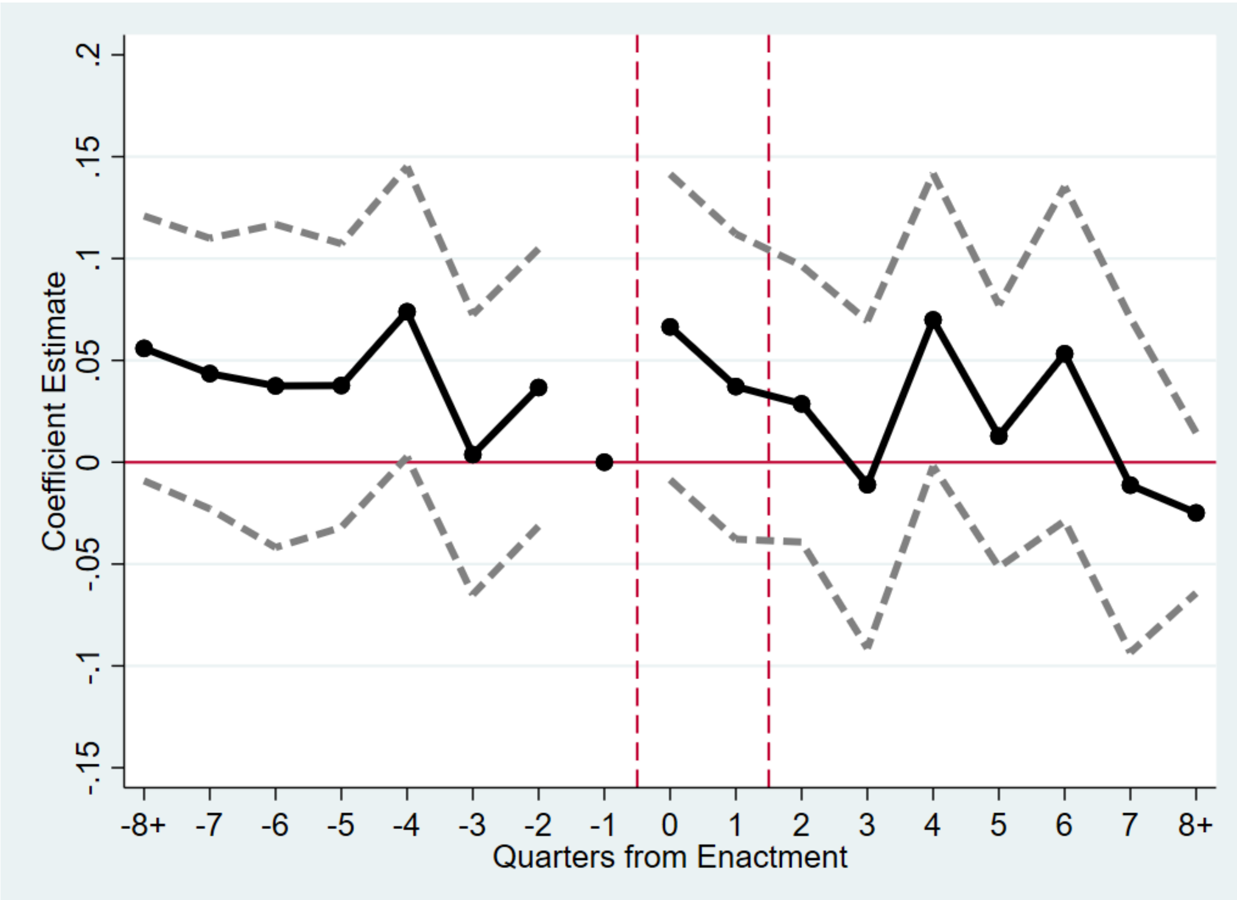
Notes: Event study coefficients are plotted from the resulting regression outlined in equation (2), using data from the NIBRS. The dependent variable is the number of criminal incidents leading to an arrest for total crime. The sample is restricted to a strongly-balanced panel. The dashed lines tracking the plotted coefficients are the 95 percent confidence intervals. The first dashed vertical line represents the ``enactment'' period, when the DCM was announced but not fully implemented. The second dashed vertical line represents the ``implementation'' period, when the DCM was fully implemented.

Figure 5. Event Study Analysis of the Estimated Relationship Between the Dependent Coverage Mandate and Criminal Incidents Leading to an Arrest, Property Crime, 19-to-25 vs. 27-to-29 year-olds, NIBRS, 2008 to 2013



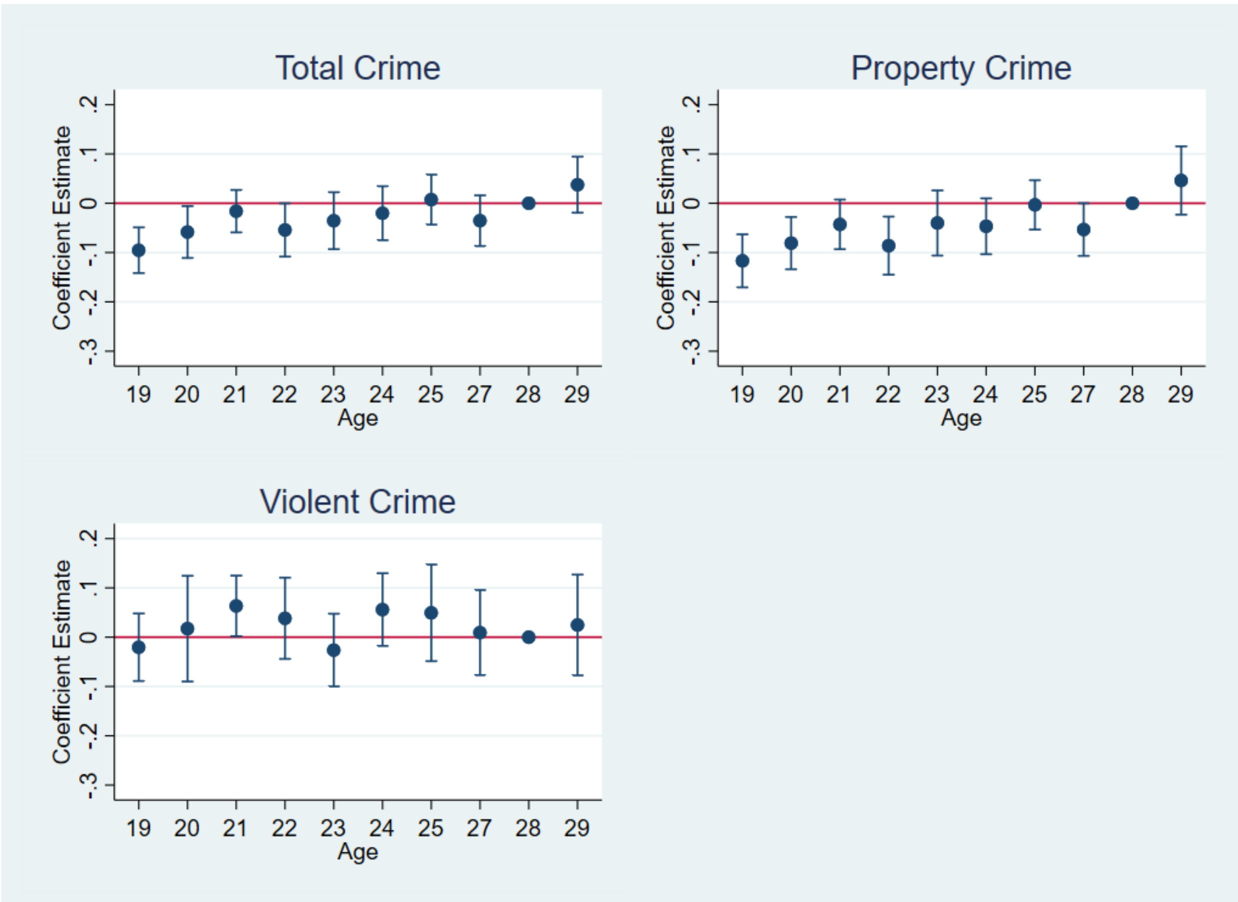
Notes: Event study coefficients are plotted from the resulting regression outlined in equation (2), using data from the NIBRS. The dependent variable is the number of criminal incidents leading to an arrest for property crime. The sample is restricted to a strongly-balanced panel. The dashed lines tracking the plotted coefficients are the 95 percent confidence intervals. The first dashed vertical line represents the ``enactment'' period, when the DCM was announced but not fully implemented. The second dashed vertical line represents the ``implementation'' period, when the DCM was fully implemented.

Figure 6. Event Study Analysis of the Estimated Relationship Between the Dependent Coverage Mandate and Criminal Incidents Leading to an Arrest, Violent Crime, 19-to-25 vs. 27-to-29 year-olds, NIBRS, 2008 to 2013



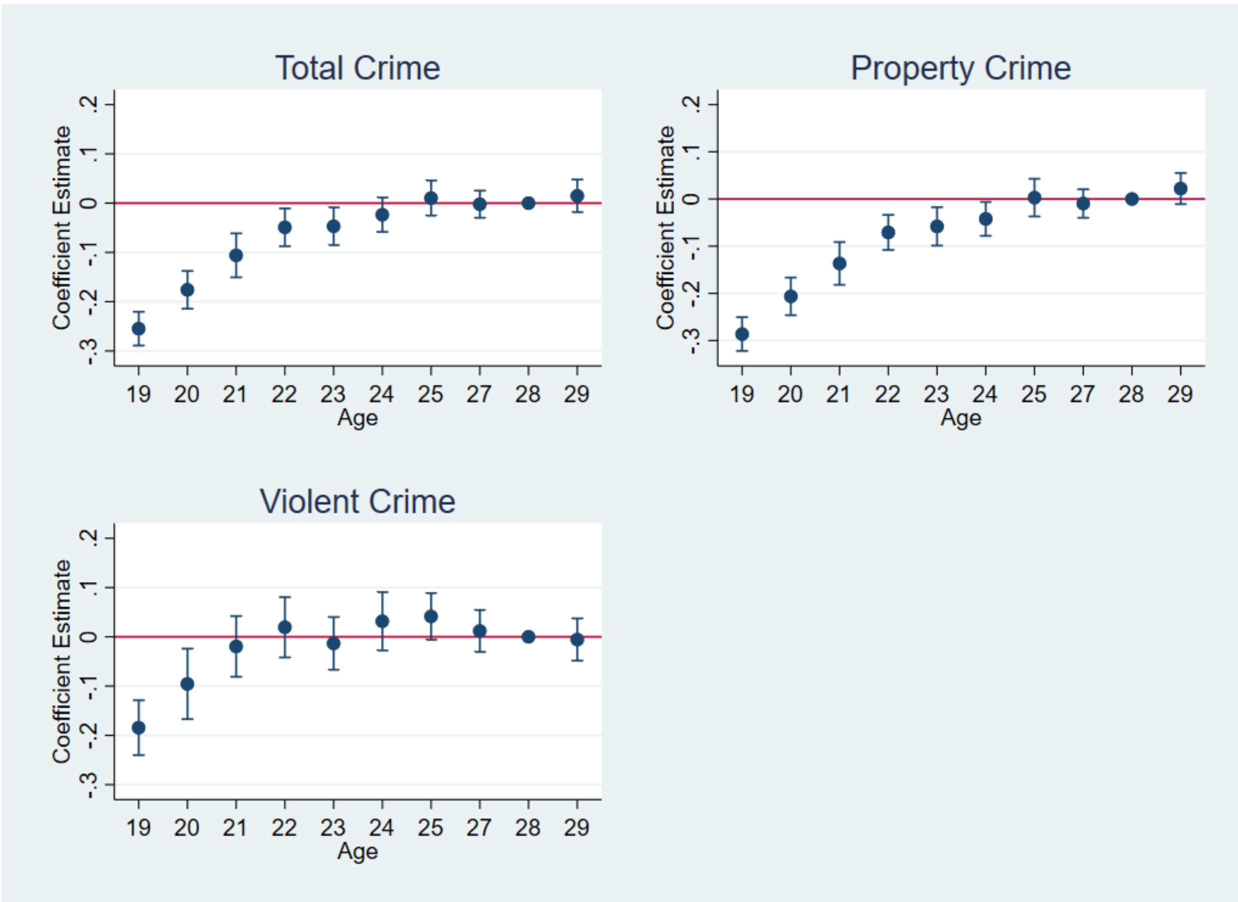
Notes: Event study coefficients are plotted from the resulting regression outlined in equation (2), using data from the NIBRS. The dependent variable is the number of criminal incidents leading to an arrest for violent crime. The sample is restricted to a strongly-balanced panel. The dashed lines tracking the plotted coefficients are 95 percent confidence intervals. The first dashed vertical line represents the ``enactment'' period, when the DCM was announced but not fully implemented. The second dashed vertical line represents the ``implementation'' period, when the DCM was fully implemented.

Figure 7. Single-Year Age Difference-in-Differences Estimates (Reference Age = 28), Enactment Effect, NIBRS, 2008 to 2013



Notes: Coefficient estimates are presented from equation (3). Each age has its own difference-in-differences estimate (using 28 year-olds as the reference group). In this figure, coefficient estimates are plotted for the "enactment" period effects. Error bars are 95 percent confidence intervals.

Figure 8. Single-Year Age Difference-in-Differences Estimates (Reference Age = 28), Implementation Effect, NIBRS, 2008 to 2013



Notes: Coefficient estimates are presented from equation (3). Each age has its own difference-in-differences estimate (using 28 year-olds as the reference group). In this figure, coefficient estimates are plotted for the "implementation" period effects. Error bars are 95 percent confidence intervals.

Table 1. Summary Statistics, NIBRS, 2008 to 2013

	19-to-25			27-to-29		
	Mean	SD	N	Mean	SD	N
			Full Sample			
Total	0.993	2.342	639,996	0.664	1.578	274,284
Property	0.783	1.841	639,408	0.512	1.259	274,032
Violent	0.232	0.818	612,948	0.167	0.587	262,692
			Pre-DCM Enactment Period			
Total	0.978	2.347	231,140	0.610	1.504	99,060
Property	0.761	1.838	230,944	0.457	1.175	98,876
Violent	0.241	0.824	221,228	0.170	0.588	94,812
			DCM Enactment Period			
Total	1.027	2.371	53,340	0.670	1.603	22,860
Property	0.792	1.834	53,256	0.505	1.248	22,824
Violent	0.259	0.866	51,324	0.181	0.632	21,996
			DCM Implementation Period			
Total	0.998	2.334	355,516	0.697	1.619	152,364
Property	0.796	1.844	355,208	0.549	1.311	152,232
Violent	0.223	0.806	340,396	0.164	0.579	145,884

Notes: Means of counts of criminal incidents leading to an arrest presented for the NIBRS. The unit of observation is an agency-age-month. The sample is limited to a strongly-balanced panel. "Pre-DCM Enactment Period" presents means from January 2008 through February 2010. "DCM Enactment Period" presents means for the period March 2010 through August 2010. "DCM Implementation Period" presents means from September 2010 through December 2013.

Table 2. Effect of ACA Dependent Coverage Mandate on Criminal Incidents Leading to an Arrest, NIBRS, 2008 to 2013

	(1)	(2)	(3)
		Panel I: Total Crime	
$\text{Treat}_i * \text{Enact}_t$	-0.048***	-0.045***	-0.045***
	(0.010)	(0.010)	(0.010)
$\text{Treat}_i * \text{Implement}_t$	-0.118***	-0.115***	-0.114***
	(0.012)	(0.013)	(0.013)
N	914,400	914,400	914,280
		Panel II: Property Crime	
$\text{Treat}_i * \text{Enact}_t$	-0.066***	-0.064***	-0.063***
	(0.013)	(0.013)	(0.013)
$\text{Treat}_i * \text{Implement}_t$	-0.143***	-0.140***	-0.139***
	(0.013)	(0.013)	(0.013)
N	914,400	914,400	913,440
		Panel III: Violent Crime	
$\text{Treat}_i * \text{Enact}_t$	0.008	0.012	0.012
	(0.018)	(0.018)	(0.018)
$\text{Treat}_i * \text{Implement}_t$	-0.046***	-0.042**	-0.041**
	(0.018)	(0.019)	(0.020)
N	912,960	912,960	875,640
State DCM	No	Yes	Yes
Month-by-Year FE	Yes	Yes	Yes
Agency FE	Yes	Yes	No
Agency-by-Year FE	No	No	Yes

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: The dependent variable is the count of criminal incidents leading to an arrest in an agency-age-month. Models are estimated via Poisson and the sample is limited to a strongly-balanced panel. All models include age and month-by-year fixed effects. Standard errors are clustered at the state level.

Table 3. Sensitivity of Estimates in Table 2 to Controls for Business Cycle and Labor Market Conditions, NIBRS, 2008 to 2013

	(1)	(2)	(3)	(4)	(5)
			Panel I: Total Crime		
$\text{Treat}_i * \text{Enact}_t$	-0.045***	-0.035***	-0.049***	-0.039***	-0.045***
	(0.010)	(0.012)	(0.010)	(0.013)	(0.012)
$\text{Treat}_i * \text{Implement}_t$	-0.114***	-0.114***	-0.120***	-0.120***	-0.114***
	(0.013)	(0.013)	(0.016)	(0.015)	(0.014)
N	914,280	914,280	914,280	914,280	914,280
			Panel II: Property Crime		
$\text{Treat}_i * \text{Enact}_t$	-0.063***	-0.050***	-0.068***	-0.055***	-0.063***
	(0.013)	(0.013)	(0.012)	(0.014)	(0.014)
$\text{Treat}_i * \text{Implement}_t$	-0.139***	-0.139***	-0.145***	-0.146***	-0.139***
	(0.013)	(0.013)	(0.018)	(0.017)	(0.014)
N	913,440	913,440	913,440	913,440	913,440
			Panel III: Violent Crime		
$\text{Treat}_i * \text{Enact}_t$	0.012	0.011	0.010	0.009	0.012
	(0.018)	(0.023)	(0.019)	(0.023)	(0.020)
$\text{Treat}_i * \text{Implement}_t$	-0.041**	-0.041**	-0.044**	-0.044**	-0.042**
	(0.020)	(0.019)	(0.019)	(0.019)	(0.021)
N	875,640	875,640	875,640	875,640	875,640
County UR*Treat	No	Yes	No	Yes	No
State HP*Treat	No	No	Yes	Yes	No
UR-by-Age group	No	No	No	No	Yes

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: The dependent variable is the count of criminal incidents leading to an arrest in an agency-age-month. Models are estimated via Poisson and the sample is limited to a strongly-balanced panel. Models control for age, month-by-year, and agency-by-year fixed effects, as well as state dependent coverage mandates. County UR*Treat is an interaction between the county-monthly unemployment rate and treatment group status. State HP*Treat is an interaction between the state-by-quarter housing price index and treatment group status. UR-by-Age group is a control for the state-year-age group (19-to-25 and 27-to-29) unemployment rate. Standard errors are clustered at the state level.

Table 4. Sensitivity of Estimates in Table 2 to Controls for Opioid Epidemic, NIBRS, 2008 to 2013

	(1)	(2)	(3)	(4)	(5)
			Panel I: Total Crime		
$\text{Treat}_i * \text{Enact}_t$	-0.045***	-0.051***	-0.041***	-0.043***	-0.043***
	(0.010)	(0.013)	(0.012)	(0.010)	(0.012)
$\text{Treat}_i * \text{Implement}_t$	-0.114***	-0.090***	-0.090***	-0.110***	-0.113***
	(0.013)	(0.015)	(0.019)	(0.012)	(0.016)
N	914,280	742,680	909,960	849,600	561,600
			Panel II: Property Crime		
$\text{Treat}_i * \text{Enact}_t$	-0.063***	-0.074***	-0.059***	-0.063***	-0.053***
	(0.013)	(0.017)	(0.015)	(0.013)	(0.016)
$\text{Treat}_i * \text{Implement}_t$	-0.139***	-0.114***	-0.111***	-0.138***	-0.140***
	(0.013)	(0.015)	(0.021)	(0.013)	(0.016)
N	913,440	742,200	909,120	848,880	560,880
			Panel III: Violent Crime		
$\text{Treat}_i * \text{Enact}_t$	0.012	0.019	0.013	0.015	-0.003
	(0.018)	(0.020)	(0.019)	(0.018)	(0.023)
$\text{Treat}_i * \text{Implement}_t$	-0.041**	-0.026	-0.036*	-0.035*	-0.037
	(0.020)	(0.020)	(0.021)	(0.020)	(0.029)
N	875,640	712,920	871,320	815,160	538,320
Opioid Control	None	Treat*27-to-29	Treat*30+	None	None
Sample Restriction	None	None	None	Drop Top 5	Drop Top 10

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: The dependent variable is the count of criminal incidents leading to an arrest in an agency-age-month. Models are estimated via Poisson and the sample is limited to a strongly-balanced panel. Treat*27-to-29 (Treat*30+) is the state-by-year opioid-related overdose mortality rate of 27-to-29 (30+) year-olds interacted with treatment group status. Drop Top 5 (10) refers to dropping from the sample the 5 (10) highest states as ranked by their opioid-related overdose mortality rate for 30+ year-olds. Models control for age, month-by-year, and agency-by-year fixed effects, as well as state dependent coverage mandates. Standard errors are clustered at the state level.

Table 5. Effect of ACA Dependent Coverage Mandate on Criminal Incidents Leading to an Arrest, Disaggregated Types of Crime, 19-to-25 vs. 27-to-29 year-olds, NIBRS, 2008 to 2013

	(1)	(2)	(3)	(4)
	Panel I: Property Types of Crime			
	Larceny	Burglary	MVT	Arson
$\text{Treat}_i * \text{Enact}_t$	-0.068***	-0.087**	0.051	0.034
	(0.014)	(0.035)	(0.050)	(0.173)
$\text{Treat}_i * \text{Implement}_t$	-0.148***	-0.111***	-0.155***	-0.077
	(0.013)	(0.026)	(0.026)	(0.106)
N	909,840	863,340	648,360	197,040
	Panel II: Violent Types of Crime			
	Murder	Rape	Assault	Robbery
$\text{Treat}_i * \text{Enact}_t$	-0.062	0.095	0.028	-0.030
	(0.127)	(0.102)	(0.025)	(0.048)
$\text{Treat}_i * \text{Implement}_t$	0.038	-0.058	-0.048**	-0.015
	(0.070)	(0.056)	(0.021)	(0.036)
N	169,080	360,480	846,600	578,760
	Panel III: Other Types of Crime			
	Drug	Stolen Property	Weapon Violations	Vandalism
$\text{Treat}_i * \text{Enact}_t$	-0.021	-0.009	-0.068**	-0.005
	(0.019)	(0.061)	(0.033)	(0.027)
$\text{Treat}_i * \text{Implement}_t$	-0.039**	-0.089**	-0.102***	-0.102***
	(0.018)	(0.036)	(0.027)	(0.021)
N	911,760	604,920	778,680	893,040
Treatment Group	19-to-22	23-to-25	19-to-25	19-to-25
Control Group	27-to-29	27-to-29	27-to-34	30-to-34

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

The dependent variable is the count of criminal incidents leading to an arrest in an agency-age-month. Models are estimated via Poisson and the sample is limited to a strongly-balanced panel. Models control for age, month-by-year, and agency-by-year fixed effects, as well as state dependent coverage mandates. Standard errors are clustered at the state level.

Table 6. Effect of ACA Dependent Coverage Mandate on Criminal Incidents Leading to an Arrest by Gender and Race/Ethnicity, 19-to-25 vs. 27-to-29 year-olds, NIBRS, 2008 to 2013

	(1)	(2)	(3)	(4)	(5)
	Male	Female	White	Black	Hispanic
			Panel I: Total Crime		
$\mathrm{Treat_i} * \mathrm{Enact_t}$	-0.053***	-0.029*	-0.070***	0.020	-0.048*
	(0.015)	(0.016)	(0.019)	(0.020)	(0.029)
$\mathrm{Treat_i} * \mathrm{Implement_t}$	-0.100***	-0.139***	-0.192***	-0.013	-0.032
	(0.018)	(0.013)	(0.023)	(0.017)	(0.022)
N	913,800	893,640	810,840	622,812	533,028
			Panel II: Property Crime		
$\mathrm{Treat_i} * \mathrm{Enact_t}$	-0.084***	-0.030*	-0.092***	0.025	-0.047
	(0.019)	(0.017)	(0.023)	(0.031)	(0.044)
$\mathrm{Treat_i} * \mathrm{Implement_t}$	-0.131***	-0.151***	-0.206***	-0.032	-0.052**
	(0.019)	(0.014)	(0.025)	(0.021)	(0.026)
N	912,240	879,720	805,440	579,720	478,464
			Panel III: Violent Crime		
$\mathrm{Treat_i} * \mathrm{Enact_t}$	0.022	-0.019	0.024	0.013	-0.053
	(0.023)	(0.044)	(0.045)	(0.025)	(0.059)
$\mathrm{Treat_i} * \mathrm{Implement_t}$	-0.030	-0.081***	-0.152***	0.002	0.010
	(0.022)	(0.022)	(0.024)	(0.020)	(0.036)
N	866,160	652,200	727,080	466,632	343,956

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: The dependent variable is the count of criminal incidents leading to an arrest in an agency-age-month. Models are estimated via Poisson and the sample is limited to a strongly-balanced panel. Models control for age, month-by-year, and agency-by-year fixed effects, as well as state dependent coverage mandates. Standard errors are clustered at the state level.

Table 7. Robustness Checks on Estimated Crime Effects, NIBRS

	(1) Weighted	(2) Unbalanced Panel	(3) Agency Population ≥ 20,000	(4) Omit CT, MA, WA	(5) Only Years 2008-2012	(6) Only Years 2008-2015
			Panel I: Total Crime			
$\text{Treat}_i * \text{Enact}_t$	-0.031**	-0.041***	-0.046***	-0.051***	-0.050***	-0.038***
	(0.013)	(0.010)	(0.017)	(0.010)	(0.010)	(0.011)
$\text{Treat}_i * \text{Implement}_t$	-0.057***	-0.121***	-0.111***	-0.111***	-0.085***	-0.177***
	(0.020)	(0.014)	(0.014)	(0.014)	(0.013)	(0.014)
N	914,280	2,351,320	495,120	797,640	804,000	1,093,320
			Panel II: Property Crime			
$\text{Treat}_i * \text{Enact}_t$	-0.040***	-0.058***	-0.070***	-0.067***	-0.069***	-0.056***
	(0.013)	(0.013)	(0.020)	(0.014)	(0.012)	(0.014)
$\text{Treat}_i * \text{Implement}_t$	-0.088***	-0.147***	-0.139***	-0.134***	-0.107***	-0.207***
	(0.020)	(0.014)	(0.014)	(0.014)	(0.013)	(0.015)
N	913,440	2,278,770	495,120	796,800	803,280	1,092,360
			Panel III: Violent Crime			
$\text{Treat}_i * \text{Enact}_t$	-0.002	0.010	0.025	0.004	0.008	0.017
	(0.044)	(0.018)	(0.018)	(0.021)	(0.018)	(0.020)
$\text{Treat}_i * \text{Implement}_t$	0.022	-0.049***	-0.035*	-0.040*	-0.028	-0.083***
	(0.026)	(0.018)	(0.020)	(0.022)	(0.020)	(0.018)
N	875,640	1,889,340	489,720	761,520	768,480	1,054,320

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

The dependent variable is the count of criminal incidents leading to an arrest in an agency-age-month. Models are estimated via Poisson and the sample is limited to a strongly-balanced panel (except for column 2). All models control for age, month-by-year, and agency-by-year fixed effects, as well as state dependent coverage mandates. Standard errors are clustered at the state level. The estimates from column (1) are population weighted using an estimated age-specific agency population.

In column (3), the sample is restricted to agencies with a total population of at least 20,000. In column (4), the early Medicaid expansion states in our NIBRS sample (Connecticut and Washington) are omitted, as well as Massachusetts, for its pre-ACA health care reform.

Table 8. Exploration of Mechanisms through which the Affordable Care Act's DCM Reduced Crime, 2008 to 2013

	(1)	(2)	(3)	(4)	(5)	(6)
	Health Insurance[a]	Dependent Health Insurance[a]	OOP[b]	OOP > $2000[b]	Visit Health Professional[c]	Needed, Not Afford RX[c]
$\mathrm{Treat}_i * \mathrm{Enact}_t$	0.024***	0.028***	n/A	n/A	0.006	-0.011
	(0.008)	(0.006)			(0.012)	(0.021)
$\mathrm{Treat}_i * \mathrm{Implement}_t$	0.054***	0.096***	-63.431*	-0.020**	0.022***	-0.011
	(0.012)	(0.006)	(37.808)	(0.008)	(0.007)	(0.011)
Pre-DCM Treatment Mean	0.665	0.234	339.258	0.036	0.101	0.110
N	162,647	162,647	23,249	23,249	74,365	28,839

	(7)	(8)	(9)	(10)	(11)	(12)
	K6 Screening For SMI[c]	Live with Parents[a]	Received SNAP Benefits[a]	Full-Time Student[a]	Employed[a]	Substance Use Treatment[d]
$\mathrm{Treat}_i * \mathrm{Enact}_t$	0.003	0.051***	-0.011*	0.038***	0.004	n/A
	(0.011)	(0.007)	(0.006)	(0.009)	(0.015)	
$\mathrm{Treat}_i * \mathrm{Implement}_t$	-0.003	0.066***	-0.012*	0.036***	-0.032***	-0.762**
	(0.006)	(0.009)	(0.007)	(0.006)	(0.010)	(0.372)
Pre-DCM Treatment Mean	0.026	0.524	0.108	0.339	0.651	6.726
N	28,554	162,647	162,647	162,647	162,647	741

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: Superscripts above denote the dataset from which each outcome variable is measured and the model specifications used for each.

[a] Survey of Income and Program Participation, 2008 to 2013, Treatment Group (19-to-25), Control Group (27-to-29)
All SIPP models (columns 1, 2, 8, 9, 10, and 11) are estimated via OLS, weighted by SIPP person weights, and cluster standard errors at the state level. All models control for state, age, and month-by-year fixed effects, state-specific linear time trends, state-year-age group unemployment rate, share of people ages 25+ with a college degree (state-

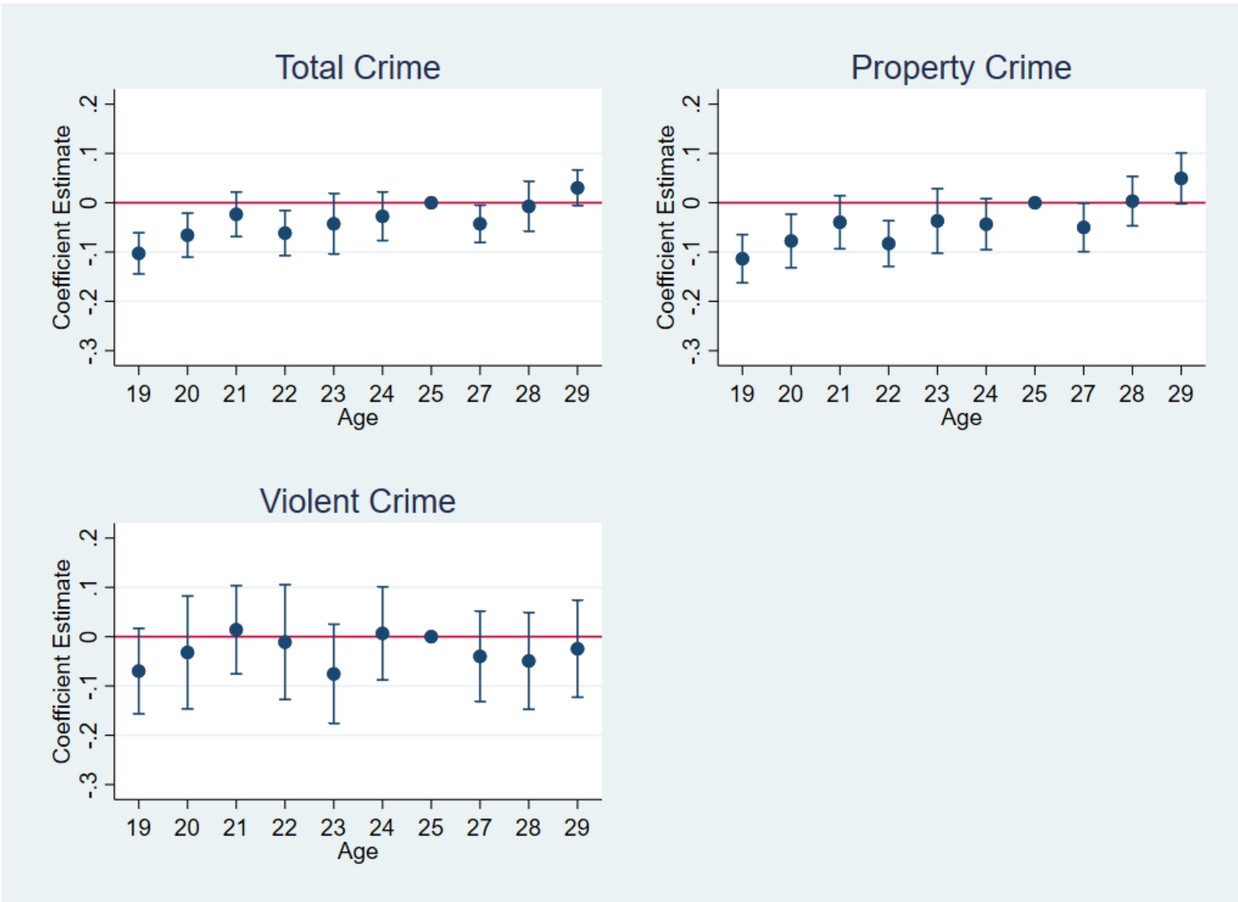
year), as well as individual-level controls for gender, race/ethnicity, marital status, and student status (except for column 10). Data are restricted to the fourth reference month of each wave in the SIPP (following Antwi et. al 2013).

[b] Medical Expenditure Panel Survey, 2008 to 2013, Treatment Group (19-to-25), Control Group (27-to-29)
All MEPS models (columns 3 and 4) are estimated via OLS and weighted using survey weights. Models include age and census region-by-year fixed effects, as well as controls for gender, race, and marital status. Data from 2010 are omitted from the sample. Taylor-linearized variance estimation is used to calculate standard errors, which accounts for the complex survey design of the MEPS. We cannot separately identify the enactment window in the MEPS data.

[c] National Health Interview Survey, 2008 to 2013, Treatment Group (19-to-25), Control Group (27-to-29)
All NHIS models (columns 5, 6, and 7) are estimated via OLS and weighted using survey weights. Models include age, month-by-year, and census region-by-year fixed effects, as well as controls for gender, race, and marital status. Taylor-linearized variance estimation is used to calculate standard errors, which accounts for the complex survey design of the NHIS.

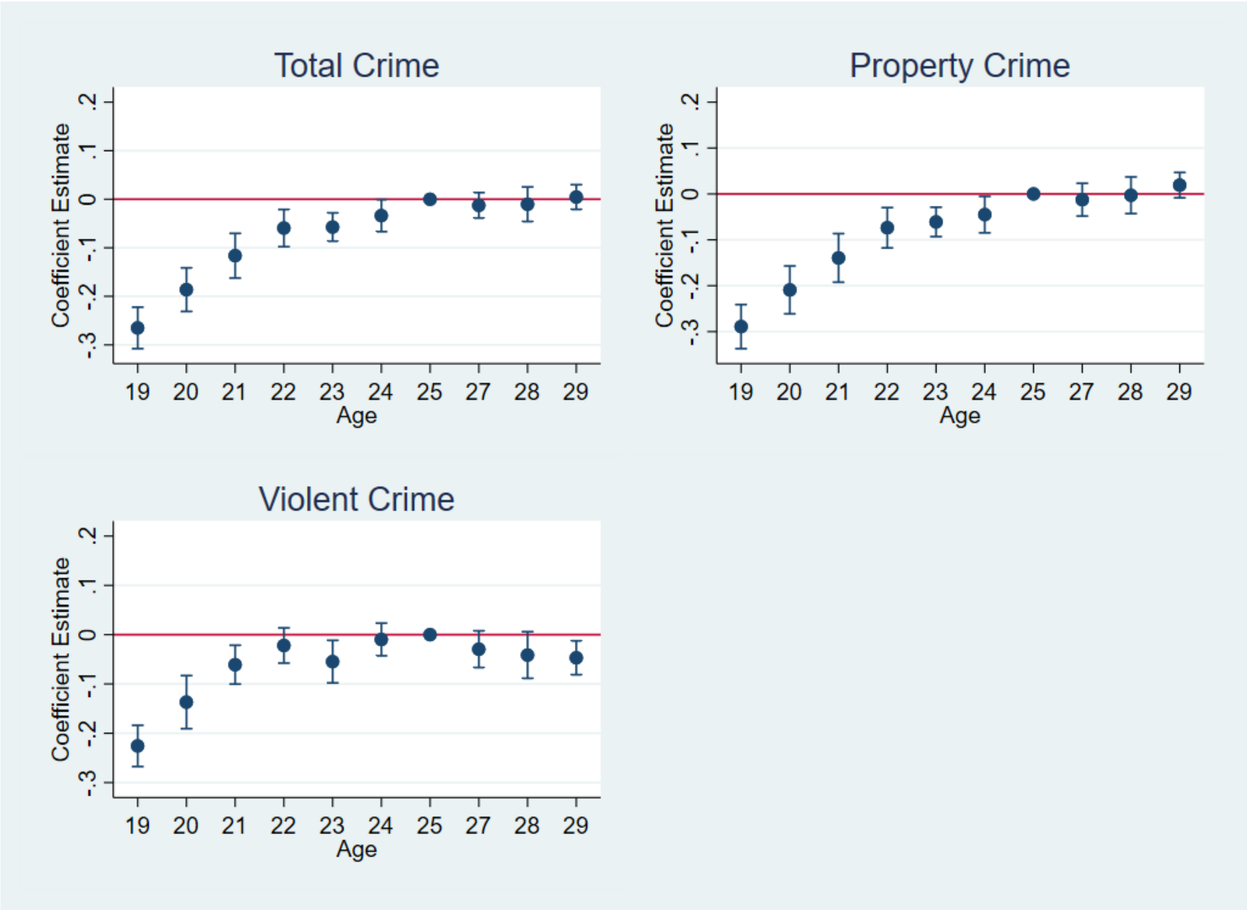
[d] Treatment Episode Data Set, 2008 to 2013, Treatment Group (18-to-20 and 21-to-24), Control Group (25-to-29)
Column (12) is estimated via OLS and weighted by state-year-age group population, with standard errors clustered at the state level. Controls include state, age group, and year fixed effects, as well as state-specific linear time trends, state-level demographics for gender, race/ethnicity, marital status, high school diploma or greater, rural population, and state-year-age group unemployment rate. Data from 2010 are omitted from the sample as we are not able to isolate the enactment window using these annual data.

Appendix Figure 1. Single-Year Age Estimates of the Enactment Effect of the DCM, NIBRS, 2008 to 2013 (Reference Age = 25)



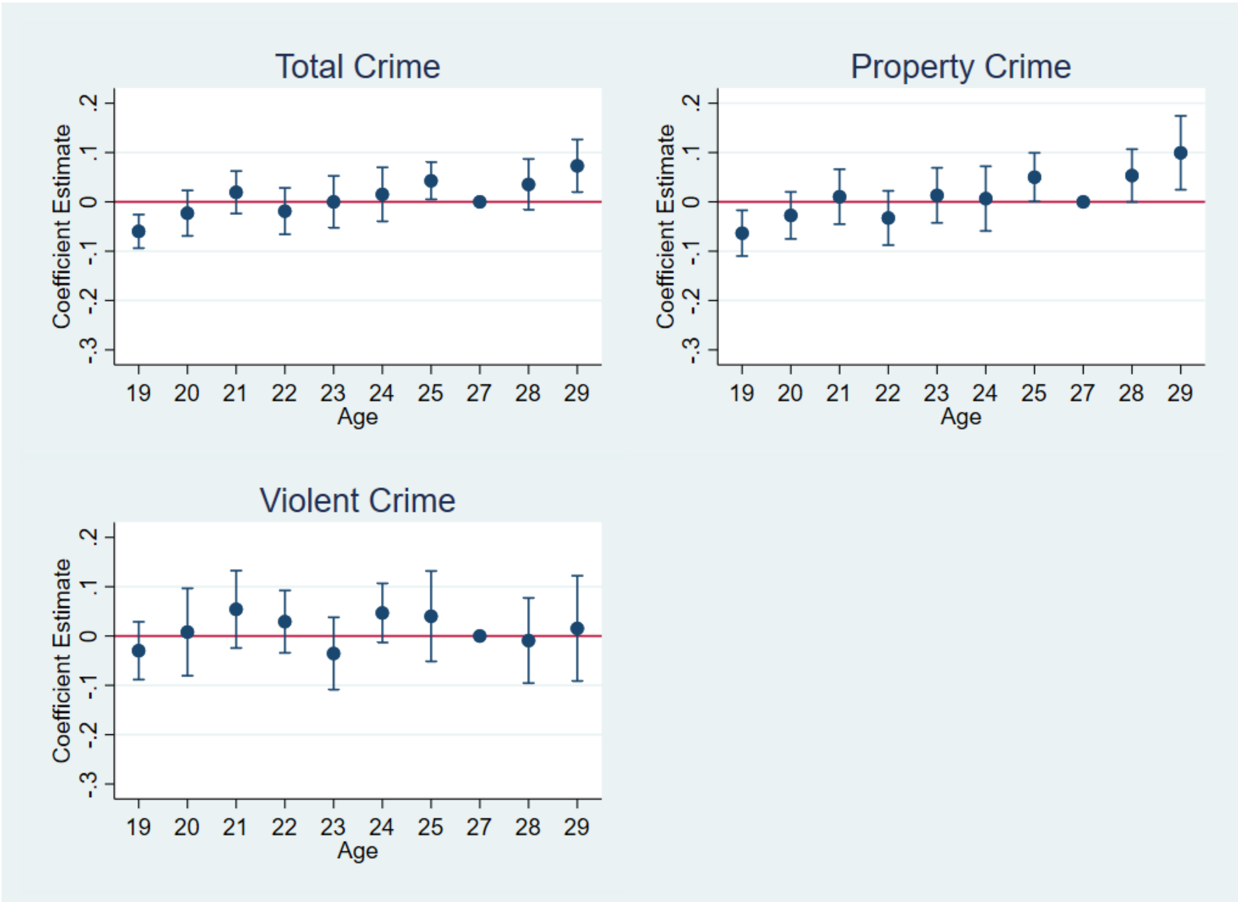
Notes: Coefficient estimates are presented from equation (3) using a Poisson model. Each age has its own difference-in-differences estimate (using 25 year-olds as the reference group). In this figure, coefficient estimates are plotted for the "enactment" period effects. Error bars are 95 percent confidence intervals.

Appendix Figure 2. Single-Year Age Estimates of the Implementation Effect of the DCM, NIBRS, 2008 to 2013 (Reference Age = 25)



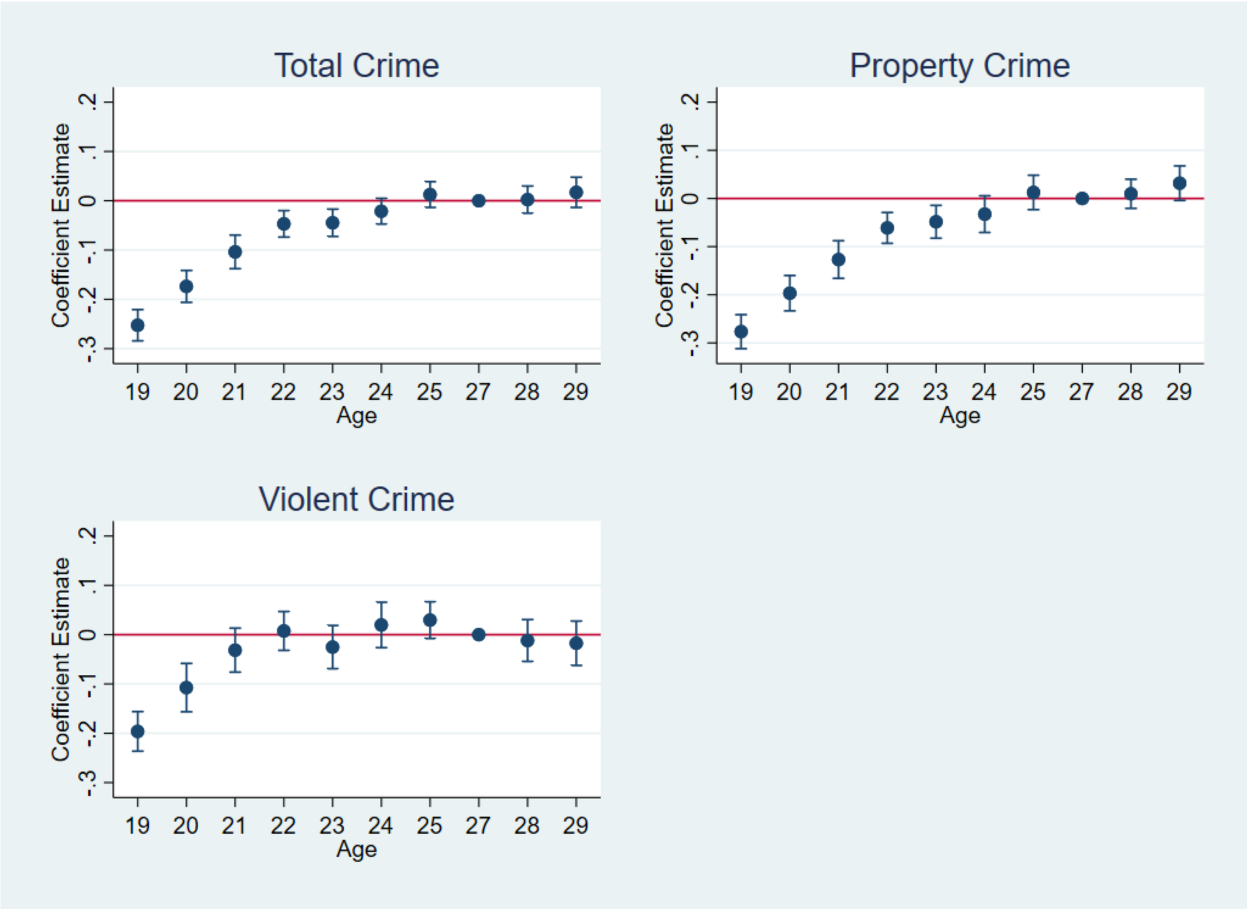
Notes: Coefficient estimates are presented from equation (3) using a Poisson Model. Each age has its own difference-in-differences estimate (using 25 year-olds as the reference group). In this figure, coefficient estimates are plotted for the "implementation" period effects. Error bars are 95 percent confidence intervals.

Appendix Figure 3. Single-Year Age Estimates of the Enactment Effect of the DCM, NIBRS, 2008 to 2013 (Reference Age = 27)



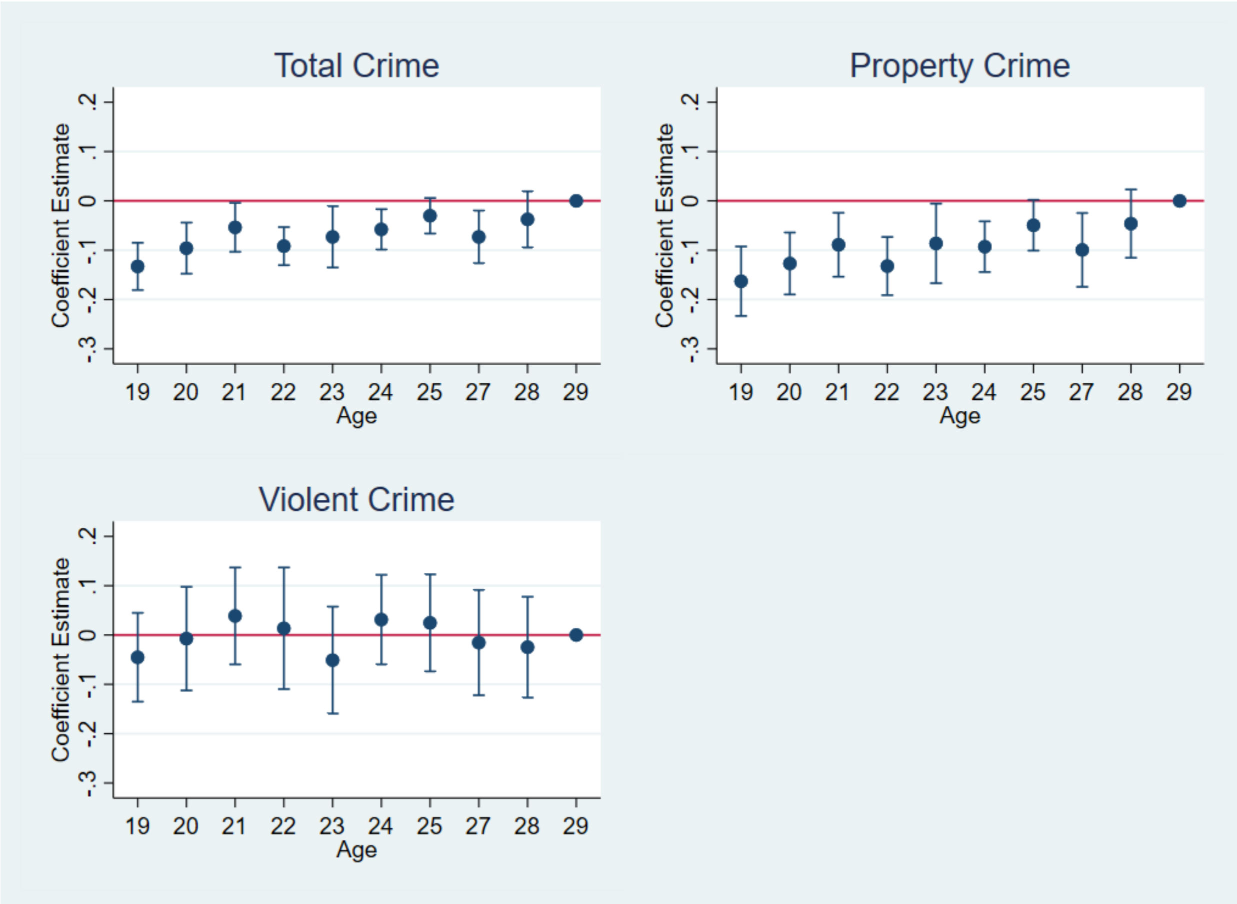
Notes: Coefficient estimates are presented from equation (3) using a Poisson Model. Each age has its own difference-in-differences estimate (using 27 year-olds as the reference group). In this figure, coefficient estimates are plotted for the "enactment" period effects. Error bars are 95 percent confidence intervals.

Appendix Figure 4. Single-Year Age Estimates of the Implementation Effect of the DCM, NIBRS, 2008 to 2013 (Reference Age = 27)



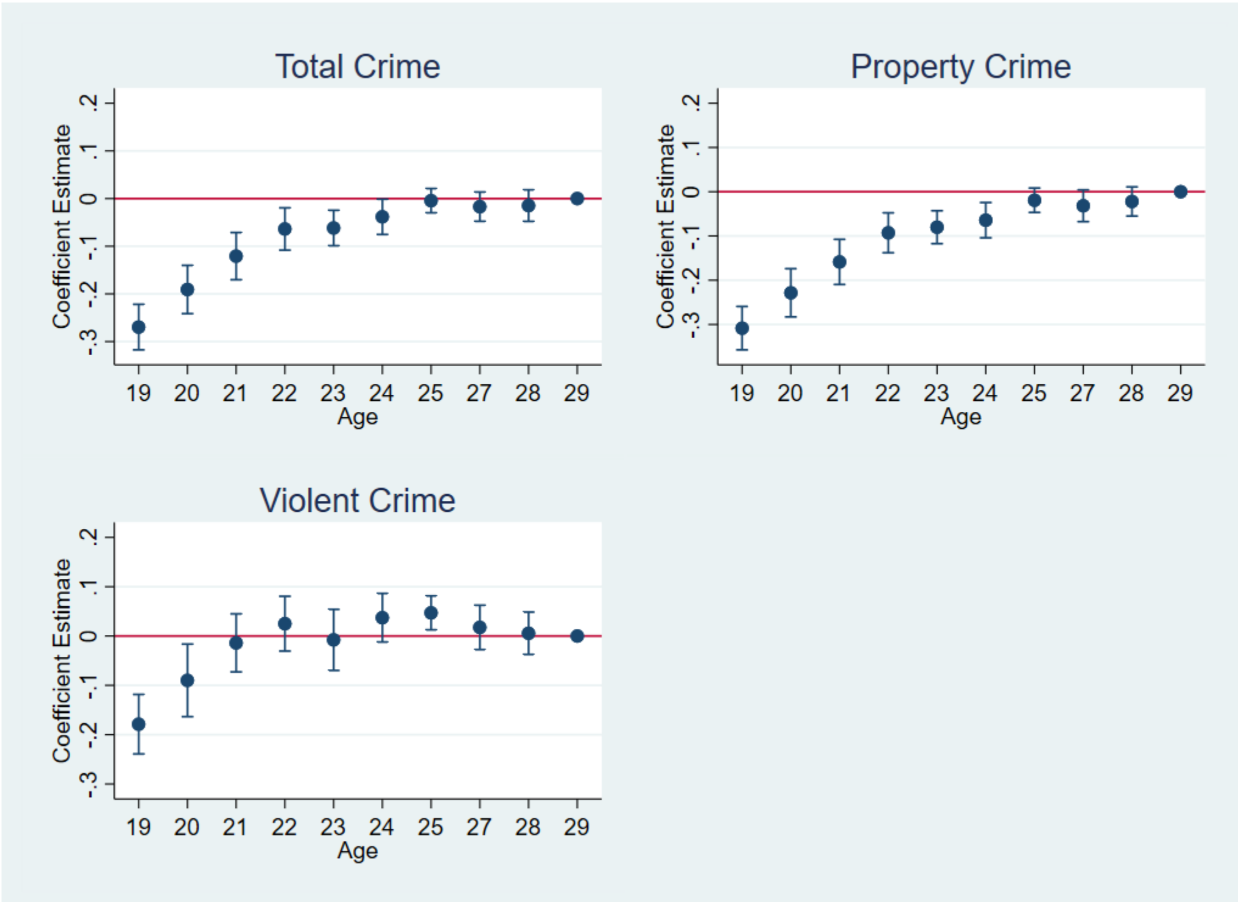
Notes: Coefficient estimates are presented from equation (3) using a Poisson model. Each age has its own difference-in-differences estimate (using 27 year-olds as the reference group). In this figure, coefficient estimates are plotted for the "implementation" period effects. Error bars are 95 percent confidence intervals.

Appendix Figure 5. Single-Year Age Estimates of the Enactment Effect of the DCM, NIBRS, 2008 to 2013 (Reference Age = 29)



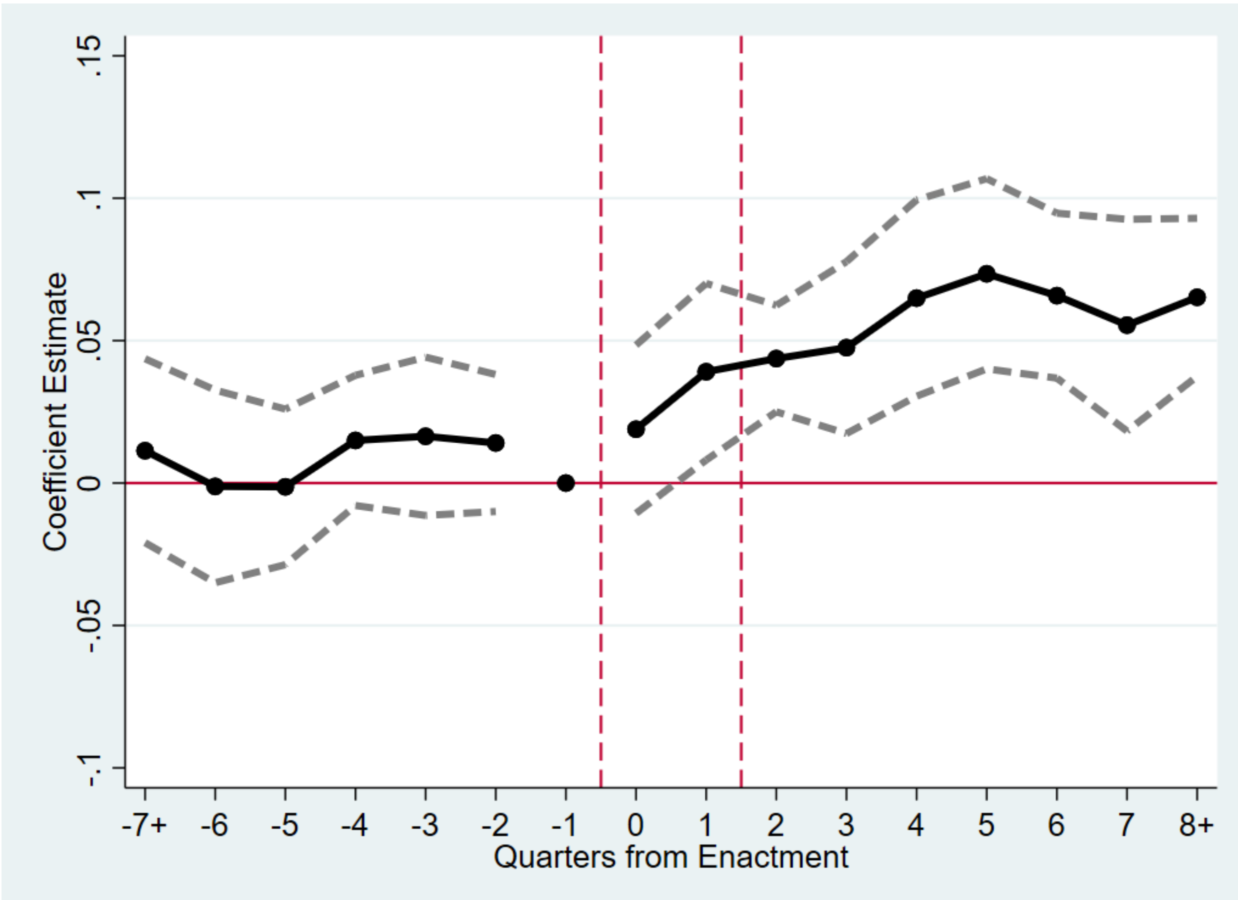
Notes: Coefficient estimates are presented from equation (3) using a Poisson model. Each age has its own difference-in-differences estimate (using 29 year-olds as the reference group). In this figure, coefficient estimates are plotted for the "enactment" period effects. Error bars are 95 percent confidence intervals.

Appendix Figure 6. Single-Year Age Estimates of the Implementation Effect of the DCM, NIBRS, 2008 to 2013 (Reference Age = 29)



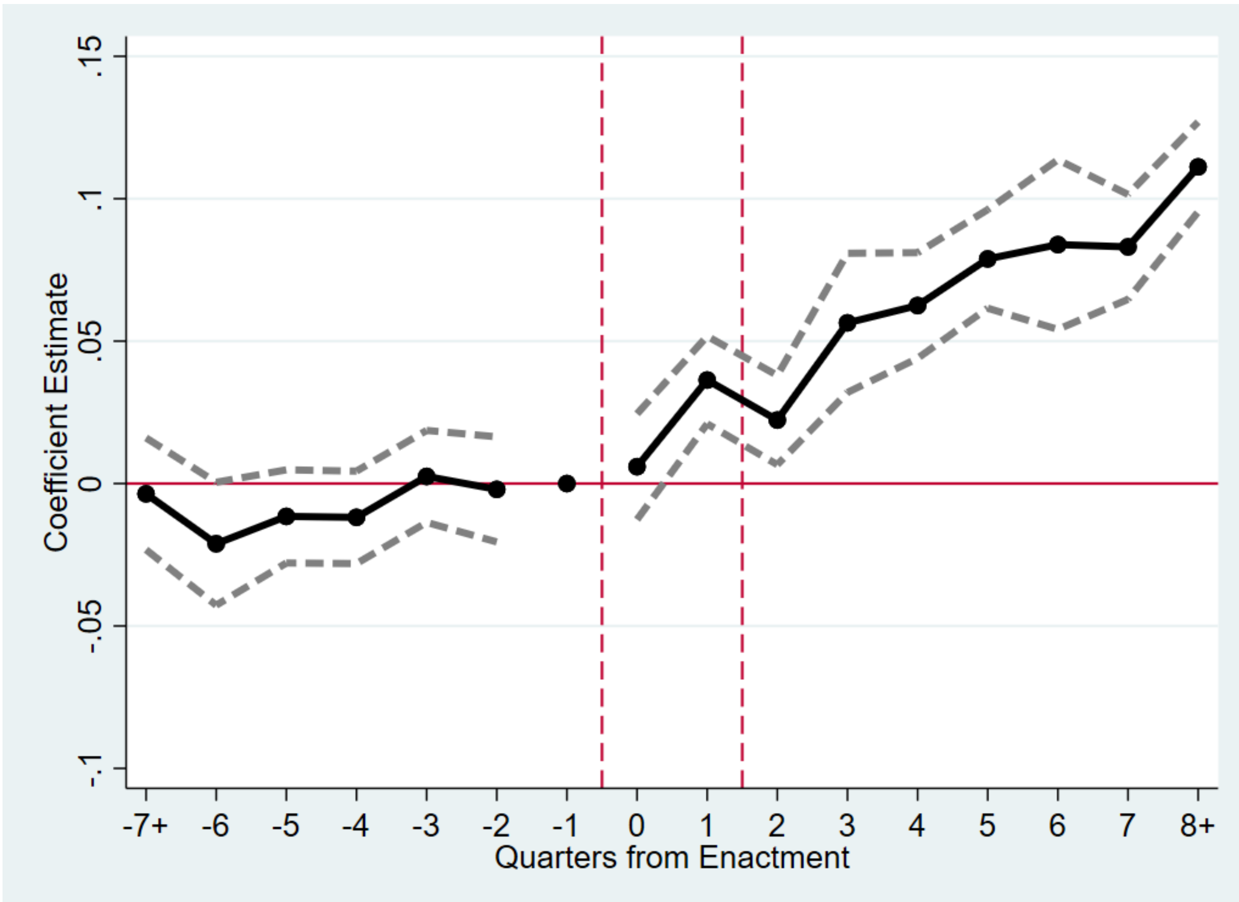
Notes: Coefficient estimates are presented from equation (3) using a Poisson model. Each age has its own difference-in-differences estimate (using 29 year-olds as the reference group). In this figure, coefficient estimates are plotted for the "implementation" period effects. Error bars are 95 percent confidence intervals.

Appendix Figure 7. Event Study Analysis of the Estimated Relationship Between the Dependent Coverage Mandate and Any Health Insurance Coverage, 19-to-25 vs. 27-to-29 year-olds, SIPP, 2008 to 2013



Notes: Event study coefficients for any health insurance coverage are plotted from an event study framework similar to equation (2), using data from the SIPP. The dependent variable is an indicator for any health insurance coverage. The dashed lines tracking the plotted coefficients are the 95 percent confidence intervals. The first dashed vertical line represents the "enactment" period, when the DCM was announced but not fully implemented. The second dashed vertical line represents the "implementation" period, when the DCM was fully implemented.

Appendix Figure 8. Event Study Analysis of the Estimated Relationship Between the Dependent Coverage Mandate and Dependent Health Insurance Coverage, 19-to-25 vs. 27-to-29 year-olds, SIPP, 2008 to 2013



Notes: Event study coefficients for dependent health insurance coverage are plotted from an event study framework similar to equation (2), using data from the SIPP. The dependent variable is an indicator for dependent health insurance coverage. The dashed lines tracking the plotted coefficients are the 95 percent confidence intervals. The first dashed vertical line represents the "enactment" period, when the DCM was announced but not fully implemented. The second dashed vertical line represents the "implementation" period, when the DCM was fully implemented.

Appendix Table 1. Summary Statistics, NIBRS, 2008 to 2013

	19-to-25			27-to-29		
	Mean	SD	N	Mean	SD	N
			Full Sample			
Total	3.858	6.522	639,996	2.666	4.250	274,284
Property	2.808	4.720	639,408	1.918	3.140	274,032
Violent	1.109	2.417	612,948	0.790	1.615	262,692
			Pre-DCM Enactment Period			
Total	3.822	6.516	231,140	2.519	4.102	99,060
Property	2.751	4.682	230,944	1.762	2.972	98,976
Violent	1.134	2.403	221,228	0.802	1.604	94,812
			DCM Enactment Period			
Total	4.071	6.613	53,340	2.765	4.339	22,860
Property	2.880	4.673	53,256	1.927	3.088	22,824
Violent	1.256	2.596	51,324	0.881	1.793	21,996
			DCM Implementation Period			
Total	3.849	6.512	355,516	2.746	4.326	152,364
Property	2.832	4.750	355,208	2.018	3.247	152,232
Violent	1.072	2.396	340,396	0.768	1.593	145,884

Notes: Weighted means (by estimated age-specific agency population) of counts of criminal incidents leading to an arrest presented for the NIBRS. The unit of observation is an agency-age-month. The sample is limited to a strongly balanced panel. “Pre-DCM Enactment Period” presents means from January 2008 through February 2010. “DCM Enactment Period” presents means for the period March 2010 through August 2010. “DCM Implementation Period” presents means from September 2010 through December 2013.

Appendix Table 2. Robustness of Estimated Crime Effects to Inclusion of Controls for Agency-by-Month-by-Year Fixed Effects, 19-to-25 vs. 27-to-29 year-olds, NIBRS, 2008 to 2013

	(1)	(2)	(3)
	Total Crime	*Property Crime*	*Violent Crime*
$\text{Treat}_i * \text{Enact}_t$	-0.045***	-0.063***	0.012
	(0.010)	(0.013)	(0.018)
$\text{Treat}_i * \text{Implement}_t$	-0.114***	-0.139***	-0.041**
	(0.013)	(0.013)	(0.019)
N	802,910	758,860	474,060

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: The dependent variable is the count of criminal incidents leading to an arrest in an agency-age-month. Models are estimated via Poisson and the sample is limited to a strongly balanced panel. All models include age, month-by-year, and agency-by-month fixed effects, as well as a control for state dependent coverage mandates. Standard errors are clustered at the state level.

Appendix Table 3. Robustness of Crime Estimates to Use of Various Control Groups, NIBRS, 2008 to 2013

	(1)	(2)	(3)
	Control Group = Age 27-to-29	Control Group = Age 30-to-34	Control Group = Age 35-to-39
		Panel I: Total Crime	
$\text{Treat}_i * \text{Enact}_t$	-0.045***	-0.068***	0.010
	(0.010)	(0.015)	(0.018)
$\text{Treat}_i * \text{Implement}_t$	-0.114***	-0.224***	-0.048*
	(0.013)	(0.021)	(0.025)
N	914,280	1,178,208	1,177,920
		Panel II: Property Crime	
$\text{Treat}_i * \text{Enact}_t$	-0.063***	-0.071***	0.017
	(0.013)	(0.017)	(0.023)
$\text{Treat}_i * \text{Implement}_t$	-0.139***	-0.244***	-0.063**
	(0.013)	(0.020)	(0.025)
N	913,440	1,176,624	1,176,192
		Panel III: Violent Crime	
$\text{Treat}_i * \text{Enact}_t$	0.012	-0.058***	-0.008
	(0.018)	(0.019)	(0.022)
$\text{Treat}_i * \text{Implement}_t$	-0.041**	-0.174***	-0.016
	(0.020)	(0.031)	(0.031)
N	875,640	1,129,680	1,122,624
Control Group	27-to-29	30-to-34	35-to-39

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: The dependent variable is the count of criminal incidents leading to an arrest in an agency-age-month. Models are estimated via Poisson and the sample is limited to a strongly balanced panel. All models include age, month-by-year, and agency-by-year fixed effects, as well as a control for state dependent coverage mandates. The treatment group for each model comprises of 19-to-25 year-olds. Standard errors are clustered at the state level.

Appendix Table 4. Estimated Effect of DCM on Crime Arrests Using Uniform Crime Reports Data, 2008 to 2013

	(1)	(2)	(3)	(4)
		Panel I: Total Crime		
$\mathrm{Treat}_i * \mathrm{Enact}_t$	-0.058***	-0.004	-0.065**	-0.010
	(0.020)	(0.014)	(0.031)	(0.024)
$\mathrm{Treat}_i * \mathrm{Implement}_t$	-0.193***	-0.038**	-0.190***	-0.030
	(0.025)	(0.014)	(0.036)	(0.053)
Pre-DCM Treatment Mean	1.606	1.261	1.606	1.261
N	405,412	405,652	405,412	405,652
		Panel II: Property Crime		
$\mathrm{Treat}_i * \mathrm{Enact}_t$	-0.041**	0.001	-0.041*	0.003
	(0.016)	(0.010)	(0.024)	(0.019)
$\mathrm{Treat}_i * \mathrm{Implement}_t$	-0.158***	-0.026***	-0.135***	0.001
	(0.023)	(0.010)	(0.028)	(0.047)
Pre-DCM Treatment Mean	1.187	0.889	1.187	0.889
N	405,412	405,652	405,412	405,652
		Panel III: Violent Crime		
$\mathrm{Treat}_i * \mathrm{Enact}_t$	-0.016**	-0.004	-0.025***	-0.013*
	(0.008)	(0.006)	(0.008)	(0.007)
$\mathrm{Treat}_i * \mathrm{Implement}_t$	-0.035***	-0.012*	-0.055***	-0.031***
	(0.007)	(0.006)	(0.011)	(0.010)
Pre-DCM Treatment Mean	0.419	0.372	0.419	0.372
N	405,412	405,652	405,412	405,652
Treatment Group	19-to-24	23-to-24	19-to-24	23-to-24
Control Group	25-to-29	25-to-29	30-to-34	30-to-34

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: The dependent variable is the criminal arrest rate per thousand population in a county-month-age group. Models are estimated via OLS and are weighted by the age group-specific county population. Models include controls for age group, month-by-year, and county-by-year fixed effects, as well as state dependent coverage mandates. Standard errors are clustered at the state level.

Appendix Table 5. Robustness of Estimated Crime Effects to Omission of Recession Years

	(1)	(2)	(3)
		Panel I: Total Crime	
$\text{Treat}_i * \text{Enact}_t$	-0.045***	-0.036***	-0.012
	(0.010)	(0.011)	(0.018)
$\text{Treat}_i * \text{Implement}_t$	-0.114***	-0.106***	-0.082***
	(0.013)	(0.014)	(0.021)
N	914280	685440	609480
		Panel II: Property Crime	
$\text{Treat}_i * \text{Enact}_t$	-0.063***	-0.055***	-0.028
	(0.013)	(0.013)	(0.020)
$\text{Treat}_i * \text{Implement}_t$	-0.139***	-0.131***	-0.105***
	(0.013)	(0.018)	(0.029)
N	913440	684480	608880
		Panel III: Violent Crime	
$\text{Treat}_i * \text{Enact}_t$	0.012	0.036	0.049
	(0.018)	(0.024)	(0.031)
$\text{Treat}_i * \text{Implement}_t$	-0.041**	-0.019	-0.006
	(0.020)	(0.016)	(0.021)
N	875640	652560	584040
Omitted Period	None	Jan 2008 through June 2009	Jan 2008 through Dec 2009

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: The dependent variable is the count of criminal incidents leading to an arrest in an agency-age-month. Models are estimated via Poisson and the sample is limited to a strongly balanced panel. All models include age, month-by-year, and agency-by-year fixed effects, as well as a control for state dependent coverage mandates. Standard errors are clustered at the state level

Appendix Table 6. Heterogeneity in Health Insurance Effects of the DCM by Race/Ethnicity, SIPP, 2008 to 2013

	(1)	(2)	(3)
	Non-Hispanic White	Black	Hispanic
	Panel I: Any Health Insurance		
Treat*Enact	0.021*	0.007	0.021
	(0.011)	(0.028)	(0.021)
Treat*Implement	0.054***	0.067*	0.039
	(0.010)	(0.036)	(0.027)
N	100,102	21,131	27,079
Pre-DCM Treatment Mean	0.736	0.584	0.469
	Panel II: Dependent Health Insurance		
Treat*Enact	0.031***	0.017*	0.005
	(0.008)	(0.010)	(0.012)
Treat*Implement	0.118***	0.048***	0.059***
	(0.010)	(0.011)	(0.010)
N	100,102	21,131	27,079
Pre-DCM Treatment Mean	0.282	0.171	0.109

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: Estimates presented for linear probability models weighted using SIPP person weights, with standard errors clustered at the state level. Models control for state, age, and month-by-year fixed effects, state-specific linear time trends, state-year-age group unemployment rate, as well as individual-level controls for gender, marital status, student status, and educational attainment. SIPP data are restricted to the fourth reference month (intended to reduce recall bias).