

Protecting Postville? The Impact of Deportation and Immigration on Crime

Abstract

A common perception is that the deportation of immigrants results in less crime and increased immigration raises crime. Using a 2008 worksite raid in Postville, Iowa and a 2006 worksite raid in Marshalltown, Iowa as natural experiments, we use a synthetic control method to study how deportation impacts local crime. Because the Postville worksite raid was followed by increases in immigrant workers in 2009, we also analyze how increased immigration impacts crime. Our analysis of both deportations suggests the removal of immigrants increased violent crime and decreased property crime, at least in the short term. Estimated effects of the 2009 inflow of immigrants suggest that new arrivals were accompanied by lower crime rates. Sensitivity analysis, however, suggests neither deportation nor increased immigration were the root causes of documented changes in crime. Rather, other latent factors influenced crime rates in Postville, Marshalltown, *and* other areas, irrespective of immigrant deportation or new immigrants. Our results imply major changes in immigration do not impact crime.

Keywords: immigration, deportation, crime, synthetic control method

JEL Codes: J1, J6, H7

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1. Introduction

Many voters consider deportation an appropriate policy response to unauthorized immigration (Talev and Contreras, 2024), in part because immigrants are associated with criminality (Einhorn, 2023). However, even though nearly three-quarters of U.S. citizens believe immigration increases crime (Spenkuch, 2014), much of the existing literature suggests otherwise. These studies span multiple disciplines, including criminology (Martinez and Lee, 2000; Wadsworth, 2010; Ousey and Kubrin, 2018), law (Mears, 2001), management (Butcher and Piehl, 1999), sociology (Reid et al., 2005), political science (Martinez and Stowell, 2012), and economics (Bell et al., 2010; Bell and Machin, 2013).

We add to the conversation surrounding immigrant deportation and crime with a novel and notably transparent natural experiment. Specifically, we study how a sudden worksite deportation in Postville, Iowa, which resulted in the arrest of 17% of the city's population, influenced local violent and property crime rates. This raid was the largest worksite raid in the United States at the time and quickly resulted in the bankruptcy of Postville's primary employer, Agriprocessors, Inc. The raid also resulted in a large outflow of Hispanic immigrants who were not employed by Agriprocessors. While the owners of Agriprocessors attempted to recover after the raid, the company quickly went bankrupt due to legal issues and an inability to replace their lost workforce. Almost two years after the raid, a relatively small influx of authorized Somali immigrants would begin work at Agriprocessors' replacement, Agri Star (Jones, 2012). These Somali immigrants numbered a quarter of the Hispanic population that was displaced from Postville due to the raid. As a robustness test, we also study the impacts of a relatively small raid in Marshalltown Iowa at the end of 2006. While the Marshalltown raid was much smaller than the Postville raid relative to

local populations, it represents a cleaner experiment in that there was no immediate influx of immigrants afterwards.

Understanding how deportations and immigration impact crime is important for several reasons. First, crime can have significant detrimental impacts on an economy (Detotto and Otranto, 2010; Carboni and Detotto, 2016). For example, crime increases uncertainty (Goulas and Zervoyianni, 2012), decreases business creation (Rosenthal and Ross, 2010), and lowers trust (Blanco, 2013). Thus, there are significant economic and social implications associated with a possible connection between deportation, immigration, and crime. Second, the removal of immigrants through deportation can cause social disorganization by breaking up established social ties and community integration (Leyro, 2013). Thus, sudden forced outflows of immigrants may have different impacts on crime compared to sudden voluntary inflows of immigrants. Finally, despite evidence on the contrary (Ousey and Kubrin, 2018; Ajzenman et al., 2023), immigrants are associated with increased crime by voters, resulting in a political demand for deportation or immigration restrictions as means of lowering crime (Einhorn, 2023; Ajzenman et al., 2023; Talev and Contreras, 2024). Therefore, it is important to develop systematic evidence on the actual impacts of deportation on criminal activity to better gauge the costs and benefits of deportation as a policy tool.

Our research lies at the intersection of a few separate, but related, lines of literature. On one hand, we build on a rich body of work that considers how immigration influences crime in host countries. At the micro level, some have compared the arrest rates of natives and immigrants and found that immigrants, particularly unauthorized immigrants, are less likely to engage in criminal behavior, though the probability of institutionalization is higher for unauthorized immigrants who arrive at younger ages (Bersani, 2014; Light et al., 2020; Gunadi, 2021). Others

have studied how changes in immigrant flows influence crime at the macro level. For example, Masterson and Yasenov (2021) study a change in law that drastically decreased refugee flows into the U.S. and find no impact of the policy on crime rates; Billy and Packard (2022) rely on a sudden increase in immigration in Miami induced by the Mariel Boatlift and find increases in crime that are driven by the demographic composition of new arrivals. More related to our work on unauthorized immigrants, Gunadi (2021) demonstrates that unauthorized immigration is not statistically significantly related to violent or property crime at the macro level. On the other hand, Knight and Tribin (2023) document increases in homicide following large increases in immigration from Venezuela; however, they also find these increases are driven by homicides involving Venezuelan immigrants as victims, not perpetrators. We add to this literature directly by studying how inflows of replacement immigrants late in 2009, almost two years after the Postville raid, influenced crime rates.

More recently, and more related to our analysis of the Postville raid, researchers have studied how immigration enforcement, deportation, and the forced removal of immigrants impact crime. Some have studied sanctuary policies, which make deportation and immigration enforcement more difficult, and secure community policies, which facilitate deportation, and found that increased (or decreased) deportation has no discernable effect on crime rates (Hausman, 2020; Miles and Cox, 2014; Kubrin and Bartos, 2020; Treyger et al., 2014). On the other hand, other research finds immigration enforcement does indeed impact crime. Chalfin and Deza (2020) show that an Arizona law requiring employers to check the legal status of workers resulted in less property crime, and Kang and Song (2024) demonstrate secure community policies lower local crime if also enacted in neighboring communities. In contrast, an Arizona law that allowed local law enforcement permission to enforce federal immigration law resulted in an *increase* in crime

(Norris, 2020). Stowell et al. (2013) provide more mixed evidence by showing the effects of deportation on crime depend on local context. Specifically, in MSAs along the U.S.-Mexico border, deportation is associated with less aggravated assault; in MSAs within the U.S. interior, deportation is associated with more violent crime and aggravated assault. Finally, Gunadi (2020) finds the implementation of the Deferred Action for Childhood Arrivals [DACA] program, which allowed some unauthorized immigrants who arrived as children to work, resulted in less property crime, with no noticeable changes to incarceration rates. Taken together, the current research on deportation, immigration, and crime suggests results depend on both local context and the nuances of different policies.

On the other hand, because our natural experiment involves the closure of a major employer and subsequent unemployment, we build off literature that examines the impact of unemployment on crime. Economic theory suggests unemployment should increase crime, because a lack of legal work decreases the opportunity costs of illicit activities (Becker, 1968). Empirical evidence supports theoretical predictions. While violent crime is less responsive to employment opportunities, a positive relationship between unemployment and property crime has been documented across multiple countries (Edmark, 2005; Raphael and Winter-Ebmer, 2001; Aaltonen et al., 2013).

Highly relevant to our study, the literature on immigration and crime has begun to merge with the broader literature on unemployment and crime. Indeed, recent research suggests the influence of immigration on crime is dependent on broader employment conditions and how immigrants impact, and respond to, those conditions (Reid et al., 2005). For instance, Borjas et al. (2010) argue that immigration increases crime at an aggregate level by increasing unemployment among workers with little education, who commit more crime as a result. Similarly, Freedman et

al. (2018) show that Hispanic crime rates increased significantly after employment opportunities for unauthorized immigrants were limited by the Immigration Control and Reform Act of 1986. Gunadi (2020) buttresses that finding by showing expanded employment opportunities provided by DACA increased the opportunity cost of property crime for DACA recipients and resulted in less profit-motivated crime.

We add to the above conversations by studying a notably transparent and sudden deportation that resulted in the immediate removal of a large portion of the local population. Specifically, we study how this deportation impacted property and violent crime. We recognize it is a criminal act for employers to hire unauthorized immigrants. At the same time, we are uninterested in how illegal immigration increases criminal activity in a tautological sense. Instead, we are interested in criminal activity that is not criminal solely based on the legal status of an immigrant. As such, we focus on popular conceptions of crime that involve a victim, which include property and violent crime. We follow a long strand of literature that focuses on violent and property crime (Wadsworth, 2010; Ousey and Kubrin, 2018; Treyger et al., 2014; Chalfin and Deza, 2020; Gunadi, 2020; Knight and Tribin, 2023; Billy and Packard, 2022)

We are the first, to our knowledge, to use a synthetic control method to analyze the impact of deportation on crime. While studies of sanctuary cities and deportations are similar in that both can be used to study the impact of immigrant removals (removals are higher in cities that are not sanctuary cities), our event of interest represents a much more sudden, exogenous event in that there is a mass removal of immigrants all at once (whereas removals are simply allowed or easier in cities that are not sanctuary cities). A similar argument can be made when comparing analyses of deportation with analyses of secure communities programs. This deportation is also more

exogenous than sanctuary laws or secure communities programs, because it is not an endogenously created local law/ordinance, but an exogenous raid carried out by Federal authorities.

Our work also contributes to more general ongoing debates surrounding immigration and its impacts on host countries. Despite fears of adverse effects, researchers typically find few. For example, extant literature typically concludes that immigrants have negligible impacts on the wages of natives, though the wages of earlier immigrants are negatively affected (Card, 1990; Butcher and Card, 1991; Ottaviano and Peri, 2012; Peri & Yasenov, 2019). Similarly, no meaningful effect of immigration on unemployment is documented, and those who look at the effects of deportation on native unemployment find a slight positive association (Withers and Pope, 1985; Winegarden and Khor, 1991; Lee et al., 2022). Recent literature also documents a medley of economic benefits for natives that are associated with increased immigration, including a lower gender wage gap, higher native education attainment, and higher economic growth (Cortés and Pan, 2019; Hunt, 2017; Clemens, 2011).

More recently, scholars have examined how immigration affects a destination country's institutional quality. Borjas (2015) argues that the economic benefits of immigration are overstated because immigrants may bring along their low-productivity culture and institutions. A number of studies conclude that no negative cultural or institutional effects are found (Clark et al. 2015; Nowrasteh, 2015; Powell et al., 2017; Clark et al., 2018; Padilla and Cachanosky, 2018; Forrester et al., 2019; Pavlik et al., 2019; Nowrasteh and Powell, 2020; Nowrasteh et al., 2020; Cachanosky et al., 2021; Yao et al., 2021; Yao et al., 2022). In each case, immigration has either a negligible or a positive impact on host countries' institutions. Our research complements the immigration-institutions literature by following others in studying one potential way immigration can impact

institutions: by impacting crime (Hausman, 2020; Miles and Cox, 2014; Leyro, 2013; Kang and Song, 2024; Norris, 2020).

Overall, we expect this sudden worksite deportation either increased crime or had no impact on crime. The Postville raid represents a discrete treatment with multiple reactions. The first reaction is the unanticipated and immediate removal of unauthorized immigrants, who made up a substantial portion of the population. Unauthorized immigrants are less likely to engage in criminal behavior compared to both legal immigrants and natives (Bersani, 2014; Light et al., 2020). This is likely because unauthorized individuals face a form of “double jeopardy” if caught – they risk not only prison sentences and fines but also deportation and the psychological and monetary costs that come with forced removal from the host country (Kidane, 2006). Because they face double jeopardy, unauthorized immigrants have a much higher incentive to avoid police attention (Bersani et al., 2018). Thus, if unauthorized immigrants make up a significant portion of the population and they are removed, we expect crime rates to increase.

The second reaction to this treatment involves a major employer closing down, resulting in more unemployment. Because of the robust relationship found between unemployment and higher crime (Edmark, 2005; Raphael and Winter-Ebmer, 2001; Aaltonen et al., 2013), we expect this reaction also leads to increases in crime.

On the other hand, we expect the 2009 increase in authorized Somali workers to lead to decreases in crime, for similar reasons. Because immigrants are generally less likely to commit crime compared to natives, a sudden influx of immigrants, particularly immigrants who already maintain gainful employment, should result in decreases in crime rates (Martinez and Lee, 2000; Wadsworth, 2010; Ousey and Kubrin, 2018).

Overall, we find minimal evidence that either the 2008 Postville raid or subsequent influxes of Somali immigrants almost two years later had significant impacts on crime in Postville. We do record scattered statistically significant results that suggest the raid led to increases in violent crime and decreases in property crime. We also report statistically significant estimates that indicate violent and property crime went down following influxes of Somali immigrants in 2010. However, our sensitivity analyses indicate other latent factors contributed to changes in crime within our sample period, as crime changed in cities outside Postville despite a lack of immigration raids or large inflows of immigrants. As a robustness check, we also study a smaller raid in Marshalltown, Iowa, at the end of 2006. While this raid was smaller than the Postville raid relative to local populations, it comes with the advantage of being a cleaner natural experiment in that the effects of the Marshalltown raid were not contaminated by inflows of immigrants soon after. Documented effects of the Marshalltown raid are remarkably consistent with those of the Postville raid.

Our results carry important policy implications. Namely, they imply changes in immigration, authorized or unauthorized, do not significantly influence crime rates. It seems the crime rates of immigrants are not significantly different enough from the crime rates of natives to impact crime, at least in our context. Given the Postville raid cost taxpayers at least five million dollars, our results also imply officials would do well to carefully consider the costs and benefits of raids, especially considering there could be other negative social impacts that accompany worksite raids like business closures and community disruptions (Juby and Kaplan, 2011).

2. Background and Theory

2.1 Background

Located in the northeast corner of Iowa, Postville is a small town with a population of roughly 2,270 (in 2008) spanning between Allamakee and Clayton counties; the east boundary of

Postville is one mile away from two other Iowa counties – Winneshiek to the northeast and Fayette to the southeast. The town lies less than 20 miles west of Wisconsin across the upper Mississippi River and within 30 miles south of the Minnesota border.

Before the raid, Agriprocessors, a meat packing plant and kosher slaughterhouse, was a local employer in the Postville area. Founded by a Hasidic butcher from Brooklyn in 1987 as northeast Iowa underwent an employment crisis, Agriprocessors created some 350 jobs at its founding and around 1,000 jobs just before the raid (Toral and Contreras, 2019). The opening of this plant also led to Postville and northeast Iowa attracting historically large numbers of immigrants from a variety of countries to a state otherwise known for its racial and ethnic homogeneity (Popper, 2008).

On May 12, 2008, the U.S. Immigration and Customs Enforcement [ICE] division of the Department of Homeland Security [DOH] conducted its largest immigration raid at that time at Agriprocessors, Inc. Approximately 900 ICE agents descended on the plant in a coordinated operation that was planned for months in order to maintain an element of surprise and avoid alerting any of Agriprocessors' employees or management (U.S. Attorney's Office Northern District of Iowa, 2010). By the time helicopters could be heard buzzing overhead, ICE was already making arrests (Crowder and Elmer, 2018). In the process, 398 employees were taken into custody, or 17% of Postville's population. ICE did not have complete knowledge of which workers were unauthorized, but presumed Hispanic ethnicity to identify suspected unauthorized immigrants (Novak et al., 2016). Out of 389 arrested employees, 300 were convicted on document fraud charges, and 297 served a five-month prison term before being deported. Most of those arrested were from Guatemala and Mexico (Juby and Kaplan, 2011). Because of family separation, disruptions to work and school, and anxiety about further potential immigration raids, the Postville

raid drove close to 1,000 Mexican and Guatemalan residents, about a third of Postville's population, out of the town (Jones, 2012).

After the raid, Agriprocessors tried to recover, but an inability to replace their lost workforce, coupled with legal issues arising from the raid, forced the plant to close down five months later. This resulted in the displacement of the rest of Agriprocessors' workers, including a large portion of Postville's native Jewish population (Tapper, 2016). A little less than two years later, the plant was bought by Hershey Friedman and renamed Agri Star. In many ways, Agri Star resembles its predecessor. Agri Star continues to be a meat and poultry processing plant, and it remains Postville's largest employer. The company uses E-Verify (a web-based employment eligibility verification system) to avoid hiring unauthorized workers; however, a large number of immigrants from developing countries, like Somalia, were hired in late 2009 after the company changed hands and the owner invested in new technology. Agri Star is unlike its predecessor in that unauthorized immigrants are not hired, but the tradition of hiring immigrants from developing countries continues. Postville saw significant decreases in and disruptions to its immigrant population in the initial years following the raid; however, by 2010, we observe increases in immigrant populations that totaled about two-thirds of Agriprocessors' immigrant workforce immediately before the raid, mostly consisting of Somali refugees (Jones, 2012; Crowder and Elmer, 2018).

When considering the impact of this sudden, concentrated deportation effort, there are a few reactions to consider. First, the raid resulted in the forced removal of a substantial portion of the local population, all of whom were unauthorized immigrants, and an exodus of many other Hispanic immigrants. Second, because the raid resulted in a major employer closing down, unemployment increased. There are theoretical reasons, and accompanying empirical support,

suggesting both responses should lead to increases in crime at the local level. On the other hand, when we consider the large inflow of replacement immigrants in late 2009, we expect to document decreases in local crime.

2.2 Theoretical Mechanisms

First, a sudden deportation may influence crime rates because it results in the removal of unauthorized immigrants. If unauthorized immigrants are more prone to crime than others, successful deportation efforts should curb local crime rates; if the reverse is true, successful deportation efforts should either have no effect on crime or lead to increases in local crime rates as individuals less prone to committing crimes are removed from the population. Immigrants may also increase or decrease local crime through their influence on social life (Ousey and Kubrin, 2018).

Historically, sociologists and economists have presumed increased immigration leads to more crime through selection mechanisms and impacts on social life and labor markets (Ousey and Kubrin, 2009). For example, some have argued immigrants are, on average, more likely to possess a crime-prone demographic profile, because immigrants are, on average, younger compared to natives (Ousey and Kubrin, 2018). Others suggest that immigration causes social disorganization by increasing ethnic heterogeneity, poverty, and residential turnover (Stowell et al., 2013). This heterogeneity is thought to stifle informal mechanisms of crime control by disrupting close social ties.

However, extant literature and accompanying empirical evidence largely suggests immigrants are less likely to commit crime than natives, and unauthorized immigrants are less prone to committing crime compared to both natives and legal immigrants. These results also hold across multiple country contexts with few exceptions (Butcher and Piehl 2007; Light et al. 2020;

Bersani 2014; Marie and Pinotti, 2024). This common finding is often attributed to the fact that unauthorized immigrants face different forms of deterrence compared to others. While unauthorized immigrants face prison terms *and* deportation if caught committing a crime, others have a more secure stay (Kidane, 2006). Thus, based on the idea that crime is marginally more likely when the expected punishment for being caught committing that crime is relatively low (Becker 1968), unauthorized immigrants should be *less* likely to commit crimes relative to others. A large body of literature supports this notion, including literature showing crime rates of second-generation immigrants assimilate *upward* to match near parity with high native crime rates relative to first-generation immigrants (Bersani, 2014).

Furthermore, a large-scale deportation of immigrants can impact crime because of disruptions in social cohesion. These disruptions, as well as the fear of further deportation, often incentivize unauthorized immigrants to withdraw from key aspects of community life, impeding integration and unauthorized immigrants' willingness to aid in local efforts to informally control crime (Leyro, 2013; Leyro, 2017). Deportation also causes social disruptions by removing established members of local communities. In the case of Postville, the sudden removal of a large portion of established members of the community had ripple effects, including the foreclosure of houses and disruptions to local small businesses (Baxter, 2017).

One major social disruption that accompanied the Postville raid was an increase in unemployment. Prior to the raid, Agriprocessors did not only employ unauthorized immigrants – they also employed authorized immigrants and natives, including around 90% of Postville's Jewish adults (Tapper, 2016). And because the raid resulted in disruptions to local small businesses (Juby and Kaplan, 2011), many were forced to close, leaving town natives and authorized immigrants alike unemployed (Olivo, 2009). Economic theories of crime (Becker, 1968) predict

unemployment to cause more crime, because the expected opportunity costs of committing crime are lower for the unemployed, who have less to lose if caught. Indeed, empirical work largely suggests unemployment increases crime, particularly property crime (Edmark, 2005; Raphael and Winter-Ebmer, 2001; Aaltonen et al., 2013). More closely related to our study, work has also been done on the intersection between immigration, unemployment, and crime. For instance, Borjas et al. (2010) provide suggestive evidence that immigration increased crime in the late 20th century by leading to unemployment among black people, who then became more likely to commit crime. And Freedman et al. (2018) demonstrate that Hispanic crime rates increase after legislation making it more difficult for unauthorized immigrants to find work. Gunadi (2020) supports that finding by showing expanded employment opportunities provided by DACA increased the opportunity cost of property crime for DACA recipients and resulted in less profit-motivated crime. Collectively, these results suggest that the theoretical and empirical relationship between immigration and crime is heavily dependent on employment dynamics. When immigration or increases in immigration enforcement lead to unemployment, crime should increase.

In light of the above theories and empirical evidence, we hypothesize the Postville raid led to increases in crime. Not only did the raid result in fewer unauthorized immigrants and authorized immigrants, who commit less crime compared to others (Light et al., 2020), but the raid also resulted in unemployment, which should result in more crime (Edmark, 2005; Raphael and Winter-Ebmer, 2001; Aaltonen et al., 2013). Related, we hypothesize subsequent influxes of Somali immigrants in 2010 resulted in less crime, as immigrants are generally less likely to commit crime compared to natives (Martinez and Lee, 2000; Wadsworth, 2010; Ousey and Kubrin, 2018).

3. Data and Methodology

3.1 Data

The size and location of Postville, which is essentially located at the corner of four counties, make it difficult to empirically examine the effects of the ICE worksite raid on local crime rates when using city/town-level data, as any impact on crime rates could easily spill across city/town lines. The fact that we are unable to track where unauthorized immigrants reside compounds this issue. More importantly, there is limited city/town-level data in our treatment area. Therefore, we rely on a county-level dataset.

Given Postville's location at the corner of four counties, we construct a treated area that includes the Iowa counties of Allamakee, Clayton, Winneshiek, and Fayette. This range happens to largely overlap with a radius of 20 miles centered around Postville. Refer to Figure 1 for a map of the location of Postville and the four treated counties in Iowa.

[Insert Figure 1 about here]

Our choice to include a 20-mile radius outside Postville in our treated area is intentional. One challenge of this research is the impossibility of tracing the addresses of unauthorized immigrants. Therefore, we cannot rule out the possibility that some of them may live outside Postville. At the same time, it is not unreasonable to assume that unauthorized immigrants live relatively close to their workplaces. Indeed, unauthorized immigrants are more likely to live close to their workplace in order to avoid unnecessary driving and the possibility of being caught after being pulled over for a traffic violation (Blumenberg and Smart, 2011). So, we opt for a treatment area that comprises a 20-mile radius centered around Postville. Doing so accounts for potential spillover effects and the possibility that some unauthorized immigrants working for Agriprocessors lived outside Postville without imposing an unrealistic treatment radius.

The data for our dependent variables, annual crime rates at the county level, are collected from the National Neighborhood Data Archive: Crimes by County dataset [NaNDA]. The NaNDA

is a dataset compiled from the Universal Crime Reporting Program Data Series at the National Archive of Criminal Justice Data [NACJD]. This dataset includes crime rates between 2002 and 2014 and differentiates between violent crime and property crime. Violent crime aggregates four types of offenses involving force or threat of force: murder and nonnegligent manslaughter, rape, robbery, and aggravated assault. Property crime refers to offenses involving taking money or property without force or threat of force against the victims and includes burglary, larceny-theft, motor vehicle theft, and arson.¹ Although similar to the crime data from the FBI's Uniform Crime Reporting [UCR] program, the NaNDA is preferable in our analyses for two reasons: first, UCR data is only available at the police department level, while the NaNDA records data at the county level; second, the NaNDA accounts for missing police department data by subtracting those jurisdictions' populations from total population counts. This is vital for accurately calculating county-level per capita crime rates. Without this adjustment, missing crime data would cause crime rates to be underestimated.

Following Billy and Packard (2022), we also collect data on county-level criminal incident predictors, including law enforcement employees per capita, the logarithm of median income, the percentage of the population with less than a high school diploma, the unemployment rate, population density, the share of the population that is black, the share of the population that is Hispanic/Latino, the share of the population that is born abroad, and the poverty rate. Except for data on law enforcement employees per capita, which we collect from the FBI's UCR data, data for the other eight variables are all from the U.S. Census Bureau and its intercensal estimates. Additionally, we also include five lagged terms of our outcome variables of interest, crime rates,

¹ While arson is technically a property crime offense, it is *excluded* from aggregate property crime rates. However, arson offenses are calculated separately in the dataset, so we are able to analyze them.

in the preintervention period as control variables.² Refer to Table 1 for a summary of descriptive statistics.

[Insert Table 1 about here]

Since ICE raided Agriprocessors in Postville, IA, on May 12, 2008, the year 2008 is the intervention year when we study the Postville Raid as a natural experiment; during this analysis, 2002-2007 (2002-2009 when studying the subsequent influxes of Somali immigrants) constitutes the preintervention period and 2008-2014 (2010-2014 when studying increases in Somali immigration) the postintervention period. To lend more internal validity to our results, we restrict our sample to include only counties in Iowa – this ensures our results are not contaminated by differences in state-level institutions between Iowa and border states.

Our final sample includes a total of 1,157 balanced panel observations from 92 Iowa counties between 2002 and 2014. While there are a total of 99 counties in Iowa, we further exclude seven Iowa counties that hosted ICE raids from 2002-2014. These counties can be identified in Figure 1 and include Woodbury, Black Hawk, Marshall, Tama, Benton, Polk, and Pottawattamie (Webb, 2012; Henderson, 2010; The Daily Iowan, 2008; Kammer, 2009). While the ICE raids in these seven counties were minor compared to the Postville Raid, relative to local populations, it is still important to exclude them from our analysis to avoid bias in our empirical estimations.

3.2 Methodology

To determine the effects of the 2008 Postville raid as well as the inflow of replacement immigrants in late 2009, this study employs a popular policy-impact evaluation technique in

² The method this paper relies on, the synthetic control method, requires dropping one lagged outcome variable from the preintervention period as a control to avoid overfitting the model; therefore, in each specification, we drop the one lagged outcome variable from our preintervention period that optimizes the fitness of the specification and minimizes the root mean square prediction error [RMSPE] in the preintervention period. See our methodology section for more details. Appendixes 1 and 2 show which lagged outcome variables are dropped in each of the specifications.

economics and political science – the Synthetic Control Method [SCM] developed by Abadie and Gardeazabal (2003), Abadie et al. (2010), and Abadie et al. (2015). The SCM is a data-driven statistical method that constructs a “synthetic” control group based on a weighted combination of “donors” from a pool of observations that has not been affected by the shock. This “synthetic counterfactual” is then contrasted with the treated group to investigate possible policy effects. Related to this study, the SCM is also often used to study the effects of immigration on host country institutions (Powell et al., 2017; Pavlik et al., 2019; Nowrasteh et al., 2020; Yao et al., 2021; Yao et al., 2022).

A more recent and closely related study utilizing the SCM is work by Billy and Packard (2022). This study evaluates the crime effects of the 1980 Cuban refugee crisis, better known as the Mariel Boatlift, and suggests that a large influx of Cuban immigrants seemingly increased property crime and murder rates in the Miami metropolitan area. At the same time, the authors find the effects are entirely compositional and explained by young adult male migrants. The findings of this paper motivate our current analysis.

We build from this work by utilizing a different case study focusing on the sudden deportation of immigrants. Thus, we can account for impacts a sudden deportation of immigrants may have that a sudden influx of immigrants may not have, including immediate disruptions to employers and a broad displacement of the local population. We further build on Billy and Packard (2022) by also studying how a subsequent inflow of replacement immigrants in late 2009 impacts crime rates.

Following constructions in pioneering SCM research, we define $J + 1$ ($j = 1, \dots, J$ and $J \leq 91$) as the total number of counties employed in the SCM specification³, where j

³ J in our paper is up to 91, as we have a total of 92 counties in the sample.

=1 denotes the treated group (the four counties surrounding the city of Postville) and the rest of the j counties are candidates for the donor pool used to construct a “synthetic” counterfactual control group. This “synthetic” control group is calculated as a weighted sum of donor counties, where the “optimal weight” vector $W^* = (w_1, \dots, w_j)$ is determined such that differences between the treated group and the “synthetic” control group are minimized in the *preintervention* period. This optimal weight, W^* , in the *preintervention* period is then used to construct the synthetic counterfactual in the *postintervention* period.

Mathematically, the treatment effect is estimated as $Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt}$, where t ($t = 1, \dots, 13$) denotes any year between 2002 and 2014, Y_{1t} denotes crime rates of the treated group at year t , Y_{jt} denotes crime rates of county j from the donor pool at year t , and w_j^* is the optimal weight assigned for a donor county j from the relevant *preintervention* period. This estimator measures the differences in crime rates between our treated counties and the crime rates of a synthetic region that would have been observed had ICE never raided Agriprocessors in Postville, IA, on May 12, 2008.

4. Empirical Results

This section presents our SCM estimated effects. We begin with an analysis of the 2008 Postville raid and continue with an analysis of the subsequent influx of legal Somali immigrants in 2010.

4.1 The Postville Raid

A critical step to obtaining effective SCM estimators is to verify the preintervention fitness of the predictors - the characteristics of the synthetic control group should resemble that of the actual treated group in the preintervention period. In Appendix 1, we provide the goodness of fit for our crime rate predictors. Our synthetic predictors match the treated predictors well in the

preintervention period, 2002-2007. The root mean square percentage error [RMSPE], measuring the overall deviation of the synthetic control characteristics from characteristics of the treated area in the preintervention period, is low in all specifications. Specifically, we document an RMSPE of 0.704 percent for our total violent crime specification and an RMSPE of 0.637 percent for our total property crime specification. There is no specification in which the RMSPE exceeds 1.606, except for our specification focusing on larceny. In that case, the RMSPE is 13.969. Appendix 2 provides an overview of donor counties and their estimated weights.

Figure 2 visualizes our main SCM findings related to the 2008 Postville raid, with each panel focusing on a different type of crime as our outcome variable of interest. In every panel, we observe relatively close pre-treatment crime trends for both our treated area and our synthetic control.

In Panel A, we observe a closely coincident synthetic control aggregate violent crime rate trend (maroon dashed line) with the actual treated aggregate violent crime rate trend (navy solid line) before 2007.⁴ However, there is a distinct diversion after 2007 (the last year of the preintervention period). The lower synthetic control aggregate violent crime rate relative to actual treated group crime rates during most of the post-intervention period (2008-2014) suggests that violent crime in Postville would be lower had ICE not conducted their raid in 2008.

In Panel B, the actual treated aggregate property crime rate seems to be lower than the synthetic control property crime rate in 2008 but higher than synthetic control property crime rates in subsequent years. These results suggest that the Postville raid may have *increased* violent crime rates and had mixed effects on property crime rates.

⁴ Small deviations in single years throughout our specifications are due to the intentional omission of crime rates that year from the predictors to avoid removing the power of the other predictors. This slight deviation disappears once we include crime rates in the omitted year back into the specifications, indicating the importance of these lagged outcome variables as predictors in the pre-intervention periods.

Following Panels A and B, Panels C-J investigate eight sub-index crime measures: per capita rates of murder and nonnegligent manslaughter, rape, robbery, aggravated assault, burglary, larceny theft, motor vehicle theft, and arson. These panels suggest that the Postville raid led to decreases in rates of murder, rape, robbery, and burglary; increases in assault and larceny; and mixed effects on arson.

[Insert Figure 2 about here]

While Figure 2 provides visually intuitive results, results are not statistically significant in all specifications. To determine the significance, we calculate the standard p-values of the pseudo t-statistics for the treatment effects. These p-values can be found in Appendix 3, which reports standard joint post-treatment p-values as well as p-values by year for each crime indicator.

Only results related to aggregate property crime and burglary exhibit a standard joint post-treatment p-value of less than or equal to 0.1, indicating significance within the 10% level. We also see standard joint post-treatment p-values approach significance for overall violent crime and arson (p-value = 0.102) as well as motor theft (p-value = 0.114) and assault (p-value = 0.125). Indeed, when we focus on individual years, we document significant results immediately after the raid (2008 and 2009) that suggest total violent crime and assault increased. This tracks well with case-study evidence showing arrests increased in Postville in the fall and winter following the raid (Jones, 2012). We also document scattered significant results that suggest murder, larceny, motor theft, and arson decreased after the raid, but none of these results are consistent over more than one year, and most begin to approach statistical insignificance. Overall, these results provide suggestive evidence that the Postville raid led to increases in violent crime and decreases in property crime.

Still, these significant pseudo p-values may not mean that the Postville raid is behind changes in crime rates. To determine if the Postville raid is a likely cause of documented crime rate trends, we perform a few sensitivity checks.

First, we wish to rule out the possibility our results are sensitive to donor selection. To do this, we perform a “leave-one-out” test, which iteratively excludes the donors with the largest weights from our donor pool and replicates Figure 2. This analysis can be found below in Figure 3. Figure 3 contrasts the treated trend (navy solid line) and synthetic control trend (maroon dashed line) from Figure 2 with multiple replicated trends (grey solid lines), where each replicated trend is a synthetic control trend with one influential donor left out. If our results in Figure 2 are credible, the maroon dashed trend representing our synthetic control crime trends in Figure 3 should be relatively closer to the cluster of the replicated grey trend lines than it is to the blue solid line that represents actual crime rates in our treated area. The majority of replicated trends are closer to the synthetic control trends. While some replicated trends show major deviations from our synthetic control trends, these replicated trends also come with significantly worse pretreatment fits compared with our synthetic control trends. These results suggest our findings are not influenced by single donors, and when they are, pretreatment fits become significantly worse. Overall, our leave-one-out tests justify our donor pool selection and lend validity to our empirical design.

[Insert Figure 3 about here]

We also perform in-place placebo tests to help determine if the effects we document are genuine treatment effects from the Postville raid. An in-place placebo test contrasts the treatment effects from the treated counties with those from all untreated donors, where we pretend each of these donor counties was also raided by ICE on May 12, 2008. If the effects we document in Figure 2 actually are due to the Postville raid, we should see the greatest treatment effects when

identifying the four counties surrounding Postville as treated. On the other hand, we should see minimal treatment effects when we pretend the donor counties were treated because these counties did not actually host the raid.

The results of these tests are shown below in Figure 4. Figure 4 shows effects of the immigration raid (navy line) on the violent and property crime rates of the four counties surrounding Postville are neither significantly larger nor smaller compared to placebo effects that are documented when we pretend donor counties were raided (grey lines). The navy line in all panels hovers around zero relative to lines signifying placebo effects, meaning that crime rates changed in our donor counties irrespective of the raid in Postville. This suggests that even though we have observed some fragmentary significant treatment effect estimates in Figure 2, they are unlikely due to the Postville raid on May 12, 2008. Instead, documented changes in crime rates are likely due to some other factors that we have not captured in our analysis; otherwise, we would have observed the most significant treatment effects within the actual treated counties.

Combined, these tests suggest that the Postville raid does not appear to have impacted violent or property crime. Instead, crime rates changed across Iowa irrespective of any immigration raids or large increases in immigrant workers. It seems some other latent factors influenced crime rates in Postville *and* other areas, irrespective of immigrant deportation.

[Insert Figure 4 about here]

4.2 Influx of New Replacement Immigrants in 2009

Though some authorized Somali immigrants came immediately after the raid to fill in vacancies left by recently deported Hispanic immigrants, the bankruptcy of Agriprocessors delayed this influx of Somali immigrants until just under two years after the Postville raid, in October 2009, when the owner of Agriprocessors' successor, Agri Star, updated the technology of

the plant and helped reintroduce immigrant workers. These authorized immigrant workers totaled about two-thirds of the unauthorized workers Agriprocessors employed in 2008 and about a quarter of the total Hispanic population that left after the raid (Jones, 2012). Though these Somali workers constituted only a fraction of the Hispanic population that left immediately after the raid, it is important to test the extent to which the reintroduction of immigrant workers impacted local crime rates because this reintroduction could attenuate the effects of the Postville raid on crime. To test that possibility, we perform an additional synthetic control analysis using the reintroduction of immigrant workers as our natural experiment of interest⁵.

Figure 5 provides a visual representation of this second set of results. Because the plant reopened at the end of 2009, 2010 becomes our first post-treatment year in this second synthetic control analysis. Standard joint post-treatment p-values are 0.10 or below, suggesting statistical significance within the 10% level, for six of our crime indicators: violent crime, murder, rape, assault, burglary, and arson.⁶ For each of these six indicators, our results mostly suggest crime went down after new immigrants arrived in Postville in 2010. Therefore, it could be the case that the wave of new 2010 immigrants attenuated the crime effects of the Postville raid.

[Insert Figure 5 about here]

To get an idea of the extent to which significant p-values in Figure 5 are due to the wave of immigrant arrivals in late 2009, we perform a few sensitivity analyses. We start with a leave-one-out analysis, found below in Figure 6. Much like our sensitivity analyses of the 2008 raid, while some replicated trends show major deviations from our synthetic control trends when we analyze inflows of immigrants in late 2009, these replicated trends also come with significantly

⁵ Appendix 4 provides goodness of fit for our crime predictors in this second SCM analysis and indicates a low RMSPE. Appendix 5 provides an overview of donor counties and their estimated weights.

⁶ P-values for this analysis can be found in Appendix 6, which reports standard joint post-treatment p-values as well as p-values by year for each crime indicator.

worse pretreatment fits compared with our synthetic control trends. These results suggest our findings are not influenced by single donors, and when they are, pretreatment fits become significantly worse. Overall, our leave-one-out tests justify our donor pool selection in this analysis and lend validity to our empirical design.

[Insert Figure 6 about here]

Finally, for added measure, we perform in-place placebo tests, where we pretend donor counties were the destinations of the influx of Somali immigrants in late 2009. These results can be found below, in Figure 7. If the effects we document in Figure 5 are due to increases in immigration, we should see the greatest treatment effects when identifying the four counties surrounding Postville as treated. On the other hand, we should see minimal treatment effects when we pretend the donor counties were treated.

[Insert Figure 7 about here]

Violent and property crime rates of the four counties surrounding Postville (signified by the navy line) are neither significantly larger nor smaller compared to placebo effects that are documented when we pretend donor counties received an influx of immigrants (grey lines). The navy line in all panels hovers around zero relative to lines signifying placebo effects, meaning that crime rates changed in our donor counties irrespective of subsequent inflows of immigrants. This suggests that even though we have observed some significant treatment effects in Figure 5, they are unlikely due to increased immigration. Again, as in our analysis of the 2008 Postville raid, documented changes in crime rates are likely due to some other latent factors that we have not captured in our analysis; otherwise, we would have observed the most significant treatment effects within the actual treated counties. Overall, influxes of immigrants a few years after the Postville raid do not appear to have impacted violent or property crime.

Our results indicate that the ICE worksite immigration raid in Postville is not robustly related to local violent or property crime rates and certainly does not seem to lower crime rates in a meaningful way. We also find subsequent inflows of replacement immigrants into Postville in 2010 did not impact crime rates in a meaningful way.

4.3 Robustness Check – 2006 Marshalltown Immigration Raid

Our results indicate neither the Postville Raid, accompanied by a large exodus of Hispanic immigrants, nor subsequent increases in Somali immigrants impacted crime. However, we must be careful in our interpretation of this finding. In particular, the null result we document for the years 2010 onward could arise if the Postville raid resulted in more crime and the subsequent inflow of Somali immigrants resulted in less crime; however, the null result we document from 2010 onward could also arise if both treatments had the opposite effect. That is, our results are also consistent with the Postville raid lowering crime rates in conjunction with subsequent inflows of Somali refugees increasing crime rates.

These competing explanations for our results limit what our analysis can definitively say about the effects of both the Postville raid and subsequent inflows of Somali workers. Still, there is little evidence that the raid affected crime one way or the other, though we document weak evidence that it caused an increase in crime in the very short term (2008-2009). If the raid did cause any increase in crime after 2009, that increase was not large enough to be detected amidst other shocks to Postville, including the 2009 arrival of Somali workers.

Our results also provide little evidence that the increase in Somali immigrants at the end of 2009 increased crime rates, and we provide weak evidence these Somali immigrants lowered crime rates. Still, our results are also consistent with long-term negative effects of the raid on crime in conjunction with increases in crime due to the Somali immigrants. However, if the Somalis caused

any increase in crime, it must have been fully offset by longer-term negative effects on crime from the removal of Hispanic immigrants. Therefore, our empirical exercises have given us novel evidence about the short-run impact of removing unauthorized immigrants while simultaneously restricting the space of things it is possible to believe about the longer-term impacts, without definitively establishing those.⁷

For more definitive results, we would ideally need a natural experiment like the Postville raid with no immediate inflow of immigrants afterwards. Another similar ICE enforcement and removal operation, a 2006 worksite raid in Marshalltown, Iowa, can provide further insight into the true effects of worksite raids. On December 12, 2006, ICE conducted raids at six Swift & Co. meat processing plants, resulting in the arrest of about 1,282 unauthorized immigrant workers across six states. As part of this large, interstate operation, 129 immigrants were arrested at the Swift & Co. facility located in Marshalltown. The raid in Marshalltown resembled the raid in Postville in that both targeted the largest local employer, leading to an exodus of mostly Hispanic immigrant workers and heightened unemployment (Flora et al., 2011). However, a few notable differences remain. First, though many migrants who were not detained also left Marshalltown after the raid, the Marshalltown raid still resulted in the loss of much fewer migrants relative to the local population compared to the Postville raid, since Marshalltown is almost twelve times the size of Postville (U.S. Census, 2010). Second, there was no large inflow of immigrants immediately after the Marshalltown raid. Instead, a small flow of Myanmar refugee workers trickled into Marshalltown after the reopening of the Marshalltown meat processing facility, almost four years later (Sodders, 2015; Springfield News-Leader, 2019).

⁷ We appreciate this insightful interpretation of results, courtesy one of the reviewers of *Public Choice*.

When contrasting the merits of these two raids as natural experiments, the Postville raid provides a better natural experiment in that it resulted in a much more discrete change in immigrant stock relative to the total population. Also, note that the RMPSE estimates for some indicators in this analysis are significantly higher compared to our analysis for Postville, particularly for total violent crime, total property crime, assault, burglary, and to a lesser extent larceny⁸; on the other hand, the effects of the Postville raid on crime could be contaminated by effects of inflows of Somali refugees a few years later. Likewise, the Marshalltown raid is a better natural experiment in that its effects on crime are not likely contaminated at all by very small inflows of Myanmar refugees almost four years later; however, the Marshalltown raid resulted in a much smaller discrete change in immigrant stocks compared to the raid in Postville, and our RMPSE estimates for some crime indicators are significantly higher for this analysis compared to analysis of the Postville raid. We present our estimates of the effects of the Marshalltown raid below, and let the reader decide which raid makes for a better natural experiment.

Consistent with our estimates of the impact of the Postville raid on crime, our reported standard joint post-treatment p-values indicate the effects of the Marshalltown raid are statistically insignificant within the 10% level for all ten crime rate indicators except for larceny. Further, save a few scattered years, p-values associated with individual year estimates are also insignificant. Our estimates are only barely statistically significant, with a standard joint post-treatment p-value of 0.091, when we estimate the effect of the Marshalltown raid on rates of larceny⁹.

⁸ When we construct our synthetic control for Marshalltown, we exclude all counties that also experienced raids during our period of analysis, including the four counties surrounding Postville, from our donor pool to avoid contamination of our results. Appendix 7 provides goodness of fit for our crime predictors in this third SCM analysis. Appendix 8 provides an overview of donor counties and their estimated weights.

⁹ In addition, for comparison references, we still report all the standard joint post-treatment p-values of the pseudo t-statistics and p-values by year for all ten crime indicators in Appendix 9.

Zooming in on our significant results, found below in Figure 8, we find actual larceny trends in Marshalltown from 2006 to 2010 were higher than synthetic control estimates. This suggests the Marshalltown raid resulted in an increase in larceny per capita, and this increase did not subside until the arrival of Myanmar workers after 2010. But, of course, we must make sure this significant result holds after more scrutiny. While Figure 9 indicates that our findings in Panel 8 are not driven by any specific synthetic control donor, which justifies our empirical design. Investigating further, in-place placebo tests shown in Figure 10 suggest crime rates changed drastically in other counties around Iowa, even when there were no raids.

[Insert Figure 8 about here]

[Insert Figure 9 about here]

[Insert Figure 10 about here]

These in-place placebo tests provide evidence that statistically significantly higher estimated rates of larceny in Marshall County are not due to the 2006 Marshalltown raid. Much like our analysis of the Postville raid, our analysis of the Marshalltown raid yields sporadically significant results. But, in the end, sensitivity analysis suggests changes in crime rates in all our raided counties are not the results of the raids themselves but are likely due to other latent factors that influenced crime rates in counties that were not raided.

5. Discussion and Conclusion

We have estimated the impact of deportation and immigration on crime. We observe scattered significant results that are consistent with case study evidence and suggest violent crime increased and property crime decreased immediately after the Postville raid in 2008 (Jones, 2012); we also document significant evidence suggesting increases in authorized immigrants a few years after the raid resulted in decreases in violent and property crime. An additional robustness test that

focuses on a relatively smaller raid in Marshall County in 2006 suggests that raid led to an increase in larceny. However, sensitivity analyses and placebo tests suggest other factors may better explain changes in crime during this time – crime rates changed across Iowa irrespective of any immigration raids or large increases in immigrant workers. Thus, neither the Postville raid, accompanied by the removal of a large group of unauthorized immigrants, nor a subsequent influx of authorized immigrant workers seem to have had a noticeable impact on crime.

While our estimates of the effects of Postville should be interpreted with caution because of the presence of two treatments within close temporal proximity, these results still give us novel evidence about the short-run impact of removing unauthorized immigrants while simultaneously restricting the space of things it is possible to believe about the longer-term impacts. There is little evidence that the Postville raid affected crime one way or the other, especially given our additional analysis of the Marshalltown raid, though we document weak evidence that the raid in Postville caused an increase in crime in the very short term (2008-2009). If the raid did cause any increase in crime after 2009, that increase was not large enough to be detected amidst other shocks to Postville, including the 2009 arrival of Somali workers. Our results also provide little evidence that the increase in Somali immigrants at the end of 2009 increased crime rates, and we provide weak evidence these Somali immigrants lowered crime rates. And while our results are conceivably consistent with long-term decreases in crime due to the raid in conjunction with increases in crime due to the Somali immigrants, if the Somalis caused any increase in crime, it must have been fully offset by longer-term negative effects on crime from the removal of Hispanic immigrants. At the same time, our analysis of the Marshalltown raid provides evidence that cuts against the idea worksite raids targeting Hispanic populations lower crime in the long-term.

Our results have important research implications and suggest that unauthorized and authorized immigrant crime rates, at least in our context, are not significantly higher or lower than the crime rates of natives. We are unable to find consistent evidence of a significant impact of these two raids, or subsequent increases in immigration, on local crime rates. If anything, our results suggest the Postville worksite deportation increased violent crime and led to decreases in property crime, while subsequent increases in authorized immigrants led to decreases in both violent and property crime. We also provide very weak evidence that the Marshalltown raid increased rates of larceny. However, the evidence we do find does not pass careful sensitivity analysis.

Our results complement prior work on deportation, immigration, and crime. Though others may find decreases in crime after policies facilitating deportation are enacted (Chalfin and Deza, 2020; Kang and Song, 2024), we provide evidence that worksite deportations, which result in the removal of immigrants with gainful employment, do not reduce crime. These findings lend support to those who show broader employment conditions impact the relationship between immigration and crime (Freedman et al., 2018). Our results also replicate those of others who have found a null or negative impact of immigration on crime (Bersani, 2014; Light et al., 2020; Martinez and Lee, 2000; Wadsworth, 2010; Ousey and Kubrin, 2018; Masterson and Yasenov, 2021).

Our research also carries important and interesting policy implications. Given the Postville raid cost taxpayers at least five million dollars, officials would do well to carefully consider the costs and benefits of raids, especially considering there could be other negative social impacts that accompany worksite raids (Juby and Kaplan, 2011). For example, cursory evidence suggests the Postville raid severely hurt local businesses that lost employees and customers after the raid (Jones, 2012). Thus, deportation can impose potentially high costs with little obvious benefits. While we

do not analyze crime rates in the places where deportees are sent, others provide evidence that deportations also impose externalities by documenting significantly higher murder rates in areas where the immigrants are deported (Rozo et al., 2020).

While there may be a demand to enforce current immigration restrictions by removing unauthorized residents (Talev and Contreras, 2024), we provide no evidence of beneficial results, at least in the case of gainfully employed unauthorized residents. Indeed, our results and analysis of the Postville raid suggest when unauthorized immigrants are employed and ingrained in their communities, the best policy response may be a lack of enforcement. Amnesty, or granting unauthorized immigrants legal residence status, is another policy that can serve as an alternative to deportation (Yao et al., 2021).

Still, there are limitations to our study. While our natural experiment is notably transparent and resulted in relatively large changes in immigrants, it was accompanied by multiple reactions that make it more difficult to study. For example, while we are able to separately study the Postville raid and subsequent increases in legal immigrants, these two shocks in immigration occur close together, and we may be picking up effects from one shock when studying the other. While our analysis of the Marshalltown raid is not confounded by potential effects from subsequent influxes of immigrants, the Marshalltown raid also resulted in a lower discrete change in immigrant stock compared to the Postville raid. Our deportations of interest were also accompanied by disruptions to major employers and unemployment, making it difficult to determine the mechanisms behind our results (or lack thereof). Large-scale deportations that are not accompanied by other large changes or followed up by increases in authorized immigration may provide clearer or more precise results. Studies of other large-scale deportations can also test the external validity of our

results. Our natural experiment of interest is unique, and it is important to test if results remain consistent in the case of other major deportations.

Another limitation of our study is our inability to identify monthly crime rates at the county level. The NaNDA only measures crime data yearly. And while FBI-UCR data tracks crime rates monthly, reporting issues make aggregation of FBI-UCR data, which is collected at the agency level, problematic (Maltz and Targonski, 2003). This is important, because increases in arrests were documented by local police in Postville during the fall and winter months of 2008 (Jones, 2012) – our data on yearly crime rates may not be granular enough to pick up month-to-month impacts this deportation had on crime. And because there is limited city/town-level FBI-UCR data for our treatment area, we are unable to perform a city-level analysis. Better data is needed to fully capture the impact of deportations on crime.

Finally, our research opens the door to other lines of research. Immigration and deportation can impact a wide variety of economic and social variables, including unemployment rates, educational attainment, and wages. While there has been a fair amount of research on the impacts of mass influxes of immigration, with the Mariel Boatlift being a notable natural experiment, much more research can be done examining the mass removals of immigrants. The finer details of our case study suggest potential avenues for future research. The raid at Agriprocessors took out the largest employer in the area, causing major disruptions to employment and displacing a large percentage of the current population. Therefore, case studies like this could be used to study the impact of deportation on other important economic and social indicators like trust, employment dynamics, business creation, and voter opinion. Our research helps move this line of literature forward. Our research also provides more evidence that cuts against the idea that immigrants are harmful to the host country.

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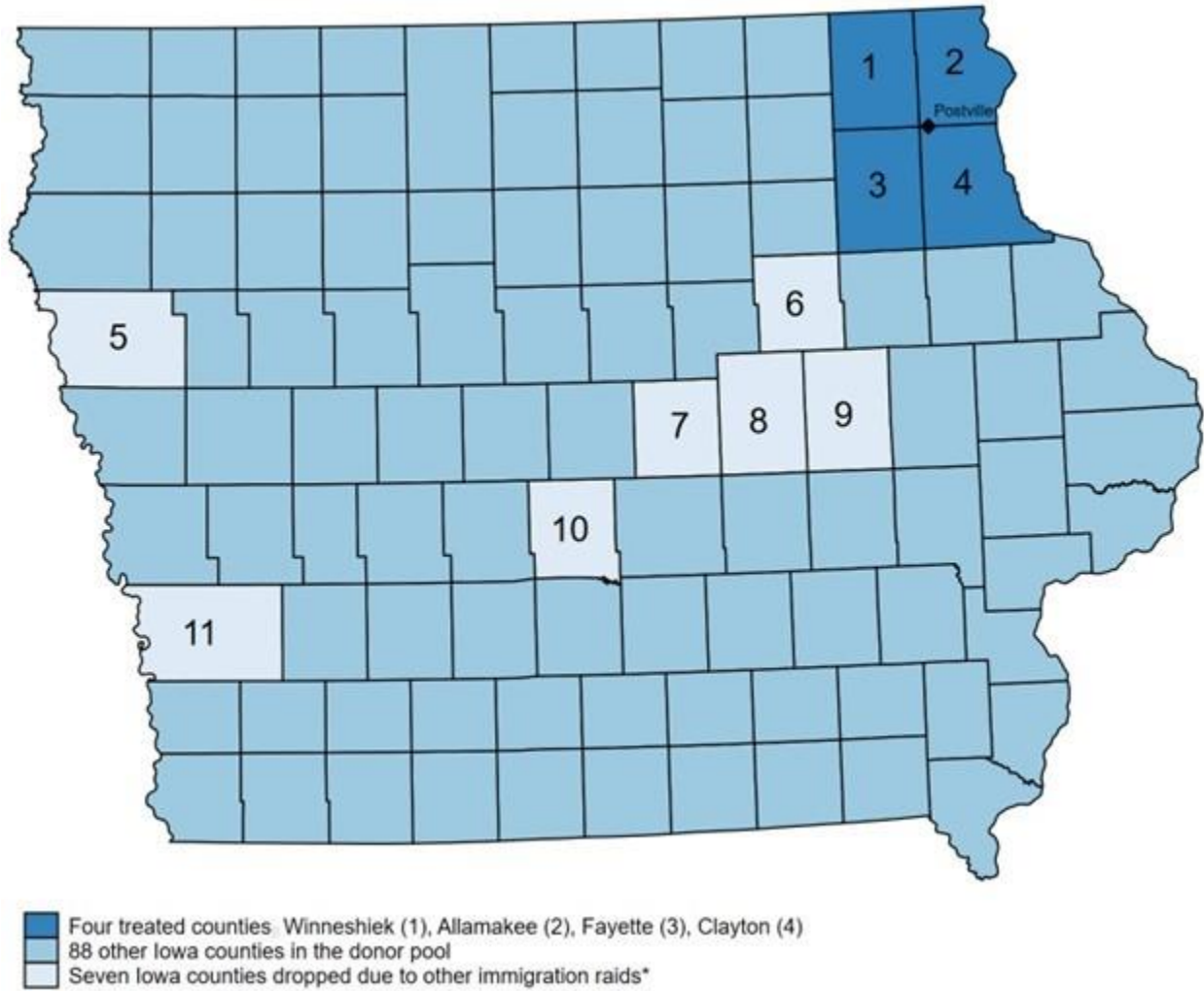
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Table 1: Summary Statistics and Data Sources

Variable	Num. of Obs.	Mean	Std. Dev.	Min	Max	Source
Violent Crime*	1157	148.072	136.83	0	1077.031	National Neighborhood Data Archive: Crimes by County [NaNDA]
Property Crime	1157	1405.158	1003.985	0	5932.778	NaNDA
Murder	1157	1.16	3.719	0	63.881	NaNDA
Rape	1157	16.539	18.605	0	148.417	NaNDA
Robbery	1157	8.383	19.569	0	184.603	NaNDA
Assault	1157	121.99	113.844	0	864.137	NaNDA
Burglary	1157	371.913	257.255	0	2048.45	NaNDA
Larceny	1157	954.568	734.957	0	4426.19	NaNDA
Motor Theft	1157	78.677	63.085	0	488.103	NaNDA
Arson	1157	13.287	15.623	0	96.748	NaNDA
Law enforcement per capita	1157	0.204	0.053	0.009	0.401	FBI Uniform Crime Reporting
Logarithm median household income	1157	10.801	0.175	10.247	11.409	U.S. Census
Below high school diploma percent	1157	11.716	2.784	5	21	U.S. Census
Unemployment percent	1157	4.089	1.396	1.7	10.2	U.S. Census
Population per square mile	1157	4312.348	5365.592	915.596	37396.58	U.S. Census
Percent black	1157	0.936	1.185	0.029	7.555	U.S. Census
Percent Latino	1157	3.241	3.894	0.459	27.2	U.S. Census
Percent foreign-born	1157	2.587	2.747	0.3	18.6	U.S. Census
Percent in poverty	1157	10.629	2.622	3.626	20.826	U.S. Census

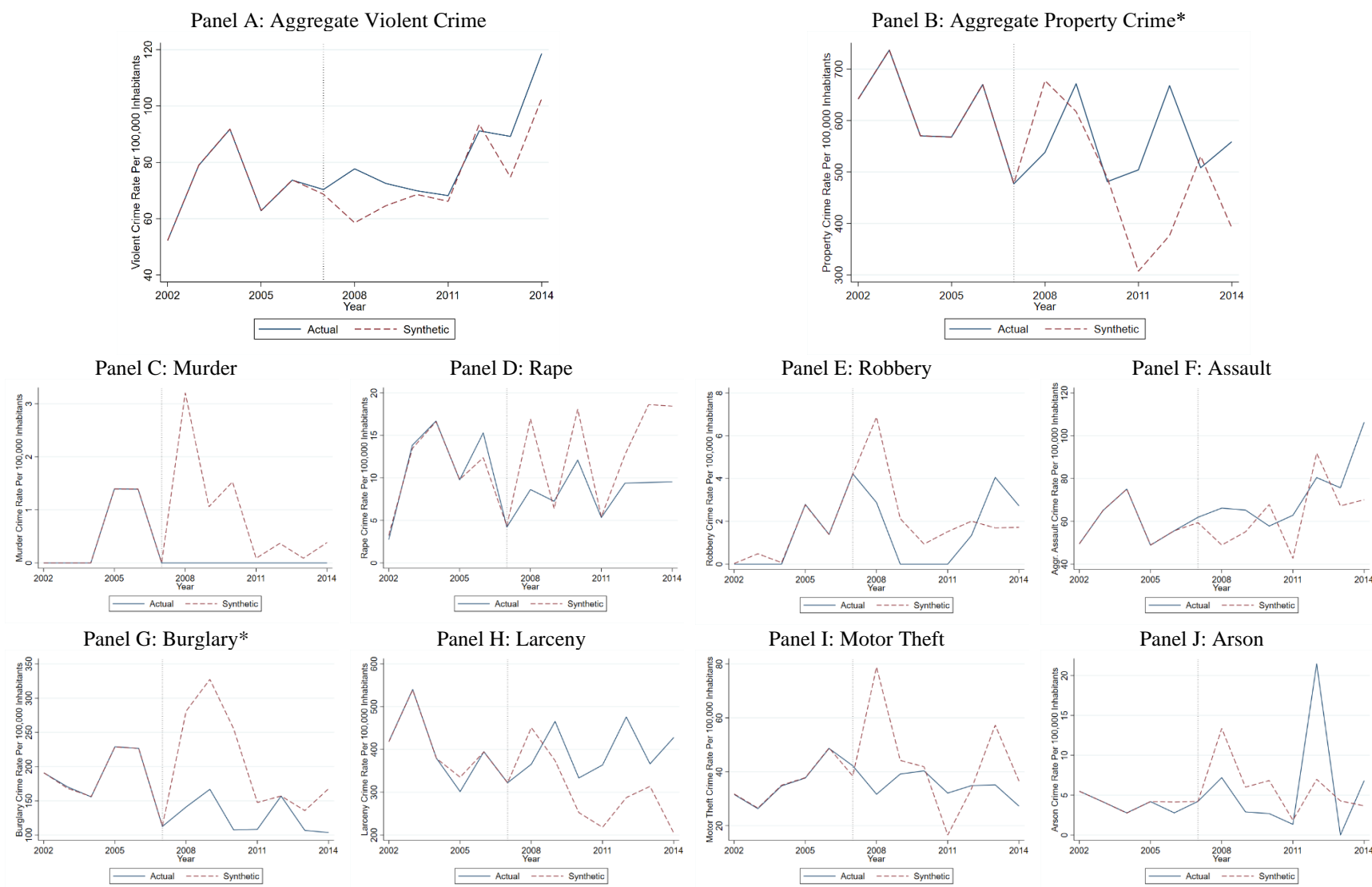
*Note: All crime indicators indicate crime per 100,000 residents.

Figure 1: Map of Treated, Donor Pool, and Dropped Iowa Counties



Note: *Due to other immigration raids during our sample period, seven Iowa counties are dropped from our donor pool: Woodbury (5), Black Hawk (6), Marshall (7), Tama (8), Benton (9), Polk (10), and Pottawattamie (11).

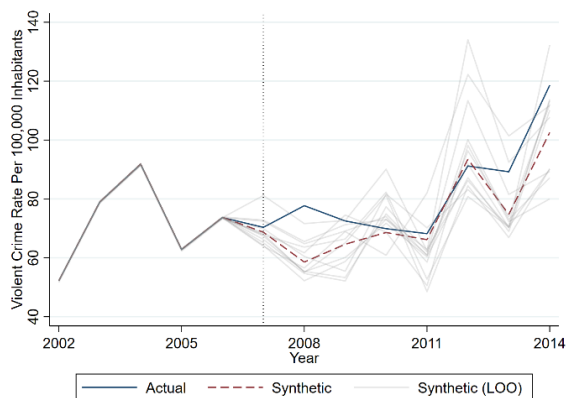
Figure 2: Crime Rate Trends Before and After Postville Immigration Raid



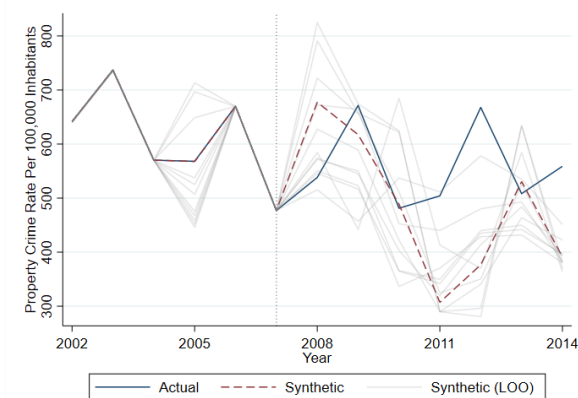
Notes: * indicates a standard joint post-treatment p-value at or below 0.1. Refer to Appendix 3 for more details.

Figure 3: Leave-one-out Tests for Postville Immigration Raid

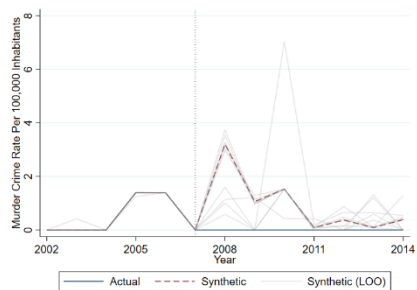
Panel A: Aggregate Violent Crime



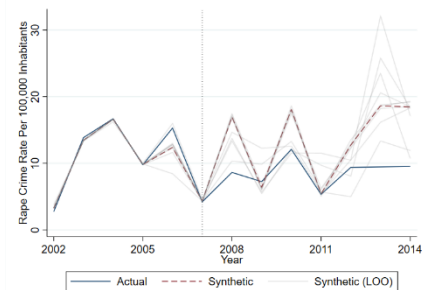
Panel B: Aggregate Property Crime



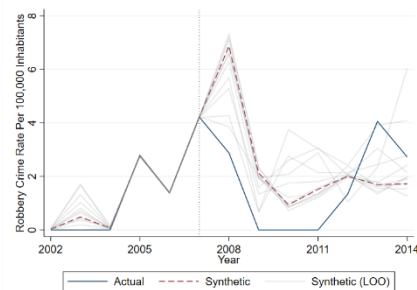
Panel C: Murder



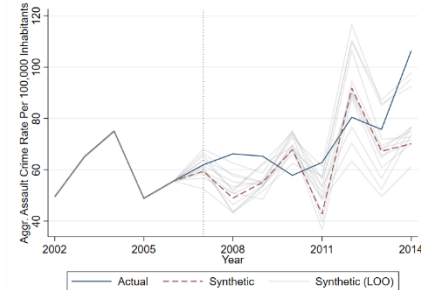
Panel D: Rape



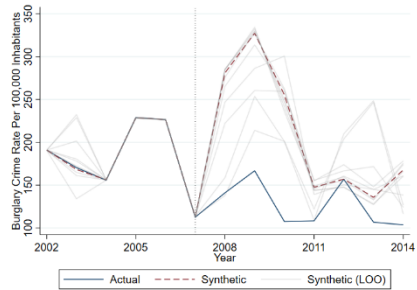
Panel E: Robbery



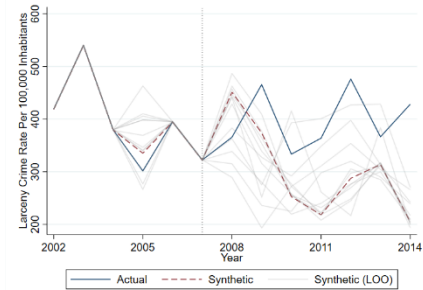
Panel F: Assault



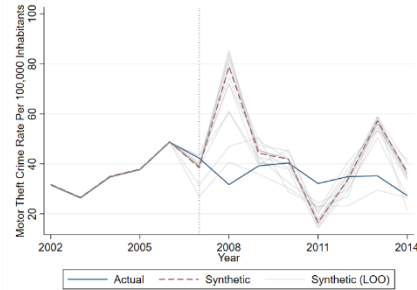
Panel G: Burglary



Panel H: Larceny



Panel I: Motor Theft



Panel J: Arson

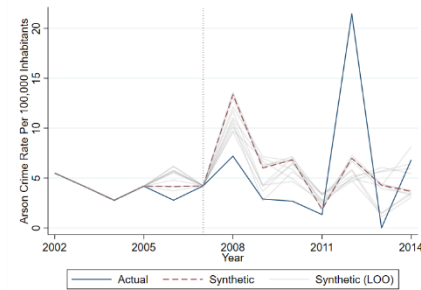
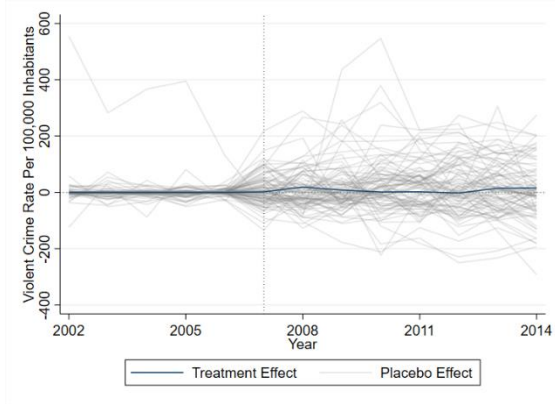
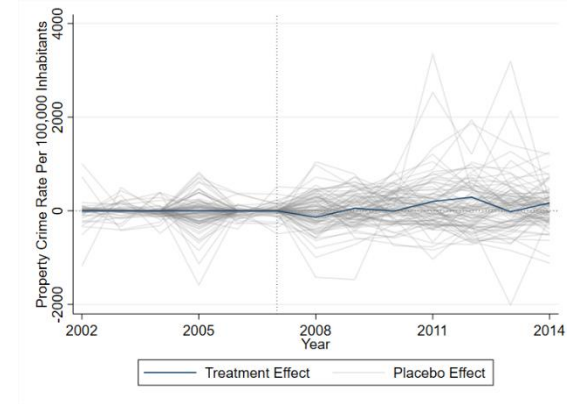


Figure 4: In-place Placebo Tests for Postville Immigration Raid

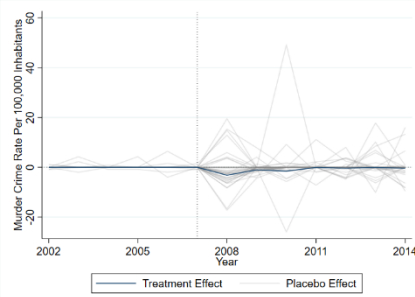
Panel A: Aggregate Violent Crime



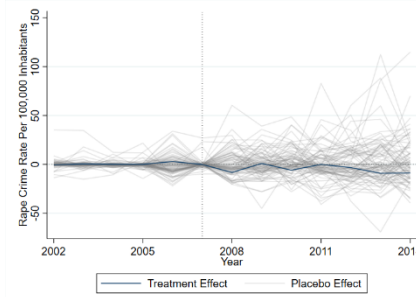
Panel B: Aggregate Property Crime



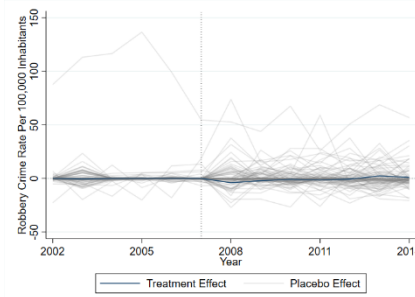
Panel C: Murder



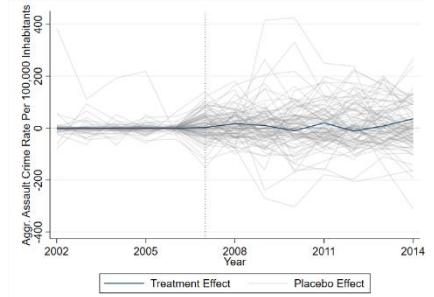
Panel D: Rape



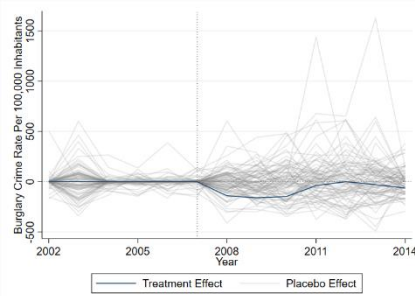
Panel E: Robbery



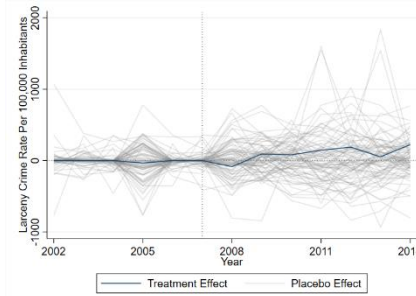
Panel F: Assault



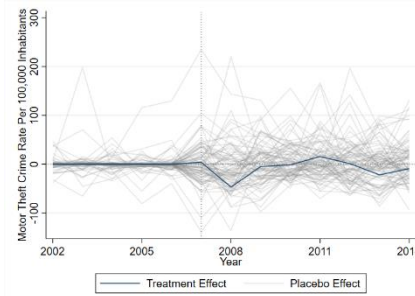
Panel G: Burglary



Panel H: Larceny



Panel I: Motor Theft



Panel J: Arson

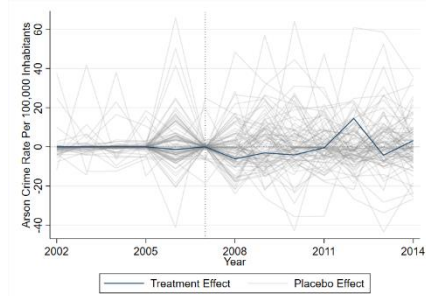
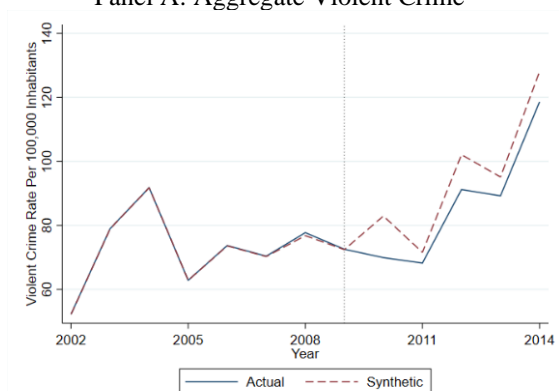
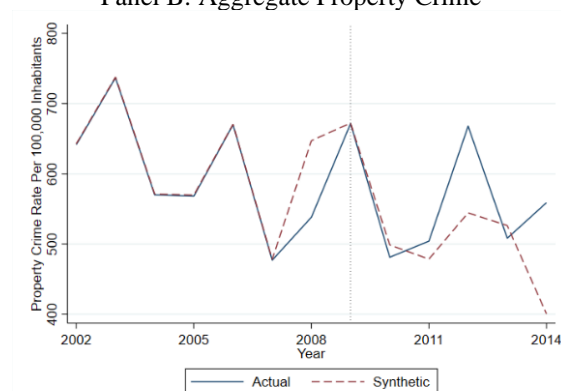


Figure 5: Crime Rate Trends Before and After 2010 New Replacement Immigration Influx

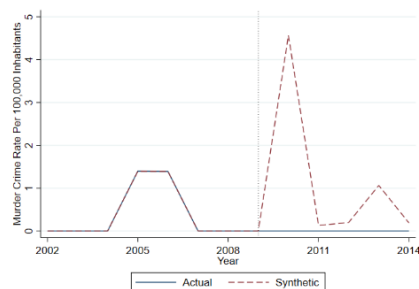
Panel A: Aggregate Violent Crime*



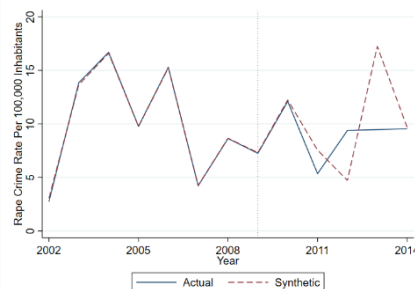
Panel B: Aggregate Property Crime



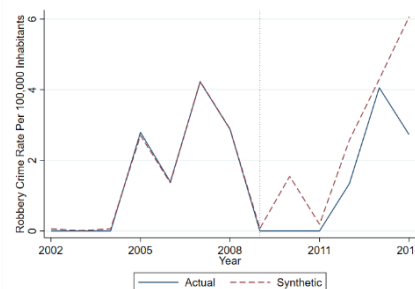
Panel C: Murder*



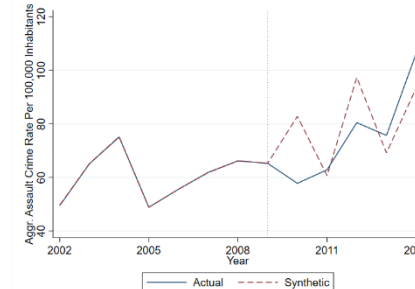
Panel D: Rape*



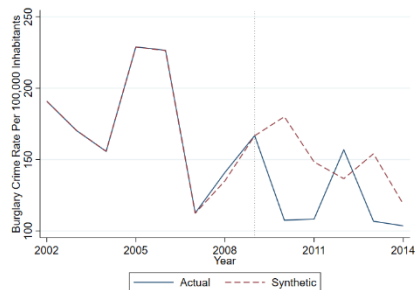
Panel E: Robbery



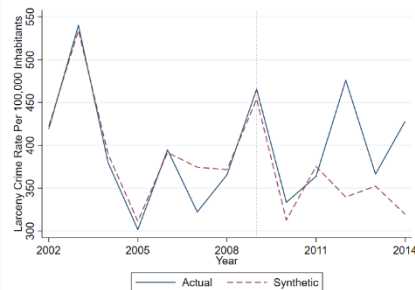
Panel F: Assault*



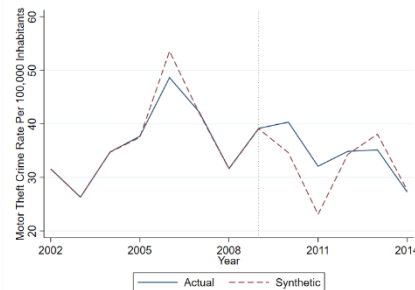
Panel G: Burglary*



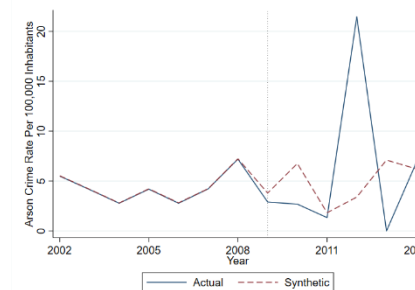
Panel H: Larceny



Panel I: Motor Theft



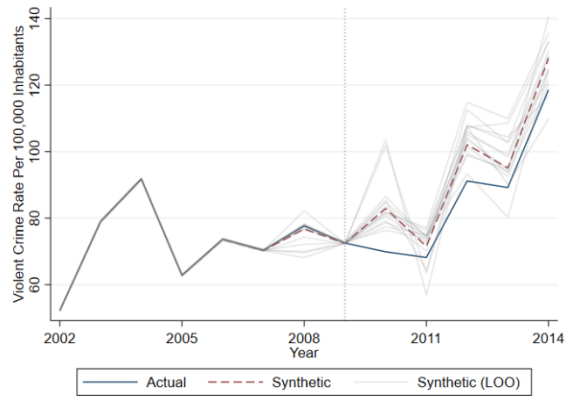
Panel J: Arson*



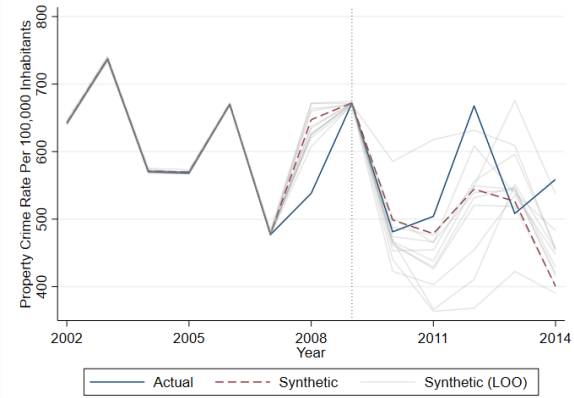
Notes: * indicates a standard joint post-treatment p-value at or below 0.1. Refer to Appendix 6 for more details.

Figure 6: Leave-one-out Tests for 2010 New Replacement Immigration Influx

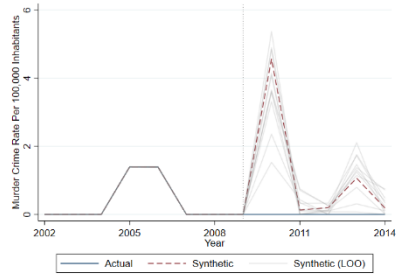
Panel A: Aggregate Violent Crime



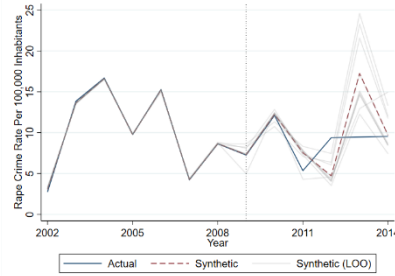
Panel B: Aggregate Property Crime



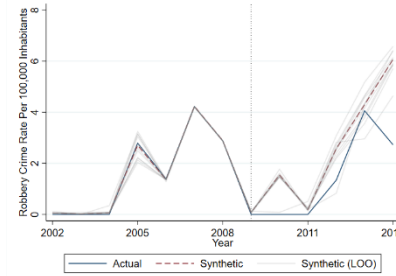
Panel C: Murder



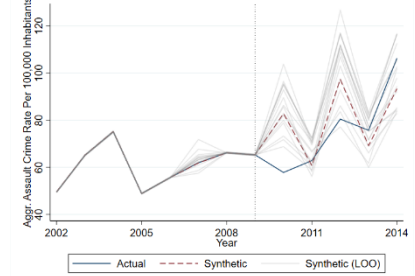
Panel D: Rape



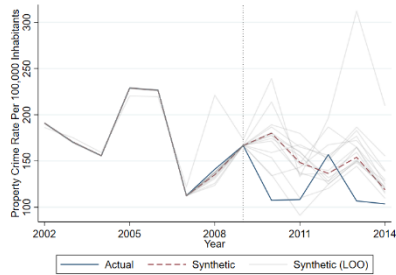
Panel E: Robbery



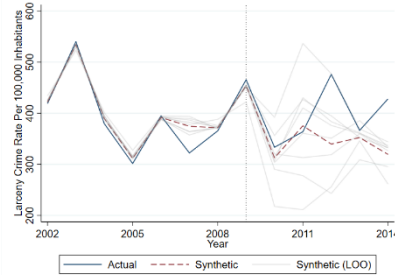
Panel F: Assault



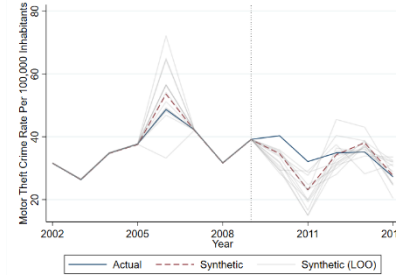
Panel G: Burglary



Panel H: Larceny



Panel I: Motor Theft



Panel J: Arson

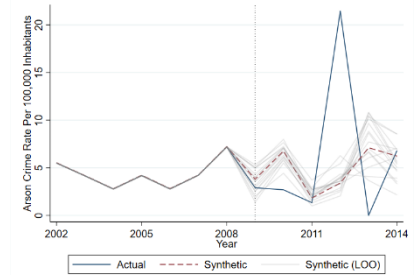
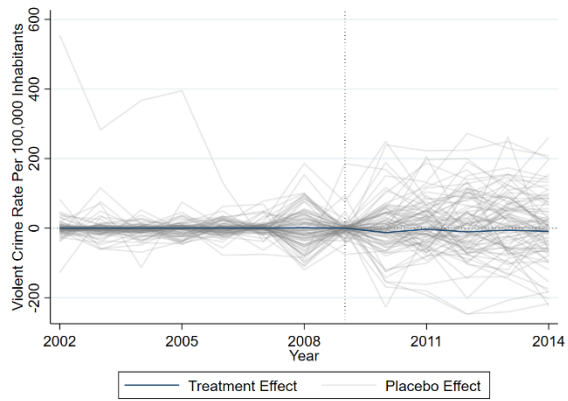
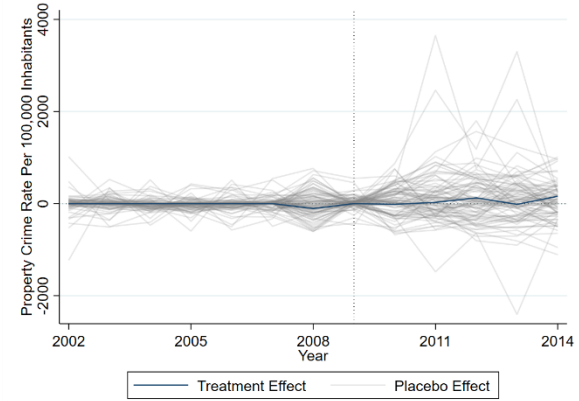


Figure 7: In-place Placebo Tests for 2010 New Replacement Immigration Influx

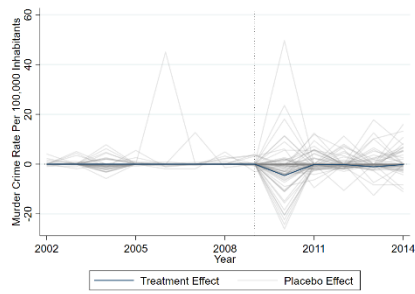
Panel A: Aggregate Violent Crime



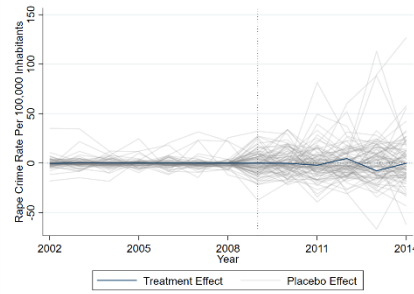
Panel B: Aggregate Property Crime



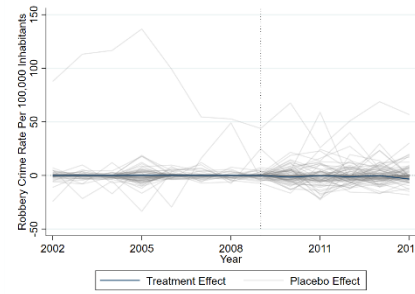
Panel C: Murder



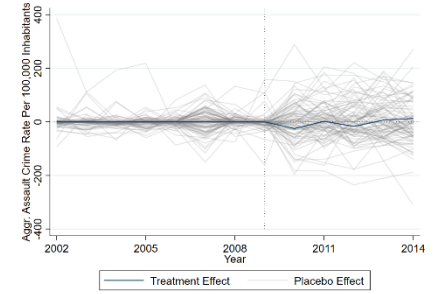
Panel D: Rape



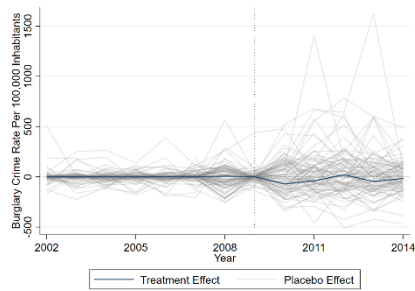
Panel E: Robbery



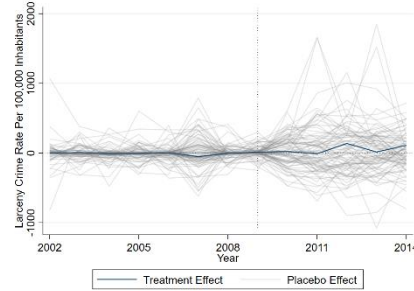
Panel F: Assault



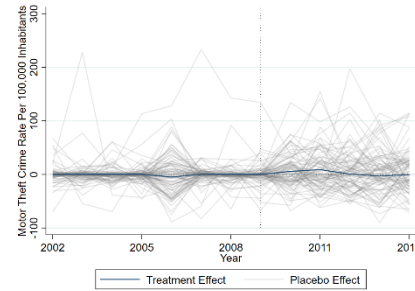
Panel G: Burglary



Panel H: Larceny



Panel I: Motor Theft



Panel J: Arson

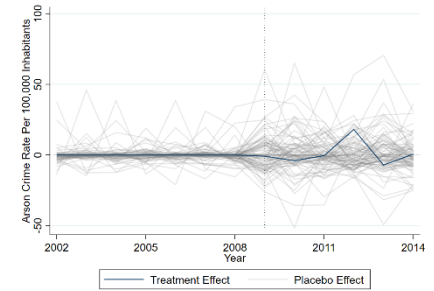
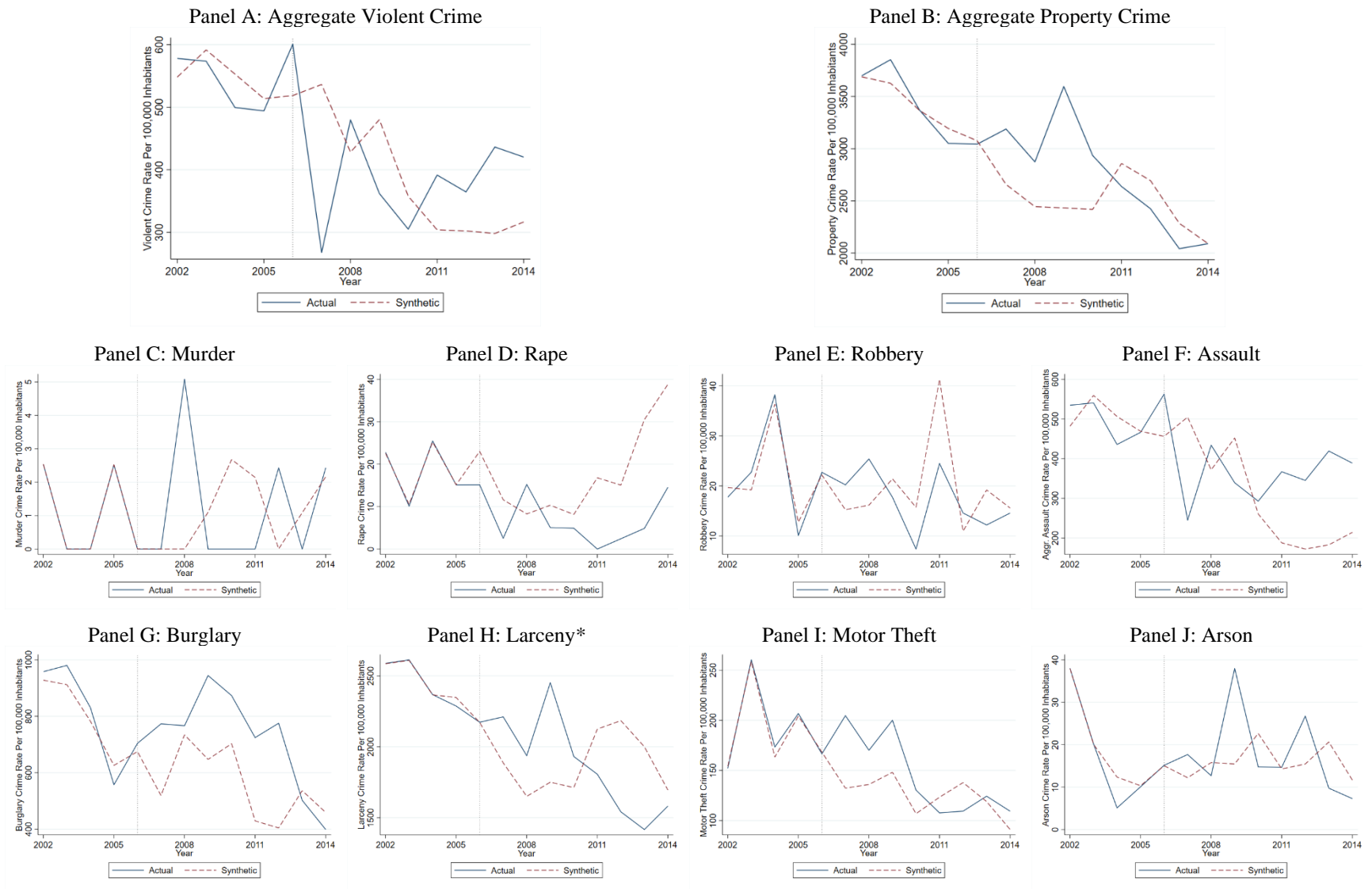


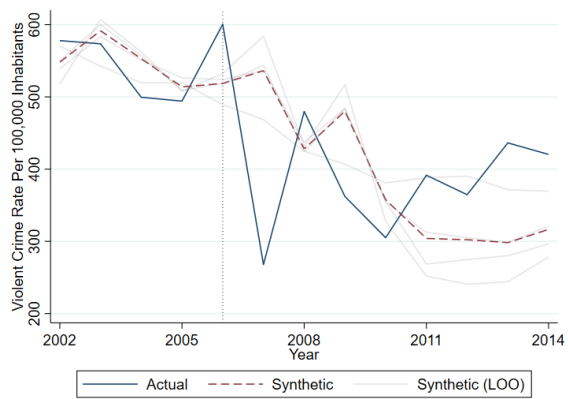
Figure 8: Crime Trends Before and After 2006 Marshalltown Immigration Raid



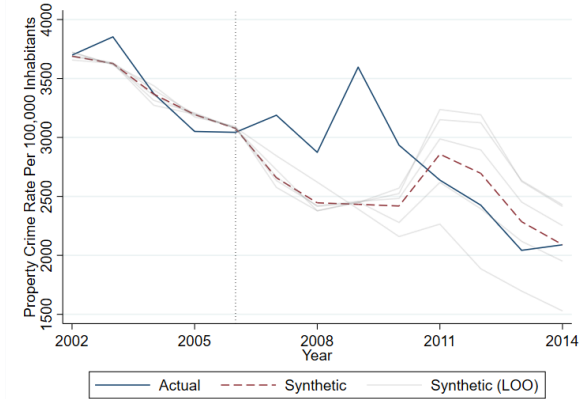
Notes: * indicates a standard joint post-treatment p-value at or below 0.1. Refer to Appendix 9 for more details.

Figure 9: Leave-one-out Tests for 2006 Marshalltown Immigration Raid

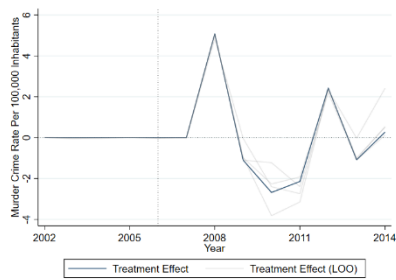
Panel A: Aggregate Violent Crime



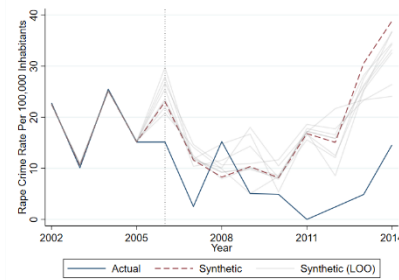
Panel B: Aggregate Property Crime



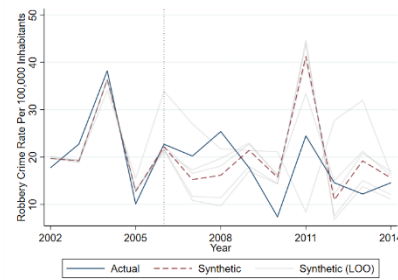
Panel C: Murder



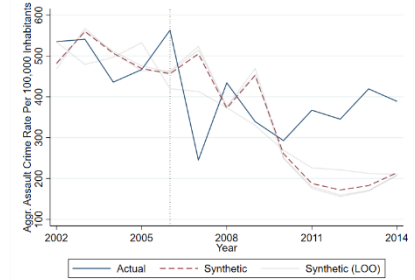
Panel D: Rape



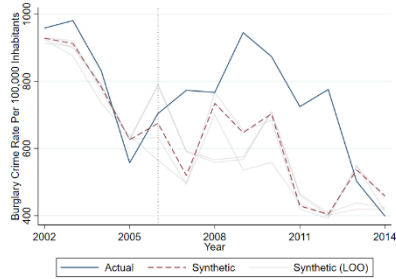
Panel E: Robbery



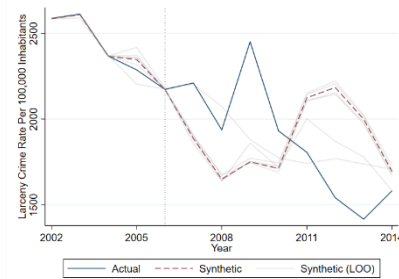
Panel F: Assault



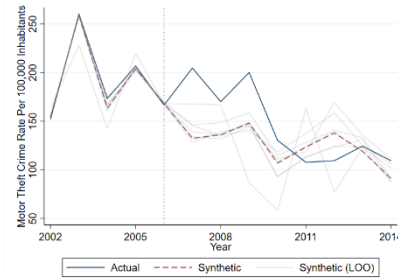
Panel G: Burglary



Panel H: Larceny



Panel I: Motor Theft



Panel J: Arson

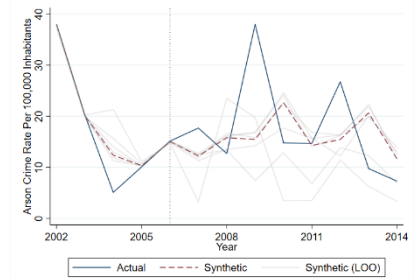
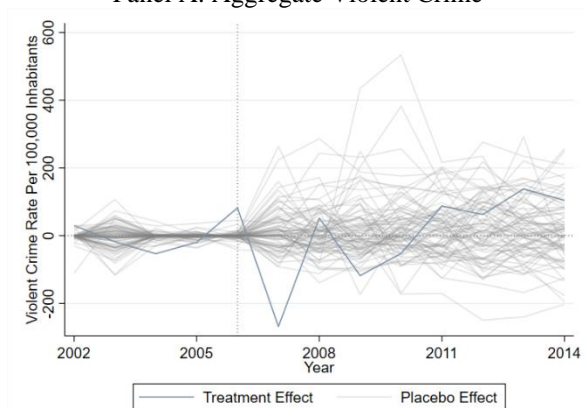
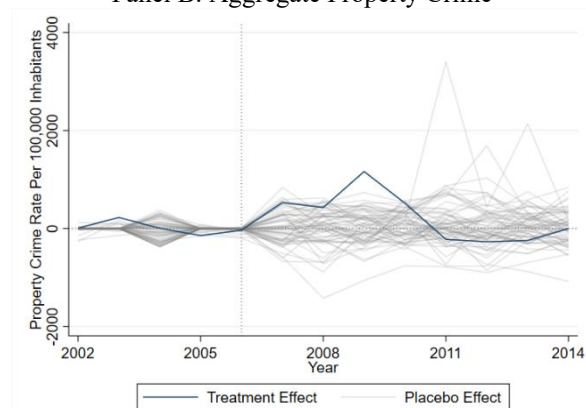


Figure 10: In-place Placebo Tests for 2006 Marshalltown Immigration Raid

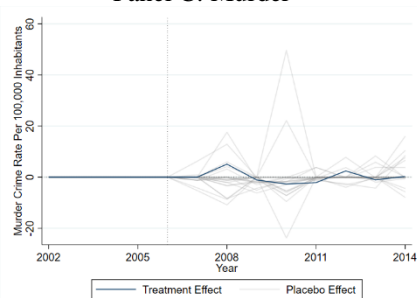
Panel A: Aggregate Violent Crime



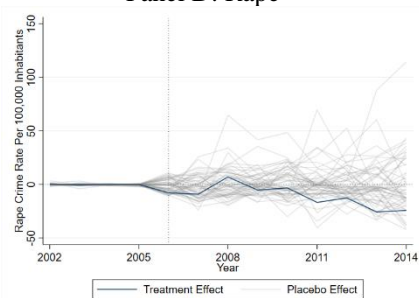
Panel B: Aggregate Property Crime



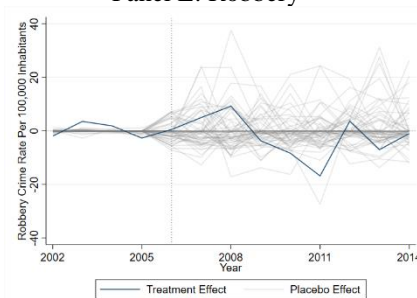
Panel C: Murder



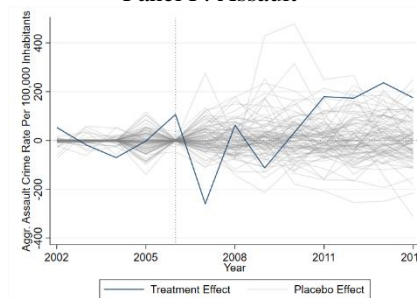
Panel D: Rape



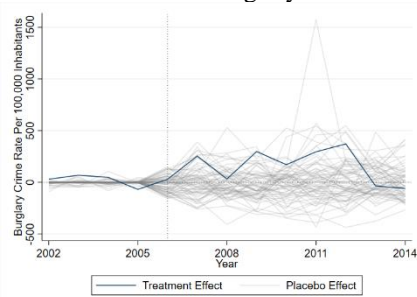
Panel E: Robbery



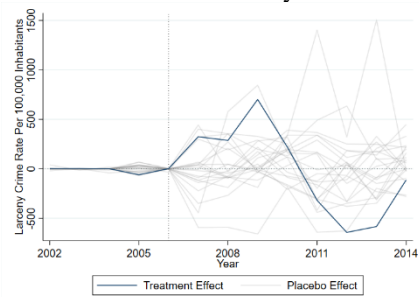
Panel F: Assault



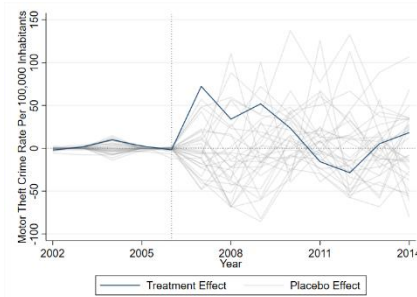
Panel G: Burglary



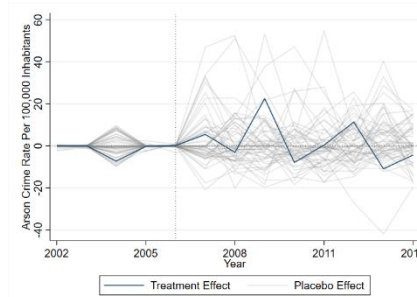
Panel H: Larceny



Panel I: Motor Theft



Panel J: Arson



Appendixes

Appendix 1: Goodness of Fit for Crime Rate Predictors in the Pretreatment Period Prior to Postville Immigration Raid

Panel A: Goodness of fit for violent crime rate predictors in the pretreatment period										
	Total Violent Crime		Murder		Rape		Robbery		Assault	
	Real	Synthetic	Real	Synthetic	Real	Synthetic	Real	Synthetic	Real	Synthetic
Viol. crime rate 2002	52.199	52.260			2.747	3.241	0.000	0.026	49.452	49.537
Viol. crime rate 2003	78.943	79.023	0.000	0.000	13.850	13.461			65.093	65.142
Viol. crime rate 2004	91.776	91.877	0.000	0.000	16.687	16.623	0.000	0.070	75.090	75.074
Viol. crime rate 2005	62.815	62.905	1.396	1.394	9.771	9.809	2.792	2.771	48.856	48.947
Viol. crime rate 2006	73.667	73.749	1.390	1.394			1.390	1.380	55.598	55.602
Viol. crime rate 2007			0.000	0.000	4.221	4.336	4.221	4.207		
Law enforcement employees per capita	0.179	0.179	0.179	0.178	0.179	0.184	0.179	0.179	0.179	0.180
Median income	10.721	10.733	10.721	10.721	10.721	10.723	10.721	10.784	10.721	10.723
Below HS education rate	12.750	12.739	12.750	12.246	12.750	12.158	12.750	11.805	12.750	12.741
Unemployment rate	5.031	4.956	5.031	4.959	5.031	4.336	5.031	4.809	5.031	4.894
Population density	2646.320	2641.850	2646.320	2647.891	2646.32	2279.091	2646.320	2511.005	2646.320	2650.820
Share black	0.524	0.524	0.524	0.460	0.524	0.550	0.524	0.478	0.524	0.523
Share Latino	2.020	4.122	2.020	3.481	2.020	2.487	2.020	2.227	2.020	2.571
Share foreign-born	1.780	1.828	1.780	1.915	1.780	2.891	1.780	2.572	1.780	1.826
Poverty rate	10.438	10.476	10.438	10.441	10.438	11.574	10.438	10.493	10.438	10.442
Pre-treatment RMSPE	0.704		0.000		1.214		0.203		1.060	
Panel B: Goodness of fit for property crime rate predictors in the pretreatment period										
	Property Crime		Burglary		Larceny		Motor theft		Arson	
	Real	Synthetic	Real	Synthetic	Real	Synthetic	Real	Synthetic	Real	Synthetic
Prop. crime rate 2002	641.501	641.807	190.939	191.028	418.968	418.200	31.594	31.784	5.495	5.485
Prop. crime rate 2003	736.801	737.231			540.136	539.543	26.314	26.546	4.155	4.159
Prop. crime rate 2004	570.125	570.381	155.742	156.201	379.620	379.239	34.764	35.019	2.781	2.746
Prop. crime rate 2005			228.926	228.681			37.689	37.821	4.188	4.202
Prop. crime rate 2006	669.956	670.154	226.562	226.545	394.746	394.346	48.648	48.831		
Prop. crime rate 2007	476.988	477.243	112.564	113.062	322.213	321.767			4.221	4.220
Law enforcement employees per capita	0.179	0.178	0.179	0.184	0.179	0.175	0.179	0.176	0.179	0.179
Median income	10.721	10.732	10.721	10.725	10.721	10.710	10.721	10.738	10.721	10.715
Below HS education rate	12.750	12.590	12.750	11.159	12.750	12.551	12.750	12.561	12.750	12.433
Unemployment rate	5.031	4.691	5.031	4.717	5.031	4.343	5.031	4.316	5.031	5.020
Population density	2646.320	1901.813	2646.320	1961.350	2646.320	2059.425	2646.320	2396.360	2646.320	2459.534
Share black	0.524	0.296	0.524	0.344	0.524	0.453	0.524	0.518	0.524	0.524
Share Latino	2.020	2.932	2.020	2.127	2.020	2.450	2.020	2.011	2.020	2.706
Share foreign-born	1.780	1.878	1.780	2.049	1.780	1.781	1.780	2.738	1.780	1.937
Poverty rate	10.438	10.458	10.438	10.859	10.438	10.440	10.438	10.515	10.438	10.341
Pre-treatment RMSPE	0.637		0.990		13.969		1.606		0.560	

Appendix 2: Donor Counties and Their Estimated Weights for Synthetic Treated Counties for Postville Immigration Raid

Panel A Violent Crime Rates and Sub-indexes									
Total Violent Crime		Murder		Rape		Robbery		Assault	
Donors	Weights	Donors	Weights	Donors	Weights	Donors	Weights	Donors	Weights
Louisa	0.138	Hancock	0.36	Hamilton	0.253	Van Buren	0.225	Butler	0.184
Winnebago	0.136	Lucas	0.136	Guthrie	0.217	Chickasaw	0.214	Page	0.156
Page	0.106	Appanoose	0.135	Lucas	0.187	Wright	0.149	Ringgold	0.105
Chickasaw	0.102	Louisa	0.11	Decatur	0.187	Madison	0.126	Louisa	0.1
Buchanan	0.097	Jones	0.067	Franklin	0.089	Humboldt	0.083	Winnebago	0.083
Worth	0.095	Winnebago	0.065	Jones	0.047	Henry	0.08	Audubon	0.078
Ringgold	0.083	Monona	0.041	Clinton	0.012	Jones	0.069	Howard	0.077
Guthrie	0.072	Cerro Gordo	0.039	Washington	0.006	Monroe	0.026	Guthrie	0.068
Butler	0.072	Plymouth	0.024	Henry	0.001	Hancock	0.017	Buchanan	0.055
Crawford	0.059	O'Brien	0.023			Appanoose	0.008	Chickasaw	0.032
Wayne	0.035					Lee	0.002	Monona	0.032
Linn	0.005							Van Buren	0.019
								Linn	0.011

Panel B Property Crime Rates and Sub-indexes									
Total Property Crime		Burglary		Larceny		Motor theft		Arson	
Donors	Weights	Donors	Weights	Donors	Donors	Weights	Donors	Weights	Donors
Butler	0.161	Winnebago	0.285	Butler	0.272	Audubon	0.376	Chickasaw	0.286
Butler	0.144	Fremont	0.217	Taylor	0.175	Taylor	0.25	Worth	0.269
Ringgold	0.129	Worth	0.135	Ringgold	0.157	Jones	0.1	Guthrie	0.088
Fremont	0.113	Wayne	0.123	Jones	0.125	Fremont	0.077	Wayne	0.087
Hancock	0.101	Guthrie	0.073	Fremont	0.068	Dickinson	0.063	Crawford	0.08
Guthrie	0.097	Hancock	0.05	Monroe	0.059	Henry	0.039	Lee	0.079
Winnebago	0.082	Taylor	0.041	Hancock	0.046	Guthrie	0.036	Audubon	0.054
Crawford	0.061	Keokuk	0.03	Winnebago	0.041	Decatur	0.016	Sac	0.021
Monroe	0.041	Decatur	0.028	Crawford	0.031	Davis	0.015	Emmet	0.021
Monona	0.039	Monona	0.018	Franklin	0.012	Ringgold	0.013	Monona	0.011
Taylor	0.033			Osceola	0.01	Scott	0.009	Butler	0.004
				Guthrie	0.003	Linn	0.006		
						Crawford	0.001		

Appendix 3: Postville Immigration Raid Treatment Effects and Standard P-Values of the Pseudo T-Statistic

Year	Crime rates									
Panel A: Total Violent Crime Rates and Sub-indexes										
	Total Violent Crime		Murder		Rape		Robbery		Assault	
	Estimates	P-values	Estimates	P-values	Estimates	P-values	Estimates	P-values	Estimates	P-values
2008	19.160	0.011	-3.205	0.184	8.634	0.182	-3.995	0.125	17.261	0.068
2009	7.921	0.068	-1.060	0.079	7.252	0.818	-2.126	0.125	10.180	0.125
2010	1.305	0.591	-1.528	0.184	12.098	0.193	-0.939	0.284	-10.085	0.136
2011	2.036	0.489	-0.088	0.000	5.349	0.955	-1.520	0.091	20.113	0.125
2012	-2.305	0.534	-0.369	0.132	9.387	0.591	-0.666	0.398	-11.398	0.159
2013	14.475	0.034	-0.089	0.289	9.462	0.284	2.361	0.250	8.462	0.227
2014	16.059	0.045	-0.384	0.184	9.543	0.307	1.006	0.330	36.220	0.080
Standard joint post-treatment p-value	0.102		0.289		0.409		0.330		0.125	
Panel B: Total Property Crime Rates and Sub-indexes										
	Property Crime		Burglary		Larceny		Motor theft		Arson	
	Estimates	P-values	Estimates	P-values	Estimates	P-values	Estimates	P-values	Estimates	P-values
2008	-139.229	0.000	-140.001	0.011	-85.665	0.080	-47.168	0.023	-6.171	0.080
2009	54.217	0.000	-160.613	0.011	91.738	0.125	-5.055	0.341	-3.104	0.193
2010	-7.603	0.091	-147.771	0.011	80.854	0.227	-1.540	0.727	-4.142	0.114
2011	196.908	0.000	-39.181	0.023	145.598	0.148	15.557	0.136	-0.514	0.625
2012	291.224	0.000	-0.151	0.932	188.521	0.102	1.175	0.807	14.471	0.045
2013	-22.218	0.045	-28.880	0.023	52.559	0.330	-22.158	0.068	-4.279	0.205
2014	167.657	0.000	-64.048	0.011	223.393	0.034	-9.305	0.216	3.159	0.170
Standard joint post-treatment p-value	0.000		0.011		0.170		0.114		0.102	

Notes: Bold p-values represent statistical significance at the 10% level or higher.

Negative estimates suggest a decrease in crime rate in a post-treatment year; positive estimates suggest an increase in crime in a post-treatment year.

Appendix 4: Goodness of Fit for Crime Rate Predictors in the Pretreatment Period Prior to 2010 New Replacement Immigration Influx

Panel A: Goodness of fit for violent crime rate predictors in the pretreatment period											
	Total Violent Crime		Murder		Rape		Robbery		Assault		
	Real	Synthetic	Real	Synthetic	Real	Synthetic	Real	Synthetic	Real	Synthetic	
Viol. crime rate 2002	52.199	52.222	0.000	0.000	2.747	3.067	0.000	0.060	49.452	49.622	
Viol. crime rate 2003	78.943	78.915	0.000	0.000	13.850	13.655	0.000	0.015	65.093	65.198	
Viol. crime rate 2004	91.776	91.761			16.687	16.598	0.000	0.061	75.090	75.234	
Viol. crime rate 2005	62.815	62.803	1.396	1.394	9.771	9.754			48.856	48.813	
Viol. crime rate 2006	73.667	73.583	1.390	1.391	15.290	15.233	1.390	1.361	55.598	55.628	
Viol. crime rate 2007	70.352	70.232	0.000	0.000	4.221	4.251	4.221	4.234			
Viol. crime rate 2008			0.000	0.000	8.634	8.660	2.878	2.890	66.194	66.175	
Viol. crime rate 2009	72.518	72.414	0.000	0.000			0.000	0.071	65.267	65.279	
Law enforcement employees per capita	0.183	0.181	0.183	0.183	0.183	0.184	0.183	0.184	0.183	0.182	
Median income	10.754	10.755	10.754	10.755	10.754	10.782	10.754	10.774	10.754	10.776	
Below HS education rate	12.750	12.737	12.750	12.376	12.750	11.795	12.750	12.788	12.750	12.752	
Unemployment rate	4.767	4.752	4.767	4.086	4.767	4.080	4.767	3.948	4.767	4.763	
Population density	2640.220	2623.969	2640.220	2645.869	2640.220	3085.971	2640.220	2305.395	2640.220	2669.339	
Share black	0.566	0.555	0.566	0.568	0.566	0.552	0.566	0.557	0.566	0.483	
Share Latino	2.130	2.149	2.130	2.139	2.130	1.597	2.130	1.989	2.130	2.267	
Share foreign-born	2.090	3.263	2.090	2.094	2.090	1.983	2.090	2.299	2.090	2.114	
Poverty rate	10.303	10.308	10.303	10.309	10.303	10.121	10.303	10.861	10.303	10.340	
Pre-treatment RMSPE	0.307		0.000		0.137		0.052		0.047		
Panel B: Goodness of fit for property crime rate predictors in the pretreatment period											
	Property Crime		Burglary		Larceny		Motor theft		Arson		
	Real	Synthetic	Real	Synthetic	Real	Synthetic	Real	Synthetic	Real	Synthetic	
Prop. crime rate 2002	641.501	642.737	190.939	190.959	418.968	422.122	31.594	31.586	5.495	5.529	
Viol. crime rate 2003	736.801	737.875	170.351	170.491	540.136	533.985	26.314	26.316	4.155	4.182	
Prop. crime rate 2004	570.125	571.242	155.742	155.784	379.620	389.776	34.764	34.712	2.781	2.800	
Prop. crime rate 2005	568.126	569.809	228.926	228.832	301.512	311.311	37.689	37.487	4.188	4.223	
Prop. crime rate 2006	669.956	670.572	226.562	226.342	394.746	391.476			2.780	2.818	
Prop. crime rate 2007	476.988	478.218	112.564	112.411			42.211	42.018	4.221	4.242	
Viol. crime rate 2008					365.504	371.571	31.658	31.614	7.195	7.236	
Viol. crime rate 2009	671.521	672.236	166.792	166.686	465.568	453.944	39.160	39.084			
Law enforcement employees per capita	0.183	0.172	0.183	0.182	0.183	0.150	0.183	0.165	0.183	0.183	
Median income	10.754	10.792	10.754	10.754	10.754	10.791	10.754	10.735	10.754	10.765	
Below HS education rate	12.750	12.708	12.750	12.753	12.750	12.338	12.750	12.534	12.750	12.217	
Unemployment rate	4.767	4.557	4.767	4.073	4.767	4.190	4.767	4.051	4.767	4.709	
Population density	2640.220	2238.848	2640.220	2470.127	2640.220	2072.283	2640.220	2378.370	2640.220	2654.237	
Share black	0.566	0.506	0.566	0.562	0.566	0.291	0.566	0.433	0.566	0.564	
Share Latino	2.130	2.375	2.130	2.165	2.130	2.667	2.130	2.106	2.130	2.416	
Share foreign-born	2.090	2.324	2.090	2.116	2.090	1.632	2.090	2.179	2.090	2.134	
Poverty rate	10.303	10.272	10.303	10.305	10.303	9.655	10.303	10.339	10.303	10.423	
Pre-treatment RMSPE	38.089		2.008		20.049		1.800		0.314		

Appendix 5: Donor Counties and Their Estimated Weights for Synthetic Treated Counties for 2010 New Replacement Immigration Influx

Panel A: Violent Crime Rates and Sub-indexes									
Total Violent Crime		Murder		Rape		Robbery		Assault	
Donors	Weights	Donors	Weights	Donors	Weights	Donors	Weights	Donors	Weights
Guthrie	0.218	Howard	0.188	Butler	0.4	Butler	0.181	Guthrie	0.117
Floyd	0.153	Grundy	0.165	Decatur	0.166	Decatur	0.176	Jasper	0.103
Howard	0.13	Lucas	0.136	Cedar	0.149	Hardin	0.141	Butler	0.097
Osceola	0.076	Decatur	0.123	Cerro Gordo	0.103	Humboldt	0.136	Buchanan	0.097
Henry	0.072	Cherokee	0.119	Hamilton	0.085	Lyon	0.132	Audubon	0.088
Ringgold	0.065	Hancock	0.079	Kossuth	0.048	Van Buren	0.111	Ringgold	0.086
Chickasaw	0.06	Osceola	0.061	Jones	0.025	Wright	0.043	Monona	0.078
Davis	0.051	Dallas	0.048	Mills	0.015	Guthrie	0.036	Louisa	0.072
Winnebago	0.05	Jones	0.046	Clinton	0.006	Plymouth	0.015	Davis	0.046
Monona	0.042	Wapello	0.023	Washington	0.002	Bremer	0.014	Chickasaw	0.046
Jones	0.034	Buena Vista	0.012			Henry	0.013	Winnebago	0.044
Louisa	0.029					Jones	0.003	Howard	0.038
Clinton	0.014							Page	0.037
Linn	0.006							Lee	0.022
								Osceola	0.018
								Clinton	0.013

Panel B: Property Crime Rates and Sub-indexes									
Total Property Crime		Burglary		Larceny		Motor theft		Arson	
Donors	Weights	Donors	Weights	Donors	Donors	Weights	Donors	Weights	Donors
Butler	0.253	Osceola	0.259	Grundy	0.267	Pocahontas	0.144	Chickasaw	0.364
Winnebago	0.133	Decatur	0.184	Keokuk	0.208	Butler	0.131	Emmet	0.118
Guthrie	0.117	Bremer	0.179	Taylor	0.117	Decatur	0.126	Guthrie	0.098
Monroe	0.107	Guthrie	0.097	Chickasaw	0.111	Audubon	0.119	Decatur	0.081
Jones	0.097	Fremont	0.053	Monroe	0.093	Guthrie	0.093	Adams	0.057
Fremont	0.089	Butler	0.048	Butler	0.08	Dickinson	0.078	Greene	0.052
Ringgold	0.063	Monona	0.046	Crawford	0.075	Sioux	0.069	Lee	0.045
Crawford	0.056	Audubon	0.03	Ringgold	0.048	Sac	0.064	Worth	0.04
Audubon	0.055	Monroe	0.027			Palo Alto	0.056	Jasper	0.037
Decatur	0.023	Worth	0.02			Floyd	0.047	Wayne	0.033
Taylor	0.008	Davis	0.02			Louisa	0.038	Franklin	0.025
		Madison	0.015			Monona	0.028	Sac	0.018
		Chickasaw	0.012			Linn	0.005	Audubon	0.014
		Lee	0.01					Crawford	0.007
								Hardin	0.006
								Scott	0.006

Appendix 6: 2010 New Replacement Immigration Influx Treatment Effects and Standard P-Values of the Pseudo T-Statistic

Year	Crime rates									
Panel A: Total Violent Crime Rates and Sub-indexes										
	Violent Crime		Murder		Rape		Robbery		Assault	
	Estimates	P-values	Estimates	P-values	Estimates	P-values	Estimates	P-values	Estimates	P-values
2010	-13.006	0.000	-4.566	0.011	-0.144	0.761	-1.547	0.057	-25.020	0.000
2011	-3.349	0.034	-0.128	0.045	-2.204	0.045	-0.188	0.273	2.160	0.000
2012	-10.894	0.011	-0.198	0.011	4.671	0.034	-1.232	0.080	-16.901	0.000
2013	-5.886	0.011	-1.063	0.023	-7.785	0.034	-0.241	0.284	6.545	0.000
2014	-9.436	0.011	-0.188	0.023	-0.140	0.818	-3.337	0.091	12.971	0.000
Standard joint post- treatment p-value	0.011		0.023		0.080		0.148		0.000	
Panel B: Total Property Crime Rates and Sub-indexes										
	Property Crime		Burglary		Larceny		Motor theft		Arson	
	Estimates	P-values	Estimates	P-values	Estimates	P-values	Estimates	P-values	Estimates	P-values
2010	-17.465	0.841	-72.483	0.011	20.705	0.693	5.696	0.284	-4.066	0.057
2011	25.549	0.841	-40.058	0.068	-11.661	0.841	8.986	0.227	-0.487	0.545
2012	123.410	0.432	20.284	0.102	136.464	0.227	0.577	0.886	18.059	0.034
2013	-18.054	0.886	-47.299	0.023	13.969	0.920	-2.947	0.466	-7.103	0.011
2014	158.510	0.193	-15.288	0.045	108.641	0.170	-0.349	0.943	0.593	0.420
Standard joint post- treatment p-value	0.534		0.034		0.420		0.386		0.034	

Notes: Bold p-values represent statistical significance at the 10% level or higher.

Negative estimates suggest a decrease in crime rate in a post-treatment year; positive estimates suggest an increase in crime in a post-treatment year.

Appendix 7: Goodness of Fit for Crime Rate Predictors in the Pretreatment Period Prior to 2006 Marshalltown Immigration Raid

Panel A: Goodness of fit for violent crime rate predictors in the pretreatment period										
	Total Violent Crime		Murder		Rape		Robbery		Assault	
	Real	Synthetic	Real	Synthetic	Real	Synthetic	Real	Synthetic	Real	Synthetic
Viol. crime rate 2002	577.932	548.178								
Viol. crime rate 2003	499.478	552.898	2.535	2.538	22.813	22.539	17.744	19.684	534.841	481.871
Viol. crime rate 2004	494.176	513.771	0.000	0.000	10.106	10.541	22.739	19.203	540.677	559.583
Viol. crime rate 2005	600.601	518.599	2.521	2.527	25.484	25.223	38.225	36.353	435.769	506.398
Viol. crime rate 2006	0.271	0.232	0.000	0.002	15.128	15.152	10.085	12.768		
Law enforcement employees per capita			0.271	0.265	0.271	0.198			562.748	456.366
Median income	10.771	10.725	10.771	10.765	10.771	10.728	0.271	0.208	0.271	0.235
Below HS education rate	17.000	14.160	17.000	12.250	17.000	14.834	10.771	10.781	10.771	10.671
Unemployment rate	4.980	5.461	4.980	4.674	4.980	4.485	17.000	11.990	17.000	14.390
Population density	6904.873	11160.155	6904.873	3956.261	6904.873	5429.973	4.980	4.440	4.980	5.988
Share black	1.341	1.721	1.341	1.283	1.341	1.514	6904.873	6713.646	6904.873	9683.911
Share Latino	12.282	6.965	12.282	4.242	12.282	5.131	1.341	1.223	1.341	1.499
Share foreign-born	1.628	4.634	1.628	1.823	1.628	1.712	12.282	1.544	12.282	5.619
Poverty rate	11.727	13.297	11.727	11.772	11.727	11.788	1.628	2.646	1.628	3.300
Pre-treatment RMSPE	499.478	552.898	0.000	0.000	0.271	0.198	11.727	11.648	11.727	13.783
	47.26815		0.00215		3.54915		2.33041		62.41095	
Panel B: Goodness of fit for property crime rate predictors in the pretreatment period										
	Total Property Crime		Burglary		Larceny		Motor theft		Arson	
	Real	Synthetic	Real	Synthetic	Real	Synthetic	Real	Synthetic	Real	Synthetic
Prop. crime rate 2002	3698.259	3689.017	958.151	928.109	2588.021	2584.960	152.087	154.165	38.022	37.940
Prop. crime rate 2003	3852.956	3627.223	980.293	912.106	2612.430	2608.716	260.233	258.705	20.212	20.101
Prop. crime rate 2004	3050.779	3194.058	830.764	782.443	2367.422	2365.918				
Prop. crime rate 2005	3043.379	3076.088	557.208	625.963			206.747	204.095	10.085	10.332
Prop. crime rate 2006	0.271	0.241			2172.761	2171.480	166.553	168.314	15.141	15.047
Law enforcement employees per capita	10.771	10.846	0.271	0.207	0.271	0.266	0.271	0.229	0.271	0.199
Median income	17.000	11.181	10.771	10.627	10.771	10.768	10.771	10.759	10.771	10.730
Below HS education rate	4.980	4.342	17.000	12.200	17.000	12.005	17.000	12.847	17.000	15.957
Unemployment rate	6904.873	6679.516	4.980	5.019	4.980	4.560	4.980	4.436	4.980	4.349
Population density	1.341	1.326	6904.873	3032.681	6904.873	6541.994	6904.873	5943.383	6904.873	3601.429
Share black	12.282	2.327	1.341	1.305	1.341	1.160	1.341	1.310	1.341	0.871
Share Latino	1.628	1.602	12.282	1.810	12.282	3.233	12.282	3.852	12.282	10.579
Share foreign-born	11.727	10.643	1.628	2.004	1.628	2.096	1.628	1.765	1.628	1.990
Poverty rate	3050.779	3194.058	11.727	11.820	11.727	11.259	11.727	11.093	11.727	10.580
Pre-treatment RMSPE	120.07557		52.01148		28.01007		4.79028		3.2599	

Appendix 8: Donor Counties and Their Estimated Weights for Synthetic Treated Counties for 2006 Marshalltown Immigration Raid

Panel A: Violent Crime Rates and Sub-indexes									
Total Violent Crime		Murder		Rape		Robbery		Assault	
Donors	Weights	Donors	Weights	Donors	Weights	Donors	Weights	Donors	Weights
Wapello	0.649	Lee	0.386	Van Buren	0.292	Howard	0.528	Wapello	0.878
Muscatine	0.183	Clarke	0.234	O'Brien	0.224	Jasper	0.216	Scott	0.065
Scott	0.112	Fremont	0.204	Webster	0.169	Johnson	0.189	Hamilton	0.057
Hamilton	0.056	Wright	0.174	Crawford	0.127	Clinton	0.053		
		Cerro Gordo	0.001	Scott	0.081	Scott	0.014		
				Buena Vista	0.072				
				Clarke	0.029				
				Muscatine	0.006				

Panel B: Property Crime Rates and Sub-indexes									
Total Property Crime		Burglary		Larceny		Motor theft		Arson	
Donors	Weights	Donors	Weights	Donors	Weights	Donors	Weights	Donors	Weights
Cerro	0.313	Greene	0.345	Cerro Gordo	0.568	Mills	0.335	Franklin	0.359
Mills	0.247	Webster	0.329	Monroe	0.274	Ringgold	0.261	Buena Vista	0.249
Buchanan	0.238	Lucas	0.235	Webster	0.051	Louisa	0.148	Howard	0.163
Webster	0.153	Monona	0.091	Lee	0.041	Scott	0.089	Crawford	0.136
Scott	0.048			Buena Vista	0.041	Lee	0.086	Louisa	0.056
				Scott	0.024	Webster	0.074	Scott	0.036
						Crawford	0.007	Franklin	0.359

Appendix 9: 2006 Marshalltown Immigration Raid Treatment Effects and Standard P-Values of the Pseudo T-Statistic

Year	Crime rates									
Panel A: Total Violent Crime Rates and Sub-indexes										
	Violent Crime		Murder		Rape		Robbery		Assault	
	Estimates	P-values	Estimates	P-values	Estimates	P-values	Estimates	P-values	Estimates	P-values
2007	-268.501	0.000	0	1	-9.097	0.534	4.978	0.466	-259.786	0.000
2008	51.701	0.352	5.075	0.205	6.958	0.602	9.228	0.239	62.257	0.307
2009	-118.033	0.114	-1.100	0.432	-5.269	0.625	-3.679	0.420	-112.964	0.159
2010	-52.662	0.466	-2.680	0.330	-3.237	0.886	-8.337	0.216	32.123	0.625
Standard joint post- treatment p-value	0.716		0.25		0.455		.5		0.636	
Panel B: Total Property Crime Rates and Sub-indexes										
	Property Crime		Burglary		Larceny		Motor theft		Arson	
	Estimates	P-values	Estimates	P-values	Estimates	P-values	Estimates	P-values	Estimates	P-values
2007	530.520	0.182	254.049	0.125	324.335	0.250	72.330	0.068	5.473	0.591
2008	428.246	0.307	32.427	0.841	286.646	0.352	34.080	0.489	-3.100	0.682
2009	1163.082	0.000	297.480	0.057	701.231	0.057	51.898	0.182	22.527	0.125
2010	516.442	0.114	170.534	0.341	219.093	0.318	23.513	0.489	-7.905	0.375
Standard joint post- treatment p-value	0.295		0.386		0.091		0.261		0.477	

Notes: Bold p-values represent statistical significance at the 10% level or higher.

Negative estimates suggest a decrease in crime rate; positive estimates suggest an increase in crime.