The Local Economic Impacts of Prisons*

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Abstract

This paper examines the local economic consequences of prisons using two complementary approaches. The first uses the openings of 230 prisons during the 1990s across the entire United States, and the second uses a quasi-experimental strategy that compares winning and rejected communities in prison site-selection competitions in Texas. I find that prisons decrease housing values by 2-4 percent and lead to substantial changes in neighborhood composition, specifically towards low socioeconomic status individuals. The negative housing value impacts are localized to the neighborhoods that are closest to the new prisons, while the economic benefits of prisons are spread across broader communities. In particular, counties where prisons opened experienced an increase in total employment driven largely by the jobs at the new prisons. Prisons thus fail to create substantial spillovers to other sectors or provide a major boost to local economies beyond the direct effect of prison employment. Lastly, I find that after the opening of a prison, local labor markets in treated counties are less responsive to macroeconomic shocks, consistent with the view that jobs at prisons are "recession proof."

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1 Introduction

During the last three decades, growth in the prison population in the United States has been the largest in the country's history. With an almost five-fold increase from 1980, the U.S. prison system in 2010 held over 1.6 million individuals.[1] To accommodate this growth, the federal and state governments nationwide have engaged in an equally dramatic boom in prison building. The number of state and federal prisons located across the United States increased from about 900 facilities in 1980 to over 1,400 in 2005. The prison construction boom has generated a lot of debate and controversy about how prisons affect the communities in which they are situated. Citizen groups opposed to prisons often cite falling property values, damage to the community's reputation and its ability to attract businesses, and other concerns associated with living close to prisoners, such as the need to share local resources. However, prisons can have positive economic impacts arising from the "boomtown" phenomenon, during which local areas experience increases in population, employment, business activities, and government revenues. In addition to these arguments, prison openings can also lead households to sort across neighborhoods based on their willingness to live near a prison. For example, prisons may attract low-educated and ethnic minority individuals, and prompt higher-income households to move out of neighborhoods. This household sorting could exacerbate income segregation and the geographic concentration of poverty in an area. Although there exists some anecdotal and correlational evidence on these questions, systematic evidence of the benefits and costs of prisons for local residents is still missing. This paper fills the gap by empirically investigating the impacts of prisons on local communities across a range of dimensions.

In addition to its direct implications for communities making decisions about allowing prison construction, this paper contributes to an important debate about the political economy of mass incarceration. To the extent that the perceived impacts of prisons, particularly the widely anticipated economic benefits to communities, inform the decisions of criminal justice officials to build more and expand existing prisons, which, in turn creates economic and political pressure to fill additional beds,[2] prison construction can play an important role in fueling mass incarceration through this "self-reinforcing" cycle. This paper takes a first

[1]This figure is the total number of prisoners under the jurisdiction of Federal or State correctional authorities, according to the Bureau of Justice Statistics, Department of Justice. The local jail population is not included.

[2]For discussion of the debate see, for example, Schlosser (1998), Chen (2009) and Cohen (2015).

step towards rigorously characterizing the consequences of prisons for local communities and improving our understanding of an essential element of the prison boom.

To estimate the impact of prisons, I employ two main empirical approaches. The first uses the openings of 230 prisons during the 1990s across the entire United States. The key empirical challenge in this analysis is the construction of a counterfactual for locations where prisons were opened. Prisons tend to be sited in neighborhoods whose housing and demographic characteristics differ from the rest of the United States. To account for these differences, my analysis controls for the lagged dependent variable and a rich set of neighborhood characteristics from the pre-treatment period, and it includes state-urban/rural area fixed effects to account for local shocks that affect all urban or all rural census tracts within a state. This strategy offers considerable advantages over a conventional approach that relies on a cross-sectional comparison between communities with and without a prison. To improve covariate balance between the treated and the control communities and to provide robustness checks to the results based on the entire Unites States, my analysis is conducted with a trimmed sample and propensity score matched sample. I also show that alternative empirical strategies that use Difference-in-Differences and Parametric Reweighting estimators (Busso, Gregory, and Kline, 2013) produce similar results.

To complement the U.S.-wide analysis, my second empirical approach uses a quasi-experimental strategy based on a prison site-selection process in Texas. During the late 1980s and early 1990s, as part of its massive state prison construction program, Texas Department of Criminal Justice (TDCJ) conducted a series of statewide site-selection competitions to select communities to host its new prisons. Many towns and counties submitted proposals for consideration. I use rejected communities from the competitions as control communities in order to estimate the impact of prisons. Given that these communities were nominated to participate in the site-selection competition by their local governments, it is likely that they shared unobserved characteristics and trends with the communities that eventually won prisons, which gives credibility to the identification assumption made for causal inference. For both the U.S. and Texas cases, I investigate the impact of prisons using two geographic definitions of a local community: census tract and county. The census tract specification captures localized impacts on communities in the immediate vicinity of prisons, and the county specification shows prisons' effects on the broader community.

There are four primary findings. First, census tracts that experienced prison openings saw a modest decline in housing values of 2-4 percent, compared to neighborhoods with similar housing and demographic characteristics. The baseline estimates imply an average loss in housing values of approximately \$6 million per prison in 2016 dollars. Although not negligible, this loss is small compared to, for example, the cost of constructing a new prison. However, it is important to note that most of the prison openings during this period took place in relatively disadvantaged neighborhoods, whose baseline property values and household incomes were much lower than in the rest of the United States. For already-poor neighborhoods, this loss could be disproportionately high relative to the wealth of local households.

Second, prison openings lead to changes in the composition of the neighborhood workforce and population as households sort in and out of neighborhoods based on their willingness to live near a prison. The results indicate that census tracts in which prisons were opened became poorer, experiencing 2-6 percent decreases in average household income. I find that this decline is explained by the net in-migration of individuals who have lower earning capacity. After accounting for these compositional changes, prisons do not significantly improve nor depress local wages and income. The evidence also reveals a small increase in the proportion of racial and ethnic minority households in the census tracts where prisons open.

Third, although there is little evidence of increased employment in neighborhoods directly proximate to prisons, I find an increase in total employment of 1-2 percent at the county level driven largely by jobs at the prisons. This pattern is consistent with anecdotal evidence that indicates that although prisons create new jobs, many workers commute to work from outside the neighborhoods that are adjacent to prisons. In other words, the employment benefits are spread out geographically, and they are not necessarily reaped by households that reside close to prisons. Moreover, the increase in jobs at the county level is mostly confined to the state government sector, which operated most of the prisons that opened during this period.[3] Aside from these direct gains in jobs at prisons and a temporary boost to the local construction industry, I find neither a positive nor negative spillover to employment and economic activities in the private sector. On the one hand, these results run contrary to claims made by prison proponents, including government officials who promote

[3]The majority of prisons that were opened in the 1990s are state operated and the majority of their employees are state government employees.

prisons to communities. On the other hand, I find no support for the opposing argument that prisons, which are large government employers, can crowd out resources and the economic activities of the private sector. Additionally, there is no evidence that prisons attract external government funds or increase local government expenditures on local infrastructure. In short, prisons bring direct employment gains but fail to create spillovers to other sectors or generate substantial boost to local economies.

The impacts of prisons on local economies can extend beyond their effects on the level of employment and economic activities. My fourth finding concerns an economic benefit that is potentially unique to the prison industry, as a "recession-proof" employer. Unlike other private employers, prisons mostly create government jobs that are thought to be more stable than private sector jobs because they provide superior job security. The prison industry also tends to be less susceptible to business cycles: indeed, sometimes they expand in size during hard times. Prisons' reputations as "recession-proof" is a key argument made by prison proponents for why prison jobs are desirable for communities. Specifically, I investigate whether economic conditions in communities with prisons are affected less by macroeconomic shocks than similar communities without prisons. I find that prisons indeed dampen the responsiveness of local labor markets to state-level macroeconomic shocks. Compared to non-prison counties with similar characteristics, unemployment in counties that have prisons are 5-11 percent less sensitive to the state unemployment level.

Overall, the negative housing value impacts and compositional changes towards lower socioeconomic status households are limited to communities immediately proximate to prisons. In contrast, the benefits of prisons, including direct job creation and reduced sensitivity to macroeconomic shocks, are spread through larger communities. The findings suggest that the prison construction boom may have had important distributional consequences because the negative impacts of prisons were borne by communities closest to prisons, which were generally much more disadvantaged compared to the rest of the country preceding the prison construction boom.

This project contributes to an extensive literature that investigates the impact of desirable or undesirable facilities on local housing markets (e.g., Gayer, Hamilton, and Viscusi, 2000; Greenstone and Gallagher, 2008; Davis, 2011; Currie et al., 2015; and Muehlenbachs, Spiller, and Timmins, 2015). To the best of my knowledge, this paper is the first large-scale study to estimate the causal effect of prisons on property values. Like many of the facilities investigated in this literature, such as airports and power plants, prisons, too, potentially affect local labor markets and businesses, and they share some characteristics with "place-based" policies that target transfers toward particular geographic areas (see e.g. Busso, Gregory, and Kline, 2013; Neumark and Kolko, 2010; Kline and Moretti, 2014; Bondonio and Greenbaum, 2007; and Ham et al., 2011). Investigating a wider set of outcomes in addition to housing values, this paper provides a fuller picture of the local economic consequences of what is commonly perceived as an undesirable facility.

This paper also makes several contributions to the interdisciplinary literature that examines the effects of prisons on local communities (see review in Glasmeier and Farrigan, 2007). First, I develop improved identification strategies for isolating the causal impact of prisons. Second, my use of both smaller and broader geographic units allows me to more accurately measure the localized and spread-out impacts of prisons. Third, I pay careful attention to household sorting behavior and the evolution of neighborhood demographics near prisons—factors that to my knowledge have not been empirically examined in this context. Fourth, by examining all prisons opened in the United States I provide a comprehensive measure of the impacts of prisons across the entire nation. Much of the previous research has focused only on certain geographic regions. My paper's estimates provide systematic evidence that could help communities and policymakers make better-informed decisions in the future about allowing prison openings and managing their consequences. Although in recent years the incarceration rate in the United Stated has dropped from the peak of 2007-8, the rate is still more than three times higher than it was for most of the 20th century. In other words, the criminal justice system across the United States and in other countries will most likely continue to build new prisons, either to expand capacity or replace older facilities.

Finally, my paper contributes to the broad literature on the prison boom, particularly with regards to the consequences of that boom and debate about the political economy of mass incarceration. To date, the literature has paid particular attention to the impact of incarceration on incarcerated individuals, including recidivism and labor market outcomes (see, for example, Kling, 2006; Chen and Shapiro, 2007; Mukherjee, 2017; and Mueller-Smith, 2015), and social inequality in incarceration (see, for example, Pettit and Western, 2004). My paper, in contrast, focuses on the expansion of the prison network into more communities. This aspect of the prison boom, which affects not only those who are directly involved with the criminal justice system, could also play an important role in processes that promote mass incarceration itself.

This paper proceeds as follows. Section 2 briefly provides relevant background about prison construction and the local impacts of prisons. Section 3 presents the analyses based on my first empirical approach for the United States; it first lays out the empirical strategy, describes the data and reports the results. Section 4 presents the analyses based on my second approach, which use quasi-experimental variation for Texas; its structure resembles that of Section 3. Lastly, Section 5 concludes and discusses some future directions.

2 Background on the Prison Construction Boom

During the last three decades of the 20th century, the corrections sector in the United States massively increased in size and scope. Factors like the "three strikes" law and longer and more severe sentences for first-time drug-related offenses exponentially increased prisoner populations. Beginning in the early 1980s, prison overcrowding became one of the most pressing issues confronting the criminal justice system in nearly every state (U.S. Department of Justice, 1988). Shortages in prison capacity led correctional authorities across the country to construct new facilities, and prison construction experienced explosive growth during the 1980s and 1990s. The number of state prisons increased from under 600 in 1979 to over 1,000 in 2000, a 73 percent increase. Likewise, prison capacity almost doubled, from under 0.7 to 1.3 million inmates during the 1990s alone.

In addition to responding to an increased demand for prison beds, prison building programs in some states also operated as an economic development strategy (Glasmeier and Farrigan, 2007). In particular, many rural communities suffering from declines in traditional industries chose to actively attract and tie their economies to prisons. In some cases, such as during the 1990s in Texas, communities engaged in intense competitions as they bid for new prisons to be sited in their communities. To increase their competitiveness, communities offered various incentives in the form of free land, infrastructure development (such as roads and hospital wings), and low utilities rates. The acquisition of prisons as an intentional economic development strategy for depressed communities may have had important implications for the rise and growth of mass incarceration in the United State. Communities' demand for prisons might have prompted correctional authorities to decide to build and expand more prisons. Importantly, the prison industry, to which communities turned for growth opportunities, is itself dependent on the continuation of crime-producing conditions and growth in incarceration.

In any case, it is not clear that pursuing prisons is a prudent economic development strategy for communities. Prison construction advocates argue that prisons bring "recession-proof" jobs, create demand for local goods and services, increase the local population, add to the tax base, and attract external government funds that improve local infrastructure (Glasmeier and Farrigan, 2007; and Doyle, 2002). On the other hand, opponents argue that prisons reduce local property values, place demand on local social, health, and public services, and increase crime in communities. Opponents also argue that prisons do not provide much economic stimulus, and can damage the reputation and ability of communities to attract and retain businesses. Because they are large government employers, prisons can also crowd out resources and the economic activities of the local private sector. The two divergent perspectives indicate that prisons can potentially bring economic benefits in terms of employment, income, migration, and local government revenue, but they also might be a source of numerous local negative externalities.

To provide systematic evidence of the costs and benefits of prisons for local communities, this paper evaluates the impact of prisons on a wide range of outcomes, including property values, employment, income, demographic composition of residents and local government finances. I employ two geographic definitions of the local community: a census tract and a county. The rationale for this approach is two-fold. First, some impacts, such as the effect on property values and the demographic composition of local residents, are likely to affect only neighborhoods closest to prisons. To detect these effects, I conduct analyses at the census tract level, which is the smallest geographic unit that can be matched across the 1980 through the 2000 Censuses. On the other hand, some impacts, including those on employment and economic activities, are likely to affect broader communities. This is so because some prison workers commute to work; moreover, there could be spillovers to nearby neighborhoods and to other sectors of the local economy. Second, the impact on the same outcome can be different in neighborhoods that are directly proximate to prisons and in broader communities. Analyzing at both levels allows me to provide a more complete picture of the local consequences of prisons.

Lastly, note that local jails are excluded from this paper's analysis. In the United States, jails are designed to hold individuals awaiting trial or serving short sentences and are run by local governments and sheriffs. Most local jails are small, with a 50-bed capacity or less.

Prisons, in contrast, house individuals convicted of crimes and are under the jurisdiction of state governments or the Federal Bureau of Prisons (BOP). Generally they are much larger in capacity and budget. My analysis includes all prisons under the authority of state governments and the BOP, including those that operate under contract with private enterprises, plus a small number of facilities jointly administered by state and county governments.

3 Analysis for the United States

This paper uses two complementary empirical approaches to estimate the local impacts of prisons. The first investigates the openings of 230 prisons during the 1990s across the entire United States.

3.1 Empirical Strategy

3.1.1 Baseline Specification

The baseline approach is described in the following systems of equations:

$$y_{i2000} = \alpha T_{i2000} + X'_{i1990}\beta + \epsilon_{i2000}, \tag{1}$$

$$T_{i2000} = X'_{i1990}\gamma + \eta_{i2000}. \tag{2}$$

Equation (1) describes the regression model I estimate. I will use equation (2) below to articulate the identification assumption. Each observation is a census tract or a county. For brevity, the following description refers to census tracts as the geographic unit but an analogous description applies to the county-level analysis. y_{i2000} is the outcome of interest, for example, log of median housing value, of census tract i in 2000. The indicator variable T_{i2000} takes the value 1 for census tracts where a prison opened between 1990 and 2000. Vector X_{i1990} includes control variables that may also determine whether the census tract experienced prison openings, including a rich set of housing and demographic characteristics. ϵ and η are the unobservable components of the outcome and prison opening status, respectively.

A few notable features of vector X_{i1990} are as follows. First, this vector is restricted to variables measured in pre-treatment periods, i.e., in 1990 or earlier, because variables from 2000 may be affected by prison openings and therefore may confound the effect of prisons if included. In all specifications, therefore, X_{i1990} includes control variables from the

1990 Census and in some specifications also includes controls from the 1980 Census. Second, vector X_{i1990} contains the lagged dependent variable (e.g., log of mean housing value in 1990) to adjust for permanent differences in the outcome across census tracts and mean reversion. Third, the vector also includes state-urban area fixed effects[4] to account for local shocks that affect all urban or all rural census tracts within a state.

The coefficient α measures the impact of prison openings on the outcome of interest after controlling for the lagged dependent variable and other covariates. The condition for consistent estimation of α based on least square estimation of equation (1) is $E(\epsilon_{i2000}\eta_{i2000}) = 0$ or that unobserved determinants of house prices are uncorrelated with prison openings after controlling for X_{i1990}. The assumption is a plausible approximation given the rich set of housing and demographic characteristics from the census data and the inclusion of state-urban area fixed effects. However, it is important to assess the robustness of the results across specifications and also address the concerns related to ordinary least square estimation when the covariate distributions differ substantially across the treated and control units (Imbens and Rubin, 2015). I discuss in the next sub-section the alternative estimation strategies that I employ to provide robustness checks to the baseline specification.

3.1.2 Alternative Strategies

First, I estimate the impact of prisons using long-difference models. Specifically, I estimate the census tract (or county) level regressions of the form:

$$\Delta y_{i2000} = \alpha T_{i2000} + X'_{i1990}\beta + \epsilon_{i2000}, \tag{3}$$

where Δy_{i2000} is the change in some outcome over the period 1990-2000 in community i, and T_{i2000} and X_{i1990} are as defined above except that vector X_{i1990} in this specification does not include the lagged dependent variable.

Second, I show robustness of the results to the Parametric Reweighting approach following Busso, Gregory, and Kline (2013). Specifically, I estimate interacted regressions of the following form to allow for flexible patterns of treatment effect heterogeneity.

$$\Delta y_{i2000} = \mu^1 T_{i2000} + (1 - T_{i2000}) \times X'_{i1990}\beta + \epsilon_{i2000}, \tag{4}$$

[4]There are two categories per state, urban or rural area. A census tract is considered to be rural if some of its population reside outside of urbanized areas and urban clusters delineated by the Census Bureau.

where $\mu^1 \equiv E[\Delta y_{i2000}|T_{i2000} = 1]$. In this specification, the mean change in outcomes among the control tracts is assumed to be a linear function of X_{i1990}. However, the mean change in outcomes among the treated tracts is not assumed to take any specific functional form of X_{i1990}. An estimate of the average treatment effect on treated tracts ($\widehat{ATT}$) can be formed as

$$\widehat{ATT} = \hat{\mu}^1 - \frac{1}{N_1}\sum_i T_{i2000}X'_{i1990}\hat{\beta}. \tag{5}$$

where N_1 is the number of treated tracts. If the control is well described by a linear model, this approach gives an estimate for the average impact of prisons on treated communities while allowing for treatment effect heterogeneity. This estimator can also be interpreted as a propensity score reweighting estimator with weights derived from a log-logistic propensity score model (Kline, 2011). I report the estimates based on these alternative empirical strategies in the Appendix.

3.1.3 Time-series Difference-in-Differences Estimation

For some of my outcomes, namely employment and number of establishment, annual data are available at the county level. This allows yearly analyses that investigate the dynamic of the impacts with respect to the timing of prison openings. My specification includes both leads and lags providing an opportunity to assess if differential pre-trends pose a challenge to my research design and examine the evolution of the treatment effects over time. Specifically, I estimate the following equation for outcome variable y_{it}, where the subscripts refer to county (i) and year (t).

$$y_{it} = \sum_{\tau=-5}^{5} \alpha_\tau \text{open}_{i,t+\tau} + C_i + \gamma_t + \epsilon_{it} \tag{6}$$

The treatment variable of interest $\text{open}_{i,t+\tau}$ takes value 1 for counties where a prison opened in year $t+\tau$. This set of indicator variables turns on at different times in different counties depending on when the prison was opened and is always 0 for counties that did not receive any prison during my sample period. I allow for a total of 10 leads and lags, running from 5 years ahead to 5 years or more after the prison was opened.[5] With the exception of $\text{open}_{i,t-5}$, each indicator turns 1 only in the relevant year and returns to 0 in

[5]The omitted category is 6 years or more before prison openings.

all subsequent years thereby capturing the effect in that particular event year. $\text{open}_{i,t-5}$ remains 1 for all subsequent years and measures the post-treatment effects 5 years and over. The specification includes county fixed effects, C_i and year fixed effects, γ_t. The standard errors are clustered at the county level to allow for arbitrary serial correlation in the error terms from the same county. In the subsequent analysis, I plot the coefficients α_{-5} to α_{+5}, which measure differences in the outcomes between counties with prison openings and control counties by event year, i.e., before, during and after prison openings. These plots show the evolution of my outcomes over time and also allow the assessment of pre-trends and the validity of my identification strategy.

3.2 Data Sources

This section describes the data I use to construct my outcome measures and treatment variables, used in both the United States and Texas analyses.

3.2.1 Prisons Openings and Characteristics

To construct prison opening status, my main treatment variable, I combine information from a number of sources. First, the 1990, 1995, 2000 and 2005 Census of State and Federal Adult Correctional Facilities (hereafter CCF), conducted by the U.S. Census Bureau for the Bureau of Justice Statistics, provide enumeration of state and federal correctional facilities in the entire United States and information on various facility characteristics such as the types of inmates housed, security level, facility operations and number of inmates and staffs. Because the CCF contains incomplete information about year of prison openings, I started off by inferring the time period by which each prison opened using its presence in each year of the CCF. I then hand-checked each one of the prisons that appeared to have opened during the 1990s based on the CCF, using information from state department of corrections' websites and annual reports to ensure that I have an accurate list of prison openings during this decade. This exercise ruled out, for example, facilities that appeared to have opened but in fact were existing facilities that underwent changes in the facility name. For my analysis, another important piece of information is the location of prisons. The geographic coordinates of each prison location were searched by hand using Google Maps combined with address information from the CCF, state department of corrections' websites and reports, and the Directory of Adult and Juvenile Correctional Departments, Institutions, Agencies, and Probation and Parole Authorities published by the American Correctional Association

(ACA).

3.2.2 Demographic and Housing Characteristics

The Decennial Census data are the source for my outcome measures on housing values and neighborhood demographic characteristics, and my pre-period housing and demographic controls.[6] Data for census tracts come from Geolytics Neighborhood Change Database (NCDB), which includes information from the 1980, 1990, and 2000 Census Summary Files normalized to form a panel of census tracts based on 2000 census tract boundaries. Census tracts contain approximately 4,000 people and are the smallest geographic unit that can be matched across 1980 to 2000 Censuses. In 1980, however, the Census Bureau only tracted "urban" areas or those belonging to a metropolitan area so the remaining areas of the country cannot be matched to census tracts in later census years. For this reason, and the fact that many prisons were built in rural areas during this time, my analysis focuses on the period 1990-2000, when we can analyze all prison openings in the United States at the census tract level.

Housing and demographic data for counties come directly from the 1980, 1990 and 2000 Census Summary Files published by the Census Bureau.

As discussed in the relevant analyses below, there are some important limitations with these public-use aggregate census data. To complement and extend the analyses in this paper, I have applied and recently obtained an approval to access the restricted census microdata. Section 5 discusses briefly the key areas of future extensions enabled by the use of the restricted data.

[6]The housing characteristics controls used in the analysis are log total owner-occupied housing units; log total renter-occupied housing units; proportion of total housing units that are owner occupied; proportion of total housing units that are occupied; proportion of housing units with zero, one, two, three, four, and five or more bedrooms; proportion of total housing units that are single-unit detached; proportion of total housing units that are single-unit attached; proportion of total housing units that consist of two, three or four, and five or more units; proportion of total housing units that are mobile homes; proportion of total housing units built within the last 5 years, six to twenty years ago, twenty to forty years ago, and more than forty years ago; and proportion of total housing units with complete plumbing facilities. Demographic characteristics include log mean household income, proportion of total persons below the poverty line; proportion of households with public assistance income last year; population density; proportion of occupied housing units with black householder; proportion of occupied housing units with Hispanic householder; proportion of population under age 18; proportion of population age 65 or older; proportion of population over age 25 without a high school diploma; proportion of population over age 25 with a college degree; unemployment rate, log number of employed individuals age 16 or older; proportion of population that is foreign born; proportion of households female headed; and proportion of population in correctional institutions.

3.2.3 County-Annual Data on Employment, Earnings and Establishments

The Quarterly Census of Employment and Wages (QCEW) is the source for my county-level annual outcomes on employment, earnings and number of establishments used in my time-series Difference-in-Differences analyses. Produced by the Bureau of Labor Statistics, this data set is based on unemployment insurance records and constitutes a near-census of employment and earnings covering more than 95 percent of all jobs in the United States.[7] I use the data from 1985 to 2005 for outcomes by sector (e.g., private and state government sectors) and from 1985 to 2000 for industry-specific outcomes.[8] Note that County Business Patterns (CBP) constitutes an alternative data source. However, it has a number of shortcomings for our purposes. Most importantly, unlike the QCEW, the CBP data excludes most government employees. Since the majority of workers in prisons are government employees, the government sector is a key sector in my analyses.[9]

In addition, to study the potential benefit of prisons in reducing the sensitivity of local economies to macroeconomic conditions, I use 2001-2010 county and state annual employment data from the QCEW and unemployment data from the Local Area Unemployment Statistics (LAUS) program. The Bureau of Labor Statistics' LAUS program provides unemployment estimates on the basis of unemployment insurance claims, data from other government surveys such as the Current Population Survey, and statistical modeling.[10]

3.2.4 Local Public Finance

Data on local government revenue and expenditure come from the U.S. Census Bureau's historical data on State and Local Government Finances. This information was collected in the Census of Governments conducted every 5 years (years ending in 2 and 7). This dataset contains financial records of individual local governments, including counties, cities, townships, special districts and independent school districts. For my analysis, I use data at the county area level, aggregating expenditures and revenues of all individual local governments

[7] Those excluded from the QCEW are primarily certain agricultural, domestic, railroad, and religious workers.

[8] The Bureau of Labor Statistics began using the NAICS-based industry classification system instead of the SICS classification in 2001. I use the SICS-based county files from 1985 to 2000 for industry-specific outcomes.

[9] In addition, the CBP is available by SIC industries from 1990-1997 and by NAICS industries from 1988 onwards. This break in the data series represents another limitation of the CBP for studying prison openings during the 1990s.

[10] The LAUS unemployment statistics are based on the place of resident, not on the place of work as is the case for the QCEW data (indeed, unemployed workers are employed in no county).

within the county. My outcome measures for 2000 are based on the averages of data from 1997 and 2002 and my pre-period controls uses data from 1987. Variables described as per capita refer to the line item divided by the county population that resides in households, which excludes incarcerated individuals. These measures are inflation adjusted to 2016 dollars using the Consumer Price Index (CPI) produced by the Bureau of Labor Statistics (BLS).

3.3 Analysis Samples and Summary Statistics for the United States

This section describes how I construct my analysis samples and presents summary statistics for tracts and counties in my samples. Following Imbens and Rubin (2015), I first assess the covariate balance of the full sample of communities and then create subsamples with improved covariate overlap by discarding some units from the original sample. These exercises form the first step in the overall strategy for robustly estimating the causal effects of interest. Since this step does not involve any outcome data and focuses solely on the treatment indicators and covariates, there is no concern with intentionally introducing biases in the subsequent causal effect estimation.

3.3.1 U.S. Census Tract Samples

The U.S.-wide analysis is conducted with three samples of census tracts. First, the full sample consists of all 2000 census tracts in the United States with nonmissing housing value data in 1990 and 2000, of which 255 tracts are treated tracts where prison was opened. As will be shown below, covariates of treated and control communities in the full sample differ significantly, reflecting the fact that prisons tend to be sited in neighborhoods with housing and demographic characteristics that differ from the rest of the United States. Limited covariate balance can make subsequent inferences imprecise and sensitive to different estimation methods and specifications. I follow Imbens and Rubin (2015) and use two systematic approaches to improve covariate balance resulting in my second and third analysis samples.

The second sample, which will be referred to as the "trimmed sample," is created by dropping observations with extreme values of the propensity score from the full sample, based on a method proposed by Crump et al. (2009). The motivation for this approach is that if there is a value of the covariate such that there are few treated units relative to control units, then the variance for an estimator of the average treatment effect for that value of the covariate will be large. Hence, dropping units with such covariate values should improve the asymptotic variance of the efficient estimator. This criterion leads to discarding observations with propensity scores outside an optimal threshold.[11] To estimate the propensity score for prison opening treatment, I follow Imbens and Rubin (2015)'s systematic approach to selecting first and second order covariates from a rich set of pre-treatment neighborhood characteristics to include in the propensity score estimation. More details on trimming can be found in the Appendix.

The third sample, which will be referred to as the "matched sample," is constructed using propensity score matching. Specifically, I construct the control group by matching each treated tract with two tracts in the same state with the closest propensity score. I now present summary statistics and assess comparability of the treated and control tracts in my three analysis samples. Overall, trimming and matching improve covariate balance significantly. The matched sample achieves better balance while the trimmed sample uses more of the available data.

Column (1) of Table 1 reports mean demographic and housing characteristics from the 1990 Census for census tracts where prisons opened during the 1990s.[12] Column (2), (3) and (4) describe these characteristics for control tracts in the rest of the United States, the trimmed sample and the matched sample respectively. The numbers reported in Columns (5) to (7) are normalized differences between the treated and control neighborhoods in each of my samples, defined as the difference in means scaled by the square root of the average of the two within-group variances. The asterisks report the significance levels of the t-tests of mean equality between the treated and control neighborhoods, with one, two and three asterisks indicating a p-value less than 0.1, 0.05 and 0.01 respectively. The normalized difference is arguably more useful for assessing covariate balance than a t-test (Imbens and Rubin, 2015). The t-statistics may be large in absolute value simply because the sample is large. Large values of the normalized difference, on the other hand, indicate that the sample means are substantially different between the treated and control groups. Using normalized differences

[11] The threshold is determined solely by the joint distribution of covariates and treatment status and does not depend on the distribution of outcomes, therefore no bias is introduced with regard to the treatment effect being analyzed.

[12] Column (1) reports mean characteristics for the treated tracts in the full and matched sample. Since trimming drops both treated and control tracts with extreme propensity scores, the treated group in the trimmed sample is a subset of the treated group in the full (or similarly the matched) sample. Specifically, trimming excludes 31 treated tracts. For brevity of exposition, I report mean characteristics of the treated tracts in the trimmed sample in the Appendix. Note that Column (6) reports the normalized differences and the results of t-tests based on comparing the treated and control tracts in the trimmed sample (and not between Columns (1) and (3)).

is especially appropriate in my setting given that sample sizes also vary across samples (e.g., the matched sample is smaller than the full sample).

In the full sample, Column (5) indicates that there are important differences between the treated and control sets of tracts. Compared to the remainder of the United States, neighborhoods with prison openings were more rural and residents had substantially lower socio-economic status; for example, mean household income in 1990 were almost 25 percent lower in these neighborhoods. The null hypothesis of equal means is rejected for the majority of the characteristics, and while most of the normalized differences do not exceed 1, there are various characteristics with substantial differences, notably median house prices and rents and demographic characteristics with regards to population density, income, and education of residents.

Column (6) compares the treated tracts with the control tracts in the trimmed sample. Covariate balance in the trimmed sample is much improved from the full sample. For example, the large differences in median housing values, average income and population density in the full sample are not evident, either statistically or economically in the trimmed sample. Although the differences for some characteristics are statistically significant, none of the normalized differences exceeds 0.3, providing little evidence of economically meaningful differences in the mean characteristics between treated and control neighborhoods in the trimmed sample.[13] Nevertheless, all of my specifications will control for the full set of these pre-treatment characteristics.

Finally, Column (7) compares tracts with prison openings and control tracts in the matched sample. Propensity score matching performs well at greatly reducing the covariate imbalance. The comparison finds no evidence of statistically or economically substantial differences between the treated and control groups. Overall, these findings suggest that the trimming and matching procedures are effective in generating plausible control neighborhoods.

3.3.2 U.S. County Samples

County-level analyses are also conducted using three samples. Table 2 compares counties where prisons were opened with control counties in the full, trimmed and matched samples constructed using similar techniques to those described in the previous section. Columns (1)

[13]For example, while the null of equal means is rejected for the proportion of residents without a high school diploma, the absolute difference in the mean is less than 2 percentage points.

to (7) in Table 2 report analogous quantities for counties as those in Table 1 for census tracts. Overall, a similar pattern applies to counties. Column (5) shows that counties that experienced prison openings are on average more rural and residents have lower socio-economic status compared to counties in the rest of the United States, although these differences are in general less economically substantial than those observed between the treated and control neighborhoods in the full sample of census tracts. While both trimming and matching techniques improve covariate balance between the treated and control counties, matching performs significantly better in this case.[14] Based on the normalized differences in Column (6), trimming performs well in terms of the economic magnitudes of the differences, with the maximum value of the normalized differences of 0.3. However, in terms of statistical significance, treated counties are still different from control counties in the trimmed sample across various dimensions, particularly with regards to demographic characteristics. This stresses the importance of controlling for observable characteristics from the pre-treatment period in the estimation that follows. Propensity score matching, on the other hand, performs well on both measures. The comparison in Column (7) finds little evidence of differences between the treated and control counties in the matched sample, either statistically or economically. This is summarized by the inability to reject the joint F-test for the hypothesis that these pre-treatment characteristics are jointly uncorrelated with prison openings (with an F-stat of 0.68 and p-value of 0.92).

3.4 Results for the United States

This section reports the empirical findings based on my first empirical approach. The section starts by presenting the tract-level results, analyzing the highly localized effects of prisons. Next it reports the county-level results, analyzing the effects on broader communities.

3.4.1 U.S. Census Tract-level Results

The analysis begins with Table 3, which investigates the impact of prison openings on median housing values and rents. Each entry gives the estimated impact on the outcome presented in each row. The table reports least squares estimates of α from two variations of the specification in equation 1 using three alternative samples. Column (1) uses the full

[14] Trimming drops 17 treated counties and 1,266 control counties with extreme values of the estimated propensity scores from the full sample of counties.

sample of census tracts; Columns (2) and (3) use the trimmed sample and Columns (4) and (5) use the propensity score matched sample. The odd columns report estimated coefficients and standard errors from a specification that controls for the 1990 dependent variable and the complete set of housing and demographic characteristics from 1990 as well as state-urban area fixed effects. The even columns also add tract-level housing and demographic characteristics from 1980 to control for differential time trends within an urban or rural area in the same state.[15] The point estimates are largely consistent across the three samples of tracts, with estimates based on the matched sample being slighter larger, and the addition of 1980 covariates has little effect on the estimates. Overall, the results indicate that neighborhoods where prisons were opened experienced a 2.8-4.3 percent decrease in housing values compared to neighborhoods with similar housing and demographic characteristics.

Rental rates, on the other hand, exhibit no perceptible decline in any specification. For these null findings, I compute the minimum detectable effect size (MDE), that is, the effect that would have been detectable with 80 percent power at the 5 percent significance level ex post (Haushofer and Shapiro, 2016; and Duflo, Glennerster, and Kremer, 2008). This approach provides an intuitive measure to distinguish tightly identified null results from those that are not statistically significant but for which we cannot rule out meaningful treatment effects with confidence. The MDEs for rents range from 2.8 - 3.6 percent based on the estimated standard errors in Columns (3) to (6). While I am powered to detect relatively small effect sizes, the decline in rents may have been smaller than my MDE. The discrepancy between rents and housing values may reflect the fact that housing values and rents of owner occupied housing units are self-reported in the Census (Busso, Gregory, and Kline, 2013). If housing markets in the neighborhoods are relatively illiquid, owners may overestimate the extent to which prisons have decreased the value of their residence. On the other hand, rents which are paid monthly may be easier to assess. I will be able to examine these conjectures in more detail using confidential census microdata. Specifically, I can re-estimate the impact of prison openings on housing values and rental rates using only households who moved into their housing units less than five years ago. Since these homeowners recently purchased their houses, they may have a more accurate sense of their market value. In any case, my current estimates of the decline in housing values reflect local residents' valuation of the amenities

[15] As noted in the data section, nonurban areas were not tracted in 1980 so data are not available for nonurban Census tracts in 1980. To keep all observations, the specifications in my even columns also include dummy variables for missing value of 1980 controls, one for each of the 1980 characteristics.

and disamenities that come with prisons and the prospects of their neighborhoods.

It is worth noting that there is an extensive literature investigating the capitalization of various amenities into local housing values and that 3-4 percent is a moderate effect for the census tract-level one, especially considering that many prisons were opened in rural areas where each census tract covers a relatively large geographic area.[16] For comparison, Pope and Pope (2015), for example, find that a new Walmart store increases house prices by 2 -3 percent for houses located within 0.5 miles of the store, a geographic unit much smaller than an average census tract in my analysis. Further, Davis (2011) finds that power plants lead to a 3-7 percent decline in housing value but over the smaller unit of census blocks, while Greenstone and Gallagher (2008) find economically small and statistically insignificant changes in residential property values at the census tract level following the Superfund cleanups of hazardous waste sites. On the other hand, Busso, Gregory, and Kline (2013) finds a much larger increase in housing value of roughly 30 percent in urban census tracts that received the Empowerment Zone program while Bartik et al. (2016) finds a 5.7 percent increase in housing values at the county level due to hydraulic fracturing operations.

The estimates in Table 3 can be used to calculate a measure of the total loss in housing values from prison openings. The mean housing values in 1990 reported in Table 1 and the average number of housing units in census tracts with prison openings (1750 units) imply that the average total value of the housing stock in treated neighborhoods is $161 million in year 2016 dollars. Multiplying this by the estimate from Column (6) of Table 3 (3.7 percent) yields an average total loss in housing value of approximately $6 millions per prison (in 2016 dollars). While not negligible, this loss is relatively small compared to the capital cost of a new prison. For example, the average cost of constructing a medium-sized prison with 1,000 beds was $118 millions. Given that state capital expenditure on prisons accounted for between 5 and 14 percent of total correctional expenditure during the 1990s, the costs of prisons associated with losses in local housing values is only a small proportion of the overall costs of the correctional system. However, it is important to note that most of the prison openings during this period took place in relatively poor neighborhoods, with much lower baseline property values and household income than the rest of the United States. The loss in housing values for these neighborhoods may be substantial relative to household assets.[17]

[16] The average land area of census tracts with a prison in my sample is 248.7 square miles (median 84.2 square miles). This is equivalent to a circle with a radius of almost 9 miles (median 5 miles).

[17] Data on household net worth is not available in the Decennial Census so unfortunately a direct com-

I turn next to discuss the impact of prison openings on neighborhood demographic characteristics. Following the opening of a prison, households will re-optimize their residence location and respond to changes in neighborhood amenities with taste-based sorting. Households that place a high value on the amenities that prisons bring, such as proximity to a large government employer, or value disamenities associated with prisons less will move in and vice versa. Table 4 reports my estimates of the effect of prisons on neighborhood characteristics. For brevity of exposition going forward, I omit the results based on the full sample and focus on the analyses based on the trimmed and matched sample. All columns control for the 1990 dependent variable, the complete set of housing and demographic characteristics from 1990 and state-urban area fixed effects. The third column also adds tract-level housing and demographic characteristics from 1980. I find that prison openings lower the socio-economic status of local neighborhoods. The results indicate that tracts where prisons were opened experienced between 1.4 and 2.2 percent decreases in average annual household income. There is also evidence of a small increase in the proportion of households with heads who are black, although estimates for proportion Hispanic are not statistically different from zero. For the proportion of housing units that are owner occupied, the match-sample estimates are also negative and significant, suggesting a modest progression toward rental properties.

Table 5 investigates the declines in household income and wages more closely. Specifically, it evaluates the extent to which these decreases are explained by changes in demographic composition of neighborhood residents and the impact of prisons on local income and wages net of the compositional changes. To do this, I first compute predicted income and wages by estimating a census tract-level regression of log average household income (or log average household wages) on a set of demographic characteristics, occupational compositions and year fixed effects using the full sample of census tracts in 1990 and 2000 excluding the treated tracts.[18] The fitted values of log average household income (or wages) from the aforementioned estimation represent the level of average income (or wages) census tracts are predicted to have given their demographic composition. I then estimate the impact of prisons on these predicted quantities.

parison with total net worth of local households cannot be made. If the average housing value losses of $6 million per treated tract is instead compared with average total annual household income of $77 million, the loss is quite significant.

[18]The set of demographic characteristics used in this estimation includes proportion of black households, proportion of Hispanic households, proportion of female-headed households, and proportion of families with

Using the matched sample of census tracts, Column (2) in Table 5 reports these estimates, which measure the impact of prisons on income and wages that is explained by changes in demographic composition.[19] Column (1), on the other hand, reports the estimated impacts on actual average income and wages (previously reported in Column (2) of Table 4). For both outcomes, comparing the estimated decreases of about 3 percent in Column (2) with the estimates in Column (1) suggest that all of the declines in income and wages are due to changes in demographic compositions. In fact, there might even be a positive impact of prisons on income and wages after accounting for their effect on demographic composition. Column (3) reports the estimate for the net impact, equal to the estimate in Column (1) minus that in Column (2). To compute the standard errors and confidence intervals for the net impacts, reported below the estimated impact, I use the seemingly unrelated estimation approach to combine parameter estimates and (co)variances matrices of the two separate models (i.e., Columns (1) and (2)) into one parameter vector and simultaneous (co)variance matrix (Weesie, 1999; and StataCorp, 2013). While the estimated impact of prison openings on average income and wages net of compositional impacts are positive, I can not reject that they are equal to zero.

Note that in this analysis so far, I have used occupation as a proxy for education or earning potential of residents. The reason I do not use data on education directly (e.g., proportional of population with a college degree) is due to an important caveat with the public-use census data. When tabulating population statistics, the Census Bureau counts prisoners as residents of the census tract (or county) in which the prisons are located. Therefore, all neighborhood characteristics tabulated based on total population, such as the proportion with a college degree, include incarcerated individuals. As a result, these characteristics are mechanically affected by prison openings through an increase in the prisoner population. To avoid introducing bias, all of my analyses that involve post-treatment demographic characteristics data, including the prediction exercise described above, only use characteristics that

children. Occupational compositions include the proportion of employed individuals age 16 or older in each of the occupation groups in the census. I use the following occupation groups, which are consistent across the 1990 and 2000 census: professional and technical occupations; executives, managers, and administrators (excluding farms); sales workers; administrative support and clerical workers; precision production, craft, and repair workers; operators, assemblers, transportation, and material moving; nonfarm laborers; service workers; and farm workers including forestry and fishing.

[19] It is worth noting that the specification used for Column (2) and (4) controls for (log of) lagged *actual* average income (or wages) not lagged predicted income (or wages). Therefore, the estimation is not subject to issues related to regressions with a generated regressor.

are of households or workers which do not include prisoners, and hence any change in which can be attributed to the impact of prison openings on non-prisoner populations alone.

Returning to Table 5, I note the concern that occupation is not a fixed characteristic but rather a choice variable. Changes in the occupational composition of local neighborhoods towards lower-paying occupations may reflect either in-migration of individuals with low-paying occupations (and out-migration of those with higher-paying occupations) or existing residents changing occupations, or both. Columns (4) and (5) show the results of the same analyses that produce Columns (2) and (3) but exclude occupational composition from the neighborhood characteristics that we use to predict average income and wages. I find that excluding occupational composition lowers the size of the estimated decrease in income and wages that are due to compositional changes, as reported in Column (4). Nevertheless, the same conclusion holds that the net impact of prison openings on neighborhood average income and wages after accounting for demographic shifts is negligible.

Table 6 provides further evidence of how prison openings change the composition of local neighborhood population, specifically towards lower socio-economic status households. In this table, I provide estimates of the impact of prison openings on average household income by when householders moved into the housing units, including in the past 10 years, 10-20 years ago and 20 or more years ago. The estimate in Column (2) indicates that compared to neighborhoods with similar characteristics, neighborhoods that experience prison openings see 3.7 percent decreases in the average income of households who recently moved in. The estimates for households who have been residing in the neighborhoods 10-20 years or 20 years or more are not statistically different from zero. Although recent movers may include existing neighborhood residents who relocate within the same neighborhood, these results suggest that the overall decreases in average household income (discussed in the previous two tables and reported again for reference in Column (1) of Table 6[20]), are driven by in-migration of households with lower income following the opening of a prison. In addition, while I cannot completely rule out existing residents changing occupations, these results also suggest that at least part of the changes in local occupational compositions are likely to be explained by in-migration of individuals with lower-paying occupations, thereby mitigating the concern associated with using occupation as a proxy for education in my compositional analyses discussed above. Lastly, Column (5) estimates the impact of prison openings on the ratio of average income among households who moved in in the past 10 years to that among households who moved in more than 10 years ago. The result indicates that prison openings lower this ratio by about 0.04. Given that this ratio is slightly below 1 among control tracts, the result suggests that prison openings induce in-migration of households who are also poorer on average than existing residents of neighborhoods where prisons were opened.

I turn next to examine the heterogeneous impact of prison openings for different types of neighborhoods, reported in Table 7. First, the effect was estimated separately for census tracts with average income below and above the median in my matched sample. There is evidence that the negative impact on housing values, rents and average income are larger in richer neighborhoods. The difference is statistically significant for housing value but insignificant for rents and income. Second, I examine whether the estimated impacts vary for neighborhoods with different population density. There appears to be somewhat larger effects on more densely populated neighborhoods, but the differences are not statistically significant. Lastly, I find suggestive evidence that rental rates decrease more in neighborhoods with largely white populations although there is no evidence of heterogeneous effects in housing values and rents.

Finally, I turn to evaluate how prison openings affect neighborhood total population and labor market outcomes of residents. Table 8 finds no evidence of substantial changes in total population in households.[21] The findings also indicate that neither employment, measured by the number of tract residents over 16 who are employed, nor the unemployment rate have been substantially improved by the opening of a prison. For these null findings, I report the minimum detectable effect size (MDE) based on the trimmed sample in the last column of Table 8. I am powered to detect relatively small effect sizes. For example, the MDE for employment is 3.6 percent which is equivalent to approximately 59 jobs, while the MDE for the unemployment rate is 0.7 percentage point. The lack of employment gains at the neighborhood level may be because the impact on employment and economic activities are spread out to broader communities. Anecdotal evidence suggests that some workers do

[20]Note that the estimate in Column (1) of Table 6 differs slightly from the estimate for the same outcome reported in Column (2) of Table 4 and in Column (1) of Table 5, despite the fact that they all use the same specification and are all based on the matched sample. This is because the sample used for Table 6 excludes a small number of census tracts with missing data on average household income by year householders moved in.

[21]I use population in households as opposed to total population because total population includes prisoners and will increase mechanically as a result of prison openings even if there is no change in non-prisoner population.

commute to work at the prison, especially in rural areas, making the direct employment benefits in terms of jobs at the prisons more spread out and not necessarily reaped by neighborhoods in close proximity to the prisons themselves. There may also be spillovers to other sectors of the local economy and to nearby neighborhoods.

Overall, my tract-level findings indicate that prisons decrease housing values and lead to substantial changes in the composition of local neighborhood workforce and population, specifically towards lower socio-economic status individuals. They also fail to bring substantial employment gains to neighborhoods closest to prisons. The next section investigates the impact at the county level which will capture broader impacts of prisons, especially on local labor markets.

3.4.2 U.S. County-level Results

Table 9 presents estimates of the impact of prison openings on employment of county residents, as measured in the Decennial Censuses. Panel A indicates that counties where prisons were opened experience some improvements in local labor market conditions, relative to counties with similar characteristics. The estimates suggest gains in employment of 1.2-1.6 percent, driven both by a small increase in population and a rise in employment to population ratio, although population estimates are less robust across specifications. There is also evidence of a 0.3 percentage point decline in the unemployment rate. To further understand the drivers of overall employment gains, Panel B investigates the impact on employment by sector. The results indicate large increases in state government sector employment of about 19-21 percent. These increases captures the direct gains in jobs at the prisons. Most of the prisons opened during this period were state-operated and while some facilities also hired private contract workers, the vast majority of prison staffs were government employees.[22] In my most reliable estimates using the matched sample presented in Columns (4) and (5), this percentage increase is equivalent to a gain of 321-328 jobs which is approximately equal to the average number of government staffs hired by prisons in each treated county (339 staff) based on prison-level data from the Census of State and Federal Adult Correctional Facilities.

Aside from direct gains driven by jobs at the prisons, there is no evidence of spillovers to other sectors of the local economies. In particular, I find neither a positive or negative

[22] On average, each treated county had 339 government employees and 11 private contractors employed by prisons, with almost half of the treated counties with zero private contractors.

spillover to private sector employment. On the one hand, these results run contrary to claims made by prison proponents, including government officials promoting prisons to communities, that prisons would provide a major boost to local economies through positive spillovers to the private sector. On the other hand, I also find no support for the opposing argument that prisons as large government employers can crowd out resources and economic activities of the private sector. Overall, my findings indicate that counties where prisons were opened experienced a direct increase in total employment driven by jobs at the prisons but saw no employment spillover to other sectors or a major boost to the local labor markets.

One question that might arise regarding my employment results, particularly related to the lack of spillovers to private sectors, is whether prisons of different sizes may have different impacts, for example if only larger prisons would create substantial spillovers. Table 10 investigates the employment effects by prison size, measured by the number of prison staffs. Each column reports the result for a specification which includes, instead of one indicator for prison openings, four indicator variables, each taking value 1 for the openings of prisons of each quartile of prison size. Column (4) shows that direct employment gains in the state government sector are indeed larger for counties that received larger prisons. The estimated increase ranges from 11 percent at the bottom quartile to 28 percent at the top quartile. However, Column (2) finds no evidence of significant changes in private sector employment across the four quartiles, suggesting that prison openings have little employment spillover to the local private sectors regardless of the prison size.

I turn next to my difference-in-differences analyses using annual data to obtain a more nuanced picture of the changes in local labor market and business activities. Specifically, I estimate equation (6) using 1985-2005 QCEW data and plot the coefficients associated with the set of treatment indicators, one for each of the 5 leads and 5 lags and the contemporaneous year of prison openings. These plots provide an opportunity to visually assess if there exists any differential pre-trend that poses a challenge to my causal inference and investigate the evolution of the impact over time. Using the matched sample of counties, Figure 1 shows the plot for the log of employment in the state government sector, laying out the evolution of the direct employment effect of prisons. There is no evidence of a trend in state employment prior to the year prisons were opened in the treated counties relative to the other counties, lending credibility to my identification strategy. The figure clearly shows a large and persistent increase in state government employment following the openings of prisons.[23]

Figure 2 then investigates the impact of prison openings on the local private sector with regards to employment, average wages and number of establishments. Again, there is little evidence of differential pre-trends for all the outcomes. In particular, the figure indicates no support for the concern that prisons were opened in counties with worse economic trends. After the prisons were opened, the estimated impacts on employment, wages and number of establishments in the private sector are small and not statistically different from zero throughout the post-treatment years. This pattern indicates that prisons neither promote nor depress economic activities in the private sector, a result in line with that in Table 9.

Lastly, Figure 3 investigates the impact of the prison construction boom on the local construction industry.[24] I find that prison buildings create a short-lived boost to the local construction industry raising employment and average wages 1-2 years ahead of when prisons were opened or during the construction period. After the prisons were opened, the estimated impacts revert back to close to zero indicating little evidence of a sustained spillover to other local construction activities outside the direct gains associated with the construction of prisons.

My findings so far have shown that prison openings have limited impacts on local labor markets and economic activities in addition to directly creating jobs in prisons and providing a temporary boost to the local construction industry. However, the economic benefits of prisons may extend beyond their impacts on the level of employment and economic activities. I turn next to evaluate a different dimension to how prisons may benefit local economies. One key argument made by prison proponents for why prison jobs are desirable for communities is that these jobs are "recession-proof". Unlike other private employers, prisons mostly create government jobs that are thought to be more stable than private sector jobs because they provide superior job security. The prison industry also tends to be less susceptible to business cycles, sometimes even expanding in size during hard times. Specifically, I investigate whether economic conditions in communities with prisons correspond less to macroeconomic shocks compared to similar communities without prisons. To do this, I

[23]The estimated increase of roughly 40-60 percent is larger than the 19-21 percent increase in state employment in Table 9. The QCEW assigns employment to a county based on the place of work as opposed to the place of residence as is the case for the census data used in Table 9. The QCEW also excludes some state employment, such as elected officials, members of a legislative body and members of the judiciary, and has lower baseline averages.

[24]Estimation in this figure uses 1985-2000 data only due to changes in the industry classifications in 2001.

estimate the following specification using 2001-2010 data from the QCEW and the Local Area Unemployment Statistics (LAUS):

$$y_{ist} = \beta_0 y_{st} + \beta_1(\text{Prison}_i \times y_{st}) + C_i + \gamma_t + \epsilon_{ist}. \tag{7}$$

y_{ist} is the outcome, i.e. log of employment or log of unemployment, in county i in state s in year t and y_{st} is the state-level outcome in the same year. The indicator variable Prison_i takes value 1 for counties that experienced prison openings during the 1990s. The specification includes county fixed effects, C_i, and year fixed effects, γ_t. The standard errors are clustered at the county level to allow for arbitrary autocorrelation. The coefficient β_0 measures the association between changes in the county outcome and the state outcome among control counties, capturing how sensitive county economic conditions are to state-level macroeconomic conditions. The coefficient of interest, β_1, measures the difference in this sensitivity between counties with prisons and control counties. If prisons dampen the responsiveness of county outcomes to macroeconomic shocks, we would expect β_1 to be negative.

Table 11 presents the results of this estimation. The odd columns include counties in my trimmed sample and the even columns includes counties in my matched sample. I find evidence that prisons indeed reduce the responsiveness of county outcomes to macroeconomic conditions. Based on Columns (1) and (2), a 10 percent change in state unemployment is associated with an approximately 9.4 percent change in county unemployment for counties without prisons and 8.9 percent change for counties with prisons. In other words, unemployment in counties with prisons are 5-7 percent less sensitive to state unemployment level, compared to counties with similar characteristics. The results for employment in Column (3) and (4) are in the same direction as those for unemployment although estimates are less precise. To summarize my analyses on the impact of prisons on local economies, I find that while having negligible spillover to other sectors in terms of jobs and economic activities, prisons bring large employment gains in the government sector and make communities less susceptible to macroeconomic shocks, a benefit potentially unique to prisons as a "recession-proof" employer.

I turn next to examine the impact on other county outcomes. Panel A and Panel B of Table 12 provide housing and demographic impacts based on estimating equation (1) using data from the Decennial Censuses. I find some evidence of a small increase in housing values and rents of 1-1.6 percent. However, unlike in census tracts near prisons, there is little evidence of changes in the composition of county residents. Average household income and and racial and ethnic composition, for example, exhibit little to no perceptible change. One potential explanation for the slight increase in housing values and rents despite no improvement in average income is the positive effect of prisons on county employment-to-population ratios and unemployment rates as shown in Table 9. In addition, the stability and "recession-proof" property of prison employment may also play a role in raising confidence in local housing markets.

Finally, Panel C of Table 12 reports estimates of the impact of prison openings on local governments' public finances. It is important to note first that these are the finances of local governments in counties (including county governments themselves and other local governments such as cities, townships, and special districts) and not of state governments, who are responsible for costs associated with the construction and operation of state prisons.[25] There are a number of potential reasons why prisons may lead to changes in revenues and expenditure of local governments. First, prison construction has been argued to create spillover to local infrastructure development, particularly the infrastructure that support the operation of prisons, many of which are shared with local residents and are the responsibility of local governments. Second, prisons may affect local government finances through its effect on local economic conditions. Particularly, local governments may grow in size as the local economies expand. This channel is unlikely to play an important role, however, as my analysis indicates that prisons fail to bring substantial boosts to local economies. Third, claims often appear in media stories arguing that prisons could bring disproportionate grants and funding from state and federal governments to communities in which they are situated. The key factor behind these claims is the Census Bureau's practice of counting prisoners as residents of the communities where they are confined. The simplistic argument goes that prison openings therefore increase the population of local communities and lead to a windfall of funding that is allocated based on local population. It turns out that most federal funding is distributed by methods too sophisticated to be tricked by where the Census counts incarcerated people, although some small federal and state programs may be affected (Kajstura, 2010). A more complex and arguably more plausible channel by which prisons may bring in larger intergovernmental transfers is through "prison-based gerrymandering". It is argued that

[25] County governments are responsible for running county jails, which are not part of this paper's analysis.

counting prisoners, most of which cannot vote, as if they were residents of local communities can have a big effect on electoral representation at the state and county level. Specifically, critics argue that this practice can lead to the violation of the "one person, one vote" rule: a vote in a prison-district now has disproportionate weight because the prisoners can not vote but count towards "representation" numbers. To the extent that political power translates into government funding, prisons could bring in extra intergovernmental revenue for local governments (Adler, 2010).

Panel C of Table 12 reports estimates for total revenue, intergovernmental revenue (which include funding from state and federal governments), and total expenditure. All of the outcomes are in per capita terms, where the item is divided by county population that resides in households, which excludes incarcerated individuals. The results indicate that prisons do not have a significant impact on the overall size of local governments' revenue and expenditure per capita. There is also little evidence that counties where prisons were opened received increased transfers per capita from state and federal governments, compared to counties with similar demographic and housing characteristics.

To conclude, my analyses of prison openings in the United States show that the negative impacts of prison in the form of falling property values and compositional changes of neighborhood residents are localized to communities with close proximity to prisons. While major "boomtown" effects were not found, the benefits in terms of direct job creation and reduced sensitivity of local economies to macroeconomic shocks are spread out to the larger communities. The findings suggests that the prison construction boom may have had important distributional consequences as communities closest to the prisons who bear the local cost of these facilities were relatively much more disadvantaged compared to the rest of the country before the prisons were built. The next section presents the analyses based on my second empirical strategy using data from Texas and provides comparisons of the results with those for the entire United States.

4 Quasi-experimental Analysis for Texas

4.1 Empirical Strategy

This research design compares the experience of neighborhoods that won a prison in the prison site-selection competition with those that applied but were rejected. Specifically, this strategy uses rejected communities from site-selection competitions as control communities in the least square estimation of equation (1) instead of using all communities that did not experience prison openings. The key advantage of this strategy is that communities that were nominated by their local governments to participate in prison site selection are likely to share similar unobserved characteristics and trends with those that eventually got to host a prison. Using rejected communities as control communities, therefore, makes the identification assumption, i.e., $E(\epsilon_{i2000}\eta_{i2000}) = 0$, more credible.[26]

I implement this empirical approach using data from Texas, the state with the largest prison population, which was the only state to conduct a series of systematic large-scale prison site-selection competitions. During the late 1980s and the early 1990s, Texas implemented the largest state prison expansion in U.S. history, tripling the number of prisons from 27 in 1984 to over 130 in 2000. To choose communities that would host their new prisons, the Texas Department of Criminal Justice (TDCJ) conducted statewide site-selection competitions in 1987, 1989, 1992 and 1994. The process began with the TDCJ's issuance of a"Request for Proposal" detailing the type of prisons to be built and site requirements such as size, road access, and environmental characteristics. Jurisdictions then submitted proposals for consideration. Often a city government and a county government co-applied.[27] The TDCJ then evaluated the proposed sites using various criteria including financial considerations and characteristics of the sites and the hosting communities. Finally, the TDCJ Board selected winning sites and communities from the short list compiled by the site-selection committee. The 1987 competition selected six sites to host two 2,250-beds maximum-security facilities and four 1,000-beds minimum/medium-security facilities. The 1989 competition selected ten sites for the construction of four 2250-beds and six 1000-beds facilities. In 1992, 23 facilities of varying sizes were sited and finally, additional 18 state jails and four privately operated facilities were sited in 1994.

At the census tract level, this empirical strategy identifies the local effects of prisons by comparing economic outcomes of the tracts that contain new prisons with outcomes of the tracts in the counties that also applied to host a prison but were rejected. At the county level, I compare outcomes of the winning counties selected to host the new prisons with those of the counties that were rejected.

[26]Recent literature that employed a similar strategy includes Greenstone, Hornbeck, and Moretti (2010) estimating the impact of large plant openings; Greenstone and Gallagher (2008) in the context of hazardous waste cleanups; and Busso, Gregory, and Kline (2013) for the impact of Empowerment Zones.

[27]Thirty-five jurisdictions submitted proposals in 1987, forty-six jurisdictions in 1989 and sixty-three in 1992.

While the use of rejected communities as controls has clear advantages over using all other communities, one may still be concerned that there are fundamental differences between the communities that won a prison and the rejected communities. To assess this possibility, first, I note that a comparison of pre-treatment demographic and housing characteristics, presented in the next section, find that winning and rejected communities are similar across a wide range of observable characteristics. Second, I carefully examine pre-trends in the treated and control communities for imbalance leading up to the opening of a prison, using county-annual data. This analysis finds no evidence of differential pre-trends that might pose a challenge to my identification strategy.

Third, this potential concern is related to the criteria by which the TDCJ selected communities in the site-selection competitions. The TDCJ rule stated that no single factor, mathematical formula, or site characteristic can be used to select a site and community. Rather, each site must be evaluated on its strengths and by the manner in which it addresses the needs of the state. The key factors that were considered included operational needs, logistics, utilities, incentives provided by the communities, construction requirements and community support. According to the site selection reports, many of the particular considerations pertain to selecting sites located in areas advantageous to the operation of the prison system. For example, in the 1989 competition, desirable sites should support the TDCJ's need to address the expanding death row population and the need to cluster units within a reasonable geographical area for mutual support capabilities. Most of these factors tend to be unrelated to community economic outcomes and so are unlikely to pose a challenge to the causal inference. However, other criteria, such as the availability of a local workforce and infrastructure (e.g., colleges and hospital facilities) and incentives provided by communities, are likely to correlate with community outcomes, stressing the importance of controlling for a rich set of demographic and housing characteristics from the pre-treatment period in the analysis that follows. Lastly, there is suggestive evidence of some degree of randomness in the selection of communities. The official audit report of the 1994 site-selection competition finds that the process did not ensure that the best proposals were selected, citing the lack of clear criteria, failure to provide sufficient objective information for the Board to consider, and inconsistency in how proposals were evaluated, among other shortcomings. In the following sections, I discuss the Texas data and provide evidence on the validity of this research design.

Finally, it is worth noting that while this quasi-experimental strategy uses arguably more credible identifying variation than the comparison on which my first empirical approach is based, it has lower power and involves communities in a particular state. The result section below documents that while there is sufficient power, the results for Texas are consistent with the results for the entire United States.

4.2 Texas-specific Data Sources

Data required to implement this empirical approach includes the Texas subset of the data described in Section 3.2. In addition, to construct the set of rejected communities for this analysis, I collected data on the identity of counties that submitted proposals and the result of the four site-selection competitions from various TDCJ reports and board meeting minutes obtained through the Texas Open Records Requests, and from archival research of newspaper articles. To analyze prison openings during the 1990-2000 decade, I use the site-selection competitions of 1989, 1992 and 1994. Although all of the prisons whose sites were selected in the 1989 competition were constructed and opened during the 1990s, six of those prison sites were selected in November 1989, prior to the census day of the 1990 Census, while the rest were selected in July 1990. This raises a concern that certain outcomes measured in the 1990 Census, such as house prices, which reflect households' long-term value of their property, may have already been affected by the selection result. My main specifications therefore exclude prisons that were selected prior to 1990. Robustness exercises indicate that including these prisons does not change our results qualitatively.

For my analysis, I use all communities that submitted a complete proposal and were seriously considered by the TDCJ to construct my control group. Restricting the control group only to communities selected in the short list that did not win a prison leaves us with too few control communities. This is because most of the communities that made the short list in one of the competitions ended up winning a prison in one of the subsequent competitions. After dropping prisons constructed in the largest cities of Texas with population greater than 100,000[28], our sample consists of 45 prisons, 43 winning counties (of which some received more than one prison) and 40 rejected counties. One caveat about the information on site-selection competitions is that I do not know the exact location within the rejected counties that would have been the location of a prison had they won the competition since

[28]Including San Antonio, Austin, El Paso, Lubbock, Laredo, Amarillo, Grand Prairie, Abilene, Wichita Falls, and Beaumont.

the TDCJ does not keep records of the rejected proposals. Nevertheless, we can still learn a lot from the analysis using the identity of the counties that participated in the site-selection competitions. The next sub-section discusses the procedures that I use to select appropriate control tracts given this caveat.

4.3 Analysis Samples and Summary Statistics for Texas

4.3.1 Texas Census Tract Sample

Ideally, this analysis would compare treated tracts with exact tracts in rejected counties that would have contained the prison site had those counties won. Given that the required information for this exercise is not available, one simple alternative is to compared treated tracts with all tracts in the rejected counties. However, some neighborhoods in the rejected counties have very different housing and demographic characteristics compared to where prisons are usually located and are very unlikely to have been the site for the new prisons. To conduct a more credible comparison, I use a prediction technique to select tracts in the rejected counties with a sufficient likelihood of containing a prison site (had their counties won) to form my control group.

Specifically, first, I predict being in direct proximity to new prisons (i.e., containing the prison site) given the observed housing and demographic characteristics using all census tracts in the winning counties. In this step, I use propensity score estimation and follow Imbens and Rubin (2015)'s systematic approach to selecting covariates to include in the propensity score estimation[29]. I then use this model to predict the propensity score for all census tracts in the rejected counties, effectively predicting which tracts in the rejected counties were likely to have contained a prison location had those counties won the competition. Census tracts in the rejected counties with a predicted propensity score between the minimum and the maximum of the predicted propensity score among treated tracts (i.e., common support) form the control group of my estimating sample. I will refer to this sample as the "site selection sample" of census tracts.

Table 13 reports summary statistics of pre-treatment covariates for census tracts in my baseline sample and provide an opportunity to gauge the credibility of my quasi-experimental research design. Column (1) shows the mean of neighborhood characteristics from the 1990 census for census tracts which contains the prisons opened during the 1990s. Column (2)

[29]See more detail in the Appendix.

describes characteristics for census tracts in the rest of Texas. Column (3) describes characteristics for control tracts in my site selection sample. Columns (4) and (5) report normalized difference and results of t-tests of mean equality between treated and control tracts respectively in the full sample and the site selection sample.

Comparing the treated census tracts with those in the rest of Texas, the null hypothesis of equal means is rejected for most of the characteristics. There are various characteristics with substantial differences, most notably house prices and rents and demographic characteristics with regards to income, poverty, education, and population density. Covariate balance is much improved if we compare treated tracts with control tracts based on the site-selection competitions. As would be expected, winning and rejected communities tend to be more rural and have poorer and lower-educated residents than the rest of Texan communities. Nevertheless, there are still a few characteristics where the treated and control neighborhoods differ significantly. I control for the full set of these 1990 characteristics in all of the following estimation, as well as a subset of characteristics from 1980 in some specifications.

Overall, my baseline sample forms the basis for a credible quasi-experiment. In the robustness exercises presented in the Appendix, I show in addition that my estimation results are robust to an alternative approach of constructing the control group using a matching technique.

4.3.2 Texas County Sample

My county level analysis compares outcomes of the counties that were selected to host Texas' new prisons with outcomes of the counties that also applied but were rejected. Table 14 presents summary statistics of county characteristics from the 1990 Census. Comparing Columns (1) and (2) demonstrate that treated counties differ substantially from the rest of Texan counties along a number of dimensions. Most notably, prisons tend to be sited in counties with larger populations and workforces and higher unemployment. Similar to the tract level analysis, my identification strategy, by comparing winning counties with rejected counties (Columns (1) and (3)), offers a large improvement in covariate balance, although a small number of characteristics still have substantial differences with normalized differences exceeding 0.3. This stresses the importance of controlling for observable characteristics from the pre-treatment period in the estimations that follow.

4.4 Results for Texas

This section presents the empirical findings for Texas. As noted above, power is lower with this empirical approach compared to my first approach, which uses data from the entire United States. I report the key estimates for Texas below and leave the rest of the results, particularly where power represents an important limitation, to the Appendix.

4.4.1 Texas Census Tract-level Results

Table 15 presents the estimates for the impact of prison openings on my key outcomes. The estimates in Column (1) are based on the comparison between census tracts where prisons were opened with tracts in the rest of Texas. Columns (2) and (3) show the results based on my quasi-experimental approach, comparing tracts where prisons were opened with control tracts in the rejected counties. All columns control for the 1990 dependent variable and the complete set of housing and demographic characteristics from 1990. The last column adds tract-level housing and demographic characteristics from 1980 to control for differential time trends.

Panel A shows the results for the impact on housing values and rental rates. The estimates for both outcomes are not statistically different from zero across the three columns. However, I cannot reject a 2-4 percent decrease in housing values, which are my point estimates for the United States. Unfortunately, given that my MDEs for housing values and rents lie in the range of 8-10 percent, I do not have sufficient power to detect relatively small effect sizes.

Panel B evaluates the impact on average income and wages and demographic characteristics of local households. Consistent with the results for the United States, I find that census tracts in winning counties where prison were opened saw a decline in average household total income and average household income from wages and salary. For both outcomes, the point estimates based on my site-selection approach in Columns (2) and (3) are somewhat larger than those based on comparing treated tracts to the rest of Texas and range from 5.8 to 7.4 percent. Compared to the results for the United States, the estimates for Texas are also larger, although with wider confidence intervals, I cannot reject the smaller decline of roughly 2 percent found for the United States. The estimates for proportion Hispanic are positive, suggesting there might be a small increase in the proportion of Hispanic households, although the estimates are imprecise. There is also some evidence of a decrease in the proportion of housing units that is owner occupied, suggesting a modest progression toward rental properties similarly to the pattern I find for the United States. Overall, the results indicate that neighborhoods appeared poorer on average after winning a prison.

Table 16 investigates the declines in household income and wages more closely. Using the same approach as Table 5, it evaluates the extent to which these decreases are explained by changes in demographic composition of neighborhood residents. Column (1) reports the estimated impacts of prisons on actual average income and wages. Column (2) reports the estimates for the predicted average income and wages given demographic characteristics and occupational compositions. For both outcomes, the estimates suggest that a significant portion of the negative impact on actual outcome is due to changes in the composition of local residents. Column (3) reports the estimates for the net impacts of prison openings after accounting for compositional effects. The estimates for both total income and wages are not statistically different from zero, although the estimates are negative. Column (4) and (5) show the results of a similar analysis but exclude occupational composition from the neighborhood characteristics that we use to predict average income and wages. I find that excluding occupational composition substantially lowers the size of the estimated decrease in income and wages that are due to compositional changes (reported in Column (4)). This, combined with the negative estimates in Column (5), suggests that changes in the occupational composition of local residents are the key drivers of the observed decline in average income and wages in Texas. In addition, the analysis of the impact of prisons on average household income by when households moved into the housing units (analogous to Table 6 for the United States) also indicates that overall decreases in average household income are driven by in-migration of lower-income households in the past 10 year and out-migration of households who have been in the tracts more than 20 years. The results are reported in the Appendix.

Lastly, returning to Table 15, Panel C reports the population and employment impacts. I find little evidence of substantial changes in total neighborhood population and employment, similar to the findings based on my U.S.-wide empirical approach. However, there is some evidence of slight improvements in local unemployment rates.

Overall, the findings based on my Texas site-selection strategy are consistent with the findings for the entire United States. While lacking sufficient power to detect small changes in housing values, the estimates for Texas indicate likewise that prisons openings lead to substantial changes in the composition of local residents, specifically towards lower-earning households, and bring little employment gains to neighborhoods closest to prisons.

4.4.2 Texas County-level Results

Table 17 reports estimates of the impact of prison openings on employment of county residents. Overall, the results are consistent with those for the entire United State although impacts are less precisely estimated. First, panel A indicates that counties where prisons were opened experience some improvements in local labor market conditions, relative to counties with similar characteristics. Based on the comparison between winning and rejected counties in Columns (2) and (3), the estimates suggest gains in employment of 4-6 percent and an increase in the employment-to-population ratio of 1-1.3 percent.

Second, Panel B indicates large increases in state government employment of about 33-47 percent, capturing the direct gains in jobs at the prisons. The estimated increases in total employment and state government employment are larger than those found for the United States. One potential explanation is that prisons in the Texas site selection sample are on average larger than prisons in the rest of the country, with an average number of staff of 445 compared to 354. However, it is important to note that estimates for Texas are generally less precise and the confidence intervals often include the point estimates based on the whole United States. Aside from these direct gains driven by jobs at the prisons, there is little evidence of spillovers to other sectors of the local economy. In particular, I find neither a positive or negative spillover to private sector employment, as is the case for the entire United States. I also find the evolution of the employment impacts for Texas to share similar patterns with those of the United States shown in Figures 1 to 3, based on estimating difference-in-differences specifications with leads and lags relative to the timing of prison openings. Specifically, there is no evidence of differential pre-trends that might pose a challenge to my identification strategy. Prisons in Texas also bring large persistent gains in state employment, create a short-lived boost to the local construction industry and have little spillover effect on the private sector. These results are presented in the Appendix.

I turn next to investigate the potential benefit of prisons in reducing the responsiveness of local economies to macroeconomic conditions. Table 18 presents the results based on the estimation of equation 7. The odd columns use the full sample of Texas counties and the even columns use the site-selection sample. The findings indicate that local labor markets in winning communities correspond less to state-level macroeconomic shocks compared to control communities. My most reliable estimate from Column (2) suggests that unemployment in winning counties are roughly 10 percent less sensitive to the state-level unemployment compared to rejected counties. The results for employment are in the same direction but the estimates for the interaction term are imprecise.

Finally, my results on the impact of prison openings on county housing markets and demographic characteristics in Texas are also consistent with those for the United States and are reported in the Appendix. Overall, there is little evidence of changes in housing and demographic outcomes in winning counties compared to rejected counties.

5 Conclusion and Future Directions

The prison construction boom in recent decades has generated substantial debate and controversy about how prisons affect the communities in which they are situated. To the extent that the perceived impacts of prisons, particularly their widely anticipated economic benefits to communities, affect criminal justice officials' decision to build more and expand existing prisons (which, in turn, creates pressure to fill additional beds), prison construction is pertinent to the debate about the political economy of mass incarceration. Using two complementary empirical approaches, this paper presents the first systematic causal evidence of the economic impact of prisons on local communities. In my first approach I examine the openings of 230 prisons during the 1990s across the entire United States; in my second I employ a quasi-experimental strategy that compares winning and rejected communities in prison site-selection competitions in Texas.

I find that neighborhoods in close proximity to prisons experienced declines in housing values and substantial changes in the composition of the local workforce and population, specifically towards lower socio-economic status individuals — an outcome consistent with taste-based sorting of households. Although these impacts are localized to neighborhoods closest to prisons, the benefits of prisons in terms of job creation and a reduced sensitivity of local economies to macroeconomic shocks are spread out over broader communities. Specifically, counties in which prisons were opened saw large and persistent gains in government employment (driven by jobs at the prisons), and temporary boosts to local construction industries during the period of new prison construction. I also find that prisons dampen the responsiveness of local labor markets to macroeconomic shocks — a benefit that is potentially unique to prisons as a "recession-proof" employer. Aside from the direct gains associated with the prisons, there is little evidence of either positive or negative spillovers to employment and economic activities in the private sector. On the whole, prisons fail to provide a major stimulus to the local economy, despite assertions to the contrary by prison proponents.

These findings have implications for policymakers and for communities that must decide whether to allow prison construction. Given, first, that prisons are unlikely to bring substantial boosts to the local economy and, second, the development needs and aspirations of local communities, planners should carefully consider whether the benefits that prisons bring (e.g., reduced cyclicality) are superior to the opportunities provided by alternative sources of economic development, such as private manufacturing plants and retailers. In addition, my findings suggest that prison construction can have important distributional consequences, particularly since the negative impacts are borne by relatively disadvantaged neighborhoods closest to prisons. Decision makers should consider these factors both during the site-selection process and after prison construction, when the consequences of prisons are felt and when appropriate compensation and transfers can be enacted. Although I leave for future work a direct investigation of whether the prison construction boom plays a role in the perpetuation of mass incarceration, I do demonstrate that the anticipated economic benefits that led communities to pursue prisons as an economic development strategy are partly realized in the form of substantial and persistent gains in relatively "recession-proof" jobs — despite the fact that there is no major boost to the local economy.

I close by noting that this paper has used aggregated census data; however, additional analyses are possible with confidential Decennial Census microdata, which I have recently obtained approval to access. The analysis can be extended in three key areas.

First, the Journey to Work component of the census microdata, which distinguishes between an individual's place of work and his or her place of residence, will allow me to obtain a more nuanced understanding of how prisons affect local labor markets and, ultimately, the overall welfare of local citizens. My findings suggest that many prison employees are commuters who live outside the neighborhoods in which prisons are located. Examining the Journey to Work component, I will be able to separate the impact of prison openings on neighborhood workers and neighborhood residents and provide a more detailed assessment of the local labor market impacts and the welfare and distributional consequences of prisons.

Second, the restricted census data identifies households at the census block, which is the smallest geographic unit tracked by the Census Bureau. Analyzing these data will allow me to more precisely identify the highly localized impacts of prisons, including housing values and rents. While this paper documents modest declines in housing values at the census tract level (a geographic unit that can cover a large area in nonurban tracts), it is possible that census blocks closest to prisons within the same tract have experienced declines larger than those documented here.

The third extension addresses the Census Bureau's practice of counting incarcerated individuals as residents of the geographic areas where correctional facilities are located. A future analysis of the confidential microdata will allow me to construct measures using non-incarcerated residents only. This should allow me to better understand how the presence of prisons affects household sorting and the demographic composition of neighborhoods.

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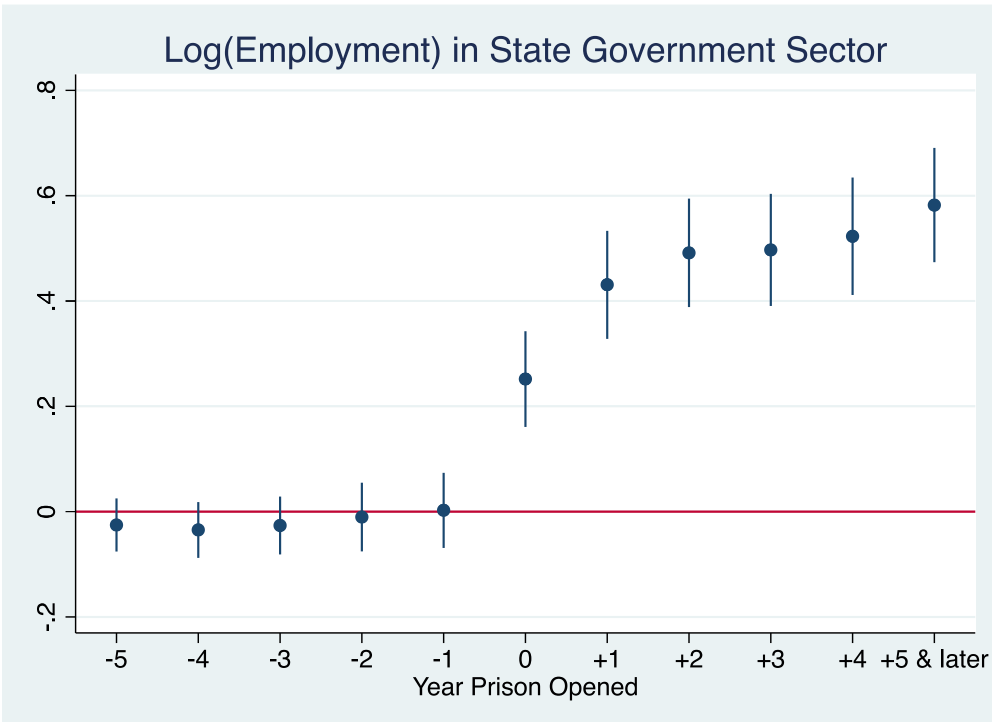
Figure 1: Impact of prisons on local employment in the state government sector, U.S. counties

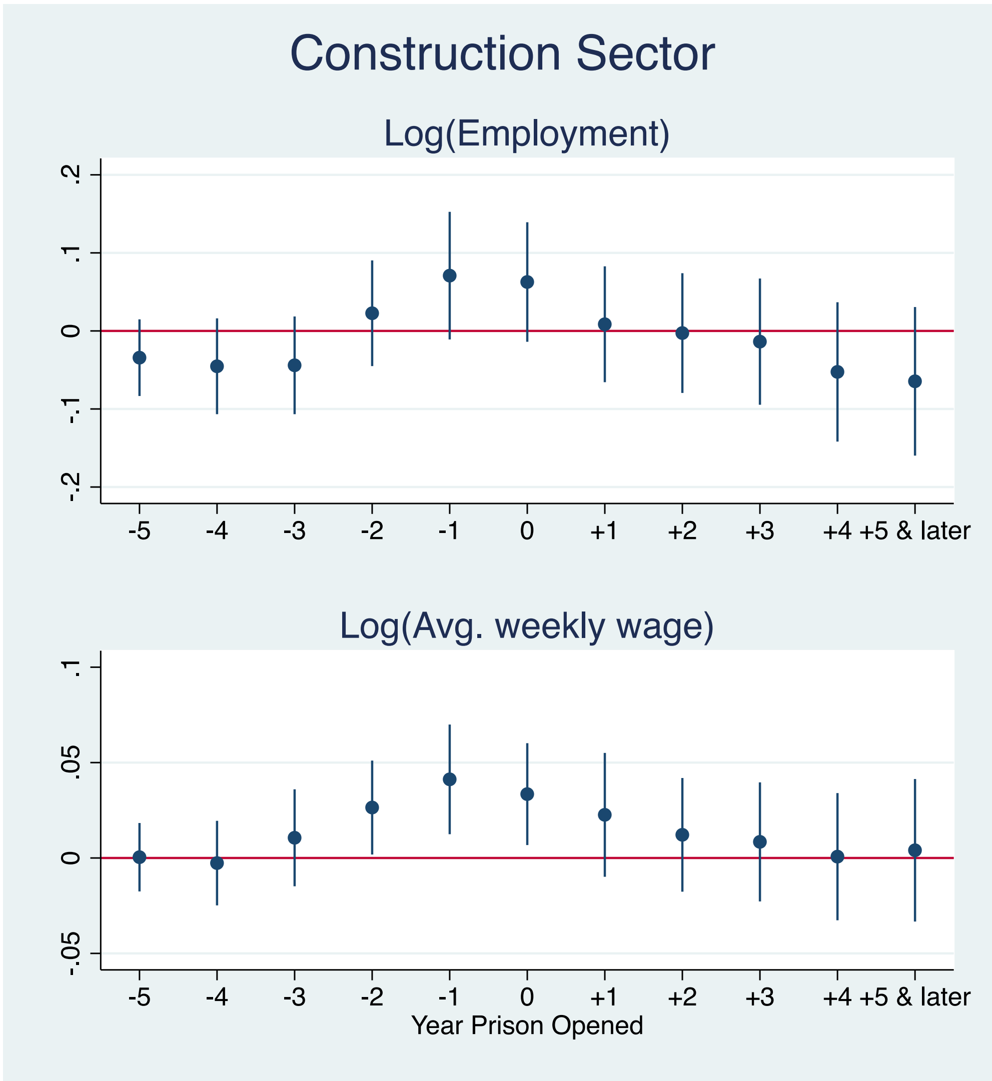


Notes: This figure plots results from the difference-in-differences analysis allowing for 5 leads and 5 lags based on the specification in Equation 6 for log of employment in the state government sector. The sample consists of U.S. counties in the propensity score matched sample. County-year data from 1985 to 2005 for the reported outcome come from the Quarterly Census of Employment and Wages (QCEW). The bars report 95 percent confidence intervals calculated using standard errors clustered at the county level.

Figure 2: Impact of prisons on local employment, wages, and number of establishments in the private sector, U.S. counties

Notes: This figure plots results from the difference-in-differences analysis allowing for 5 leads and 5 lags based on the specification in Equation 6 for log of employment, log of average weekly wages, and log of number of establishments in the private sector. The sample consists of U.S. counties in the propensity score matched sample. County-year data from 1985 to 2005 for the reported outcomes come from the Quarterly Census of Employment and Wages (QCEW). The bars report 95 percent confidence intervals calculated using standard errors clustered at the county level.

Figure 3: Temporary boost to local construction industry, U.S. counties



Notes: This figure plots results from the difference-in-differences analysis allowing for 5 leads and 5 lags based on the specification in Equation 6 for log of employment and log of average weekly wages in the construction industry. The sample consists of U.S. counties in the propensity score matched sample. County-year data from 1985 to 2005 for the reported outcomes come from the Quarterly Census of Employment and Wages (QCEW). The bars report 95 percent confidence intervals calculated using standard errors clustered at the county level.

Table 1: Pre-treatment mean characteristics of census tracts in the U.S.

	1990 Mean				Normalized Diff. & t-test		
	(1) Treated	(2) Rest of US	(3) Trimmed	(4) Matched	(5) Full	(6) Trimmed	(7) Matched
Housing Characteristics							
Median House value ($)	50,702.29	89,650.48	48,850.25	53,396.58	0.94***	0.04	0.09
Monthly rent ($)	315.07	462.76	308.20	318.71	1.04***	-0.01	0.04
Proportion owner occupied	0.62	0.60	0.64	0.61	-0.12	0.19***	-0.09
Proportion occupied	0.86	0.91	0.87	0.85	0.50***	0.10	-0.09
Proportion 0-1 bedrooms	0.10	0.14	0.10	0.10	0.35***	-0.13**	0.00
Proportion 2-3 bedrooms	0.77	0.70	0.78	0.77	-0.66***	-0.03	-0.05
Proportion 4+ bedrooms	0.12	0.16	0.13	0.13	0.38***	0.20***	0.06
Proportion consisting of 1 unit	0.70	0.67	0.71	0.69	-0.17**	0.17***	-0.07
Proportion consisting of 2 unit	0.03	0.05	0.03	0.03	0.36***	0.01	0.04
Proportion consisting of 3-4 unit	0.02	0.05	0.02	0.02	0.41***	-0.00	0.04
Proportion consisting of 5+ unit	0.05	0.15	0.04	0.05	0.66***	-0.09	0.02
Proportion mobile home	0.19	0.08	0.18	0.19	-0.89***	-0.10	0.05
Proportion complete plumbing	0.85	0.90	0.85	0.83	0.57***	0.10	-0.11
Proportion built last 5 yrs	0.10	0.11	0.10	0.11	0.16**	-0.05	0.12
Proportion built within 6-20 yrs	0.36	0.31	0.37	0.37	-0.28***	-0.00	0.03
Proportion built within 20-40 yrs	0.28	0.30	0.27	0.27	0.13*	-0.17***	-0.11
Proportion built >40 yrs	0.25	0.27	0.27	0.25	0.06	0.15**	0.00
Demographic Characteristics							
Household annual income ($)	28,483.77	36,999.45	27,859.87	29,001.56	0.72***	-0.02	0.06
Population density (/sq.km)	94.14	1,650.90	71.63	89.43	0.67***	0.03	-0.02
Proportion no high school diploma	0.35	0.25	0.35	0.36	-0.77***	-0.15**	0.06
Proportion with college	0.10	0.19	0.10	0.11	0.86***	0.08	0.04
Proportion under 18	0.27	0.26	0.27	0.27	-0.21***	0.06	0.03
Proportion 65+	0.14	0.13	0.14	0.14	-0.16**	0.08	0.05
Proportion houshold black	0.13	0.11	0.11	0.12	-0.09	-0.17***	-0.03
Proportion houshold hispanic	0.07	0.06	0.05	0.08	-0.05	-0.21***	0.05
Proportion below poverty line	0.19	0.13	0.18	0.19	-0.60***	-0.18***	0.02
Proportion public assistance	0.10	0.08	0.10	0.10	-0.38***	-0.11*	-0.01
Proportion foreign born	0.03	0.07	0.03	0.04	0.39***	-0.15**	0.03
Proportion female headed	0.21	0.22	0.20	0.21	0.04	-0.18***	0.00
Total employment (16+ persons)	1,695.52	1,829.21	1,648.29	1,640.28	0.18***	-0.03	-0.07
Unemployment rate	0.08	0.07	0.08	0.08	-0.28***	-0.03	-0.02
Proportion in correctional inst.	0.03	0.00	0.01	0.02	-0.35***	-0.26***	-0.09
Observations	255	56,766	9,305	489			

Notes: Column (1) reports mean characteristics for treated tracts in the full and the matched sample where prisons were opened during the 1990s. Column (2), (3) and (4) describe characteristics for control tracts in the rest of the United States, the trimmed sample and the matched sample respectively. The numbers reported in Column (5) to (7) are normalized differences between treated and control tracts in each of my samples, defined as the difference in means scaled by the square root of the average of the two within-group variances. The asterisks report the significance levels of the t-tests of mean equality between treated and control tracts in the corresponding sample, with one, two and three asterisks indicating a p-value less than 0.1, 0.05 and 0.01 respectively. Note that since trimming drops both treated and control tracts with extreme propensity scores, the treated group in the trimmed sample is a subset of the treated group in the full (or similarly the matched) sample. For brevity of exposition, I report mean characteristics of the treated tracts in the trimmed sample in the Appendix.

Table 2: Pre-treatment mean characteristics of counties in the U.S.

	1990 Mean				Normalized Diff. & t-test		
	(1) Treated	(2) Rest of US	(3) Trimmed	(4) Matched	(5) Full	(6) Trimmed	(7) Matched
Housing Characteristics							
Median House value ($)	44,883.56	54,138.30	46,879.70	47,562.25	0.34***	0.25***	0.15*
Monthly rent ($)	298.65	322.33	304.25	309.62	0.29***	0.18**	0.16*
Proportion owner occupied	0.62	0.62	0.62	0.61	0.03	0.14*	-0.06
Proportion occupied	0.85	0.85	0.86	0.85	-0.01	0.11	-0.04
Proportion 0-1 bedrooms	0.11	0.11	0.10	0.11	-0.03	-0.12*	0.02
Proportion 2-3 bedrooms	0.77	0.74	0.77	0.77	-0.39***	-0.11	-0.02
Proportion 4+ bedrooms	0.12	0.15	0.13	0.12	0.51***	0.27***	0.02
Proportion consisting of 1 unit	0.72	0.73	0.72	0.72	0.12*	0.03	-0.06
Proportion consisting of 2 unit	0.03	0.03	0.03	0.03	-0.05	-0.03	-0.03
Proportion consisting of 3-4 unit	0.03	0.03	0.03	0.03	0.17**	0.13*	0.07
Proportion consisting of 5+ unit	0.05	0.06	0.05	0.06	0.14*	0.07	0.07
Proportion mobile home	0.16	0.14	0.16	0.16	-0.23***	-0.08	0.01
Proportion complete plumbing	0.84	0.84	0.84	0.83	0.03	0.14*	-0.02
Proportion built last 5 yrs	0.09	0.10	0.09	0.10	0.07	0.06	0.11
Proportion built within 6-20 yrs	0.34	0.34	0.35	0.35	-0.00	0.07	0.08
Proportion built within 20-40 yrs	0.28	0.26	0.27	0.27	-0.24***	-0.13*	-0.09
Proportion built >40 yrs	0.28	0.30	0.29	0.28	0.09	0.00	-0.05
Demographic Characteristics							
Household annual income ($)	27,553.34	29,611.26	28,104.22	28,227.63	0.33***	0.22***	0.13
Population density (/sq.km)	56.97	86.06	45.13	45.14	0.06	-0.03	-0.05
Proportion no high school diploma	0.35	0.30	0.33	0.34	-0.44***	-0.28***	-0.11
Proportion with college	0.12	0.14	0.12	0.12	0.31***	0.18**	0.09
Proportion under 18	0.28	0.27	0.27	0.27	-0.22***	-0.17**	-0.11
Proportion 65+	0.14	0.13	0.13	0.13	-0.04	-0.16**	-0.04
Proportion houshold head black	0.12	0.07	0.10	0.11	-0.33***	-0.17**	-0.08
Proportion houshold head hispanic	0.07	0.03	0.04	0.05	-0.29***	-0.27***	-0.10
Proportion below poverty line	0.20	0.16	0.18	0.19	-0.45***	-0.30***	-0.12
Proportion public assistance	0.10	0.08	0.09	0.10	-0.45***	-0.28***	-0.11
Proportion foreign born	0.02	0.02	0.02	0.03	-0.07	-0.15**	0.01
Proportion female headed	0.21	0.18	0.20	0.21	-0.46***	-0.23***	-0.04
Unemployment rate	0.08	0.07	0.07	0.07	-0.36***	-0.17**	-0.06
Proportion in correctional inst.	0.01	0.01	0.01	0.01	-0.03	0.10	0.06
Observations	219	2,877	1,611	498			

Notes: Column (1) reports mean characteristics for treated counties in the full and the matched sample where prisons were opened during the 1990s. Column (2), (3) and (4) describe characteristics for control counties in the rest of the United States, the trimmed sample and the matched sample respectively. The numbers reported in Column (5) to (7) are normalized differences between treated and control counties in each of my samples, defined as the difference in means scaled by the square root of the average of the two within-group variances. The asterisks report the significance levels of the t-tests of mean equality between treated and control counties in the corresponding sample, with one, two and three asterisks indicating a p-value less than 0.1, 0.05 and 0.01 respectively. Note that since trimming drops both treated and control tracts with extreme propensity scores, the treated group in the trimmed sample is a subset of the treated group in the full (or similarly the matched) sample. For brevity of exposition, I report mean characteristics of the treated tracts in the trimmed sample in the Appendix.

Table 3: Impact of prisons on housing values and rents, U.S. census tracts

	Full		Trimmed Sample		Matched Sample	
	(1)	(2)	(3)	(4)	(5)	(6)
Log(House value)	-0.035***	-0.031***	-0.028***	-0.029***	-0.042***	-0.037***
	(0.011)	(0.010)	(0.011)	(0.010)	(0.012)	(0.012)
Log(Rent)	-0.007	-0.007	0.003	0.002	-0.007	-0.011
	(0.010)	(0.010)	(0.010)	(0.010)	(0.013)	(0.013)
Observations	57,023	57,023	9,529	9,529	744	744
1990 dependent variable	Yes	Yes	Yes	Yes	Yes	Yes
1990 characteristics	Yes	Yes	Yes	Yes	Yes	Yes
1980 characteristics	No	Yes	No	Yes	No	Yes
State-Urban FEs	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each entry reports the estimate of the impact of prison openings on the outcome presented in each row. Heteroskedasticity-robust standard errors are reported in parentheses. Asterisks denote significance: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 4: Impact of prisons on demographic characteristics, U.S. census tracts

	Trimmed	Matched	
	(1)	(2)	(3)
Log(HH Income)	-0.014*	-0.019**	-0.022**
	(0.008)	(0.009)	(0.009)
Log(HH Wage & Salary)	-0.004	-0.015*	-0.019**
	(0.009)	(0.009)	(0.009)
Prop. Black	0.006**	0.008***	0.007***
	(0.003)	(0.003)	(0.003)
Prop. Hispanic	0.003	0.003	0.002
	(0.002)	(0.003)	(0.003)
Prop. Owner Occupied	-0.004	-0.009**	-0.007*
	(0.004)	(0.004)	(0.004)
Observations	9,529	744	744
1990 dependent variable	Yes	Yes	Yes
1990 characteristics	Yes	Yes	Yes
1980 characteristics	No	No	Yes

Notes: Each entry reports the estimate of the impact of prison openings on the outcome presented in each row. Heteroskedasticity-robust standard errors are reported in parentheses. Asterisks denote significance: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 5: Income and wage impacts and compositional changes, U.S. census tracts

		Inc.occupation		Excl.occupation	
	(1) Actual	(2) Predicted	(3) Net	(4) Predicted	(5) Net
Log(HH Income)	-0.019**	-0.036***	0.017	-0.017**	-0.002
	(0.008)	(0.008)	(0.009)	(0.007)	(0.009)
			[-0.001,0.035]		[-0.020,0.016]
Log(HH Wage & Salary)	-0.015*	-0.031***	0.016	-0.014**	-0.000
	(0.008)	(0.008)	(0.009)	(0.006)	(0.009)
			[-0.002,0.034]		[-0.018,0.017]
Observations	744	744		744	
1990 dependent variable	Yes	Yes		Yes	
1990 characteristics	Yes	Yes		Yes	

Notes: Each entry reports the estimate of the impact of prison openings on the outcome presented in each row. Column (1) reports the estimates of the impact of prison openings on the actual value of the outcome. Column (2) reports the estimates on the predicted value of the outcome, given demographic characteristics and the occupational composition of census tracts. Column (3) reports the estimates of the net impact of prisons after accounting for the effect on compositional changes (equal to the estimate in Column (1) minus that in Column (2)). Standard errors and 95 percent confidence interval of the net impact, reported in parentheses and in brackets respectively, are calculated using the seemingly unrelated estimation approach (Weesie, 1999; and StataCorp, 2013). Column (4) and (5) report similar quantities to Column (2) and (3) respectively, except that I exclude occupational compositions from the neighborhood characteristics that I use to predict average income and wages. All estimates in this table are based on the propensity score matched sample of census tracts. Heteroskedasticity-robust standard errors are reported in parentheses. Asterisks denote significance: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 6: Impact of prisons on average household income by year householders moved in, U.S. census tracts

	Log(Average Income)				Ratio
	(1) All	(2) Past 10 yrs	(3) 10-20 yrs	(4) 20+ yrs	(5) Past 10 /10+
Prison opening	-0.0200**	-0.0369***	0.00560	0.0141	-0.0399***
	[0.00853]	[0.0106]	[0.0148]	[0.0174]	[0.0142]
Observations	739	739	739	739	739
R-squared	0.816	0.782	0.669	0.554	0.409
Control mean in 2000 (Level)	43,760	43,033	47,655	42,312	.97
1990 Log(Average Income)	Yes	Yes	Yes	Yes	Yes
1990 characteristics	Yes	Yes	Yes	Yes	Yes
State-Urban FEs	Yes	Yes	Yes	Yes	Yes

Notes: Column (1) to (4) report the estimates of the impact of prison openings on average household income by when householders moved into the housing units, including in the past 10 years, 10-20 years ago and 20 or more years ago, respectively. Column (5) reports the estimates the impact of prison openings on the ratio of average income among households who moved in in the past 10 years to that among households who moved in more than 10 years ago. All estimates in this table are based on the propensity score matched sample of census tracts. Heteroskedasticity-robust standard errors are reported in parentheses. Asterisks denote significance: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 7: Heterogeneity analysis, U.S. census tracts

	Income (Med. = 27,186)			Density (Med. = 20.79)			White (Med. = 0.87)		
	(1) Low	(2) High	p-value	(3) Low	(4) High	p-value	(5) Low	(6) High	p-value
Log(House value)	-0.020	-0.085***	0.016	-0.022	-0.040**	0.540	-0.039**	-0.032*	0.798
	(0.017)	(0.021)		(0.020)	(0.016)		(0.018)	(0.019)	
Log(Rent)	0.002	-0.041*	0.125	-0.002	-0.018	0.513	0.014	-0.037*	0.071
	(0.018)	(0.021)		(0.019)	(0.016)		(0.020)	(0.020)	
Log(HH Income)	-0.017	-0.034**	0.336	-0.016	-0.030**	0.490	-0.012	-0.027**	0.392
	(0.012)	(0.014)		(0.012)	(0.013)		(0.013)	(0.012)	
Observations	372	372		372	372		372	372	
1990 dependent variable	Yes	Yes		Yes	Yes		Yes	Yes	
1990 characteristics	Yes	Yes		Yes	Yes		Yes	Yes	

Notes: Each entry reports the estimate of the impact of prison openings on the outcome presented in each row. In Column (1) and (2), the impact was estimated separately for census tracts with average income below and above the median in my matched sample, respectively. The reported p-values are for the tests of the null hypothesis that the difference in the coefficients in the preceding two columns is equal to zero, obtained from running fully-interacted regressions. Column (3) and (4) reports the estimates separately for census tracts with population density below and above the median in my matched sample, respectively. Column (5) and (6) reports the estimates separately for census tracts with proportion of households with white household head below and above the median in my matched sample, respectively. All estimates in this table are based on the propensity score matched sample of census tracts. Heteroskedasticity-robust standard errors are reported in parentheses. Asterisks denote significance: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 8: Impact of prisons on employment and population, U.S. census tracts

	Trimmed	Matched		MDE
	(1)	(2)	(3)	[Based on Col. (1)]
Log(Population in HH)	0.005	0.005	-0.005	0.036
	(0.013)	(0.015)	(0.015)	
Log(Employment)	0.006	-0.001	-0.006	0.036
	(0.013)	(0.016)	(0.016)	
Unemployment rate	0.001	0.002	0.000	0.007
	(0.002)	(0.002)	(0.002)	
Observations	9,529	744	744	
1990 dependent variable	Yes	Yes	Yes	
1990 characteristics	Yes	Yes	Yes	
1980 characteristics	No	No	Yes	

Notes: Each entry in Column (1) to (3) reports the estimate of the impact of prison openings on the outcome presented in each row. The last column provides the minimum detectable effect size (MDE), the effect that would have been detectable with 80 percent power at the 5 percent significance level ex post, based on the standard errors in Column (1). Heteroskedasticity-robust standard errors are reported in parentheses. Asterisks denote significance: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 9: Impact of prisons on employment, U.S. counties

	Full	Trimmed		Matched	
	(1)	(2)	(3)	(4)	(5)
Panel A: Employment Impacts					
Log(Employment)	0.013**	0.014**	0.012**	0.016**	0.012*
	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)
Population over 16	0.008*	0.006	0.005	0.011**	0.007
	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)
Employment-to-population ratio	0.003	0.004**	0.004**	0.003*	0.003*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Unemployment rate	-0.002	-0.003**	-0.003***	-0.003**	-0.003**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Panel B: Log(Employment) by Sector					
Private	-0.002	-0.000	-0.002	0.001	-0.006
	(0.008)	(0.008)	(0.007)	(0.008)	(0.008)
Federal government	0.005	0.003	0.005	0.022	0.021
	(0.024)	(0.025)	(0.025)	(0.027)	(0.026)
State government	0.180***	0.177***	0.175***	0.194***	0.190***
	(0.019)	(0.019)	(0.019)	(0.020)	(0.020)
Local government	0.008	0.006	0.009	0.013	0.016
	(0.012)	(0.012)	(0.012)	(0.014)	(0.014)
Self-employed	0.009	0.018	0.015	0.011	0.011
	(0.012)	(0.013)	(0.012)	(0.013)	(0.012)
Observations	3,096	1,813	1,813	717	717
1990 dependent variable	Yes	Yes	Yes	Yes	Yes
1990 characteristics	Yes	Yes	Yes	Yes	Yes
1980 characteristics	No	No	Yes	No	Yes
State FEs	Yes	Yes	Yes	Yes	Yes

Notes: Each entry reports the estimate of the impact of prison openings on the outcome presented in each row. Heteroskedasticity-robust standard errors are reported in parentheses. Asterisks denote significance: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 10: Impact of prisons on Log(Employment) by sector by size of prison, U.S. counties

	(1) Total	(2) Private	(3) Loc Gov	(4) State Gov	(5) Fed Gov	(6) Self-Empl
Number of Prison Staff						
1st quartile (9-172)	0.005	0.000	-0.001	0.114***	0.016	-0.017
	[0.014]	[0.016]	[0.027]	[0.039]	[0.054]	[0.024]
2nd quartile (172-304)	0.014	-0.011	0.034	0.184***	-0.007	0.040
	[0.013]	[0.018]	[0.022]	[0.038]	[0.049]	[0.025]
3th quartile (304-453)	0.007	-0.021	0.018	0.222***	0.062	0.011
	[0.013]	[0.016]	[0.026]	[0.030]	[0.048]	[0.023]
4th quartile (453-2425)	0.015	-0.005	-0.004	0.280***	-0.067	-0.003
	[0.010]	[0.012]	[0.024]	[0.041]	[0.046]	[0.022]
Observations	705	705	705	705	705	705
1990 dependent variable	Yes	Yes	Yes	Yes	Yes	Yes
1990 characteristics	Yes	Yes	Yes	Yes	Yes	Yes
1980 characteristics	Yes	Yes	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Mean in 1990 (Level)	29,963	23,281	2,044	1,540	851	2,104

Notes: Each column reports the estimates of a single regression for four indicator variables, each taking value 1 for counties where prisons with number of prison staff in each of the particular quartile opened. All columns are estimated using the propensity score matched sample of counties. Heteroskedasticity-robust standard errors are reported in parentheses. Asterisks denote significance: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 11: Prisons and the response of local labor markets to macroeconomic conditions, U.S. counties

	(1) Log Unemployed	(2) Log Unemployed	(3) Log Employed	(4) Log Employed
State level	0.934***	0.948***	0.911***	0.921***
	[0.0137]	[0.0239]	[0.0428]	[0.0836]
Prison X State level	-0.0673***	-0.0504***	-0.167**	-0.0931
	[0.0148]	[0.0169]	[0.0796]	[0.102]
Observations	17,230	6,840	17,227	6,840
County FEs	Yes	Yes	Yes	Yes

Notes: Each column reports the estimate for the main effect of state-level outcome and the interaction terms of state-level outcome and the indicator for counties that experienced prison openings during the 1990s, based on the specification in Equation 7. State-level outcome corresponds to the outcome presented in each column. Odd columns are based on the trimmed sample and even columns are based on the propensity score matched sample, excluding counties that had prison openings before 1990 or after 2000. County-year data from 2001 to 2010 for unemployment and employment come respectively from the Local Area Unemployment Statistics (LAUS) program and the Quarterly Census of Employment and Wages (QCEW). Reported standard errors are clustered by county. Asterisks denote significance: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 12: Impact of prisons on housing and demographic characteristics, and local government finances, U.S. counties

	Trimmed	Matched	
	(1)	(2)	(3)
Panel A: Housing Impacts			
Log(House value)	0.014**	0.016**	0.010
	(0.007)	(0.008)	(0.007)
Log(Rent)	0.011**	0.011**	0.009
	(0.005)	(0.006)	(0.006)
Panel B: Demographic Impacts			
Log(HH Income)	0.003	0.004	0.000
	(0.004)	(0.005)	(0.005)
Prop. black HHs	-0.002**	-0.000	-0.000
	(0.001)	(0.001)	(0.001)
Prop. Hispanic HHs	-0.001	-0.002	-0.001
	(0.001)	(0.001)	(0.001)
Panel C: Local Government Finance Impacts			
Log(Revenue per capita)	0.005	-0.000	0.007
	(0.012)	(0.013)	(0.013)
Log(Intergov. Revenue per capita)	0.009	0.002	0.007
	(0.013)	(0.013)	(0.014)
Log(Expenditure per capita)	-0.005	-0.000	0.004
	(0.012)	(0.013)	(0.013)
Observations	1,813	717	717
1990 dependent variable	Yes	Yes	Yes
1990 characteristics	Yes	Yes	Yes
1980 characteristics	No	No	Yes
State FEs	Yes	Yes	Yes

Notes: Each entry reports the estimate of the impact of prison openings on the outcome presented in each row. In Panel C, variables described as per capita refer to the line item divided by the county population that resides in households, which excludes incarcerated individuals. Heteroskedasticity-robust standard errors are reported in parentheses. Asterisks denote significance: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 13: Pre-treatment mean characteristics of census tracts in Texas

	1990 Mean			Normalized Diff. & t-test	
	(1) Treated	(2) Rest of TX	(3) Rejected	(4) Full	(5) Site Selection
Housing Characteristics					
Median House value ($)	41,652.43	60,532.48	45,743.23	0.64***	0.26
Monthly rent ($)	326.90	400.35	336.61	0.52***	0.08
Proportion owner occupied	0.61	0.59	0.65	-0.15	0.41**
Proportion occupied	0.82	0.87	0.84	0.47***	0.21
Prop. housing units with 0-1 bedrooms	0.11	0.14	0.10	0.36*	-0.14
Prop. housing units with 2-3 bedrooms	0.81	0.74	0.81	-0.69***	0.00
Prop. housing units with 4+ bedrooms	0.08	0.12	0.09	0.45**	0.22
Prop. of housing units with 1 unit	0.74	0.72	0.74	-0.13	0.09
Prop. of housing units consisting of 2 unit	0.02	0.02	0.01	0.02	-0.33**
Prop. of housing units consisting of 3 or 4 unit	0.01	0.03	0.01	0.43**	-0.15
Prop. of housing units consisting of 5+ unit	0.03	0.12	0.03	0.76***	-0.00
Proportion mobile home	0.19	0.11	0.19	-0.70***	0.03
Proportion with plumbing facilities	0.80	0.86	0.82	0.54***	0.21
Proportion built last 5 yrs	0.11	0.13	0.13	0.19	0.29
Proportion built within 6-20 yrs	0.41	0.44	0.42	0.19	0.06
Proportion built within 20-40 yrs	0.28	0.28	0.27	-0.02	-0.09
Proportion built >40 yrs	0.20	0.15	0.18	-0.38**	-0.19
Demographic Characteristics					
Household annual income ($)	27,187.72	34,990.81	29,908.77	0.61***	0.40**
Population density (/sq.km)	43.20	698.45	67.38	1.06***	0.24
Proportion without high school diploma	0.38	0.29	0.37	-0.55***	-0.06
Proportion with college	0.10	0.18	0.11	0.77***	0.10
Proportion under 18	0.29	0.29	0.30	-0.02	0.12
Proportion 65+	0.15	0.11	0.13	-0.55***	-0.36**
Proportion household head black	0.08	0.08	0.05	-0.02	-0.32*
Proportion household head hispanic	0.21	0.19	0.20	-0.06	-0.01
Proportion below poverty line	0.24	0.18	0.21	-0.46**	-0.27
Proportion public assistance	0.10	0.07	0.08	-0.40**	-0.23
Proportion foreign born	0.05	0.07	0.05	0.37**	0.03
Proportion female headed	0.19	0.19	0.16	-0.01	-0.51***
Total employment (16+ persons)	1,618.35	1,706.03	1,587.50	0.11	-0.05
Unemployment rate	0.08	0.07	0.08	-0.08	-0.03
Prop. in correctional institutions	0.02	0.00	0.02	-0.21*	-0.00
Observations	40	3,119	195		

Notes: This table reports summary statistics of pre-treatment covariates for census tracts in the Texas analysis. Column (1) shows the mean characteristics from the 1990 census for census tracts which contains prisons opened during the 1990s. Column (2) describes characteristics for census tracts in the rest of Texas. Column (3) describes characteristics for tracts in the rejected counties with predicted propensity scores of containing a prison in the common support of the predicted propensity scores among the treated tracts. The numbers reported in Column (4) and (5) are normalized differences between the treated and control tracts in each of my samples, defined as the difference in means scaled by the square root of the average of the two within-group variances. The asterisks report the significance levels of the t-tests of mean equality between the treated and control tracts in the corresponding sample, with one, two and three asterisks indicating a p-value less than 0.1, 0.05 and 0.01 respectively.

Table 14: Pre-treatment mean characteristics of counties in the Texas

	1990 Mean			Normalized Diff. & t-test	
	(1) Treated	(2) Rest of TX	(3) Rejected	(4) Full	(5) Site Selection
Housing Characteristics					
Median House value ($)	41,141.86	41,635.09	43,210.00	0.04	0.15
Monthly rent ($)	311.56	305.09	310.68	-0.11	-0.01
Proportion owner occupied	0.58	0.57	0.59	-0.15	0.20
Proportion occupied	0.82	0.79	0.82	-0.40**	-0.02
Proportion 0-1 bedrooms	0.13	0.12	0.13	-0.11	0.07
Proportion 2-3 bedrooms	0.79	0.79	0.78	0.04	-0.16
Proportion 4+ bedrooms	0.08	0.09	0.09	0.12	0.20
Proportion consisting of 1 unit	0.74	0.76	0.74	0.20	0.03
Proportion consisting of 2 unit	0.03	0.02	0.02	-0.42***	-0.52**
Proportion consisting of 3 or 4 unit	0.02	0.02	0.02	-0.44***	-0.27
Proportion consisting of 5+ unit	0.06	0.04	0.05	-0.31*	-0.10
Proportion mobile home	0.14	0.15	0.15	0.25	0.23
Proportion with plumbing facilities	0.81	0.78	0.81	-0.37**	-0.02
Proportion built last 5 yrs	0.09	0.10	0.10	0.10	0.25
Proportion built within 6-20 yrs	0.39	0.38	0.39	-0.12	-0.01
Proportion built within 20-40 yrs	0.30	0.29	0.31	-0.08	0.14
Proportion built >40 yrs	0.22	0.23	0.20	0.13	-0.27
Demographic Characteristics					
Household annual income ($)	27,514.55	28,633.90	28,519.56	0.22	0.19
Population density (/sq.km)	26.76	18.87	21.68	-0.18	-0.14
Proportion without high school diploma	0.36	0.36	0.38	0.01	0.19
Proportion with college	0.13	0.13	0.12	0.04	-0.12
Proportion under 18	0.28	0.28	0.30	-0.01	0.33
Proportion 65+	0.13	0.14	0.12	0.12	-0.27
Proportion houshold head black	0.07	0.06	0.05	-0.22	-0.35
Proportion houshold head hispanic	0.20	0.19	0.25	-0.05	0.22
Proportion below poverty line	0.24	0.21	0.24	-0.27	0.01
Proportion public assistance	0.09	0.08	0.09	-0.31*	-0.01
Proportion foreign born	0.05	0.05	0.06	0.14	0.23
Proportion female headed	0.20	0.17	0.18	-0.58***	-0.43*
Total employment (16+ persons)	25,711.60	17,727.27	22,355.75	-0.17	-0.09
Unemployment rate	0.08	0.07	0.08	-0.44**	0.02
Proportion in correctional institutions	0.01	0.01	0.01	-0.12	0.01
Observations	43	208	40		

Notes: This table reports summary statistics of pre-treatment covariates for counties in the Texas analysis. Column (1) shows the mean characteristics from the 1990 census for counties which contains prisons opened during the 1990s. Column (2) describes characteristics for counties in the rest of Texas. Column (3) describes characteristics for rejected counties from the site-selection competitions. The numbers reported in Column (4) and (5) are normalized differences between the treated and control counties in each of my samples, defined as the difference in means scaled by the square root of the average of the two within-group variances. The asterisks report the significance levels of the t-tests of mean equality between the treated and control counties in the corresponding sample, with one, two and three asterisks indicating a p-value less than 0.1, 0.05 and 0.01 respectively.

Table 15: Impact of prisons on housing, demographic and employment outcomes, Texas census tracts

	Full	Site Selection	
	(1)	(2)	(3)
Panel A: Housing Impacts			
Log(House value)	-0.007	0.007	0.027
	(0.030)	(0.033)	(0.036)
Log(Rent)	0.005	0.011	0.016
	(0.034)	(0.030)	(0.035)
Panel B: Income & Demographic Impacts			
Log(HH Income)	-0.030*	-0.061***	-0.059***
	(0.017)	(0.021)	(0.021)
Log(HH Wage & Salary)	-0.043**	-0.075***	-0.063***
	(0.018)	(0.020)	(0.023)
Prop. Black	-0.003	0.002	0.000
	(0.004)	(0.006)	(0.006)
Prop. Hispanic	0.010*	0.009	0.012
	(0.006)	(0.007)	(0.007)
Prop. Owner Occupied	-0.011	-0.019**	-0.010
	(0.008)	(0.008)	(0.008)
Panel C: Population & Employment Impacts			
Log(Population in HH)	0.002	-0.039	-0.013
	(0.024)	(0.026)	(0.028)
Log(Employment)	0.030	-0.007	0.024
	(0.026)	(0.031)	(0.034)
Unemployment rate	-0.009**	-0.008**	-0.008
	(0.004)	(0.004)	(0.005)
Observations	3,156	233	233
1990 dependent variable	Yes	Yes	Yes
1990 characteristics	Yes	Yes	Yes
1980 characteristics	No	No	Yes

Notes: Each entry reports the estimate of the impact of prison openings on the outcome presented in each row. Heteroskedasticity-robust standard errors are reported in parentheses. Asterisks denote significance: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 16: Income and wage impacts and compositional changes, Texas census tracts

		Inc.occupation		Excl.occupation	
	(1)	(2)	(3)	(4)	(5)
	Actual	Predicted	Net	Predicted	Net
Log(HH Income)	-0.061***	-0.040*	-0.020	-0.026	-0.035**
	(0.019)	(0.022)	(0.020)	(0.016)	(0.017)
			[-0.059,0.019]		[-0.068,-0.002]
Log(HH Wage & Salary)	-0.075***	-0.045**	-0.030	-0.022	-0.053***
	(0.018)	(0.022)	(0.020)	(0.014)	(0.018)
			[-0.069,0.009]		[-0.089,-0.017]
Observations	233	233		233	
1990 dependent variable	Yes	Yes		Yes	
1990 characteristics	Yes	Yes		Yes	

Notes: Each entry reports the estimate of the impact of prison openings on the outcome presented in each row. Column (1) reports the estimates of the impact of prison openings on the actual value of the outcome. Column (2) reports the estimates on the predicted value of the outcome, given demographic characteristics and the occupational composition of census tracts. Column (3) reports the estimates of the net impact of prisons after accounting for the effect on compositional changes (equal to the estimate in Column (1) minus that in Column (2)). Standard errors and 95 percent confidence interval of the net impact, reported in parentheses and in brackets respectively, are calculated using the seemingly unrelated estimation approach (Weesie, 1999; and StataCorp, 2013). Column (4) and (5) report similar quantities to Column (2) and (3) respectively, except that I exclude occupational compositions from the neighborhood characteristics that I use to predict average income and wages. All estimates in this table are based on the site selection sample. Heteroskedasticity-robust standard errors are reported in parentheses. Asterisks denote significance: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 17: Impact of prisons on employment, Texas counties

	Full	Site Selection	
	(1)	(2)	(3)
Panel A: Employment Impacts			
Log(Employment)	0.033**	0.057**	0.041
	(0.014)	(0.024)	(0.026)
Population over 16	0.010	0.032*	0.026
	(0.010)	(0.018)	(0.020)
Employment-to-population ratio	0.010***	0.013***	0.010*
	(0.004)	(0.005)	(0.005)
Unemployment rate	-0.005**	-0.002	-0.000
	(0.002)	(0.004)	(0.004)
Panel B: Log(Employment) by Sector			
Private	0.010	0.029	0.014
	(0.018)	(0.034)	(0.036)
Federal government	-0.107	-0.115	-0.111
	(0.070)	(0.129)	(0.139)
State government	0.289***	0.386***	0.366***
	(0.040)	(0.065)	(0.062)
Local government	-0.007	-0.029	-0.064
	(0.032)	(0.050)	(0.053)
Self-employed	0.032	0.043	0.048
	(0.031)	(0.046)	(0.047)
Observations	254	79	79
1990 dependent variable	Yes	Yes	Yes
1990 characteristics	Yes	Yes	Yes
1980 characteristics	No	No	Yes

Notes: Each entry reports the estimate of the impact of prison openings on the outcome presented in each row. All columns control for the full set of 1990 housing and demographic characteristics. Column (3) adds the following 1980 characteristics: proportion with no high school diploma, log average household income, log employed individuals over 16 and unemployment rate. Heteroskedasticity-robust standard errors are reported in parentheses. Asterisks denote significance: * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 18: Prisons and the response of local labor markets to macro conditions, Texas counties

	(1) Log Unemployed	(2) Log Unemployed	(3) Log Employed	(4) Log Employed
State level	0.932***	0.956***	0.718***	0.912***
	[0.0160]	[0.0284]	[0.0883]	[0.171]
Prison X State level	-0.0688**	-0.100**	-0.100	-0.292
	[0.0340]	[0.0427]	[0.182]	[0.241]
Observations	2,460	780	2,460	780
County FEs	Yes	Yes	Yes	Yes

Notes: Each column reports the estimate for the main effect of state-level outcome and the interaction terms of state-level outcome and the indicator for counties that experienced prison openings during the 1990s, based on the specification in Equation 7, except that year fixed effects are excluded. State-level outcome corresponds to the outcome presented in each column. The odd columns use the full sample of Texas counties, comparing the experience of winning counties with that of all other Texan counties during 2001-2010 after the prisons were opened. The even columns use the site-selection sample, comparing the experience of winning counties with that of the rejected counties. County-year data from 2001 to 2010 for unemployment and employment come respectively from the Local Area Unemployment Statistics (LAUS) program and the Quarterly Census of Employment and Wages (QCEW). Reported standard errors are clustered by county. Asterisks denote significance: * $p < .10$, ** $p < .05$, *** $p < .01$.