Essays in Public Economics and Empirical Political Economy

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Abstract

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This dissertation consists of three essays in applied microeconomics, focusing and topics in public economics and empirical political economy. Chapter 1 studies how the accessibility of subsidized housing impacts the probability that a recently incarcerated person returns to prison. Exploiting orthogonality of release timing to local subsidized housing conditions, I show that the probability of recidivism increases as average local wait time for public housing units decreases, suggesting potentially criminogenic effects of public housing. These effects are strongest for individuals released to counties where public housing projects contain more units and are located in high minority share neighborhoods. In contrast to the findings for public housing, I find no relationship between wait time for housing choice vouchers on return prison stays. Results from this study suggest increased re-incarceration as a negative externality associated with place-based housing subsidies but not associated with tenant-based subsidies. Chapter 2 investigates how partisan gerrymandering impacts turnout for US House elections. Common measures of gerrymandering are a function of turnout, making assessments of the impacts on turnout difficult. We present evidence from two natural experiments. First, in a nationwide sample, we construct a state-level measure of gerrymandering based on the partisan composition of districts and leverage variation stemming from Congressional redistricting. Second, we draw on Pennsylvania and Ohio voter files and leverage the court-ordered redrawing of Pennsylvania districts in 2018 aimed at undoing partisan gerrymandering. Both approaches reveal that higher levels of partisan gerrymandering causally reduce turnout. Chapter 3 presents work analyzing the effects of shocks to the resources of non-government resettlement organizations on refugee outcomes. We use reductions in refugee support provided by the largest partner entity, the United States Conference of Catholic Bishops, resulting from revelations of sexual abuse allegations across U.S. dioceses. Combining this information with recent administrative data and a novel approach to identify refugees at the diocese level, we find that resource strain resulting from newly disclosed abuse scandals leads to reductions in refugee participation in federal social safety net programs. We also find suggestive evidence of negative effects on labor market outcomes such as employment and wages.

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1.1 Introduction

According to the Bureau of Justice Statistics, 1,430,165 individuals were under supervision of the US state and federal correctional system in 2019. Individuals often return to prison soon after release. Over 600,000 people return from prison annually with roughly one third returning to prison within three years of reentry (Agan and Makowsky, 2021).

A robust literature in social work and criminology points to the importance of stable housing in preventing future repeat criminal action and subsequent prison stays (Jacobs and Gottlieb, 2020). Housing is associated with factors likely to reduce recidivism. An address, for example, is often prerequisite to obtaining employment. Simes (2019) notes the importance of neighborhood attachment in preventing return to crime. Securing housing is also lowers mental strain and levels of stress, both of which are important for preventing criminal recidivism. Lastly, standard economic models of crime emphasize opportunity cost as crucial to the decision of whether to engage in criminal activity (Doleac, forthcoming). Given that criminal activity – especially when perpetrated by individuals with previous records – carries the risk of incarceration, the presence and quality of housing is a potentially important opportunity cost.

However, *where* the housing is located may matter as much as gaining access to housing itself. Recently released prisoners disproportionately move into areas of concentrated poverty and high population density, features that are also associated with higher crime rates (Simes, 2019) and may promote repeat criminal activity. Recent work has highlighted the importance of neighborhood peers and on the probability of re-offense Kirk et al. (2018); Billings and Schnepel (2022). The neighborhood sorting may be further exacerbated by housing subsidy programs. Collinson, Ellen and Ludwig (2015) note that public housing tends to draws families to segregated and economically disadvantaged neighborhoods, and that housing voucher recipients typically do not use their subsidy to move to neighborhoods with observably different characteristics. Despite the benefits associated with housing after incarnation, the potential for neighborhood and peer effects makes relationship between subsidized housing policy and prisoner recidivism theoretically ambiguous.

Very little is known about how existing housing policy levers impact recidivism. The gap in knowledge is especially apparent given recent studies highlighting the capacity of means-tested programs and other social safety net policies to reduce rates of recidivism. For example, restrictions on SNAP and welfare benefits stemming from 1996 welfare reforms appear to lead to sizable increases in rates of recidivism (Tuttle, 2019; Yang, 2017*a*). Similarly, Agan and Makowsky (2021) tie increases in minimum wage to reductions in recidivism and find that access to state EITC benefits reduces re-incarceration for female re-entrants. This paper focuses on subsidized housing, which offers solutions to re-entrants facing housing barriers in the private market. At 1.1% of total federal outlays, housing assistance is among the largest low-income assistance programs in the United States (Falk, 2014).[1]

This paper investigates two related questions. First, I ask whether access to subsidized housing impacts the probability of return prison stays. Second, does this answer changes depending on the method of administering affordable housing? To answer these questions, I focus on comparing effects for the two major modes of subsidized housing: publicly owned project-based housing (hereafter referred to as public housing) and tenant based housing choice vouchers (HCVs).

To evaluate the effect of subsidized housing on re-incarceration, I exploit variation in access to housing programs across time and counties as generated by average wait time reported by local Public Housing Authorities (PHAs). I match the local wait time to returning prisoners based on county and year of release. Identification rests on the assumption that timing of release is orthogonal to local subsidized housing conditions. Recent work has relied on similar identifying assumptions regarding orthogonality of release timing to local conditions and policies,[2] noting that an individual's release from prison, when at the discretion of a parole board, is decided by an individuals risk profile and behavior while under supervi-

[1]In fiscal year 2013, federal housing assistance outlays totaled to $36 billion. By way of comparison, outlays for family support programs (including Temporary Assistance for Needy Families), Earned Income Tax Credit, and Supplemental Nutrition Assistance Program made up 0.8%, 1.7%, and 2.4% of federal outlays respectively

[2]See Yang (2017*b*) and Schnepel (2018) for effects of local labor markets, Agan and Makowsky (2021) for EITC and minimum wage, and Sherrard (2020) for local Ban-the-Box policies.

sion. Other individuals are released without the discretion of state boards, including those who serve their maximum sentence and those incarcerated in states with mandatory parole release after a minimum number of years is served. Under each of these scenarios, release timing is unlikely to be related to local subsidized housing conditions.

Using administrative state prison spell data from the Bureau of Prisons and annual reports from the U.S. Department of Housing and Urban Development (HUD) from 2004 to 2016, I find that longer wait time for public housing at time of release is associated with a *reduction* in recidivism probability. The effects are nonlinear, with two and three year recidivism probability flat when wait time is between 0 and 14 months, and then decreasing. For very long wait times, over 3 years, the relationship fades. However, the relationship between one year recidivism probability and wait time appears to be structured differently. Wait time is positively correlated with on year recidivism probability between 7 and 13 months, but negatively correlated after. Wait times greater than the median are associated with 0.004 decrease in three year recidivism, or about 1.4% relative to the mean. I find that these effects are highly dependent on the neighborhoods characteristics of public housing projects in a county. In particular, the effects are stronger where the neighborhoods around public housing projects are more racially segregated and have higher share of residents in poverty. The effect is largely driven by counties with dense public housing: the effect is largest and only significant in the fourth quartile of average units per public housing project. High county level property and violent crime rates within a county exacerbate the effect as well. Lastly, the re-incarceration effect decreases with age and is four to five times greater for non-whites compared to whites.

Although the US Department of Housing and Urban Development (HUD) officially encourages PHAs to consider people with criminal records, neither public housing nor HCVs are designed with post incarceration outcomes in mind. While overall housing access is measured by average wait time, it should be noted that the actual participation in the program by released prisoners in my sample is unobservable. Because of this, coefficients should be viewed as intent to treat analysis, and are likely attenuated compared to the effect of the treatment on the treated.

One potential threat to identification is that local economic conditions are likely related to demand for subsidized housing and the likelihood for released prisoners to re-offend. If high local unemployment rates drive applications to local PHAs, regressing re-incarceration on wait times may pick up the effect of unemployment instead of subsidized housing access. Notably however, this would my bias my results toward finding a *positive* association between wait time and re-incarceration. For example, Lower rates of recidivism are associated with greater availability of high paying, low skilled jobs (Schnepel, 2018) and affordable rent. To mitigate this attenuation bias, I include a vector of local economic conditions in my regressions. The inclusion of these controls does little to change the significance or effect sizes of the estimates.

I run the same analysis on wait times for HCVs, which are subsidies tied to tenants rather than projects. Wait times for HCVs are highly correlated with wait times for public housing. However, there is no equivalent effect of HCV wait time on re-incarceration rates. This null effect is unsurprising for two reasons. First, HCVs have generally not been found to meaningfully decrease criminal activity among recipients (Carr and Koppa, 2020). Second, private landlords often aggressively screen applicants based on criminal history (Evans, Blount-Hill and Cubellis, 2019), which would limit any treatment effect of voucher receipt.

Overall my results suggest that, despite providing affordable living arrangements, public housing exposes recently incarcerated individuals to greater recidivism risk, at least in the relative long term of 3 years. These effects are mitigated when public housing units are spread out within a county, implying that PHAs may be able reduce recidivism by diffusing housing across their area of coverage. Even so, the lack of a relationship between re-incarceration and housing vouchers indicates that vouchers may provide a means of subsidized housing that does not carry the negative externality of increased recidivism. This lends support to the general movement from HUD away from housing projects toward voucher based programs. Neither housing policy appears to meaningfully reduced rates of recidivism. The complicated nature of post incarcerating housing involves navigating both negative peer effects and discrimination on the private market. Policy solutions aimed at reducing re-incarceration through housing will need to contend with these issues.

1.2 Background

Returning citizens face many barriers to obtaining housing after incarceration spells. With low wages and limited employment opportunities upon release, many released prisoners have difficulty affording monthly rent (Geller and Curtis, 2011; Herbert, Morenoff and Harding, 2015; Petersilia, 2003; Roman and Travis, 2006). Racial discrimination and criminal background checks present significant obstacles as well (Evans and Porter, 2015; Evans, Blount-Hill and Cubellis, 2019). Surveys of ex-offenders reveal that large shares move in with family, friends, or intimate partners upon release (Simes, 2019). Many live with a parent or sibling (Visher and Courtney, 2007), with female relatives being especially common (Western et al., 2015). Homelessness is common among after release as well, with large shares of released prisoners reporting shelter stays shortly after release (Metraux and Culhane, 2004; Roman and Travis, 2006)

1.2.1 Criminal history and access to subsidized housing

Individuals released from prison on average earn much less than the general population even before any incarceration spell (Looney and Turner, 2018), and criminal records further restrict access to employment and limit income potential (Pager, 2003; Mueller-Smith, 2015). Returning prisoners overwhelmingly meet the income criteria for accessing federally subsidized housing,[3] yet ex-offenders experience two distinct sources of difficulty in accessing federally subsidized housing. First, public housing and voucher scarcity often generates long wait lists, effectively preventing receipt of housing assistance. Second, federal and local policies place restrictions on the applicants with certain categories of criminal offenses. The strategy of this paper is to leverage the variation in the first barrier as measured by average wait time during the year of release

Those seeking access to affordable housing are often faced with local scarcity in available units and vouchers, regardless of criminal history. PHAs manage the gap between the number

[3]Looney and Turner (2018) use IRS data linked to administrative data from federal state prisons to create a national census of prisoner records. They find that 45% of released prisoners have no reported income in the first year of release. Only 20% of those with jobs reported earning more than $15,000

of applicants and available subsidies by maintaining wait lists.[4] The difference in time between application and receipt of housing subsidy can be long – greater than ten years in some extreme cases. Panel A of figure 1 shows the distribution of average wait time individuals spent waiting for federally subsidized housing by county and year. Panel B shows the distribution of average wait times faced by returning prisoners in the county and year they were released. Notably, prisoners tend to be released to counties with longer weight times. For all county year pairs, the average weight time for public housing (HCVs) is 10 months (19 months), while returning prisoners face an average weight time of 16 months (24 months). The median average public housing for released prisoners is about 14 months, and 22 months for HCVs. Wait times vary substantially within county over time, a fact exploited in my identification strategy.

Ex-offenders may also be screened from receiving housing assistance by local PHAs. Nationally, HUD imposes lifetime bans for two types of offenses: manufacturing methamphetamine and offenses leading to inclusion on the national sex offender registry. Additionally, those who have been evicted from federally assisted housing for drug-related criminal activity are barred for three years from accessing vouchers and public housing assistance, unless they complete drug rehabilitation or the circumstances that led to the conviction are no longer relevant. As a result of the Housing Opportunity Program Extension Act of 1996, local PHAs were provided the legal framework to implement criteria for evicting tenants for criminal activity and screening applicants for criminal history. PHAs responded by increasing evictions and application denials (Roman and Travis, 2004). However, blanket policies barring access to all criminal record holders were potentially subject to litigation under the Fair Housing Act.[5] Written policies rarely exclude all types of offenses, and often do not consider offenses that occurred more than five or ten years in the past. Commonly, these policies consider offenses that impact the public peace and safety, often mentioning drug and violent offenses.[6]

[4]PHAs manage wait lists in three ways: first come first serve, random lotteries, and through use of preference lists.

[5]Those convicted of felony and misdemeanor offenses are not considered a protected class under the Fair Housing Act. However, criminal record screening policies disproportionately impact minorities and people of color (Oyama, 2009).

[6]Some PHAs consider drug and violent offenses that occurred up to 25 years in the past, although these appear to be exceptions. There are no nationally available data on PHA policies regarding applicants with

Despite potential restrictions in place stemming from HUD and local PHA policies, returning prisoners in practice gain access to subsidized housing at fairly high rates. In their study of prisoners recently released to Chicago, La Vigne and Parthasarathy (2005) find that 10.4% were living in Section 8 or public housing one to two years after release. In a similar study of men released in Cleveland, Visher and Courtney (2007) also find that one in ten respondents residing in public or section 8 housing one year after release. In a study of the Robert Taylor Homes in Chicago, Venkatesh (2002) document that 29% of household reported the return or expected return of an incarcerated person to their home, and an additional 12% expected a returning prisoner within two years.

1.2.2 Subsidized housing and crime

Project based pubic housing has long been accused of concentrating poverty and disadvantage in inner-city neighborhoods, leading to high rates of crime. Numerous studies in the economics literature have sought to test the hypothesis that concentrated public housing leads to crime, and whether policy interventions can undo the relationship.

By studying the demolition of 20,000 public housing in Chicago from 1990 through 2011, Aliprantis and Hartley (2015) examine how dispersing residents impacts neighborhood crime. They find large crime rate reductions in census blocks in and adjacent to where units were demolished. Neighborhoods receiving dispersed residents experienced modest increases in some types of crime.

The Move To Opportunity for Fair Housing (MTO) experiment provides a test of whether removing individuals from public housing and into more affluent neighborhoods reduces criminal behavior. Kling, Ludwig and Katz (2005) and Sciandra et al. (2013) find an offer of a voucher reduced violent crime arrests for young males and females in the short run, but only for females in the long run. Results for property crime was more mixed, with property crime arrests declining for females, but increasing for young males. Using moves induced by MTO, Ludwig and Kling (2007) find that racial segregation was the most important neighborhood characteristic in explaining differences in violent crime arrests; a finding the

criminal records. For a review of the types of policies implemented by PHAs, see Tran-Leung (2016) authors attribute to higher drug market activity in high-minority neighborhoods.

While the MTO literature test compares the receipt of a voucher relative to public housing residence, more recent studies explore the crime effects of receiving a HCV for low income families more generally. Jacob, Kapustin and Ludwig (2015) use a randomized voucher lottery in Chicago to explore the effects of receiving vouchers on children, finding little effects on a variety of outomes, including crime. Carr and Koppa (2020) use a similar strategy for heads of households on voucher waiting lists in Houston, and also find no effect on arrest rates.

1.2.3 Post-incarceration residence and peer effects

Neighborhood and peer effects may be particularly important for the recently incarcerated. Kirk (2015) for example, finds that parolees often concentrate in the similar neighborhoods and that peer effects contribute to the probability of recidivism. Kirk uses residential changes induced by Hurricane Katrina and finds evidence suggesting that an additional parolee per 1000 residents increases one year re-incarceration rates within a zip code by 11%. Using variation in the number of neighborhood peers incarcerated at the time of a prisoner's release, Billings and Schnepel (2022) show that a decrease in the presence of one neighborhood peer leads to a 20 percent decrease in probability of rearrest within three months.

Recent literature has also revealed that even programs explicitly aimed at successful prisoner reentry can actually increase recidivism as a result of interacting with offenders. As part of a series of replications of reentry RCTs, Doleac et al. (2020) find evidence to suggest that residential substance abuse programs may actually *increase* the probability of rearrest for recently released prisoners. Lee (Forthcoming) finds that plausibly exogenous assignment to halfway houses accelerates the timing of re-incarceration relative to direct release prisoners.

1.3 Data

1.3.1 National Corrections Reporting Program

The primary source of data for this paper comes restrictive access administrative data from the Bureau of Justice Statistics (BJS) as a part of the National Corrections Reporting Program (NCRP). These data compile records from state department of corrections from 43 states over the years 2000 to 2018. The data include records for every person admitted or released from state prison during these years. Each individual is given a unique identifier, so that it is possible to track both an individual's release and readmission into state prison. Therefore, this dataset allows me to identify instances of recidivism that occur within a state.[7] Importantly, these data include information on offenses associated with each prison spell, state and county of offense, race, education, and age of release.

Following Yang (2017*b*), I impose several sample restrictions to create the analysis sample. First, I keep only first prison spell. Individuals with multiple prison spells, all else equal will be less likely to be granted access to public housing and vouchers by local PHAs. In order to target the population most likely to gain access to subsidized housing, I only include records for and individual's first release from prison observed in the data. Second, I drop observations if the county of offence is not present. Due to a data-processing error when state records were compiled by the National Corrections Reporting Program, county codes from several states are missing. For these states, analysis of the local subsidized housing on re-incarceration is impossible. I drop these records from the analysis[8] Third, I drop observations from counties that released less than 100 offenders during the full sample period. Fourth, I do not include records with missing release dates. It is impossible to determine whether these individuals are still in incarcerated or if their lack of subsequent prison terms indicates they have not been re-incarcerated. Fifth, I drop records for prisoners who were released before the year 2000. Sixth, I do not include records for prison terms that

[7] The structure of these data do not allow for tracking individuals who re-offend across state lines or who are sent to county prisons upon release from state prisons. In a study of prisoners released in 2005 in 30 states, Durose, Cooper and Snyder (2014) document 10.9% of released prisoners were re-arrested in another state following release.

[8] These states are Ohio, Oklahoma, Oregon, Pennsylvania, Rhode Island, South Carolina, South Dakota, Virginia, Washington, West Virginia, Wisconsin, and Wyoming.

ended because of death. Lastly, I drop the state of California from the analysis. To address overcrowding in state prisons, the California Department of Corrections and Rehabilitation transferred a large numbers of inmates to county jails in 2011. Because county jails do not report to the NCRP, it is impossible to accurately assess whether individuals released were re-incarcerated and under what time frame. Recidivism for each released prisoner is measured by whether a subsequent prison term exists. The main outcome variables are binary variables for whether new prison spell exists for the prisoner within a given number of years or months since release.

Figure 2 shows the estimated hazard rate of re-incarceration by month since release. Similar to Yang (2017*b*), these rates are calculated by the number of offenders returning to prison in a given month over the total risk pool at the beginning of that month. This figure indicates that offenders are most at risk for returning to prison within the first several years after release. The first year, as indicated by the spike is particularly important. Given the importance of the first few years after release. I will primarily report coefficients from models defining recidivism as re-incarceration within one year, two years, and three years. I also present results for recidivism defined over longer time periods, up to 5 years after release.

1.3.2 HUD Picture of Subsidized Households Data

To create a measure of subsidized housing accessibility at the time of release, I draw data from HUD's Picture of Subsidized Households. These data have been published annually from 2004 through 2021, compiling local program characteristics from annual surveys of PHAs. As the treatment in my analysis, I make use of the reported average time spent by tenants on the waiting list for HCVs and public housing as a measure of accessibility for each program. This variable is calculated by taking the difference in months between the date new residents are admitted and the date they entered the waiting list. This is difference is then averaged to the county level. Figures 11 through 13 show the geographic distribution of wait time across the US, shown separately for 2004, 2010, and 2016. The maps reveal substantial variation in wait time both across counties and within counties across time.

In addition to average wait time, the Picture of Subsidized Households report characteristics of the neighborhoods surrounding public housing projects. In particular, the share of minority residents and the share of residents below the federal poverty line in the project census tract is reported. These shares are then averaged across all projects to the county level. These characteristics allow me to control for neighborhood conditions as well as test for heterogeneity in the treatment effect.

I restrict the data to years before 2018, the last reported in the NCRP data. I then match the subsidized housing data to my sample of released prisoners by year and county of release.

1.3.3 Uniform Crime Reports County-Level Detailed Arrest and Offense Data (UCRC)

In order to test the hypothesis that higher crime rates in is the mechanism at work, I make use of the Uniform Crime Reports County-Level Detailed Arrest and Offense Data (UCRC) . The UCR contains data from the FBI's Uniform Crime Reporting program (UCR) aggregated to the county level by Inter-University Consortium for Political and Social Research (ICPSR). The UCR compiles arrest and report data from local and state law enforcement agencies, resulting in heterogeneity in coverage across counties. I make use of these data for the years 2004 through 2016, the last year for which it is available. I linearly interpolate within county to construct the county crime measure for 2015, which is also not available. Following Freedman and Owens (2011), I limit the analysis to counties where imputation by ICPSR is less than 50%.

After linking the above data sources, the resulting dataset includes 2,661,386 inmates released to counties with public housing data available and 2,645,390 with Housing Choice Voucher data available. Because I use project level data as controls in my main specification for analysis of both public housing wait time and HCV wait time, the HCV sample includes only those inmates released to counties with both public housing programs and HCV programs. Table 1 presents summary statistics for the resulting samples. The sample consists largely of low-educated men, and about half are non-white. The average age of release is 34. The average time served is 28 months, and most common sending offenses include violent crimes (26%), drug crimes (28%), and property crimes (16%).[9] Columns 1 and 2 in Panel B show means for county by year level variables, weighted by the number of prisoners released. The third column shows means for the same variables for all counties. Prisoners are released to counties with higher wait times for both public housing and HCV subsidies and higher rates of crime. They are much less often released to a county with a single public housing project, but more often released to counties whose public housing projects are in poorer and high minority share neighborhoods.

One threat to identification is that time varying county characteristics may be associated with both wait time and re-incarceration probability. To test for this issue, I regress average wait time on county level characteristics, controlling for county and year fixed effects. Figure 4 shows results from this exercise. Each coefficient is from a separate regression, and the sample is the county and year pairs that are matched to prisoners in the data. Reassuringly, there is very little relationship between these county characteristics and wait time. Though a one standard deviation increase in unemployment rate is associated with about a 20 day increase in HCV wait time on average. Given high employment is generally found to decrease rates of recidivism (Schnepel, 2018; Yang, 2017*b*), bias introduced from not including unemployment rate is likely to bias my results toward finding a positive relationship between wait time and recidivism. Nevertheless, I include these county level controls in my main specification.

1.4 Empirical strategy

To estimate the effect of subsidized housing access on repeat incarcerations, one would ideally randomly assign recently incarcerated individuals to counties with various levels of subsidized housing access. To approximate this experiment, I exploit the fact that the timing of release is determined by parole board decisions and likely to be orthogonal to local subsidized housing conditions. To capture the accessibility of subsidized housing assistance, I use the average wait time reported by PHAs for project based public housing and HCVs.

[9] Here, sending offense is the offense carrying the highest sentience for each given prison spell.

In addition to county and year fixed effects, the main specification includes a vector of time varying county level controls to account for factors that may be correlated with subsidized housing wait lists.

While average reported time on waiting list provides a measure of accessibility at the time of release, it is not clear that the relationship between the length of wait list and the probability of re-offending should be linear. The effect of a change from 6 month to 12 month wait list is likely different than the change from a 30 month to a 36 month wait list. To test for such a non-linear relationship, I first estimate a linear spline model with knots spaced at quartiles of average wait time, including minimal controls:

$$Recid_{ict} = \beta_0 + \sum_{i}^{4} \beta_i h(ave\_wait_{ct}, q_i, q_{i-1}) + \gamma_t + \alpha_c + pop_{ct} + \epsilon_{ct}$$

where

$$h(ave\_wait_{ct}, q_i, q_{i-1}) = \left\{ \begin{array}{ll} q_i - q_{i-1}, & \text{if } ave\_wait_{ct} \geq q_i \\ \max(0, ave\_wait_{ct} - q_{i-1}), & \text{if } ave\_wait_{ct} < q_i \end{array} \right\}$$

where q_i is the ith quartile of average county level wait time experienced by released prisoners in my sample and $q_0 = 0$. γ_t and α_c are year and county fixed effects respectively. $pop_{c,t}$ is the county c's population in year t. $Recid_{ict}$ is a binary variable set to one if individual i – released in county c in year t– is re-incarcerated within a given time period. I focus primarily on one, two year and three year recidivism rates. β_1 through β_4 represent the slope of the linear relationship between average wait time and recidivism probability for each quartile of the data. I observe a released person's re-commitment to prison only if it occurred during the years 2000 through 2018. In all analyses, I drop individuals for whom it is impossible to observe the entire relevant period. For example, I drop individuals who were released in 2017 and 2018 from the 2 year recidivism analysis.

Tables 2 and 3 show the results of the linear spline regressions for public housing wait time and HCV wait time respectively. Notably, evidence that increased wait time in the second quartile is associated with an increase in re-incarceration within one year of release, in general the relationship is insignificant in this range. However, the relationship appears to change around the median, with each additional month in the third quartile in this range *reducing* re-incarceration probability. The relationship fades by the final quartile; for two and three year re-incarceration, indicating that the the marginal influence of an additional month is negligible when wait lists are already long. In contrast, I find very little indication that wait time for HCVs influences re-incarceration, though again there is a positive relationship in the second quartile for one year recidivism.

It is important to note that the average wait time reported variable reflects the length of a PHA wait list rather than actual time spent waiting for public housing or HCV. In my main specification, I define treatment, "Low Access", as equal to one for a released prisoner who enters a county with average wait time above the median of all released prisoners in my sample. The results from the spline regressions (Table 2) indicate that the relationship between wait time and re-incarceration appears to shift directions near the median, and the cutoff for the binary treatment is is chosen to reflect this relationship. The following is the primary specification:

$$Recid_{ict} = \beta_0 + \beta_1 \mathbb{1}[\text{Low Access}_{ct}] + X_{it} + Z_{ct} + \gamma_t + \alpha_c + \epsilon_{ct}$$

In addition to year and county fixed effects, I include a set of both county and individual level controls. I include race, sex, age at time of release, the square of age at time of release, a dummy for whether the released prisoner held a GED or high school diploma when entering prison, and detailed offense fixed effects. At the county level, I include total population, income per capita, share of low skilled employment, employment rate, total government transfers, unemployment rate, and violent and property crime per 1000 residents.

I also include measures of neighborhood characteristics of the public housing projects, aggregated to the county level. To account for segregation in the project neighborhood, I include the percent of minority residents living in the same census tract as a public housing project, averaged across all projects in a county. Similarly, I include the county average of the percent of individuals living below the federal poverty line in census tracts with public housing projects. Lastly, I include a dummy for whether a county has a single location for public housing. About 12% of prisoners in my sample are are released to such counties.

1.5 Results

Table 4 presents results for the main specification on public housing access. Even columns display models with minimum controls, including only year and county fixed effects and a linear control for county level population. Odd columns report the full specification for all controls. Columns 1 and 2 report effects on one year re-incarceration rates. In both the minimal and full controls model, there are little difference in re-incarceration between released prisoners who experience high and low access to public housing.

Column 3 and 4, reporting effects on 2 year re-incarceration, similarly show a negative but insignificant relationship between a low access public housing environment and re-incarceration, though with larger coefficients. The relationship between access and three year recidivism, however, is both negative and significant, with the preferred model estimating that low access public housing reduces re-incarceration by probability by .4 percentage points (1.4% relative to the mean of 30%).

While public housing is an explicitly place-based policy solution to affordable housing, housing vouchers are tied to individuals. If the results in the previous section are indeed driven by public housing directing re-entrants toward disadvantaged neighborhoods, such effects should be absent for vouchers. Dis-amenities associated with neighborhoods are likely capitalized into rental values. Therefore, even if re-entrants experience discrimination on the private market, housing subsidies in the form of vouchers should weakly expand their set of housing options to include lower crime neighborhoods.

Table 5 is similar to Table 4, but reporting results of the effect of HCV accessibility. However, in sharp contrast to the estimates for public housing access, coefficients on HCV access are small and never statistically significant. These specifications set the treatment equal to one when average wait time for HCVs is above the median experienced by released prisoners in my sample. Figure 3 shows the correlation between public housing and HCV average wait time. Because the average wait times for the two programs reflect the same underlying PHA administrations and local housing demand, the wait times between the two programs are tightly correlated. However, prisoners experience substantially shorter median wait times for public housing compared to HCVs. To test for whether the null effects on HCV access is driven by the difference in the cutoff used to define the treatment rather than the effect of program access, I re-run the HCV analysis defining treatment based on the public housing treatment definition. If the results from the public housing analysis are driven by other factors correlated with subsidized housing access generally rather than public housing access in particular, constructing the treatment in this way should yield similar results to those reported in 4. Instead, Table 6 shows small and insignificant relationship between re-incarceration and the treatment defined in this way.

Tables 7, 8, and 9 are similar to tables 4, 5, and 6, but with the treatment defined by terciles of wait time rather than median. Each regression is run on dummy variables for tercile of wait time, with the second tercile the omitted category. Table 7 presents the results for terciles of public housing wait time on one, two and three year recidivism. Results from the one year recidivism regressions mirror the inverted U-shaped relationship found from the one year spline regressions in table 2. Relative to individuals released to counties in the second tercile of wait time, those in the first tercile are about quarter of a percentage point less likely to recidivate within one year and those in the third tercile about a half of a percentage point. When estimating the effect of wait time on two and three year recidivism, however, the coefficient on the first tercile becomes smaller and insignificant. In the preferred specification, Being in the third tercile of wait time is associated with a .6 percentage point decrease in the probability of being re-incarcerated within two years (2.5% relative to the mean) and a .7 percentage point decrease in being re-incarcerated within 3 years (2.4% relative to the mean).

Table 8 is similar to table 7, but uses terciles of wait time for housing choice vouchers. The direction of the estimates are generally similar to those in table 7. Notably, the coefficient on the first tercile of voucher wait time is negative, significant, and similar in magnitude to the corresponding public housing coefficient. Once controls are added, however, the estimate becomes and insignificant. Table 9 repeats the analysis in table 8 using public housing terciles cutoffs with housing choice voucher wait times to assign "placebo" terciles. Again, the coefficient on first tercile is positive and significant for one year recidivism before controls are added. The magnitude is similar to the estimate from corresponding public housing specification. This suggests that perhaps underlying factors correlated with subsidized housing environment common to vouchers and public housing are driving this estimate.

To investigate whether the effect changes over time, I re-estimate the full model with with the re-incarceration measure defined by increasingly longer time periods after release, again defining treatment as above median wait time. Figure 5 shows results for both public housing and for HCVs. Each figure shows a plot of the treatment coefficients on re-incarceration rates from 6 months to 60 months after release. The effect size of public housing access on re-incarceration peaks for three year recidivism rates, and begins to fade afterward. consistent with earlier findings, the effects are null for HCV access through for re-incarceration defined through 5 years after release.[10]

1.5.1 Mechanisms

1.5.1.1 Neighborhood and county characteristics

Next, I test whether the negative relationship between re-incarceration rates and lower access to public housing is driven by neighborhood disadvantage. I separately re-run the main specification including interaction terms accounting for racial segregation, poverty and crime rate. As a measure of racial segregation, I include the average share of minority residents in the census tracts containing public housing projects. I similarly construct a measure of neighborhood poverty by including the average share of residents below the federal poverty line in the census tracts containing public housing projects. These variables reflect neighborhood characteristics of public housing projects for counties in my sample. I also include county level violent and property crime per 1000 residents.[11] I normalize each of these measures for ease of interpretation.

Table 10 presents results for models including neighborhood characteristic interaction terms. Each panel represents a separate regression. Allowing the treatment to interact with local characteristics increases the effect substantially in terms of magnitude and significance

[10]Repeating this exercise defining HCV access using the same cutoff in the public housing analysis similarly yields null results.

[11]Crime data are not readily available at the census tract level, and so I am unable to construct a measure that reflects crime rates specific to neighborhoods occupied by public housing projects.

compared to the main specification. Results presented in Panel A suggest that, relative to mean rates of re-incarceration, low access to public housing at time of release corresponds with about a 3% decline in re-incarceration for one, two and three year recidivism rates relative to mean rates. This relationship is highly sensitive to the level of segregation present in the housing projects in the county of release. For example, a standard deviation decrease in the segregation measure erases three quarters of the effect size on one year re-incarceration. This moderation is smaller in magnitude, but still economically significant for two and three year rate. Panel B shows results for my measure of project neighborhood poverty. Coefficients are similar in magnitude to the those in the previous panel, though the interacted terms are insignificant for the two and three year rates.

Heterogeneity by county crime rate as reported in panel C is particularly striking. A one standard deviation decrease in index crime more than erases the main effect. In the case of one year re-incarceration rates, an increase in the crime rate one standard deviation from the mean more than triples the effect estimated effect size. The degree to which high crime rates magnify the effect size decreases for the two and three year recidivism measures.

Taken all together, the results in this table reveal that the estimated treatment effects are substantially moderated by neighborhood characteristics. The pattern that emerges is that low access to public housing decreases recidivism, and that the neighborhood disadvantage sizably increases this relationship.

Recent literature on crime and public housing has focused on densely populated high rise housing projects (Aliprantis and Hartley, 2015). To test whether my effects are driven by densely build projects, I create a measure of public housing density by taking the average number of units per housing project in a county. I then run the main specification separately for each quartile. Figure 6 displays the confidence intervals for estimated effect of low public housing access on 6 month through 60 month re-incarceration rates, shown separately for each quartile of project density. Notably, the treatment effect is only present for individuals in the fourth quartile, where they are both larger and more persistent than the main estimated effects. These results indicate that counties with densely built public housing projects are driving the main result.

An alternative explanation for the negative relationship between low public housing access and re-incarceration is that subsidized housing increases recidivism probability through its employment disincentives. Recent literature in economics has underscored the importance of the role that employment and well paying jobs play in reducing rates of recidivism (Schnepel, 2018; Yang, 2017*b*). Both housing voucher and public housing generally cap benefits at 30% of a recipients income, which creates a substantial implicit labor market tax. Susin (2005) provide evidence of this effect using a matched sample, estimating that public housing reduced adult earnings by 19%. Strong empirical evidence suggests the effect is present among housing voucher recipients as well. Jacob and Ludwig (2012) show significant declines in employment and labor income following receipt of housing vouchers in a random lottery. However, given that this disincentive is present in both project and tenant based subsidized housing, it is unlikely that the results in this paper are driven by reductions in labor market participation.

1.6 Individual heterogeneity

Table 11 presents results for models including interaction terms for offender characteristics. Each panel represents a separate regression, estimating the differential effects by race, sex, age, and high school or GED attainment. Panel A includes the dummy "White" (relative to non-white) as an interaction term. Results from this model indicate that there is substantial differences the low access effect size across race. For a white prisoner, the one year re-incarceration effect of being released to a low access public housing county is one fifth of the effect size for non-white individuals. The effect size for white individuals is 26% that of non-whites for two year recidivism rates, and about 45% for three year rates. The models in Panels B and D imprecisely estimate coefficients on the interaction term, but qualitatively suggest that effect of low public housing access is greater for males and for individuals with lower levels of education. Lastly, panel C suggests that the effect of public housing access on re-incarceration is smaller for individuals who are older at the time of release.

Figure 7 shows results by sending offense type. Each figure shows a plot of the treatment coefficients on re-incarceration within X months after release, estimated using the fully specified model. HUD policy prohibits subsidized housing applicants with two types of criminal records: manufacturing methamphetamine and sex offenses leading to inclusion in the national sex offender registry. While the NCRP data is not sufficiently detailed to identify methamphetamine offenses from other drug offenses, I am able to test whether the effect of public housing access on re-incarceration is present for individuals convicted of sex offenses. Given that these individuals are banned from accessing subsidized housing in anywhere in the United States, the "low access" treatment should have no effect on these individuals. Figure 7d reveal a coefficient pattern similar to the main specification in 5a, however, all coefficients are insignificant.

Next, I turn to drug and violent crime. Although there is no blanket ban on admitting individuals with these criminal histories, violent and drug offenses are commonly mentioned in PHA screening policies. As PHAs cite the safety and peace of residents as motivation for screening, violent offenders likely are to be screened particularly aggressively (Tran-Leung, 2016). Figures 7b and 7c plot coefficients for drug and violent crime respectively. While as expected, there are no treatment effects for violent crime, the pattern for drug crimes appears to mirror that of the main effects. However, given that drug offense class incorporate a wide range of severity, it likely includes offenses not screened by PHAs. Lastly, Figure 7a shows negative effects for individuals convicted of property property crimes, which are less commonly screened by PHAs.

1.7 Conclusion

Overall, my results suggest that higher local access to the main housing subsidies administered by HUD do nothing to reduce rates of re-incarceration among recently released state prisoners. To the contrary, I find evidence that lower public housing availability as measured by average wait time reduces the probability of re-incarceration within 3 years. I find these effects are largely driven by counties whose projects are situated in high minority neighborhoods, and to a lesser extent by high poverty neighborhoods. Counties with higher density public housing projects and counties with high crime rates also experience the effect more intensely. I argue that public housing may expose recently incarcerated individuals to disadvantaged neighborhoods, increasing recidivism probability.

On the other hand, HCVs provide a mode of subsidized housing that does not carry the negative externality of increased recidivism. HCVs are tied to the tenant rather than a housing unit, and so allow the recipient the choice to move away from poor neighborhoods. Unlike public housing, renters with HCV rent on the private market. Private landlords have been found to aggressively screen tenants with criminal history (Evans, Blount-Hill and Cubellis, 2019), which would limit any impact of vouchers on returning prisoners and potentially explains the null results for HCVs.

This study focuses on recidivism as defined by a return to state prison after a previous stay in a state prison. It is important to note that this measure is a particularly severe form of recidivism that combines the offense allegedly committed by the individual, the role of law enforcement, and the courts ultimately settling on (state) prison time. Whether similar relationships exist between subsidized housing access and other measures of recidivism – such as county jail spells or repeat arrests – is left to future work.

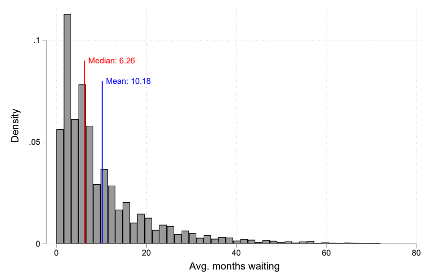
The primary policy goal of HCVs and public housing is not to reduce recidivism, and neither program appears to be effective toward that end. Policies aimed at reducing recidivism through housing should be targeted and carefully designed to account for neighborhood effects.

1.8 Tables and figures

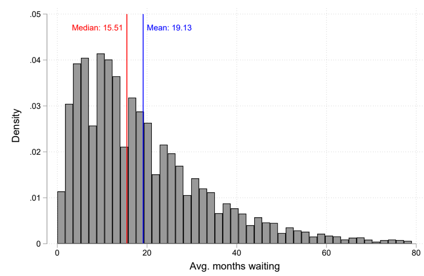
Figure 1: Distribution of average wait time for federal subsidized housing

Panel A: Average wait time by county and year

(a) Public housing

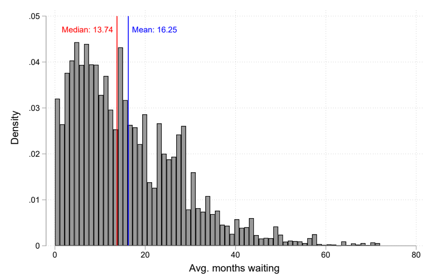


(b) Housing choice vouchers

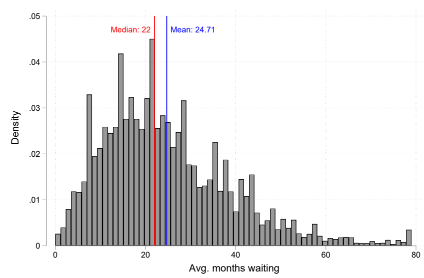


Panel B: Average wait time by county and year weighted by released prisoners

(c) Public housing

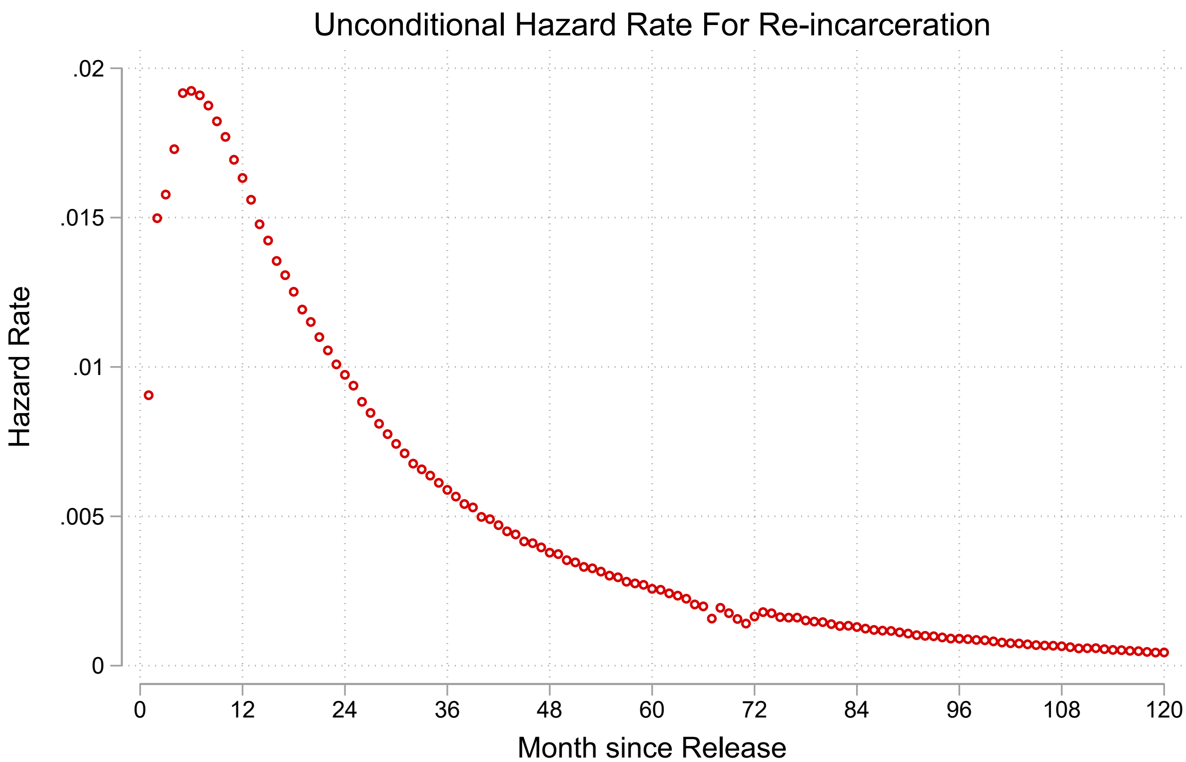


(d) Housing choice vouchers



Data are from the HUD Picture of Subsidized Households from 2004 to 2020. The distribution of average wait times in counties is displayed in Figure 1a for public housing and Figure 1b for housing vouchers. Figures 1c and 1d display the density of average county wait times for released prisoners in the NCRP.

Figure 2: Unconditional hazard rate for re-incarceration



Notes:Data are from the NCRP for prisoners released from 2000–2018. This figure represents the unconditional hazard rate for re-incarceration by months since release. For each month after release, the hazard rate is calculated by dividing total number of prisoners re-incarcerated over the total not re-incarcerated.

Figure 3: Binned scatterplot with linear fit

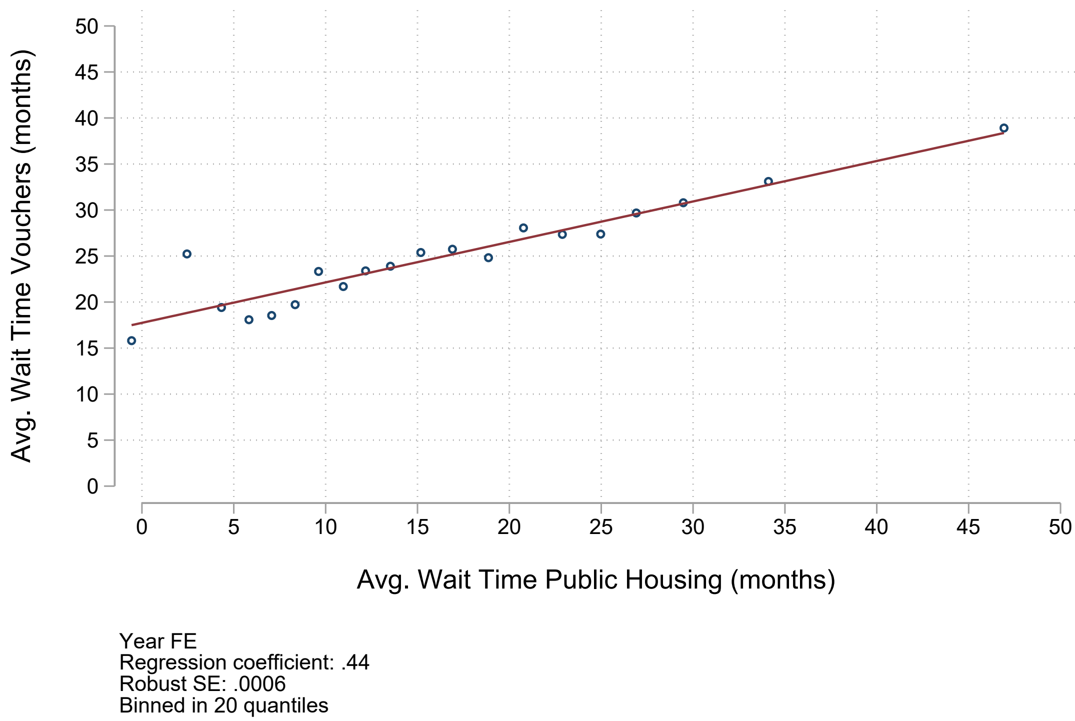
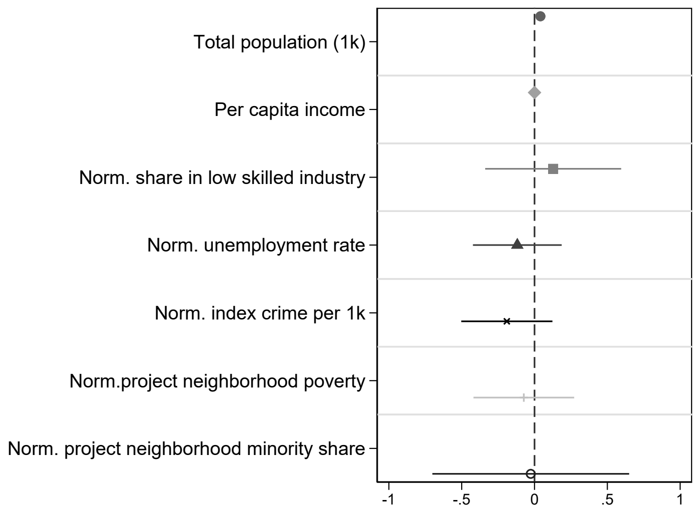


Table 1: Summary statistics

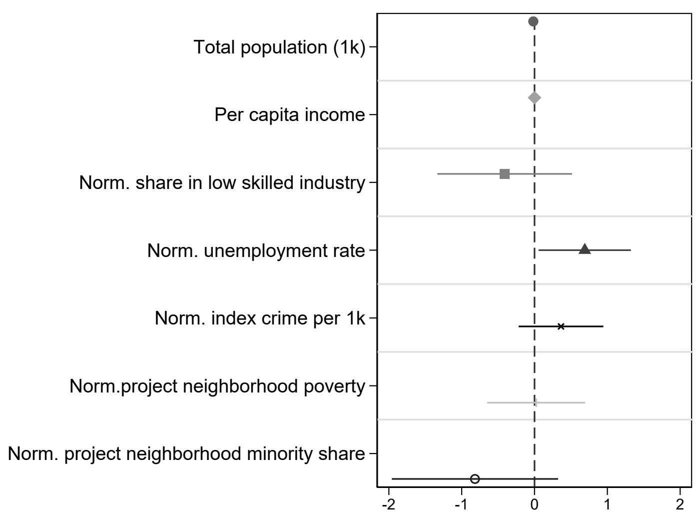
	Public Housing	HCV	All Counties
Panel A			
Individual Characteristics			
White	0.50	0.49	-
Male	0.86	0.86	-
Age at release	34.37	34.35	-
GED/High School	0.37	0.37	-
Time Served (Months)	28.08	28.26	-
Violent Offense	0.26	0.26	-
Drug Offense	0.28	0.28	-
Property Offense	0.16	0.17	
Panel B			
County Characteristics			
Avg. PH wait time (months)	15.20	15.40	9.62
Avg. HCV wait time (months)	24.38	24.41	18.91
County population	890168.52	932457.07	98562.93
County per capita income	39250.52	39606.16	35463.67
County share in low-skilled jobs	0.27	0.27	0.23
County unemployment rate	6.79	6.76	6.48
Reported index crimes per 1k	38.29	38.72	22.19
Single project in county	0.12	0.12	0.29
Pct. below poverty in project census tract	28.30	28.62	20.61
Pct. minority in project census tract	51.72	52.60	28.75
Observations	2661386	2645390	44002

Figure 4: Balance between county characteristics and average wait time

(a) Public housing



(b) Housing choice vouchers



Notes: This figure reports coefficients from separate regressions estimating the effect of various county level characteristics on the county average months spent waiting for subsidized housing. The sample is every county/year to which prisoners in my sample are released, totalling to 10,299 county/year observations. All regressions include year and county fixed effects. 90% confidence intervals are calculated using robust standard errors.

Table 2: Linear spline regression: average wait time for public housing in county

	Re-incarceration within:		
	One Year	Two years	Three years
Quartile 1: (0,7)	-0.0002	0.0002	0.0006
	(0.00040)	(0.00050)	(0.00057)
Quartile 2: (7,13)	0.0007**	0.0006	0.0002
	(0.00030)	(0.00038)	(0.00040)
Quartile 3: (14,23)	-0.0005**	-0.0007**	-0.0008***
	(0.00024)	(0.00028)	(0.00030)
Quartile 4: (24,72)	-0.0002*	-0.0001	-0.0001
	(0.00011)	(0.00015)	(0.00016)
Observations	2847250	2683233	2516518
Mean dep. var	.136	.24	.302

Standard errors in parentheses

Year FE, county FE, county population

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports estimated linear spline coefficients for the effect of county average wait time for public housing on re-incarceration probability. All regressions include year fixed effects, county fixed effects, and control for county population. Standard errors are clustered at the county by year level and are displayed in parenthesis. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 3: Linear spline regression: average wait time for HCV in county

	Re-incarceration within:		
	One Year	Two years	Three years
Quartile 1:	-0.0002	-0.0004	-0.0002
	(0.00020)	(0.00025)	(0.00029)
Quartile 2:	0.0004*	0.0004	0.0002
	(0.00021)	(0.00027)	(0.00028)
Quartile 3:	-0.0001	-0.0001	-0.0000
	(0.00019)	(0.00023)	(0.00023)
Quartile 4:	-0.0001	-0.0002*	-0.0002**
	(0.00008)	(0.00010)	(0.00011)
Observations	3193361	3003348	2807279
Mean dep. var	.137	.24	.301

Standard errors in parentheses

Year FE, county FE, county population

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports estimated linear spline coefficients for the effect of county average wait time for housing choice vouchers on re-incarceration probability. All regressions include year fixed effects, county fixed effects, and control for county population. Standard errors are clustered at the county by year level and are displayed in parenthesis. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4: Effect of public housing access on re-incarceration

	One year		Two years		Three years	
Low Access	0.0001	-0.0009	-0.0011	-0.0023	-0.0031*	-0.0042**
	(0.00131)	(0.00125)	(0.00163)	(0.00154)	(0.00175)	(0.00166)
White		0.0009		-0.0034**		-0.0091***
		(0.00108)		(0.00160)		(0.00195)
Male		0.0226***		0.0506***		0.0676***
		(0.00076)		(0.00088)		(0.00099)
Age at release		-0.0080***		-0.0129***		-0.0141***
		(0.00030)		(0.00039)		(0.00042)
Age at release squared		0.0001***		0.0001***		0.0001***
		(0.00000)		(0.00000)		(0.00000)
GED/High School		-0.0212***		-0.0323***		-0.0356***
		(0.00098)		(0.00138)		(0.00163)
Observations	2661386	2661386	2661386	2661386	2496172	2496172
Mean dep. var	.136	.136	.24	.24	.301	.301
Offense. FE	NO	YES	NO	YES	NO	YES
County Population	YES	YES	YES	YES	YES	YES
County level controls	NO	YES	NO	YES	NO	YES
Ind. controls	NO	YES	NO	YES	NO	YES

Notes: This table reports the coefficient on low access to public housing as measured by high average wait time at in the county and time of release. Regressions in this table define the binary treatment "Low Access" using the median wait time for public housing among recently released prisoners in my sample. All regressions include year and county fixed effects. Standard errors are clustered at the county by year level and are displayed in parenthesis. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 5: Effect of HCV access on re-incarceration

	One year		Two years		Three years	
Low Access	0.0012	0.0006	0.0014	0.0006	-0.0003	-0.0008
	(0.00122)	(0.00117)	(0.00148)	(0.00145)	(0.00156)	(0.00157)
White		0.0010		-0.0035**		-0.0092***
		(0.00109)		(0.00162)		(0.00197)
Male		0.0231***		0.0511***		0.0679***
		(0.00078)		(0.00090)		(0.00101)
Age at release		-0.0080***		-0.0129***		-0.0141***
		(0.00031)		(0.00040)		(0.00043)
Age at release squared		0.0001***		0.0001***		0.0001***
		(0.00000)		(0.00000)		(0.00000)
GED/High School		-0.0231***		-0.0350***		-0.0386***
		(0.00113)		(0.00162)		(0.00190)
Observations	2645390	2645390	2645390	2645390	2474713	2474713
Mean dep. var	.137	.137	.241	.241	.302	.302
Offense. FE	NO	YES	NO	YES	NO	YES
County Population	YES	YES	YES	YES	YES	YES
County level controls	NO	YES	NO	YES	NO	YES
Ind. controls	NO	YES	NO	YES	NO	YES

Notes: This table reports the coefficient on low access to housing choice vouchers as measured by high average wait time at in the county and time of release. Regressions in this table define the binary treatment "Low Access" using the median wait time for housing choice vouchers among recently released prisoners in my sample. All regressions include year and county fixed effects. Standard errors are clustered at the county by year level and are displayed in parenthesis. $^{*}p < 0.10,\ ^{**}p < 0.05,\ ^{***}p < 0.01$

Table 6: Effect of HCV access on re-incarceration: placebo

	One year		Two years		Three years	
Low Access	-0.0006	-0.0011	-0.0011	-0.0016	-0.0021	-0.0023
	(0.00111)	(0.00109)	(0.00137)	(0.00135)	(0.00155)	(0.00151)
White		0.0010		-0.0034**		-0.0091***
		(0.00109)		(0.00162)		(0.00197)
Male		0.0231***		0.0511***		0.0679***
		(0.00078)		(0.00090)		(0.00101)
Age at release		-0.0080***		-0.0129***		-0.0141***
		(0.00031)		(0.00040)		(0.00043)
Age at release squared		0.0001***		0.0001***		0.0001***
		(0.00000)		(0.00000)		(0.00000)
GED/High School		-0.0231***		-0.0350***		-0.0386***
		(0.00113)		(0.00161)		(0.00190)
Observations	2645390	2645390	2645390	2645390	2474713	2474713
Mean dep. var	.137	.137	.241	.241	.302	.302
Offense. FE	NO	YES	NO	YES	NO	YES
County Population	YES	YES	YES	YES	YES	YES
County level controls	NO	YES	NO	YES	NO	YES
Ind. controls	NO	YES	NO	YES	NO	YES

Notes: This table reports the coefficient on low access to housing choice vouchers as measured by high average wait time at in the county and time of release. Regressions in this table define the binary treatment "Low Access" using the cutoff used to define the public housing treatment. All regressions include year and county fixed effects. Standard errors are clustered at the county by year level and are displayed in parenthesis. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 7: Effect of public housing access on re-incarceration: terciles

	One year		Two years		Three years	
Third tercile of wait time	-0.0052***	-0.0055***	-0.0051**	-0.0061***	-0.0060**	-0.0073***
	(0.00185)	(0.00180)	(0.00224)	(0.00223)	(0.00234)	(0.00242)
First tercile of wait time	-0.0028**	-0.0025**	-0.0015	-0.0015	-0.0004	-0.0006
	(0.00130)	(0.00125)	(0.00163)	(0.00157)	(0.00177)	(0.00171)
White		0.0008		-0.0034**		-0.0091***
		(0.00108)		(0.00160)		(0.00195)
Male		0.0226***		0.0506***		0.0677***
		(0.00076)		(0.00088)		(0.00099)
Age at release		-0.0080***		-0.0129***		-0.0141***
		(0.00030)		(0.00039)		(0.00042)
Age at release squared		0.0001***		0.0001***		0.0001***
		(0.00000)		(0.00000)		(0.00000)
GED/High School		-0.0213***		-0.0323***		-0.0357***
		(0.00098)		(0.00138)		(0.00163)
Observations	2661386	2661386	2661386	2661386	2496172	2496172
Mean dep. var	.136	.136	.24	.24	.301	.301
Offense. FE	NO	YES	NO	YES	NO	YES
County Population	YES	YES	YES	YES	YES	YES
County level controls	NO	YES	NO	YES	NO	YES
Ind. controls	NO	YES	NO	YES	NO	YES

Notes: This table reports the coefficient on terciles of public housing wait time in the county and time of release. Second tercile is omitted category. All regressions include year and county fixed effects. Standard errors are clustered at the county by year level and are displayed in parenthesis. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 8: Effect of HCV access on re-incarceration: top tercile vs bottom tercile

	One year		Two years		Three years	
Third tercile of wait time	-0.0011	-0.0016	-0.0007	-0.0013	-0.0003	-0.0006
	(0.00143)	(0.00139)	(0.00176)	(0.00171)	(0.00181)	(0.00179)
First tercile of wait time	-0.0020*	-0.0014	-0.0014	-0.0008	-0.0001	0.0002
	(0.00122)	(0.00120)	(0.00150)	(0.00149)	(0.00160)	(0.00163)
White		0.0010		-0.0035**		-0.0092***
		(0.00110)		(0.00162)		(0.00197)
Male		0.0231***		0.0511***		0.0679***
		(0.00078)		(0.00090)		(0.00101)
Age at release		-0.0080***		-0.0129***		-0.0141***
		(0.00031)		(0.00040)		(0.00043)
Age at release squared		0.0001***		0.0001***		0.0001***
		(0.00000)		(0.00000)		(0.00000)
GED/High School		-0.0231***		-0.0350***		-0.0386***
		(0.00113)		(0.00162)		(0.00190)
Observations	2645390	2645390	2645390	2645390	2474713	2474713
Mean dep. var	.137	.137	.241	.241	.302	.302
Offense. FE	NO	YES	NO	YES	NO	YES
County Population	YES	YES	YES	YES	YES	YES
County level controls	NO	YES	NO	YES	NO	YES
Ind. controls	NO	YES	NO	YES	NO	YES

Notes: This table reports the coefficient on terciles of housing choice voucher wait time in the county and time of release. Second tercile is omitted category. All regressions include year and county fixed effects. Standard errors are clustered at the county by year level and are displayed in parenthesis. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

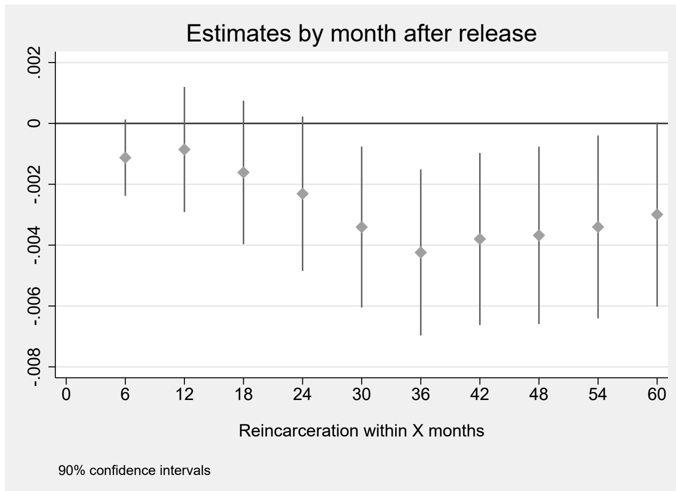
Table 9: Effect of HCV access on re-incarceration: top tercile vs bottom tercile (Placebo using public housing terciles)

	One year		Two years		Three years	
Third tercile of wait time:	0.0018	0.0012	0.0010	0.0003	-0.0012	-0.0016
(placebo)	(0.00123)	(0.00117)	(0.00150)	(0.00148)	(0.00160)	(0.00162)
First tercile of wait time:	0.0027*	0.0023	0.0030	0.0021	0.0023	0.0010
(placebo)	(0.00154)	(0.00151)	(0.00197)	(0.00190)	(0.00224)	(0.00217)
White		0.0010		-0.0035**		-0.0092***
		(0.00109)		(0.00162)		(0.00197)
Male		0.0231***		0.0511***		0.0679***
		(0.00078)		(0.00090)		(0.00101)
Age at release		-0.0080***		-0.0129***		-0.0141***
		(0.00031)		(0.00040)		(0.00043)
Age at release squared		0.0001***		0.0001***		0.0001***
		(0.00000)		(0.00000)		(0.00000)
GED/High School		-0.0231***		-0.0350***		-0.0386***
		(0.00113)		(0.00161)		(0.00190)
Observations	2645390	2645390	2645390	2645390	2474713	2474713
Mean dep. var	.137	.137	.241	.241	.302	.302
Offense. FE	NO	YES	NO	YES	NO	YES
County Population	YES	YES	YES	YES	YES	YES
County level controls	NO	YES	NO	YES	NO	YES
Ind. controls	NO	YES	NO	YES	NO	YES

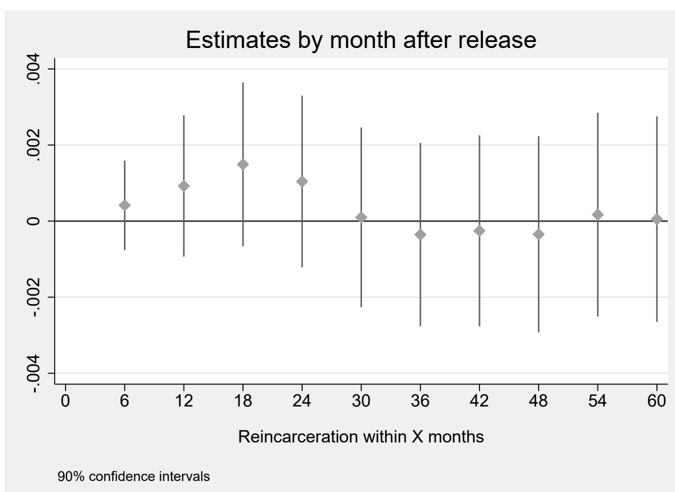
Notes: This table reports the coefficient on terciles of housing choice voucher wait time in the county and time of release. Second tercile is omitted category. Regressions in this table define the tercile dummies using the terciles of public housing wait time. All regressions include year and county fixed effects. Standard errors are clustered at the county by year level and are displayed in parenthesis. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Figure 5: Estimated coefficient by month after release

(a) Public housing



(b) Housing choice vouchers



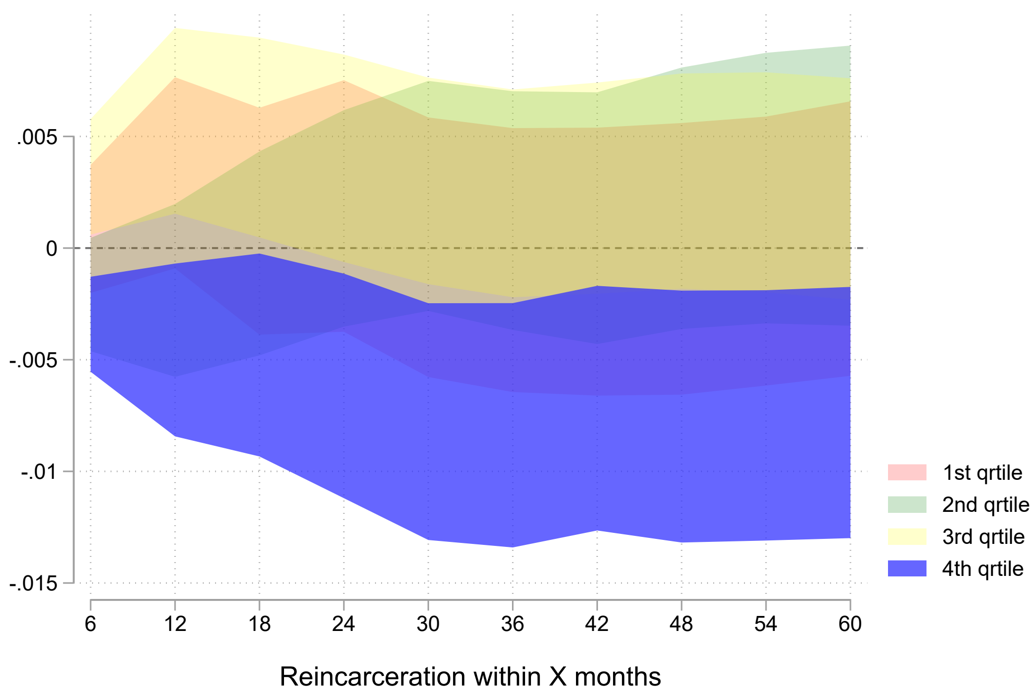
Notes: This figure displays the coefficient on being above median wait time by months since release. All regressions include year FEs, county FEs, offense dummies, and full set of county and individual controls. 90% confidence intervals displayed, calculated using standard errors clustered at the county by year level.

Table 10: Heterogeneity by county characteristics

	Re-incarceration within:		
	One Year	Two years	Three years
Panel A			
Norm. Perc. minority X Low access	-0.0031**	-0.0044**	-0.0038*
	(0.0014)	(0.0019)	(0.0021)
Low access	-0.0042***	-0.0063***	-0.0090***
	(0.0015)	(0.0019)	(0.0022)
Norm. Perc. minority in census tract	-0.0085***	-0.0152***	-0.0188***
	(0.0012)	(0.0015)	(0.0017)
Panel B			
Norm. Perc. below poverty X Low access	-0.0035**	-0.0030	-0.0019
	(0.0015)	(0.0020)	(0.0022)
Low access	-0.0044***	-0.0063***	-0.0090***
	(0.0015)	(0.0020)	(0.0022)
Norm. Perc. below poverty in census tract	0.0082***	0.0113***	0.0119***
	(0.0012)	(0.0015)	(0.0016)
Panel C			
Norm. Index crimes X Low access	-0.0076***	-0.0083***	-0.0077***
	(0.0016)	(0.0020)	(0.0022)
Low access	-0.0031**	-0.0050***	-0.0076***
	(0.0013)	(0.0018)	(0.0020)
Norm. reported index crime	0.0104***	0.0145***	0.0160***
	(0.0010)	(0.0012)	(0.0013)
Observations	2632806	2632806	2468712
Mean dep. var	.14	.24	.30

Notes: All regressions include year FEs, county FEs, offense dummies, and full set of county and individual controls. Standard errors are clustered at the county by year level and are displayed in parenthesis.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Figure 6: Estimated coefficients for public housing by months after release: split by quartiles of public housing density



Notes: This figure displays the confidence intervals for coefficients on the "low access" treatment. Each shaded region represents estimates for separate quartiles of public housing concentration experienced by released prisoners in my sample. All regressions include year FEs, county FEs, offense dummies, and full set of county and individual controls. 90% confidence intervals displayed, calculated using standard errors clustered at the county by year level.

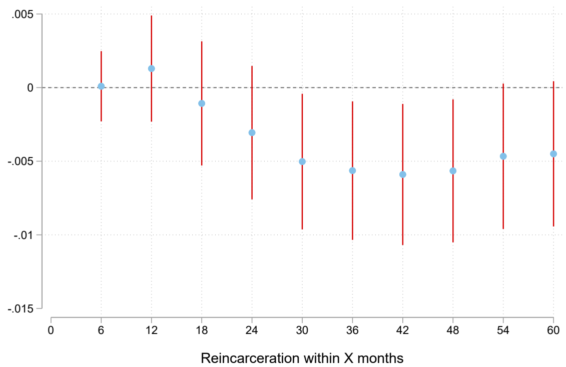
Table 11: Heterogeneity by individual characteristics

	Re-incarceration within:		
	One Year	Two years	Three years
Panel A			
White X Low Access	0.0053***	0.0070**	0.0066*
	(0.0020)	(0.0029)	(0.0036)
Low Access	-0.0067***	-0.0095***	-0.0121***
	(0.0019)	(0.0025)	(0.0029)
White	-0.0003	-0.0049***	-0.0100***
	(0.0009)	(0.0012)	(0.0014)
Panel B			
Male X Low Access	-0.0016	-0.0018	-0.0024
	(0.0018)	(0.0021)	(0.0023)
Low Access	-0.0026	-0.0045*	-0.0067**
	(0.0019)	(0.0024)	(0.0027)
Male	0.0221***	0.0496***	0.0666***
	(0.0009)	(0.0011)	(0.0013)
Panel C			
Age at release X Low Access	0.0001	0.0003*	0.0004**
	(0.0001)	(0.0002)	(0.0002)
Low Access	-0.0072	-0.0168***	-0.0233***
	(0.0047)	(0.0063)	(0.0070)
Age at release	-0.0079***	-0.0129***	-0.0142***
	(0.0003)	(0.0003)	(0.0004)
Panel D			
Highschool or GED X Low Access	0.0003	0.0008	0.0036
	(0.0020)	(0.0029)	(0.0034)
Low Access	-0.0041**	-0.0063***	-0.0101***
	(0.0017)	(0.0023)	(0.0026)
GED/High School	-0.0220***	-0.0332***	-0.0379***
	(0.0011)	(0.0014)	(0.0016)
Observations	2632806	2632806	2468712
Mean dep. var	.14	.24	.30

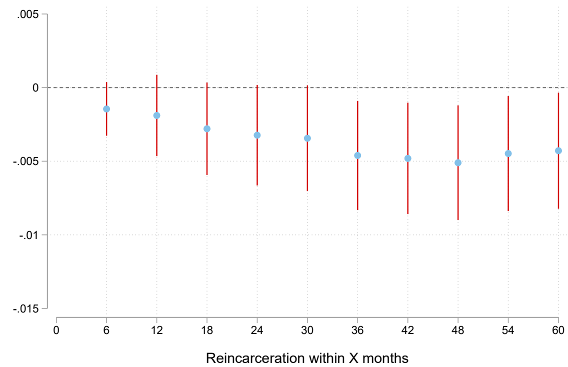
Notes: All regressions include year FEs, county FEs, offense dummies, and full set of county and individual controls. Standard errors are clustered at the county by year level and are displayed in parenthesis.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Figure 7: Estimated coefficient by month after release and original offense

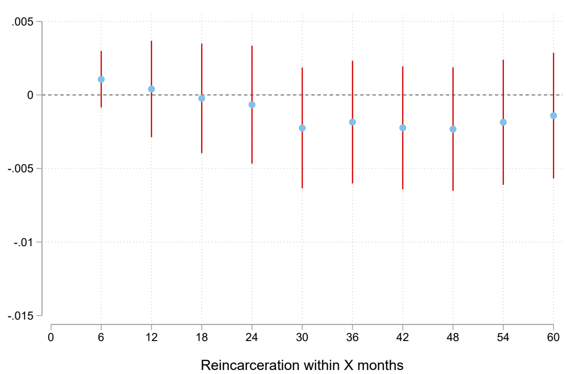
(a) Property crimes



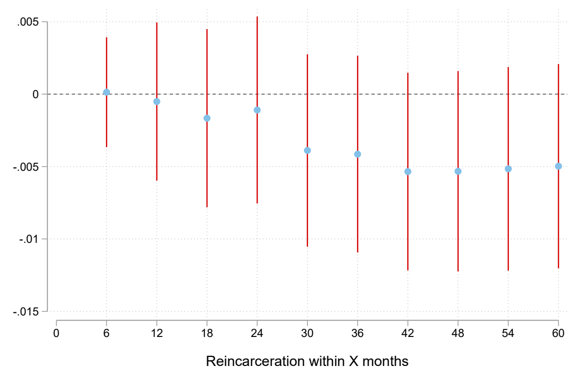
(b) Drug crimes



(c) Violent offenses



(d) Sex offenses



Notes: This figure displays the coefficient on being above median wait time by months since release. All regressions include year FEs, county FEs, offense dummies, and full set of county and individual controls. 90% confidence intervals displayed, calculated using standard errors clustered at the county by year level.

2.0 Partisan Gerrymandering and Turnout

Work presented in this chapter is the result of research conducted with co-authors Daniel Jones of the University of Pittsburgh Graduate School of Public and International Affairs and Carly Urban of Montana State University Department of Agricultural Economics and Economics.

2.1 Introduction

How does partisan gerrymandering—the reshaping of Congressional boundaries to benefit a specific party—affect political participation? Increased partisanship may reduce turnout for voters on the margin of participating in Congressional—and potentially even higher ballot—elections. Specifically, policymakers may be concerned that a gerrymandered district could reduce participation because district competition has decreased. It is also possible that the act of gerrymandering itself disengages voters, as they may feel that their state government has designed the system such that their vote cannot matter.

While at first estimating the effects of partisan gerrymandering on turnout appears straightforward, this question remains largely unanswered in the literature. This is because measures of partisan gerrymandering, with the most common being the efficiency gap, often rely in part on ex post measures of turnout (Stephanopoulos and McGhee, 2015).[1] This makes regressing turnout on standard gerrymandering measures impossible. This paper overcomes previous challenges in measuring gerrymandering across two settings by (1) constructing an ex ante predictive measure of ex post gerrymandering to assess the impacts of gerrymandering nationwide and (2) using a natural experiment where a state was court-ordered to reduce partisan gerrymandering.

We begin with a nationwide state-level panel that includes variation before (2008 and

[1]The efficiency gap is based on the number of votes "wasted" by each party in a state. We provide detail on the construction of the measure in section 2.3.1.1. Other measures have also been discussed. Stephanopoulos and McGhee (2018) examine the pros and cons of these alternative measures.

2010) and after (2012 and 2014) redistricting. We build upon work by Jones and Walsh (2018), to design a measure of gerrymandering that does not rely upon ex post vote shares. Specifically, we use their predicted efficiency gap to measure predicted gerrymandering and show that it correlates with actual gerrymandering. We find that a one standard deviation unit increase in partisan gerrymandering reduces turnout by 1.6%; these effects are concentrated in midterm years, within which turnout falls by 3.6%, when presidential candidates are not on the ballot.

We then assess the impacts of a 2018 court-ordered redistricting to undo partisan gerrymandering in Pennsylvania, drawing on voter registration files from Pennsylvania and Ohio (where district lines did not change in 2018). We compare the turnout of voters in the two states, before and after the 2018 redistricting. The setting offers several advantages. First, it offers the opportunity to test the extent to which changes in turnout driven by changes in gerrymandering occur throughout a state or more so through district-level factors (namely, changes in competitiveness), therefore speaking to the mechanism driving our nationwide results. Second, the voter-level data allows us to test which voters are most impacted by gerrymandering. Third, we document in a variety of ways that Pennsylvania is in fact less gerrymandered in 2018 than before, so our analysis does not rely on any particular researcher-constructed measure of gerrymandering. Therefore, concerns about whether our results from the first portion of the paper are driven by our measure of gerrymandering are addressed in the second portion of the paper, which is agnostic on measurement of gerrymandering. And, finally, from a policy perspective, while creating more partisan Congressional districts within a state may have a negative impact on turnout, it is not clear that redrawing the boundaries in a way that reduces partisan gerrymandering can reverse the negative effects. We find that removing partisan gerrymandering increased participation. These effects are largest for those voters living in areas that became competitive and marginal voters—the young and voters not registered as Democrats or Republicans.

2.1.1 Related work and contribution

We contribute to two main literatures. First, to the best of our knowledge we are the first to look at the causal effects of partisan gerrymandering on political participation. In related work, Stephanopoulos and Warshaw (2019), consider the effects of partisan gerrymandering on candidate entry and find that gerrymandering increases the likelihood that a seat is uncontested and attracts less qualified candidates. Caughey, Tausanovitch and Warshaw (2017), using the efficiency gap as their measure of gerrymandering, impacts roll-call voting behavior of elected legislators, suggesting that it is reasonable to think that voter behavior (and turnout in particular) is impacted; our paper fills that gap.

Second, our paper is related to other work studying the impacts of redistricting (but not explicitly partisan gerrymandering) on political participation. Some existing work uses redistricting as a natural experiment, providing a source of variation to study how various features of a district impact turnout. For example, Fraga (2016) leverages reassignment of voters to new districts (due to redistricting) to study how turnout of different race groups responds to district racial composition and to the opportunity to vote for a co-ethnic incumbent. Fraga, Moskowitz and Schneer (2021) use a similar approach to study whether being assigned to a partisan-aligned district impacts turnout, finding that it does. We argue that our paper is primarily related to those noted here in the methods employed and less so in the substance of the research question: namely, they too use post-Census redistricting as a shock to some electoral feature faced by voters (incumbency, district race composition, district partisan composition). In our case, unlike the papers mentioned here, the substantive feature of interest is the degree of partisan gerrymandering (as measured by the efficiency gap, for instance) voters face.

Hunt (2018) and Hayes and McKee (2009) are related to the papers above, but more explicitly study the impacts of redistricting itself. Hunt (2018) draws on voter files from Florida and studies the consequences of the 2012 wave of redistricting, finding that changes in competitiveness and exposure to incumbent candidates have small impacts on the likelihood of voting, whereas (echoing Fraga et al.) changes in the partisan composition of a voter's district leads to larger impacts on turnout. Hayes and McKee (2009) on the other hand find that voters assigned to a district with a new incumbent as a result of redistricting are less likely to know the name of their incumbent and less likely to vote in their House race. Again, while we too study a consequence of redistricting, we are examining the impacts of a different consequence than these papers.

Finally, we implicitly contribute to work examining the effects of competitiveness on participation that uses variation in redistricting to examine competitiveness (some of which fall in the category described in the previous paragraph using redistricting as a source of variation). The most related paper in this space is Moskowitz and Schneer (2019), who use redistricting beginning in 2012 and voter file records to understand how individuals who are moved into a more competitive district change their turnout behavior in US House races. They estimate a precise null effect of competitiveness on participation, except when the House race is at the top of the ticket and even then, the effect is small in magnitude. However the authors do not consider effects due to (un)gerrymandering explicitly, only changes in competition. In our Pennsylvania natural experiment, we are able to separate the effects of gerrymandering and competition and can show that the effects of un-gerrymandering are concentrated among districts that became competitive.

Prior to Moskowitz and Schneer (2019), many papers studied the effects of competition or perceived closeness of elections on turnout. See, for example Gerber et al. (2020) or Enos and Fowler (2014) and citations therein – both conduct field expeirments and find that providing information on closeness of upcoming elections has no mobilizing effect. Shachar and Nalebuff (1999) explores closeness and turnout in presidential contests. As noted above, Hunt (2018) also considers the impacts of competitiveness.

2.2 Theoretical framework

We consider two main mechanisms through which partisan gerrymandering may impact turnout which we briefly outline in this section.

First, gerrymandering in a state may have a general depressive effect on turnout throughout the impacted state through reduction in trust in government and the elections process.

We refer to this as the behavioral demobilization channel, in that voters are not responding to changes directly impacting the electoral context within their district (though such changes may occur). First we note that very recent evidence suggests a link between the method of redistricting, its impact on gerrymandering, and perceptions of fairness. Grose and Nelson (2021) find that independent redistricting commissions "improve voter attitudes toward the fairness of the process of redrawing lines" and that voters are less likely to perceive lines drawn by commissions (versus state legislatures) as gerrymandered.

Next, we note that existing comparative work has documented a positive relationship between trust in government and turnout (Grönlund and Setälä, 2007) and perceptions of electoral integrity and turnout (Birch, 2010).[2] Alvarez, Hall and Llewellyn (2008) provide evidence from the US that confidence in the electoral process is positively associated with turnout. Thus, if partisan gerrymandering within a state diminishes a voters' confidence in the process, they may be less likely to vote (independent of, or in addition to, any changes to the electoral environment they face in their district).

In short, if the behavioral demobilization channel is prominent, we would expect turnout to decline throughout a state that has become more gerrymandered, rather than just in the most impacted districts.

The second potential channel we have in mind is this: gerrymandering clearly alters the district-specific electoral environment – for example, and most notably, changing the level of competitiveness of some districts. To the extent that voters respond to those changes, then gerrymandering will impact turnout. Moskowitz and Schneer (2019) outline several ways that competitiveness of a district may impact turnout. The first is instrumental voting, wherein voters respond to their likelihoood of being decisive. A second is elite mobilization, wherein more parties and candidates put forth more effort to mobilize voters in more competitive races. A third explanation they offer is that voters in competitive districts are simply different in a number of ways which motivates them - and us later in this paper - to estimate models with individual voter fixed effects. However, as already noted in the previous subsection, a long line of literature has empirically explored the impact of competitiveness or perceived closeness of elections on turnout, yielding mixed results. Thus, though

[2]See Norris (2013) for a broader discussion.

it is clear that gerrymandering will impact the competitiveness of some districts, from prior work it remains ambiguous whether that will in turn impact turnout. However, to the extent that this channel is active, we would expect to find that gerrymandering in a state does not impact turnout statewide, but instead only in districts that have experienced large changes in competitiveness.

Finally, we also highlight that the two mechanisms above need not be mutually exclusive.

2.3 Nationwide analysis

We first assess the effects of partisan gerrymandering on turnout in U.S. House of Representatives elections from 2008–2014.[3] Our basic approach is a continuous treatment difference-in-difference strategy, leveraging variation in the degree of partisan gerrymandering stemming from post-Census redistricting that took effect in 2012. We base our analysis around the efficiency gap measure, which has become a key metric of partisan gerrymandering in academic work and legal cases in recent years (Stephanopoulos and McGhee, 2018; Stephanopoulos and Warshaw, 2019; Stephanopoulos and McGhee, 2015). As the efficiency gap is a state-level measure for each election, we use a state panel for the nationwide results.

2.3.1 Data

2.3.1.1 The Efficiency Gap

The efficiency gap measures the degree of state level gerrymandering in a given election. The measure counts the "wasted votes" for each party in each district throughout the state. For one party in a district that party won, all votes above the number of votes needed to win the district (half the electorate plus one) are "wasted." In a district the party lost, all of its votes are "wasted." For example, if Democrats win a very narrow race in a district, very few votes were "wasted," but many Republican votes were wasted, in the sense that those votes could have been used to help Republicans win in a neighboring district. If Democrats

[3] Our data on state-level voting-age population turnout rate is available through 2014.

win by a large margin in that district, many Democratic votes were "wasted" and some Republican votes were wasted. This definition of "wasted votes" is meant to identify areas where substantial "cracking" and "packing" of voters in redistricting has occurred.[4]

Wasted votes for each party are then summed throughout the state for the year in question, and the efficiency gap (EG) for state s in year y is calculated as

$$\text{EG}_{sy} = \frac{\text{Total D Wasted Votes}_{sy} - \text{Total R Wasted Votes}_{sy}}{\text{Total Votes}_{sy}}$$

In this formulation, a positive efficiency gap implies that district lines were drawn such that more Democratic votes were ultimately wasted, or a Republican-favoring gerrymander; the reverse is true for a negative value. Values close to zero imply that district lines are "fair" in the sense that one party does not systematically have more "wasted" votes.

Stephanopoulos and McGhee (2015), in proposing the efficiency gap, explain that it is useful for describing the partisan direction of a gerrymander, while the absolute value of the gap reveals the presence and magnitude of gerrymandering. As our paper is focused on the latter—whether *more* gerrymandering impacts turnout generally—we take the absolute value of the efficiency gap as our measure of gerrymandering:

$$\text{Gerry}_{sy} = |\text{EG}_{sy}|.$$

In the resulting measure, smaller values indicate fairly drawn district lines and larger values indicate more severe partisan gerrymandering.

[4]In "cracking," one party's geographic stronghold will be "cracked" into multiple districts where they may be in a minority in each resulting district, thereby diluting and "wasting" the votes of that party's members. In "packing," district lines are drawn to concentrate one party's voters into a single district that that party would likely win regardless, thereby pulling those votes away from neighboring districts and "wasting" them in a district that already leaned towards that party. See Friedman and Holden (2008) for more on optimal cracking and packing.

2.3.1.2 Predicted gerrymandering

The key aim of our paper is to assess how gerrymandering affects turnout. Unfortunately, the efficiency gap is an *ex post* measure of gerrymandering; it uses the turnout that has occurred to measure how gerrymandered a state is. The standard efficiency gap measure therefore takes as an argument the variable we aim to take as an outcome. Moreover, as the efficiency gap is calculated based on realized turnout, it partially captures voters' responses to the particular candidates who chose to run in the newly-drawn districts. To overcome those challenges, we construct a new measure of *predicted gerrymandering* based on the partisan composition of voters in Congressional districts. Rather than calculating efficiency gaps based on the number of Democrats and Republicans that turned out, we can instead calculate a predicted efficiency gap based on the number of Democrats and Republicans within the districts.

We draw on data from Jones and Walsh (2018), who constructed "predicted (two-party) Democrat shares" for every US Congressional district in both pre-2012 and post-2012 district maps; these data are valuable because systematic nationwide data on the partisan composition of Congressional districts are not otherwise readily available. For instance, for each district prior to 2012, the authors report a share of voters within the district who are likely Democrats rather than Republicans. We direct readers to that paper for more detail about data construction, but in short the "predicted Democrat share" is based on estimating relationships between county-level registered voter party shares (in states where such data are available) and county-level demographic variables, then using the resulting coefficients to construct predictions of district-level party shares based on district-level demographics. Importantly, in the construction of their measure the demographic data are held constant, using the 2010 Census, so any variation in party shares after vs. before redistricting in a given district is driven solely by the new district maps. Constructing party shares this way allows us to avoid concerns that our measure partially captures migration or other demographic shifts.

With those data, we can calculate "predicted wasted votes" for each party-district-election combination. To do so, we replicate the process described in the previous section. We calculate "predicted Democrats" and "predicted Republicans" in each district by multiplying the "predicted Democrat share" (and one minus that measure) by the district population. Then "predicted wasted Democrat votes" in a district that is majority Democratic is:

$$\text{Predicted Ds} - (\text{District Pop} \times 0.5 + 1),$$

or the number of Democrats in excess of what would be required to barely hold a majority. In districts where Democrats are in the minority, the predicted wasted Democrat votes is simply the total number of predicted Democrats. Predicted wasted Republican votes is produced in the same way. Then, we calculate a predicted efficiency gap (PredEG_{sy}) using the standard efficiency gap formula, but entering *predicted* wasted votes in place of actual wasted votes. As our primary interest is in the level of gerrymandering and not the partisan lean, we take the absolute value of PredEG_{sy} to define our main "treatment" variable:

$$\text{PredGerry}_{sy} = |\text{PredEG}_{sy}|$$

for state y in election year y.[5]

For ease of interpretation, we convert both Gerry_{sy} and PredGerry_{sy} to standard normals. As we will show later, state-level changes in our PredGerry_{sy} measure are in fact strongly predictive of changes in actual, ex post gerrymandering (Gerry_{sy}). We drop all states with a single Congressional district, as they cannot be gerrymandered. Appendix Figure A.3 plots both measures across the states prior to the 2012 wave of redistricting (top maps) and after the new maps have taken effect (bottom maps).

[5]We have also conducted analysis constructing a similar efficiency gap/gerrymandering measure taking as an input the 2008 Obama Vote Share in place of the "predicted Dem. share" measure described here. Results are in the same direction, and we cannot reject that they differ from our preferred estimates reported in our paper, but they are less precisely estimated. We argue though that our version of the measure using "predicted Dem. share" has some distinct advantages over using presidential vote share. Most notably, presidential vote share is a turnout response to a particular candidate, whereas our predicted Dem. share measure is a measure of underlying partisan composition - and therefore offers the possibility to, for instance, measure *potential* competitiveness - divorced from any particular candidate or race.

2.3.1.3 Outcome and other data

Finally, our outcome in the analysis in this section is state-by-election year-level turnout. We draw on turnout data from the United States Election Project (McDonald, 2018) for the years 2008, 2010, 2012, and 2014. In these data, a statewide turnout rate is calculated by dividing the total number of votes cast for the highest office on the ballot throughout the state for a given year by the state's voting-age population.[6]

In supplementary analysis presented in the appendix, we take political donations as an outcome to provide an alternative measure of political participation. Using data from the Center for Responsive Politics, we include aggregate donations to general election candidates for US House, Senate, and President or the corresponding committees (e.g., Democratic Senatorial Campaign Committee, Democratic Congressional Campaign Committee, and Democratic National Committee) based on individual donors' locations.

2.3.2 Empirical strategy

Our empirical approach is a continuous treatment difference-in-differences model for the 2008, 2010, 2012, and 2014 elections. Our main estimating model is:

$$Turnout_{sy} = \alpha + \beta_1 PredGerry_{sy} + guber_{sy} + senate_{sy} + \delta_s + \tau_y + \epsilon_{sy},$$

where PredGerry_{sy} is our standard-normalized measure of predicted gerrymandering for state s in election year y; higher values indicate that a state is more gerrymandered, based on the partisan composition of districts. $guber_{sy}$ and $senate_{sy}$ are dummies to indicate whether there is a gubernatorial or senate election in the state-year. δ_s are state fixed effects; τ_y are year fixed effects. Standard errors are clustered at the state-level.

Within a state, PredGerry_{sy} varies only once redistricting has occurred. That is, the measure is constructed based on pre-2012 district maps and 2010 demographics in 2008 and 2010 elections, so the measure is fixed within those years within a state; likewise, the measure is constructed based on post-2012 district maps and 2010 demographics in 2012 and 2014, so the measure is fixed within those years. Thus, 2008 and 2010 form a pre-period in

[6]If we instead use voting-eligible population as the denominator, our results are nearly identical.

our analysis, while 2012 and 2014 represent a post-period. Carrying through the standard difference-in-differences framework, states that experience large changes in the degree of partisan gerrymandering in the 2012 wave of redistricting are our "treatment" group; states that experience smaller changes are our "control" group.

Since we in essence use the shift in gerrymandering that comes with redistricting, we are implicitly running a difference-in-difference model. This means we also need to ensure that states that did and did not become gerrymandered were not trending towards a certain turnout level anyway. In other words, we must test the parallel trends assumption. We do this with a placebo test, where we use data from 2004 through 2010.[7] We assign the correct predicted gerrymandering values for 2004 and 2006 election years, and we assign the post-redistricting 2012 and 2014 predicted gerrymandering values for the 2008 and 2010 election years. We then estimate the model using the true turnout data. Since the 2008 and 2010 election years come prior to redistricting, applying the post-redistricting scores should result in a null provided that the two were not trending differently prior to the change.

2.3.3 Results

Table 12 reports our results from the nationwide analysis of the impact of partisan gerrymandering on turnout. We begin by documenting that our predicted, ex ante measure of gerrymandering indeed predicts actual (ex post) gerrymandering. To do so, we simply estimate the equation described in the previous section, except that we take actual gerrymandering, rather than turnout, as the outcome. Recall that both predicted and actual gerrymandering are standard-normalized. The results in Column 1 imply that a one standard deviation increase in our predicted measure is associated with an 0.289 standard deviation increase in ex post gerrymandering.

We now turn to our main result in Column 2. A one standard deviation increase in predicted gerrymandering measure decreases turnout by roughly 0.78 percentage points—a 1.6% drop.

[7] Testing parallel trends via an event study is not immediately applicable here given our continuous treatment. Moreover, this placebo test captures essentially the same idea. Indeed, a pre-period placebo test of this sort is essentially the test of pre-trends proposed in De Chaisemartin and d'Haultfoeuille (2020).

In Columns 3 and 4, we divide the sample into the presidential election years (2008 and 2012) and the midterm years (2010 and 2014). There we find that our main result is largely driven by changes in turnout in the midterm election years; in those years a one standard deviation increase in gerrymandering reduces turnout by 1.35 percentage points (or 3.5%). In presidential years, the change is both not statistically significant and small in magnitude: a one standard deviation increase in gerrymandering reduces turnout by less than 0.01%. This is intuitive, as turnout is typically driven by the highest election on the ballot.

Column 5 adopts a two-stage least squares framework. Insofar as our predicted gerrymandering measure is essentially an instrument for actual gerrymandering, Columns 2-4 are reduced form analyses. However, Column 1 shows that predicted gerrymandering does not perfectly predict actual gerrymandering, and as such the estimates of Columns 2-4 may underestimate the true magnitude of the effect and should be rescaled accordingly. That is the aim of the 2SLS estimation in Column 5. That specification shows that a one standard deviation increase in actual gerrymandering reduces turnout by 2.4 percentage points or 5%.

Finally, Column 6 validates our difference-in-difference design by showing that states with and without shifts in gerrymandering were not trending differently prior to its occurrence. We show that when we (falsely) apply the predicted gerrymandering measure after gerrymandering occurred (2012-2014) to years before it occurred (2008-2010) but allow the 2004-2006 elections to have their correct values. We estimate a coefficient whose magnitude is close to zero. While the standard error is not small, it is statistically different from the estimates we calculate in Columns (2)-(3). This result gives us confidence that differences in trends across states that were and were not gerrymandered are not driving our results.

In this section, we have documented that gerrymandering depresses one form of political participation: turnout. In Appendix Table A.1, we document that it also has negative effects on another measure of participation: political donations. In particular, we find that a one standard deviation increase in gerrymandering depresses contributions overall by roughly 4% and depresses contributions to House candidates in particular by roughly 6%.

2.4 2018 court-ordered redistricting in Pennsylvania

Next, we take advantage of a natural experiment where Pennsylvania, in 2018, was ordered by its State Supreme Court to redraw its district lines in response to a lawsuit claiming that the state was gerrymandered in favor of Republicans. The redrawn districts were dramatically altered and—in measurable ways—left the state substantially less gerrymandered.

The US House of Representative electoral district lines that prevailed in Pennsylvania from 2012-2018 were drawn by a Republican legislature in 2011 in the normal course of post-Census redistricting. The League of Women Voters of Pennsylvania filed a lawsuit in 2017 alleging that "Republican mapmakers used sophisticated computer modeling techniques, in Pennsylvania and elsewhere, to manipulate district boundaries with surgical precision to maximize the number of seats their party would win in future elections" and that the resulting maps in Pennsylvania violated the state Constitution (Levy, 2017). Based on our efficiency gap calculations, the district lines in place in the 2012 elections generated a value of $Gerry_{sy}$ that is in the top 10% in the country. The case was heard by the Pennsylvania State Supreme Court, which—in late January of 2018—ruled in favor of the plaintiffs, ordering that new House district lines be drawn by mid-February. New maps were in place for the May 15th primary elections and the November general elections.

We draw on data on the universe of registered voters in Pennsylvania and neighboring Ohio and conduct analyses at the individual voter-level. Using a difference-in-differences framework that includes individual voter fixed effects we ask: Did turnout markedly change in Pennsylvania, relative to Ohio, once gerrymandering was removed? In the previous section, we documented that gerrymandering suppresses turnout. In this section, we hypothesize that Pennsylvanians are more likely to turn out in 2018 after gerrymandering was removed.

We add to our nationwide analysis in three ways. First, it is conceivable that either: (1) gerrymandering reduces turnout on a statewide (and not district-specific) level by reducing voters' trust and attachment to the democratic process, even if their own district is not personally impacted or (2) gerrymandering reduces turnout specifically in districts that become less competitive because of it. Our analyses in this section allow us to test for district-specific responses and assess these two possibilities. Second, we can assess the types of voters more or less likely to turn out to better understand *whose* vote is being depressed by gerrymandering. Third, while some argue that ex post efficiency gap measures are limited in detecting gerrymandering (Krasno et al., 2019), the natural experiment presented in Pennsylvania's court-ordered redistricting allows us to estimate the effect of un-gerrymandering on turnout without constructing any measures of gerrymandering itself. We simply use the existence of a court decision to determine how un-gerrymandering affects political participation.

2.4.1 Data

2.4.1.1 Pennsylvania and Ohio voter files

Our main sources of data in this section are the Pennsylvania and Ohio state voter files, obtained from the respective states' Secretary of State offices. These provide our basic outcome variable, indicating whether or not a voter voted in a given election. The data include the rosters of the universe of registered voters in each state as of 2019 (when we obtained the data). For each voter, we can observe some basic demographic information (e.g., gender, age), precise residential location, and registered party affiliation, if voters have registered with a party.

In addition to this basic information, we observe the dates of the five previous general and primary elections in which the voter participated in Ohio, and participation in general and primary elections since 2000 in Pennsylvania. We only observe that a voter cast a ballot on the election dates listed with no information about the parties or candidates they supported. However, in the voter history identifying primary participation, the data do note whether the voter participated in the Republican or Democratic primaries. We use this information to fill in partisan affiliation of voters in Ohio, which has lower rates of party affiliation compared to Pennsylvania.[8] We mark voters as Republican (Democrat) either if they registered as such, or if—in each of their primary appearances—they vote in the

[8] Pennsylvania has a closed primary system, while Ohio has an open system. Thus, far fewer voters in Ohio are registered with a particular party. The ratio of observations in the voter file to voting age population is similar in each state, so it is not clear that the difference in primary systems leads to lower rates of voter registration in Ohio relative to Pennsylvania. Given that neither state changed primary systems during our analysis period, any underlying difference in registration rates due to open versus closed primaries will be accounted for in our difference-in-differences strategy.

Republican (Democratic) primary. This yields rates of party identification similar to that of Pennsylvania.

As is true in most analyses drawing on voter files, we note that these files are a snapshot of registered voters at one point in time (2019, in our case). While we observe past election participation of all voters registered in 2019, we do not observe the participation of voters who were present prior to 2019 but left the state or otherwise became unregistered by 2019. Likewise, the residential location of voters is as of 2019. In our analysis, to assign voters to Congressional districts, we are therefore forced to assume that voters were in the same location in prior elections that we see them in in 2019. This will introduce some noise into our analysis, attenuating our estimates. For these reasons, we restrict our attention to recent elections, specifically, those in 2014, 2016, and 2018, as the issues generated by using data that is a snapshot in time are only magnified in elections that are farther in the past.

2.4.1.2 Measuring congressional district competitiveness

Some analyses in this section are aimed at testing how voters differentially respond to the removal of gerrymandering based on changes in the competitiveness of their own district. Just as we required a pre-election prediction of gerrymandering, we now require a pre-election prediction of how closely contested Congressional races in Pennsylvania and Ohio will be. This measure must be abstracted from the candidates running in those districts, which may be a response to (un)gerrymandering.

To construct a measure of competition, we build from the "predicted Democrat share" measure and construct a measure similar to that of Besley, Persson and Sturm (2010). They define the competitiveness of an election as $-|0.5 - DemocratShare_{jt}|$ for electoral jurisdiction j in year t, where $DemocratShare_{jt}$ is a two-party Democrat share. Thus, an area that is 50% Democratic and 50% Republican is maximally competitive, with competitiveness value of zero under their construction (the maximum possible value). For increasingly uncompetitive jurisdictions, the measure becomes increasingly negative.

We could theoretically implement this measure directly, by inserting predicted Democrat shares in place of $DemocratShare_{jt}$. However, this is not ideal for two reasons. First, the measure ignores potential non-linearity in the relationship between the partisan composition of a district and the expected degree of competitiveness; for example, the measure treats increases from 0% to 5% Democratic and 44% to 49% Democratic as equivalent increases in expected competitiveness. Second, while Jones and Walsh (2018) document that *increases* in predicted Democrat share successfully predict an *increased* likelihood of Democratic victory in US House elections, there is nothing in the construction of the measure to ensure that a predicted Democrat share of 0.5 implies that the district (likely) is comprised of exactly 50% Democrats. In other words, we are confident in the ordering generated by the predicted Democratic share measure but do not claim that levels of any particular point in the measure are accurate.

To solve both of these problems, we take the full sample of US House election results *outside* of Pennsylvania and Ohio from 2012 through 2018. We estimate a nonlinear least-squares specification, with a dummy for Democratic victory as the outcome variable and predicted Democrat share as the explanatory variable. The regression is estimating how our predicted Democrat share measure translates into the probability that a Democratic candidate will win, which is arguably more relevant to a measure of competitiveness. We therefore use the resulting coefficients and the predicted Democrat share values in Pennsylvania and Ohio to construct *predicted probability of Democratic victory* for each district in the two states before and after 2018; call this $Pr(\widehat{\text{DemWin}})_{dy}$ for district d in year y. Because the predicted Democrat share measure only varies when redistricting occurs, both that measure and the resulting probability measure are constant within Ohio districts throughout our sample period, but change in Pennsylvania districts in 2018. Finally, we then construct a measure of predicted competitiveness for these districts as:

$$\widehat{\text{Comp}}_{dy} = -|0.5 - Pr(\widehat{\text{DemWin}})_{dy}|.$$

Like the predicted Democrat share measure, this measure is an *ex ante* measure of competitiveness based only on district composition and abstracted from candidate characteristics, quality, or effort.

2.4.2 Empirical strategy

With the data on voter electoral participation from 2014-2018 described above, we estimate two forms of models. The first leverages the surprise redistricting of Pennsylvania to estimate, within a difference-in-differences framework, how individual-level turnout changes in response to a voter's state becoming less gerrymandered:

$$V_{isdy} = \alpha + \beta_1 PA_s \times Post_y + \beta_2 \text{DInc}_{dy} + \beta_3 \text{RInc}_{dy} + \delta_i + \tau_y + \epsilon_{isdy}$$

The outcome V_{isdy} is a dummy variable equal to one if voter i in state s-district d voted in the general election in year y. β_1 is the coefficient of primary interest, as it estimates how the turnout of Pennsylvania voters changed in 2018 (the year where new districts were in place) relative to any change in turnout of Ohio voters in the same year, where district lines did not change. As the model includes individual voter fixed effects (δ_i) these changes in turnout are interpreted as changes at the individual-voter level and account for any individual time-invariant differences in propensity to vote. The model also controls for whether the presence of a Democratic incumbent, DInc_{dy}, and the presence of a Republican incumbent, $RInc_{dy}$; both of these dummies are equal to zero for districts where neither candidate is an incumbent.[9]

We note that the interaction term $PA_s \times Post_y$ will capture the effects of anything that leads Pennsylvania turnout to differ from that of Ohio in 2018. That includes the change in the drawing of Congressional district lines—our main focus—but may also include other factors. However, we feel that Ohio serves as a reasonable control group for estimating the effects of redrawing lines both due its geographic proximity and demographic similarity, but—perhaps more importantly—because the timing of other important elections coincide in 2018 across the two states. In particular, both had US Senate and gubernatorial elections in 2018.[10]

[9] Note that we define incumbents for the purposes of these controls simply as individuals who were present in the US House in the prior term, and not as individuals who represented the same district they are currently running. The reason for this is that districts in Pennsylvania change substantially in 2018, rendering the latter potential definition irrelevant.

[10] Ohio's gubernatorial election in 2018 featured two candidates running for an open seat, whereas in Pennsylvania, Governor Tom Wolf ran for and won reelection. If this distinction has an effect, we expect that it would be to put downward pressure on the estimate, thereby leading us to underestimate any positive effect of the 2018 redistricting.

One of the main aims of the Pennsylvania-Ohio analysis is to explore how district-specific factors contribute to changes in turnout resulting from changes in the degree of gerrymandering. Specifically, we test the degree to which changes in the competitiveness of districts contribute to changes in turnout. To do so, we use the competitiveness measure to create an additional treatment variable MC_i equal to one if the district that voter i is assigned to in 2018 is more competitive than the district that voter was assigned to prior to 2018. Because Ohio does not experience a redistricting in 2018, $\mathrm{MC}_i = 0$ for all Ohio voters, but equals 1 for roughly two-thirds of Pennsylvania voters. We then estimate a triple-differences model:

$$V_{isdy} = \alpha + \beta_1 PA_s \times Post_y + \beta_2 PA_s \times Post_y \times \mathrm{MC}_i + \beta_3 \mathrm{DInc}_{dy} + \beta_4 \mathrm{RInc}_{dy} + \delta_i + \tau_y + \epsilon_{isdy}$$

In this model β_1 and β_2 are of primary interest. β_1 identifies the increase in turnout in PA in 2018 for voters that did not experience an increase in the competitiveness of their district; β_2 identifies the differential increase for voters that did experience an increase in competitiveness. If there is not a district-specific effect of gerrymandering, and the impacts of gerrymandering that we documented in the previous section occur statewide through a behavioral voter (de)mobilization channel, then we expect that $\beta_1 > 0$ and $\beta_2 = 0$. If, on the other hand, part of the effect of changing the level of gerrymandering is driven by district-specific responses changes in the competitive environment, then we expect $\beta_2 > 0$. If that is the case, it is possible that there is both a statewide effect and a district-specific effect ($\beta_1 > 0$ and $\beta_2 > 0$), but it is also possible that any effect of un-gerrymandering is entirely explained by the district-specific competitive effect ($\beta_1 = 0$ and $\beta_2 > 0$).[11]

2.4.3 Results

2.4.3.1 Is Pennsylvania less gerrymandered in 2018?

Since we use the 2018 redistricting in Pennsylvania to study the impacts of reducing gerrymandering, it is important to document that the "treatment" worked. Was Pennsylvania in fact less partisan gerrymandered in 2018 than it was in 2014 and 2016? We discuss

[11] Our results remain comparable if we control for demographic characteristics at the district-by-post level, including the share of owner-occupied housing, vacant housing, urban, female, over 18 years of age, Hispanic, Black, Native American, Asian/Pacific Islander, and other race, as well as population. These results are in Appendix Table A.4.

three pieces of convincing evidence that indeed the redrawn boundaries reduced partisan gerrymandering.

First, Cervas and Grofman (2020) compare the 2018 Pennsylvania map to the pre-2018 Pennsylvania map using multiple metrics of partisan gerrymandering, including the efficiency gap. They show that the 2018 map is less gerrymandered regardless of the metric used. In particular, the efficiency gap falls from 0.24 under the 2011-drawn map to 0.08 under the 2018-drawn map.

Second, both academic work and legal arguments have pointed to geographic compactness (or lack thereof) as an indicator of gerrymandering (See, for example, Niemi et al. (1990) and Polsby and Popper (1991)). Kaufman, King and Komisarchik (2017) propose a method for quantifying compactness; using their code, we calculated the average compactness of Pennsylvania districts before and after 2018, as well as Ohio during the same time period. We find that the average Pennsylvania and Ohio districts are similarly compact prior to 2018, but Pennsylvania districts become 34% more compact on average in 2018.

Third, we propose a novel test of gerrymandering. Details are provided in the appendix, but in short, we estimate spatial regression discontinuity models, testing whether there are discontinuities at district boundaries in the partisan composition of Census block groups in crossing from Republican-leaning districts to Democratic-leaning districts. We argue that the systematic presence of such discontinuities within a state would be evidence of "packing," wherein partisan gerrymanderers carefully draw some districts lines to add as many members of one party as possible to a district that already leans in favor of that party, thereby "wasting" those votes. Our results (Appendix Figure A.4 and Appendix Table A.2) reveal that, in Pennsylvania before 2018, the average block group barely within a Democratic-leaning district is 6 percentage points more Democratic than a block group barely on the Republican-leaning side. In the district lines in 2018 in Pennsylvania, there is no significant jump at district lines in the partisan composition of block groups.

2.4.3.2 Impacts on turnout

We now report our main results of this section in Table 13. The odd-numbered columns report the results from our difference-in-difference model, testing whether there is an overall statewide effect on turnout in Pennsylvania resulting from reducing partisan gerrymandering. In Column 1, which includes elections in 2014, 2016, and 2018, we see that voters are 3.4 percentage points more likely to turn out to vote in Pennsylvania in 2018; barring other factors driving higher turnout in Pennsylvania in that year, we interpret this as a strongly suggestive positive effect on turnout of reducing the degree of partisan gerrymandering within the state. Column 3 drops the 2014 election, as elections farther in the past are more prone to noise driven by voters who have moved since the observed election. Doing so implies that we are comparing a presidential election (2016) to a midterm election (2018). In Column 5 we compare only midterm elections (2014 and 2018). Both reveal a positive effect on turnout, albeit of different magnitudes (four versus two percentage points).

What drives the increased turnout? The even-numbered columns report the results from our triple-differences model, testing whether the degree to which the statewide results are in fact driven by the two-thirds of voters who experienced an increase in the competitiveness of their districts. Across Columns 2, 4, and 6, we see that changes in district competitiveness are an important part of the story, with PA residents who experienced an increase roughly two percentage points more likely *than other Pennsylvanians* to turn out to vote in 2018. As the $Post \times PA$ coefficient differences out any general differences driving higher turnout in 2018 in PA, we are confident in interpreting these results as more than merely suggestive.

Based on Columns 2 and 4, voters in districts that *did not* increase in competitiveness were also more likely to turnout, consistent with voter mobilization broadly combined with district/contest-specific effects; however, Column 6 (restricting the sample to the two midterm elections) is more consistent with an explanation where statewide increases in turnout are entirely explained by changes in district-specific factors. We can push that comparison further, by comparing the estimates from the odd and even columns. In doing so, our results imply that the statewide increase in turnout in 2018 is between roughly 75% to 90% explained by voters who experienced an increase in the competitiveness of their districts.[12] Therefore, regardless of the coefficients on "Post × PA" in Columns 2 and 4, it is clear across specifications that changes in competitiveness are not just an important part of the story, but that they in fact largely drive the results.

In Columns 7 and 8, we report the results of a placebo specification. We restrict to the 2014 and 2016 election years and assume that the 2018 lines were in fact in place in 2016. Another way to think about this placebo experiment is that we replicate our models, lagging our outcome measure by one election cycle. As before, this is a test of the parallel trends prior to treatment. The results reveal no statistically or economically significant effects, suggesting that trends prior to the redrawing of maps in 2018 do not drive our main results.

To provide further evidence of the robustness of our results and to ensure that we are comparing "like" voters, we repeat the same specifications in Pennsylvania, but restricting the sample to voters who are within 2 kilometers of a post-2018 district boundary. In doing so, we can no longer identify the "Post × PA" coefficient, but we can identify the differential "Post × PA × MC." effect. Results are reported in Table 15, Panel A, and reveal a *larger* effect of a redistricting-driven increase in district competitiveness on the likelihood of turning out.[13]

In Table 14 and in corresponding analysis for voters within 2km of district boundaries in additional panels of Table 15, we probe how different types of voters differentially respond to the redrawn district lines. Part of our motivation is to assess how gerrymandering impacts the turnout of voters typically thought to be less politically active, so we pay particular attention here to voters who are not affiliated with a particular party and also younger voters.

Other than estimating on subsamples of voters, all specifications in the table match Columns 1 and 2 of Table 13. In Panel A of Table 14, we show that unaffiliated/third-party voters are perhaps the most responsive, with strong responses both statewide and specifically

[12]Since roughly two-thirds of voters in PA experienced an increase in competitiveness, the overall effect is a weighted average of the effects on non-MC voters ($Post \times PA$) and the MC voters ($Post \times PA + Post \times PA \times MC$). For example, in Column 2, the overall effect is $\frac{1}{3} * 0.021 + \frac{2}{3} * (0.021 + 0.02) = 0.034$, which coincides with the overall effect. Therefore, the fraction of the effect driven by the "more competitive" voters is $\frac{2}{3} * (0.021 + 0.02)/0.034 = 0.796$ or 79.6%. Columns 3-4 generate a percentage roughly equal to 75%; Columns 5-6 generate a percentage roughly equal to 90%.

[13]Appendix Table A.3 documents that results are very similar taking other distance thresholds from the boundary.

in districts that became more competitive. Amongst voters registered with one of the two major parties, only Republicans increase their turnout overall and only Republicans increase their turnout specifically in response to changes in the competitiveness of their districts. This may make sense, as the redrawing was meant to undo a Republican-leaning gerrymander. However, in considering voters close to district boundaries only (Table 15, Panel B), we note that *both* Republicans and Democrats are more likely to turn out in districts that have become more competitive.

In Panel B of Table 14, we split the sample into three roughly equally sized groups of voters based on their year of birth. Paralleling the result wherein unaffiliated voters show the strongest response to reducing gerrymandering, Panel B shows that the younger voters in our sample (those who were under 40 in 2014) show stronger responses than other age groups. Amongst that group, voters in districts that did not become more competitive are four percentage points more likely to turnout, and voters in districts that *did* become more competitive are seven percentage points more likely. Both estimates are statistically significant. This is in stark contrast to the oldest group of voters (over 57) who turn out at higher rates generally but are no more likely to turnout in PA in 2018 in districts that did not become more competitive and 1.5 percentage point more likely to turnout in districts that did (significant at only the 10% level). Panel C of Table 15 reveals a similar pattern of effects by age for voters close to district boundaries.

2.5 Conclusions

This paper uses two methods to document that partisan gerrymandering reduces voter engagement in elections.

First, we show that being in a state with relatively more gerrymandering reduces turnout in a nationwide state-panel approach. We build upon work by Jones and Walsh (2018) and use a measure of the predicted gerrymandering to show that a one standard deviation unit increase in gerrymandering reduces turnout by roughly 1.6%.

Second, we show that in Pennsylvania—where courts ordered US House boundaries to be redrawn to removing partisan gerrymandering—participation increased after the boundaries were un-gerrymandered. These effects are concentrated among those whose own-district became more competitive, as opposed to the state change as a whole. Recent work by Moskowitz and Schneer (2019) indicates that district competitiveness in general has little influence on turnout, but our results suggest that while that may be true on average, it is not universally true. The results in Pennsylvania show that there may be heterogeneity in the effect of competitiveness on turnout—perhaps due to gerrymandering itself.

We also document larger effects of gerrymandering on turnout for Independent or Third-party registered voters and young voters. Given that voting can be habitual (Meredith, 2009; Coppock and Green, 2016), reducing gerrymandering may have an unintended consequence of engaging young voters in the long-run.

2.6 Tables and figures

Table 12: Nationwide impact of partisan gerrymandering, as measured by the efficiency gap, on state-by-year turnout (2008-2014)

	(1) Gerry. (std. norm.)	(2) Turnout (VAP)	(3) Turnout (VAP)	(4) Turnout (VAP)	(5) Turnout (VAP): 2SLS	(6) Turnout PLACEBO
Outcome mean	0	47.3	37.7	56.6	47.3	48.5
Pred. Gerry.	0.289**	-0.777**	-1.347***	-0.027		-0.017
(std. norm.)	(0.108)	(0.304)	(0.358)	(0.215)		(0.266)
Gerry.					-2.383**	
(std. norm.)					(0.965)	
Sample	Full	Full	Midterms	Pres. Years	Full	2004-2010
Observations	171	172	86	86	171	171
R-squared	0.500	0.955	0.922	0.985	0.901	0.960

Notes: All specifications include state and year FEs, as well as dummies to indicate Senate and Gubernatorial elections occurring in the same state-by-year. Turnout (VAP) is the turnout percentage amongst the voting-age population. The placebo specification is a test of pre-trends: we pull data from 2004–2010 and apply the predicted gerrymandering score—the correct score for those years—for 2004–2006; we then falsely apply the 2012–2014 "post" score to the 2008–2010 elections (before gerrymandering occurred). Robust standard errors (clustered at state-level) in parentheses *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 13: Shift in turnout in PA & OH following 2018 court-ordered redistricting to eliminate partisan gerrymandering in PA, individual voter-by-election level

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	2014-2018		2016&2018		2014&2018		PLACEBO 2014 &2016	
Post X PA	0.034***	0.021***	0.041***	0.030***	0.019***	0.003	-0.004	-0.006
	(0.005)	(0.006)	(0.005)	(0.005)	(0.007)	(0.008)	(0.006)	(0.007)
Post X PA X More Comp.		0.020***		0.017***		0.023***		0.003
		(0.006)		(0.006)		(0.008)		(0.007)
Observations	34,216,515	34,046,910	23,384,368	23,271,298	21,664,294	21,551,224	21,664,294	21,551,224
R-squared	0.679	0.679	0.780	0.780	0.742	0.742	0.738	0.738

Notes: All specifications include individual voter FEs, year FEs, and dummies to indicate incumbency status of the Democratic and Republican candidates. In Columns 1-6, Post= 1 in 2018. In Columns 7-8, Post= 1 in 2016. In Columns (7)-(8) we falsely assign "Post" status to 2016. Robust standard errors (clustered at district-pair level) in parentheses ** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 14: Heterogeneity in shift in turnout in PA & OH following 2018 court-ordered redistricting to eliminate partisan gerrymandering in PA, individual voter-by-election level

PANEL A: Split by party

	Republican		Democrat		Other Party/Unaffiliated	
	(1)	(2)	(3)	(4)	(5)	(6)
Post X PA	0.017***	0.014***	0.004	-0.017**	0.048***	0.031***
	(0.003)	(0.003)	(0.008)	(0.008)	(0.007)	(0.007)
Post X PA X		0.005		0.031***		0.027***
More Comp.		(0.004)		(0.009)		(0.008)
Observations	12,505,855	12,449,203	12,696,267	12,606,369	9,014,393	8,991,338
R-squared	0.637	0.637	0.664	0.664	0.660	0.660

PANEL B: Split by age

	YOB< 1957		YOB 1957—1974		YOB> 1974	
	(1)	(2)	(3)	(4)	(5)	(6)
Post X PA	0.012***	0.005	0.017***	0.005	0.063***	0.042***
	(0.003)	(0.005)	(0.005)	(0.006)	(0.008)	(0.008)
Post X PA X		0.010*		0.019***		0.032***
More Comp.		(0.005)		(0.006)		(0.009)
Observations	11,061,256	11,022,133	11,551,280	11,500,685	11,603,958	11,524,071
R-squared	0.660	0.660	0.661	0.661	0.654	0.654

Notes: All specifications include individual voter FEs, year FEs, and dummies to indicate incumbency status of the Democratic and Republican candidates. YOB is year of birth. Party affiliation is based on party registration in Pennsylvania (where primaries are closed) and based on a combination of party registration and primary participation in Ohio (where primaries are open). Robust standard errors (clustered at district-pair level) in parentheses ** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 15: Shift in turnout in PA amongst voters within 2 km of post-2018 district boundary lines, individual voter-by-election level

PANEL A: Main analysis

	2014-2018	2016&2018	2014&2018
	(1)	(2)	(3)
Post X More. Comp.	0.035***	0.022**	0.043***
	(0.009)	(0.010)	(0.009)
Observations	3,649,554	. 2,433,036	2,433,036
R-squared	0.660	0.764	0.729

PANEL B: Split by party

	GOP	Dem	Other/Unaff.
	(1)	(2)	(3)
Post X More. Comp.	0.018***	0.046***	0.035***
	(0.005)	(0.013)	(0.010)
Observations	1,099,524	2,144,610	405,420
R-squared	0.647	0.658	0.657

PANEL C: Split by age

	YOB< 1957	YOB 1957—1974	YOB> 1974
	(1)	(2)	(3)
Post X More. Comp.	0.029***	0.039***	0.048***
	(0.009)	(0.011)	(0.015)
Observations	1,160,154	1,278,288	1,211,112
R-squared	0.657	0.649	0.633

Notes: All specifications include individual voter FEs, year FEs, and dummies to indicate incumbency status of the Democratic and Republican candidates. YOB is year of birth. Party affiliation is based on party registration in Pennsylvania (where primaries are closed) and based on a combination of party registration and primary participation in Ohio (where primaries are open). Robust standard errors (clustered at district-pair level) in parentheses ** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

3.0 Resettlement Agency Resource Strain and Refugee Outcomes: Evidence from Catholic Sex Abuse Scandals

Work presented in this chapter is the result of research conducted with co-authors K. Pun Winichakul of Smith College and Ning Zhang of the University of Oxford.

3.1 Introduction

Refugee resettlement requires the coordination of multiple government and non-governmental agencies. In the United States alone, such institutions have partnered to resettle over 3 million individuals since 1980 (Phillip Connor and Jens Manuel Krogstad, 2018). Government agencies such as the Departments of State, Homeland Security, and Health and Human Services, are central to the management of many steps in the resettlement process. They are responsible for duties such as identifying individuals in precarious situations around the world, screening applications, and providing financial assistance programs to support refugee families during their initial few months in the United States.

The policies and programs overseen by government agencies contribute to the well-being of refugee families. In the U.S., access to larger amounts of cash assistance through programs such as Temporary Assistance for Needy Families has been shown to improve refugee wages (LoPalo, 2019). Beyond financial resources, resettlement in locations with established social networks have also been shown to play a central role in refugee labor market outcomes (Beaman, 2012). Outside of the U.S., research has noted how government resettlement policies impact refugee adults in the labor market and refugee children performance in school (Edin, Fredriksson and Åslund, 2004; Äslund et al., 2011).

At the same time, governments rely on several key non-governmental partner agencies, who offer local knowledge and resources to aid migrant families as they acclimate to their new homes. Partner resettlement organizations, otherwise known as voluntary agencies (VOLAGs), provide a wide range of services, including, "employment, home management, crisis intervention, financial literacy and medical assistance" for an extended period beyond a refugee's first few months in their new home (Catholic Charities Atlanta, 2019).

This reliance on local community support has increased in recent years. Large scale refugee crises stemming from the U.S. withdrawal from Afghanistan and the 2022 Russian invasion of Ukraine prompted the Biden administration and the U.S. Department of State to develop a private sponsorship program for resettling refugees in the United States; allowing small groups of individuals to directly sponsor incoming evacuees by providing services including "securing housing, providing for basic necessities (such as clothing, groceries, household furnishings), assisting with access to federal, state, and local benefits, and providing initial orientation to the local community."[1]. The U.S. Department of State has noted that this increased emphasis on direct sponsorship is boldest innovation in refugee resettlement in four decades.

Although a wide network of organizations could strengthen the support structure available to refugees, over-reliance on partner organizations could also leave vulnerable communities exposed to volatility in the financial health of such institutions. In other words, unexpected resource shocks to VOLAGs could lead to cuts in the quality of the human services they can provide.

In this paper, we explore the impact of non-governmental organizations on refugee economic outcomes. In particular, we examine the effect of resource and reputation shocks to VOLAGs on refugee employment, earnings, and federal social safety net program participation. While past work in economics has emphasized how government resource generosity shapes refugee outcomes (e.g., LoPalo, 2019), we complement this work by exploring the effect of VOLAG aid on refugee well-being in the United States.

We first consider factors that impact the availability of VOLAG resources for refugee resettlement. We focus on the resources of the largest VOLAG, the United States Conference of Catholic Bishops (USCCB), an organization that has resettled nearly one-third of all refugees over the past four decades (United States Conference of Catholic Bishops, 2019). During the same time period, USCCB and its affiliates have experienced turbulent financial and social support. Allegations of sexual abuse have emerged in Catholic institutions around

[1] Blinken (2021)

the country. The scandals have crippled the Church financially, with local Church jurisdictions paying millions of dollars in damages and some dioceses filing for bankruptcy (Tom Gjelten, 2018). Beyond the direct finances of the Catholic Church, Church-affiliated charities have also experienced large declines in private contributions and participation (David Crary, 2019; Nicolas L. Bottan and Ricardo Perez-Truglia, 2015). For our work, we use data from Bishop Accountability, an organization that tracks scandal revelations, to identify the timing and location of new public disclosures (, 2019).

Next, we construct a novel dataset of refugees from the American Community Survey (ACS) between 2000-2020. We identify likely refugees by selecting observations from the ACS whose observable characteristics are found in individual refugee data from Dreher et al. (2020) for refugee arrivals from 1994 to 2008. Our refugee-identification method allows us to assign participants to locations at the county (and diocese) level based on location information provided in the individual refugee data. This fundamentally improves the identification of refugee location relative to prior work, which often relies on the strategy first used by Capps et al. (2015) and employed by Evans and Fitzgerald (2017). This method is commonly applied to data from the American Community Survey but when studying outcomes at the state level or larger geographic specificity.

We combine these data sources to examine the effect of non-governmental aid on refugees in the United States. In particular, we use variation in where *and* when refugees were resettled in the U.S. relative to the timing of new scandal revelations in dioceses to explore differences in refugee outcomes. In other words, our identification strategy compares refugees who were likely exposed to varying intensities of media coverage and shifts in community support toward Catholic institutions as a result of new scandal revelations. As USCCB remains the largest partner non-government resettlement agency, the revelation of sexual abuse scandals in the Church and the consequences resulting from these events may have affected refugees who rely on the organization's resources.

Our results suggest that new abuse allegations in dioceses, and the resulting reputation and resource shocks in the years preceding a refugee's arrival in the United States, lead to a decrease in federal social safety net program participation by refugees. In particular, we find that more allegations in a diocese reduce refugee participation in the Supplemental Nutrition

Assistance Program (SNAP), and decrease total federal welfare program participation. Lower public welfare take-up is not likely driven by improvements in labor market outcomes—we observe suggestive evidence that refugees exposed to a greater number of local Church abuse scandals are less likely to be employed, in the years immediate after arrival.

Our interpretation of the negative effects experienced by refugees depends both on how VOLAG resettlement behavior may have changed, and what types of resources the Catholic Church lost, in response to new abuse scandal revelations. First, we use administrative data from the Office of Refugee Resettlement's (ORR) Matching Grant program with information on a subset of refugees and their assigned VOLAG, and find that the relative share of refugees resettled by USCCB falls as a consequence of scandal revelations. Yet, potential quality differences across VOLAGs does not appear to explain the results, as we do not find average differences in refugee outcomes between USCCB and other VOLAGs that are consistent with our directional effects. Instead, we argue that the worsened outcomes for refugees are more likely a function of changes to the support structures within USCCB. It is worth noting, however, that the Matching Grant program data are only available for years outside of refugee arrival years in our main sample.

Next, we evaluate what types of resources the Catholic Church lost as a result of new abuse scandal revelations. Past work has documented how Catholic school enrollment and individual religious affiliation dropped precipitously as a consequence of the scandals (Hungerman, 2013; Dills and Hernández-Julián, 2012). Researchers have also highlighted how charitable contributions to Catholic-affiliated nonprofit organizations decrease in areas with greater scandals (Bottan and Perez-Truglia, 2015). In addition to these findings, we also use the Current Population Survey (CPS) September Volunteering and Civic Life Supplement Data (2010-2015) to show that scandal revelations have a significant negative impact on the amount of volunteer assistance individuals dedicate to refugee and immigrants. These outcomes represent a range of forms of support, from changes individual belief systems to volunteer service time to monetary resources. While our analysis does not identify whether a particular form of support is responsible for the decline in refugee, the collective impact felt across resource channels shows a comprehensive reduction in the ways in which the Catholic Church may be able to support refugees.

Altogether, our findings suggest that non-governmental aid – in the context of refugee resettlement – may be a complement to public welfare programs. Relying significantly on partner organizations may have the benefit of leveraging local labor market knowledge and support, but also leaves refugees exposed to fluctuations in the organizations' financial health. In circumstances where non-governmental resources diminish, new arrivals may have greater difficulty settling into their new surroundings, and navigating unfamiliar processes like applying for public welfare resources available to them.

This paper contributes to multiple strands of literature. Our primary contribution is to work that highlights key factors shaping refugee outcomes in the United States. Economists with administrative data have explored the role that social networks have on refugee labor market outcomes Beaman (2012). But due to the lack of widely-available data tracking refugees and their economic outcomes, prior empirical work on refugees in the United States is relatively limited. Without administrative data, researchers have had to develop methods to identify "likely" refugees (e.g., Capps et al., 2015). With the likely refugee classification, scholars have compared the earnings profile of refugees to other immigrants and documented characteristics associated with refugee long-term outcomes (Connor, 2010; Evans and Fitzgerald, 2017). Other work has focused on the impact of public resources and resettlement policies on refugee economic outcomes, in the United States and in Sweden (LoPalo, 2019; Edin, Fredriksson and Åslund, 2004; Äslund et al., 2011).[2] Building on past work, this paper introduces a new method of identifying refugees in Census data, using recently-available administrative information from the Office of Refugee Resettlement (Dreher et al., 2020). We then use this new method to examine the impact of VOLAGs on refugee outcomes in the United States, highlighting the partnering role of these organizations to public institutions (LoPalo, 2019).

This paper also adds to our understanding of the consequences of the Catholic Church abuse scandal revelations in the United States. As noted above, researchers have found that the negative attention created by these scandals led to a shift in religious affiliations and the general religiosity of the populace (Hungerman, 2013; Bottan and Perez-Truglia, 2015).

[2]There is additional literature that has studied similar questions but with respect to the broader immigrant population (Borjas and Trejo, 1991; Hansen and Lofstrom, 2003).

The Catholic Church also saw significant reductions in both financial and non-financial support, as measured by factors such as Catholic school enrollment and charitable contributions (Dills and Hernández-Julián, 2012; Bottan and Perez-Truglia, 2015). Beyond the Church, the scandals also changed individual preferences toward, and participation in, public social services, as well as voting behavior (Dills and Hernández-Julián, 2014). This paper introduces a novel consequence of the turbulence that has roiled the Catholic Church over the past few decades, showing how the scandals have affected a vulnerable population that relies on the Church for support.

The rest of the paper proceeds as follows. Section 2 describes the refugee resettlement process in the United States. It also includes a summary of government social programs available to refugee families. Section 3 reviews the main datasets used in the analysis. Section 4 outlines the empirical strategy, and Section 5 discusses the results. Section 6 discusses the mechanisms underlying our results, Section 7 highlights our robustness checks, and Section 8 concludes.

3.2 Background

3.2.1 Resettlement process

Refugee resettlement comprises a sizable component of immigration to the US. Between 1996 and 2019, refugees and asylum seekers accounted for approximately 12.5% of new lawful permanent residents annually.[3] Refugees, while enjoying high levels of human capital on average, arrive in the US with few resources (Capps et al., 2015). To handle the large logistical issues associated with settling and providing support for incoming refugees, the United States Office of Refugee Resettlement partners primarily with nine large nonprofit organizations to assign refugees to locations across the US and provide local support after arrival. A list of these nine organizations is provided in Appendix Table A.1.

Potential incoming refugees are referred to the United States through the United Na-

[3]Estimate produced from Yearbook of Immigration Statistics, 1996-2019.

tions High Commissioner for Refugees (UNHCR). After several rounds of screenings and background checks, a request for sponsorship assurance is sent to the Department of State's Refugee Processing Center, which coordinates with the nine resettlement agencies. Cases are assigned to agencies in a round-robin fashion according to predefined percentages related to each organization's capacity (Ahani et al., 2021; LoPalo, 2019). Although the majority of VOLAGs are religiously affiliated, the faith of refugees is not a factor in VOLAG assignment and refugees do not have a mechanism for expressing preference for resettlement organization or eventual location placement. However, though those with family in the US are likely to be placed nearby. Those without family in the United States are assigned by resettlement agencies to their local affiliates based on factors related language match, employment opportunities, and local rental market. Our empirical strategy rests in part on the assumption that, controlling for refugee characteristics and local conditions related to the above factors, assignment decisions do not select on individual refugee characteristics in response to new scandal revelations. A similar strategy is used in LoPalo (2019) to identify the effects of state-level welfare generosity on refugee labor market outcomes. In our context, it may also be the case that USCCB independently responds to abuse allegations by shifting placement of refugees assigned to them to locations less affected by scandal. We investigate this possibility in Section 5 using administrative data from the Office of Refugee Resettlement's Matching Grant program obtained via Freedom of Information Act Request.

3.2.2 Local affiliate offices and resettlement support

Upon arrival in their placement location, the first point of contact for refugees are case workers assigned by local offices affiliated with the settling VOLAG. As a part of the Reception and Placement program (R&P), VOLAGs are granted $2,175 per arrival to offset initial expenses such as housing, initial food purchases, and job placement services. Longer term funding may be provided through additional state grants or by private donations.

Local resettlement offices provide critical continuing support in the form of active case management. Local case workers play an role in connecting new arrivals with local resources. For example, caseworkers coordinate services provided through local donations and volunteers (Miller, 2022), including employment and housing assistance, mental health, youth and senior services, English classes, technology, and student programs. Local offices also rely on volunteers to serve as "cultural guides" to help navigate unfamiliar elements of life in the United States (Siddiq and Rosenberg, 2021; Wilson and Rodriguez, 2019).

Another key role of case workers is to connect new arrivals with public assistance. Case workers report that clients often experience difficulty navigating the public assistance infrastructure because of its complexity, sometimes exacerbated by lack of English proficiency (Siddiq and Rosenberg, 2021; Wilson and Rodriguez, 2019). Refugees with families are eligible for Temporary Assistance to Needy Families (TANF) for the first five years after arrival. Those meeting income requirements for TANF but without minor children are eligible for Refugee Cash Assistance (RCA) for the first eight months after arrival. Despite their non-citizen status, refugees who meet eligibility requirements may also access Supplemental Security Income (SSI), Medicaid, and the Supplemental Nutrition Assistance Program (SNAP). Take-up of public assistance programs is high, particularly in the first several months after arrival. Within the first year of residence in the US, as many as 40.3% of refugee households receive TANF benefits while 24.9% receive RCA funds (Office of Refugee Resettlement, 2015), but public assistance usage drops sharply after the first year.

Resettlement offices rely heavily on volunteers to supplement case management services. In the authors' discussions with local resettlement offices, staff reported using volunteers to alleviate overburdened caseworkers, providing rides, introducing community services, help signing up for public benefits, help connect clients to employment, and other services. In a case study of UCCSB resettlement in Oregon, Yarris, Garcia-Millan and Schmidt-Murillo (2020) reports using volunteers intensely after budget cuts forced staff reduction. While relying on volunteers can have the advantage of attaching refugees to the local community and developing wider networks, volunteers' commitment can be volatile and subject to burnout (Behnia, 2007; Bennett and Barkensjo, 2005). McAllum (2018) notes that the level of commitment of refugee service volunteers appears to be moderated by whether they had strong ties to the organization and the presence of a volunteer coordinator in the local resettlement office (McAllum, 2018). In light of the fact that refugee resettlement relies heavily on local support, and that the United Council of Catholic Bishops settles roughly one third of all refugees, we ask whether scandals have downstream impact on refugee outcomes.

3.3 Data

3.3.1 Constructing scandal measure

We use data from Bishop Accountability, an organization that collects information on allegations of sexual abuse in the Catholic Church to construct our scandal measure (, 2019). The group's website includes information on media reporting related to an accusation, and tracks the timing of media revelations and the diocese where the alleged abuse occurred.

When constructing our scandal measure, we extract the year that a revelation is first revealed and which dioceses are affected by that revelation.[4] Following Bottan and Perez-Truglia (2015), we include two types of scandals. The first type of scandal (Type A) is one where the accused is a current clergy member. The alleged abuse may have occurred in a different diocese or the one where the priest is currently employed; regardless, we define the location as the clergy member's current diocese. The second type of scandal (Type B) is one where the alleged abuse occurred in the past, and the priest may or may not be active. We define the location of these allegations as where the abuse occurred. Note that a new allegation (and an accused priest) may have multiple scandals associated with them. In such cases, we review the associated media and consider the incident as affecting both locations *only if* there are local media outlets in both dioceses covering it. Our final scandal data set covers 4,278 scandals (1,634 Type A; 2,645 Type B) from 1988 to 2014.

3.3.2 Identifying refugees

Our question of interest requires a representative refugee sample with information on individual demographics, location (e.g., counties/dioceses), year of immigration, and other economic outcomes. The American Community Survey (ACS; 2000-2020) satisfies almost all

[4]We focus on the timing of the media disclosure, rather than the timing of when the alleged abuse occurred, as we are interested in the effect of public knowledge of these incidents rather than the incidents themselves.

the requirements mentioned above. However, there are two concerns associated with using ACS data. First, the data set does not include refugee identifiers; that is, we cannot directly identify a refugee from the ACS. Second, only a subset of observations in the public-use ACS data include county identifiers, a necessary variable for matching individuals to related dioceses.

To overcome these obstacles, we take advantage of individual-level Refugee Resettlement Data (1975-2008) from Dreher et al. (2020). The data cover 2.5 million refugees from 121 origin countries that entered the U.S. between 1975 and 2008. For each individual, the data also contains identifiers for the state and county where they were resettled, as well as rich information on individual demographics. Since the Refugee Resettlement Data does not include the labor market outcomes of the refugees, we identify likely refugees in the ACS using common variables that appear in both data sets. In the first step of this process, we identify all immigrants from the ACS data by the country of birth. Next, we assign observations in the ACS to cells using demographic variables available in the Refugee Resettlement Data. The core characteristics used in this process are year of immigration, country of origin, birth year, gender, and location (state). Beyond these core variables, we leverage additional individual characteristics when available in either data set. For example, because birth quarter information is available for the ACS after 2004, we use this information to assign cells for the 2005 ACS following waves. Additionally, because the Refugee Resettlement Data has education and marital status for refugees that entered the U.S. between the years 1994 and 2001, we also use education level and marital status when considering ACS observations for individuals who arrived between 1994 and 2001.

From our methodology, we obtain 42,055 observations from the ACS that share characteristics with at least one observation in the Refugee Resettlement Data. Of observations in this sample that were collected in the 2005 ACS or later, when we have the birth quarter variable, 76% of the observations share common characteristics with a single observation from the Refugee Resettlement Data. When birth quarter is not provided in ACS waves prior to 2004, we find 36% of observations share characteristics with a single observation in the Refugee Resettlement Data.

The remaining observations from the ACS share characteristics with multiple individuals from the Refugee Resettlement Data. Given that our treatment is defined at the diocese level, a primary concern with these observations is that the resettlement locations of the multiple individuals from the Refugee Resettlement Data who matched may be different. This potentially affects our ability to accurately determine where refugees were resettled at the diocese level. To address this concern, in the third step we rely on the empirical distribution to randomly draw one observation from the multiple observations in the Refugee Resettlement Data that share the same characteristics. We assign resettlement location from that one particular observation to the ACS sample. In Section 3.7, we also run 500 simulations where we repeat the random draw creating distributions for our estimated effects under different potential resettlement counties. We show from this simulation exercise that our estimated effects on public assistance and employment outcomes are robust to different potential matched samples, while our estimated effects on earnings and hours worked are not.

With our methodology, we are left with 42,055 observations from the ACS. We plot the distribution of age, sex, continents, and country of origin for the likely refugees identified from ACS data and compare the distributions to the original ORR data of refugee populations entering the U.S. between 1991 and 2008. We show that the refugee sample we identify from the ACS data is representative of the overall refugee population in terms of age, sex, and country of origin. However in terms of country of origin, the ACS refugee sample has relatively more individuals coming from Cuba (listed as part of North America continent category) and fewer refugees coming from other countries (listed under other continents).

We believe our approach, which uses a wide array of individual characteristics and the newly released micro-level refugee population data to identify refugees in the ACS, provides an improvement over the existing method used in the literature (Capps and Newland, 2015; Evans and Fitzgerald, 2017; LoPalo, 2019). Specifically, to identify samples in the ACS as "likely" refugees, the current method commonly employed by researchers relies on the aggregate number of refugees as a share of total immigrants by country of origin and year of immigration (refugee concentration ratio, RCR) from the Department of Homeland Security's Yearbook of Immigration Statistics. The existing method classifies individuals from ACS as likely refugees if they belong to a country-year pair with an RCR of 0.7 or greater. This method is good enough to capture a reasonable share of refugees from the ACS data.

However, using the RCR ratio will certainly overestimate refugees from the country-year pair with an RCR of 0.7 or greater and underestimate refugees from the country-year pair with an RCR less than 0.7. Formally, there are two errors in identifying refugees from the existing method: one generates false positives, while the other generates false negatives. A false positive match means that an individual from the ACS is identified as a refugee when he/she actually is not; a false negative error refers to an individual who is a refugee but is not identified in the data. The existing method is subject to both errors: for the immigrants from a country-year pair with an RCR of 0.7 or greater, a share of samples between 0-0.3 (with an RCR range between 1-0.7) will be subject to false positive errors; while for the immigrants from a country-pair with an RCR ratio less than 0.7, a share of samples between 0-0.7 (with an RCR range between 0-0.7) will subject to the false negative error. One might be particularly concerned that the false negative error possible in the existing method impacts a meaningful share of refugees, as it applies to any country-year pair with an RCR less than 0.7.

In contrast, we improve on the identification of refugees in ACS data by using individual-level characteristics. At the same time, some immigrants non-refugees in the ACS data could share the same attributes with observations in the ORR data, which would lead our method could wrongly classify that individual as a refugee when actually he/she is not. Thus our method is also subject to the false positive error. In fact, because we are not restricting the sample to immigrants whose country of origin and year of arrival pair as an RCR greater than 0.7, we assign refugee status to some individuals who have low unconditional probability of being refuges. This may increase the risk of false positives relative using the RCR alone. Table 17 repeats our main results on a sample created by our method as well as restricting to RCR thresholds.

Nevertheless, the ORR data covers the universe of refugees entering the US, so refugees sampled in the ACS should also be represented in the ORR data. For this reason, our method is unlikely to suffer substantially from false negative errors, significantly improving the identification of refugees from the ACS data for those refugees who immigrated between 1991 and 2008.

We also improve on the identification of refugees in ACS data by generating county identifiers for our refugee observations. Existing literature mostly focuses on refugee outcomes at the state level (e.g., LoPalo, 2019). Our method identifies refugees at a more granular geographical level. For our work, it allows us to assign refugees to counties (and associated dioceses), and examine the effect of scandal revelations at the diocese level. Because the micro-level ORR data are not available after 2008, we cannot use it to identify refugees beyond that year.

Finally, because a primary emphasis of the resettlement process is to provide sufficient resources so that individuals can adapt as quickly as possible to their new surroundings immediately after relocating, we pay particular attention to outcomes within two years of arriving, which leaves us with 4,358 observations.[5] Our primary outcome variables include employment, usual hours worked, annual wages, annual total earnings, SNAP take-up, and participation in other public social safety net programs.

3.4 Empirical strategy

We analyze the impact of non-government partner resources on a range of refugee economic outcomes. Our baseline empirical strategy is a fixed effects specification of the following form. In particular, we estimate:

$$Y_{dti} = \beta_0 + \beta_1 S_{-5-0,dt} + \alpha_\tau + \gamma_d + \lambda_t + X_i + \eta_{st} + \epsilon_{dti} \tag{1}$$

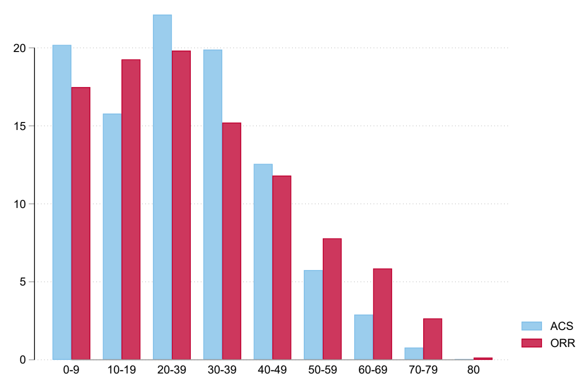
In our baseline specification, Y_{dti} is the outcome of interest for refugee i living in diocese d in year t. Our main measures of interest, $S_{-5-0,dt}$, represent the total number of scandals that were disclosed in a diocese the period $0-5$ years relative to arrival, with negative (positive) terms representing years before (after) arrival.[6]

[5] Another benefit of focusing on outcomes within the first two years of arrival is refugees may be less likely to move internally within the United States immediately after arriving, a factor that would introduce additional measurement error with respect to diocese assignment.

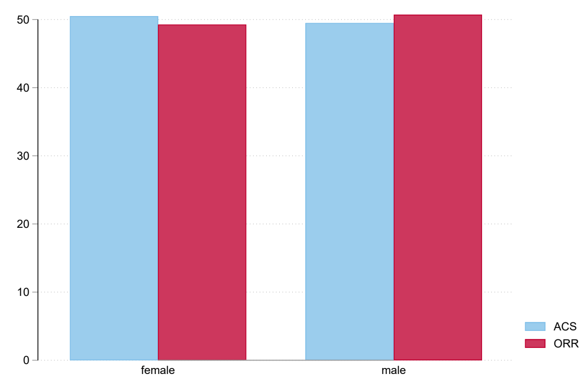
[6] We show in the Appendix that grouping scandals using different time periods (e.g. 0-4 years before arrival, 0-3 years before arrival, etc.) produce similar results.

Figure 8: Compare ACS to ORR

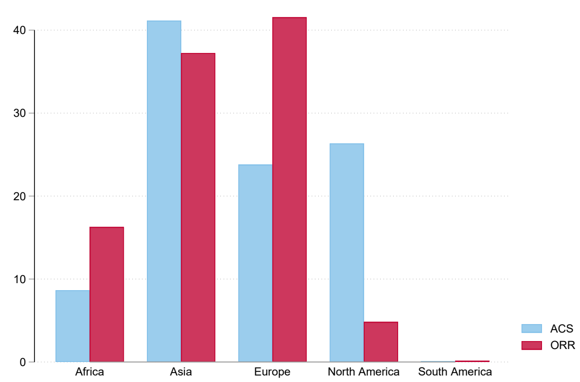
(a) Age



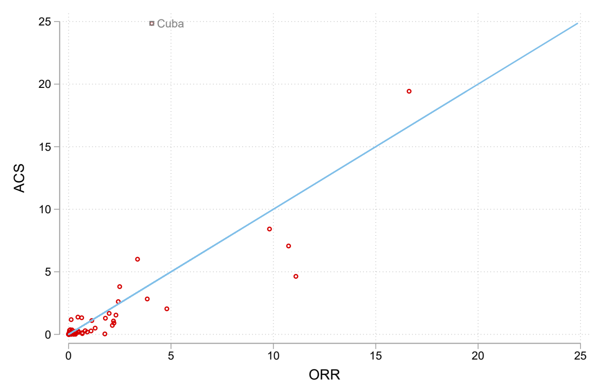
(b) Sex



(c) Continent



(d) Country



Beyond our main measures of interest, we control for year of arrival fixed effects α_τ, diocese fixed effects γ_d, and observation year fixed effects λ_t. We also control for individual characteristics X_i such as gender, age, education, English speaking ability, marital status, country of origin, and race. Finally, we include measures at the state-year level η_{st}, such as state-level unemployment in both the year of arrival and observation year, to control for any regional business cycle effects that may also influence to refugee labor market outcomes. After conditioning on these individual and state-year characteristics, our empirical strategy relies on variation from refugees who are resettled in the same diocese and arrive from the same countries of origin, but are "exposed" to varying degrees of scandal disclosure and resource shocks due to the year that they arrive.

Our primary analysis focuses on outcomes in the short term, for individuals who have arrived in the past two years. By focusing on short-term outcomes, we attempt to minimize the possibility of selection among our study population along two margins: those who internally migrate and relocate to a different place within the United States, and selection into who may remain in the United States.

While USCCB has resettled the largest share of refugees in the United States since 1980, there are eight other VOLAGs that support the resettlement process. The resource shocks that accompany a new revelation of alleged abuse should only affect Catholic organizations and not resettlement partners with no Catholic affiliation. In turn, refugees resettled by a different partner organization should not be affected by any negative resource shocks to the Catholic Church and its affiliates.

To account for the possibility that refugees are resettled by different VOLAGs, we also estimate the following equation:

$$Y_{dti} = \beta_0 + \frac{CA}{TA}_d \beta_1 S_{-5-0,dt} + \alpha_\tau + \gamma_d + \lambda_t + X_i + \epsilon_{dti} \tag{2}$$

Equation 2 is a modification of Equation 1, where each scandal measure is now multiplied by the relative presence of local Catholic resources. One might therefore interpret the scaled scandal measure as the number of "effective" scandals in a diocese.

We estimate this relative presence, represented by the term $\frac{CA}{TA}_d$, in two ways. The first method estimates the relative presence using the number of local Catholic resettlement affiliates in a diocese as a share of the total number of local resettlement affiliates across the nine VOLAGs. We identify local Catholic affiliates using the list of local offices provided by Catholic Charities USA. We use the the fact that the names of the affiliate offices follow similar naming conventions to identify them in a list of nonprofit organization data from the Urban Institute's National Center for Charitable Statistics. In particular, we use these data to tag any nonprofit with "Catholic Charities," "Catholic Social Service," or "Catholic Community Service" in the organization name and identify their location using the listed mailing address. For other VOLAGs, we identify local affiliates by using lists provided on their respective websites. While we acknowledge that the geographic distribution of local affiliates for each VOLAG may vary across time, these lists offer a best approximation for the relative presence of each VOLAG across the United States.[7]

The second method uses the relative size of the Catholic community in a location. To calculate this measure, we use the percent of the population that identifies as Catholic in 1990. These data come from the Association of Religion Data Archives (ARDA, 1990).

3.5 Results

3.5.1 Effects on welfare take-up and labor market outcomes

We begin by reporting the results from our baseline specification, described in Equation 1. We report the results for a series of outcomes in Table 16, with each outcome referenced in a separate column. The outcomes are: log wages, log total income, an indicator for employment status, usual hours worked, an indicator for participation in SNAP, and an indicator for receiving support from welfare programs (e.g., AFDC, General Assistance). Coefficients and standard errors are scaled by 100. Summary statistics for these outcomes, as well as the primary independent variable (number of new scandals revealed 0-5 years before a refugee's arrival) are reported in Appendix Table A.2.

[7] In the Appendix, we show that the results are not specific to drawing on affiliates based on individual VOLAG websites. That is, we use administrative data from the ORR's Matching Grant program as an alternative way to identify the location of local resettlement affiliates, and our results hold.

In Panel A, the results show that a greater number of new scandal revelations has negative consequences for the economic well-being of refugees. In the subsequent discussion, we interpret and scale our coefficients estimates by standard deviation changes in our scandal measure. For a one standard deviation increase in scandal revelations 0-5 years prior to arrival, refugees are 2.2 (-0.045*48.6) percentage points (pp) less likely to enroll in SNAP and 2.8pp less likely to receive other forms of welfare income. This reduction in participation in the social safety net does not appear to be a result of improving labor market outcomes that may reduce the reliance on public welfare programs. The results show that refugees are no more likely to receive greater wages, total income, or be employed. If anything, the results suggest that refugees receive lower wages (income) and are less likely to be employed if the area that they are resettled experienced a greater number of new scandal revelations within five years before their arrival.

While the results in Panel A point to the potential harmful effects of these scandals on new refugee arrivals, the results do not take into account the relatively likelihood that refugees in the study population are resettled by Catholic Church affiliates, organizations whose resources would be directly affected by new scandal revelations. In Panels B and C of Table 16, we report results from empirical specifications that estimate "effective" scandals based on the relative share of Catholic affiliates and Catholic population in an area, respectively. The results highlight that scaling scandals by the estimated presence of Catholic resources increases the magnitudes of the coefficients compared to Panel A. For every 1σ increase in scandal revelations 0-5 years prior to arrival, refugees are between 5.3-6.3 pp less likely to participate in SNAP if we account for the presence of local Catholic organizations and population. Similarly refugees are between 6.8-7.9 pp less likely to participate in other public welfare as a result of the same sized increase in scandal revelations, after accounting for the presence of local Catholic organizations and population.

When further accounting for the relative presence of local Catholic resources, the results suggest that labor market outcomes for refugees worsen in response to new scandal revelations. The results in Panels B and C suggest that refugees are approximately 2.0-2.7 pp less likely to be employed for every 1σ increase in new scandals prior to arrival. Further, refugee wages (and total income) also appear to decline by approximately 13.0-16.0 (8.5-10.5) percent with a standard deviation increase in scandals, depending on whether the relative presence of Catholic resources is approximated by local Catholic organizations or the local Catholic population. Our results are also robust to using different scandal revelation time groupings, i.e., 0-4, 0-3, 0-2, or 0-1 years before arrival. The results using different time groups are reported in Table A.3.

In order to further refine our identification of likely refugees and to mitigate the risk of false positives, we repeat the main analysis on a sample constructed using our method described above as well as restricting to individuals likely to be refugees based on year of arrival and country of origin. Following common practice (Capps and Newland, 2015; Evans and Fitzgerald, 2017; LoPalo, 2019), we use the Department of Homeland Security's Yearbook of Immigration Statistics to construct a refugee concentration ratio, RCR, based on aggregate number of refugees as a share of total immigrants by country of origin and year of immigration. Table 17 reports results from our sample, further restricting to those whose year of arrival and country origin implies an RCR of greater than 0.8. In general coefficients on public assistance take up increase in magnitude. Without accounting for Catholic presence, SNAP enrollment declines by 15.65 percentage points with a one standard deviation increase in allegations. Accounting for Catholic presence, this estimate grows to 35-57 percentage point change. When using the affiliate measure of Catholic presence, estimates from this sample suggest that a standard deviation increase in allegations leads to a reduction in other welfare take up by 27.6 percentage points.

Results for labor market outcomes are more mixed. While there is some evidence that scandals decreased employment probability (33.67 percentage points with a standard deviation change when using the Catholic population scaled measure), coefficients on wages and income are large and positive. Panel A suggests that ten scandals increase wages, conditional on being employed, by 13%.

We interpret the differences in magnitude (and in the case of wages and income, sign) of the estimates in tables 16 and 17 to be the result of refining the sample to reduce false positives. While in general non-citizen immigrants are not eligible for public assistance or SNAP within the five years of arrival, recently arriving refugees participate in public assistance and SNAP at high rates.[8] Reducing the number of false positive refugees increases the share of the sample who are eligible for public assistance, contributing to the difference in effect size. We also note that restricting the sample in this way substantially reduces the countries of origin represented in our sample. We view this as an additional explanation for the large differences in coefficients on both labor market outcomes and public assistance take-up.

Our method assigns refugee status to individuals who share characteristics with a known refugee. False positive refugee assignment, to the extent that it is an issue in our sample, is likely most concentrated among those with the lowest unconditional probability of being a refugee. Again we restrict the sample using the RCR, this time asking whether the effects differ for those least likely to be refugees. Table 18 presents results for individuals in our sample whose RCR is less than 0.2. The estimated effects of abuse allegations on income are large and positive, leading us to view coefficients on earnings in other specifications with caution. Estimates on other outcomes are imprecise, and in general share the same direction as those in table 17. Notably however, the coefficients on SNAP take up are small and positive, contrasting with the results in other specifications.

3.6 Mechanism

The preceding section highlights the detrimental effects of new Catholic Church sexual abuse scandal revelations on communities that rely on the support of the institution. In this section, we discuss underlying types of resource strain experienced by the Catholic Church as a result of scandal revelations that might contribute to the negative impacts on refugees. We then further elaborate on how to best interpret the negative effects.

[8]In 2008, the Office of Refugee Resettlement estimated that 57.7% of refugees arriving the previous year made use of public benefits. For refugees in the second year after arrival, this drops to 31.7% (Office of Refugee Resettlement, 2008). In 2015, the Office of Refugee resettlement reported that 40.3% of surveyed refugees made use of their first year of were enrolled in SNAP benefits (Office of Refugee Resettlement, 2015).

3.6.1 Non-financial resources

One way that new scandal revelations diminished local Catholic diocese resource capacity was through non-financial support. In particular, prior work has highlighted changes to multiple non-financial outcomes that signal less community support for the Catholic Church. For one, past literature has shown how enrollment in Catholic schools declined as a consequence of new revelations (Dills and Hernández-Julián, 2012). Further, researchers have found that the overall religiosity of local communities dropped; and even among those who were religious, scholars noted a significant *shift* in the affiliations of those individuals away from Catholicism toward other denominations (Hungerman, 2013). Each of these changes signal a weakening standing of the Catholic Church in local communities when new scandals were revealed, but where refugees were also still being resettled.

Beyond switches in religious affiliation and general reduction in religiosity, there are other ways in which individuals may express weaker connections with the Catholic Church and its services For instance, we use data from the Current Population Survey (CPS) September Volunteering and Civic Life Supplement (2010-2015) to examine the effect of scandal shocks on individual volunteer service behavior. Especially given the reliance of VOLAGs on volunteers to fill important caseworker positions when formal staffing is short, it may be one of the mechanisms through which disclosed abuse scandals affect refugee outcomes. We first examine the effect of scandal shocks on individual overall volunteer services to organizations, including religious organizations; children's educational, sports, or recreational groups; social and community service groups; cultural or arts organizations; immigrant/refugee assistance, etc. We then examine the effect of the scandals on volunteer services provided to refugee and immigration resettlement agencies in particular.

Table 19 reports the effect of the scandals on individual overall volunteer services and services to refugee and immigration resettlement agencies. Using different specifications, column (1) shows that Catholic scandal revelations have little impact on general volunteering. If any, the effect is negative and not statistically significant from zero. However, column (2) shows that scandal revelations have a negative impact on volunteer services provided to immigration and refugee services. Specifically, a 1σ increase in new scandals prior to refugee arrival decreases individual assistance provided to refugee/immigrants by 1-2pp. The effects are statistically significant at the 10% level when using the raw scandal measure and at the 1% level when using the affiliate-scaled scandal measure.

3.6.2 Financial resources

In addition to non-financial resources, the Catholic Church lost significant monetary resources. News agencies have reported how many dioceses have recently had to file for bankruptcy as the financial strain resulting from new revelations continues to grow larger (Gjelten, 2018; Crary, 2019). Research has documented how the church in particular has lost significant resources previously provided through private charitable donations (Bottan and Perez-Truglia, 2015). These financial troubles, combined with the reductions in non-financial support discussed previously, have left the organization with limited capacity to provide the human services it had previously offered to its vulnerable community members.

3.6.3 Ruling out alternative interpretation of negative effects

A natural response to the resource strain experienced by the Catholic Church would be to reduce the number of refugees that we resettle. Doing so would offer an alternative interpretation of our results – rather than refugees faring worse off due to overwhelmed caseworkers and less financial and non-financial support through the Catholic Church, refugees may fare worse because *other VOLAGs* other than the Catholic Church are not as effective at resettling them.

To explore this possibility we submitted a Freedom of Information Act Request and obtained data on Matching Grant program participants, beginning in 2010. Refugees in the Matching Grant program are a subset of the total number of refugees resettled by each VOLAG. Further, the data that we obtained were for out-of-sample years (2010-2015). However, these data include various metrics that measure the success success that each VOLAG has in resettling refugees allocated to them.

Figure A.1 compares outcomes for refugees resettled by USCCB compared to other outcomes. The figure describes four different outcomes: share of adults employed, average wage, self-sufficiency after 120 days, and percentage accessing cash assistance.

We first note that, on average, refugees in the Matching Grant program resettled by other VOLAGs are less likely to accessing public cash. This lower share would be consistent with the negative effects on social safety net program participation that we find. However, the Matching Grant program data suggest that the lower rate of public cash access among other VOLAGs may be due to significantly improved labor market outcomes. Employed and self-sufficiency measures are significantly better among other VOLAGs compared to USCCB. This is not consistent with the results that we observe in our analysis. If anything, recall that our primary results suggest that labor market outcomes are *worse* after scandals, which would not be likely if there were reallocated to organizations with a track record of obtaining better labor market outcomes for refugees.

Given the results regarding average outcome differences across VOLAGs from the Matching Grant program data, we do not believe our effects are driven by quality differences across VOLAGs. Rather, we view the more likely interpretation as one of worsening quality of resettlement support provided by the Catholic Church to refugees assigned to them.

3.7 Robustness

3.7.1 Simulation

In this section, we perform the simulation analysis to address the fact that some individuals in the ACS data share characteristics with multiple observations in the Refugee Resettlement data. This will affect our main results if these individuals have different county identifiers and thus expose to different scandal shocks at the diocese level. To account for the impact of the assignment of county identifiers associated with different matches, we conduct a simulation analysis that randomly assigns one of the observations that share the same characteristics to the ACS sample. We perform the random assignment 500 times and used the associated 500 refugee samples (with different assigned county identifiers) to estimate the regression coefficients. Then we plot the distribution and one-sided p-value of the coefficients in Figure 9 and 10. We find that the mean of the point estimates from the 500-simulation reported in A.5 is similar to the coefficients reported in the main tables. In addition, the p-values for the one-sided test of the coefficients for welfare program participation (SNAP, and other welfare take-up) are close to or below 0.01, suggesting the results are statistically significant at (or close to) 1% level. This suggests that the results for the social safety net participation and labor market outcomes are robust when accounting for cases where individual characteristics imply multiple potential placement counties.

3.7.2 Dropping Cuban refugees

As discussed in the data section, Cuban refugees make up 25% of refugees identified in the ACS. This group makes up the largest share of refugees when describing refugees by country of origin. To test the robustness of our results, in this section we drop Cuban refugees and rerun our primary analysis. The results are reported in Table A.6. Our estimates after dropping Cuban refugees are larger than the ones reported in the main analysis. In addition, the effects on wages, income, hours worked, SNAP program participation, and other welfare program take-up are all statistically significant at 1% level, indicating that our main results are robust to dropping Cuban refugees.

3.8 Conclusion

Refugee resettlement is a complex task that requires cooperation between multiple agencies in both private and public spheres. In the United States, non-governmental partner agencies are key contributors in the resettlement process. These volunteer agencies (VOLAGs) provide a multitude of services as refugees acclimate to their new homes.

In this paper, we explore the impact of non-governmental resources on refugee economic outcomes. To evaluate this question, we consider resource and reputation shocks to the largest VOLAG, the United States Conference of Catholic Bishops, resulting from newly disclosed allegations of sexual abuse in the Church. We introduce a novel method to identify likely refugees in the American Community Survey as well as the diocese in which they were resettled. Once identified, we analyze how refugees who were resettled in areas and during times with more negative attention surrounding abuse scandals (and therefore negative resource shocks) fared compared to others who were resettled during less tumultuous times.

Our results suggest that resource shocks to VOLAGs lead to a reduction in federal social safety net program participation in the short run. We find that this reduction in public welfare take-up is not due to improving labor market outcomes; rather, while not conclusive, we find suggestive evidence that refugees also fare worse in the labor market. The negative consequences on social safety net participation highlight the value of non-government networks to government agencies. VOLAGs are key in linking refugees to public resources that can help them acclimate to their new homes. For example, VOLAG caseworkers hold critical institutional knowledge to help refugees navigate unfamiliar steps required to access the full suite of resources available to them.

Overall, this paper improves our understanding of the institutions that shape refugee outcomes after resettlement. Building on important work that emphasizes the value and need for public resources and programs (LoPalo, 2019), our research further quantifies the role that non-government partners play in supporting this vulnerable population. At the same time, we hope our paper may also further dialogue about how best to support refugees, as our findings highlight how they are vulnerable to variability in aid of non-governmental organizations. This conversation grows more urgent over time, as the number of refugees continues to increase rapidly as a consequence of increasing threats of war, persecution, and climate change globally (UNHCR, 2022).

3.9 Tables and figures

Table 16: Effects of Catholic scandal revelations on refugee outcomes

Dep. Var.:	(1) ln(*wage*)	(2) ln(*inc*)	(3) *Employed*	(4) *Hours worked*	(5) *SNAP take − up*	(6) *Welfare take − up*
Panel A: Raw Scandal Measure						
S_{-5-0}	-0.103**	-0.078**	-0.021	-0.530	-0.045***	-0.058***
	(0.045)	(0.039)	(0.015)	(0.586)	(0.016)	(0.012)
Observations	2,640	3,148	4,358	4,358	4,358	4,358
Panel B: Affiliate-Scaled Scandal Measure						
S_{-5-0}	-0.268**	-0.175	-0.041	-0.689	-0.129***	-0.163***
	(0.120)	(0.118)	(0.041)	(1.589)	(0.042)	(0.016)
Observations	2,617	3,122	4,323	4,323	4,323	4,323
Panel C: Population-Scaled Scandal Measure						
S_{-5-0}	-0.330***	-0.219**	-0.056	-1.535	-0.109**	-0.140**
	(0.116)	(0.107)	(0.043)	(1.655)	(0.050)	(0.054)
Observations	2,640	3,148	4,358	4,358	4,358	4,358

Note: This table reports OLS estimates of the effects of Catholic affiliate resource shocks, as a result of newly-disclosed sexual abuse allegations, on a series of refugee outcomes. The sample are those in the ACS who have been in the United States for two years or less and whose characteristics are found in the Refugee Resettlement Data. Outcomes include log wages, log total income, an indicator for employment status, usual hours worked, an indicator for participation in SNAP, and indicator for receiving income from welfare programs. All specifications include year of immigration, ACS year, and diocese fixed effects. All specifications also include controls for race, marital status, education, gender, age, country of origin, state unemployment rate in year of immigration, state unemployment rate in ACS year, and TANF maximum generosity benefits in the year of immigration. All income measures are inflation-adjusted. All coefficients and standard errors are scaled by 100. Robust standard errors are clustered at the diocese level. *** p<0.01, ** p<0.05, * p<0.1

Table 17: Likely refugees (>80% arrivals from sending country are refugees)

Dep. Var.:	(1) ln(*wage*)	(2) ln(*inc*)	(3) *Employed*	(4) *Hours worked*	(5) *SNAP take − up*	(6) *Welfare take − up*
Panel A: Raw Scandal Measure						
S_{-5-0}	1.332***	4.89	-0.207	-3.469	-0.322***	-0.086
	(0.438)	(0.596)	(0.133)	(7.439)	(0.116)	(0.126)
Observations	1,094	1,654	1,654	1,654	1,654	1,654
Panel B: Affiliate-Scaled Scandal Measure						
S_{-5-0}	1.918	1.330	-0.229	3.752	-1.173***	-0.568**
	(1.1327)	(1.473)	(0.258)	(13.354)	(0.299)	(0.230)
Observations	1,093	1,262	1,650	1,650	1,650	1,650
Panel C: Population-Scaled Scandal Measure						
S_{-5-0}	4.203***	0.686	-0.693**	-0.17270	-0.730**	-0.101
	(1.268)	(1.413)	(0.282)	(17.824)	(0.359)	(0.321)
Observations	1,094	1,264	1,654	1,654	1,654	1,654

Note: This table reports OLS estimates of the effects of Catholic affiliate resource shocks, as a result of newly-disclosed sexual abuse allegations, on a series of refugee outcomes. The sample are those in the ACS who have been in the United States for two years or less, whose characteristics are found in the Refugee Resettlement Data, and who were classified as a likely refugee based on birth country and immigration year. An observation is classified a likely refugee if the total number of refugees and asylees from the sending country comprises more than 80% of the legal immigrant arrivals from the sending country and arrival year. Outcomes include log wages, log total income, an indicator for employment status, usual hours worked, an indicator for participation in SNAP, and indicator for receiving income from welfare programs. All specifications include year of immigration, ACS year, and diocese fixed effects. All specifications also include controls for race, marital status, education, gender, age, country of origin, state unemployment rate in year of immigration, state unemployment rate in ACS year, and TANF maximum generosity benefits in the year of immigration. All income measures are inflation-adjusted. All coefficients and standard errors are scaled by 100. Robust standard errors are clustered at the diocese level. *** p<0.01, ** p<0.05, * p<0.1

Table 18: Placebo: unlikely refugees (<20% arrivals from sending country are refugees)

Dep. Var.:	(1) ln(*wage*)	(2) ln(*inc*)	(3) *Employed*	(4) *Hours worked*	(5) *SNAP take − up*	(6) *Welfare take − up*
Panel A: Raw Scandal Measure						
S_{-5-0}	0.167	0.302***	-0.016	-1.387	0.050	-0.044
	(0.167)	(0.112)	(0.039)	(1.347)	(0.040)	(0.049)
Observations	360	431	674	674	674	674
Panel B: Affiliate-Scaled Scandal Measure						
S_{-5-0}	0.380	0.730***	-0.013	-2.872	0.128	-0.106
	(0.389)	(0.262)	(0.102)	(3.382)	(0.098)	(0.125)
Observations	356	427	670	670	670	670
Panel C: Population-Scaled Scandal Measure						
S_{-5-0}	0.468	0.825***	-0.046	-4.172	0.134	-0.118
	(0.447)	(0.298)	(0.113)	(3.884)	(0.107)	(0.138)
Observations	360	431	674	674	674	674

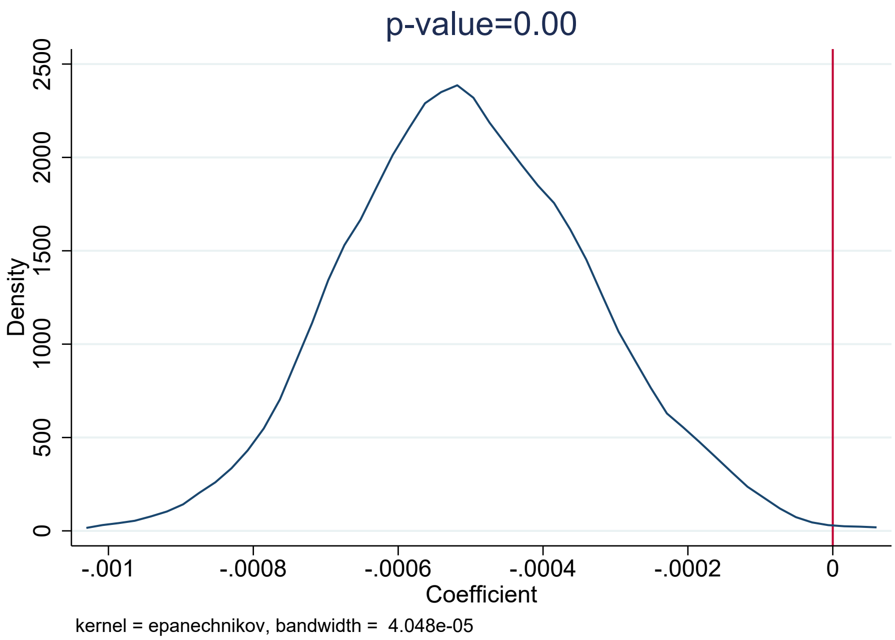
Note: This table reports OLS estimates of the effects of Catholic affiliate resource shocks, as a result of newly-disclosed sexual abuse allegations, on a series of refugee outcomes. The sample are those in the ACS who have been in the United States for two years or less, whose characteristics are found in the Refugee Resettlement Data, and who were classified as an unlikely refugee based on birth country and immigration year. An observation is classified a likely refugee if the total number of refugees and asylees from the sending country comprises less than 20% of the legal immigrant arrivals from the sending country and arrival year. Outcomes include log wages, log total income, an indicator for employment status, usual hours worked, an indicator for participation in SNAP, and indicator for receiving income from welfare programs. All specifications include year of immigration, ACS year, and diocese fixed effects. All specifications also include controls for race, marital status, education, gender, age, country of origin, state unemployment rate in year of immigration, state unemployment rate in ACS year, and TANF maximum generosity benefits in the year of immigration. All income measures are inflation-adjusted. All coefficients and standard errors are scaled by 100. Robust standard errors are clustered at the diocese level. *** p<0.01, ** p<0.05, * p<0.1

Table 19: Effects of Catholic scandal revelations on volunteer service

	(1)	(2)
Dep. Var.:	Overall volunteer service	Immig./Refugee assistance
Panel A: Raw Scandal Measure		
S_{-5-0}	-0.011	-0.002*
	(0.023)	(0.001)
Panel B: Affiliate-Scaled Scandal Measure		
S_{-5-0}	-0.006	-0.004***
	(0.028)	(0.001)
Panel C: Population-Scaled Scandal Measure		
S_{-5-0}	0.004	-0.005
	(0.060)	(0.004)
Controls	Yes	Yes
Mean of Dep. Var	0.201	0.0004
Std of Dep. Var	0.401	0.019
Observations	188,102	188,102

Note: This table reports OLS estimates of the effects of Catholic affiliate resource shocks, as a result of newly-disclosed sexual abuse allegations, on individual volunteer services. All specifications include year and diocese fixed effects. All specifications also include controls for race, marital status, education, gender, age, state unemployment rate in calendar year, and TANF maximum generosity benefits in calendar year. Robust standard errors are clustered at the diocese level. *** p<0.01, ** p<0.05, * p<0.1

Figure 9: Coefficient distribution: public assistance participation



(a) Other welfare

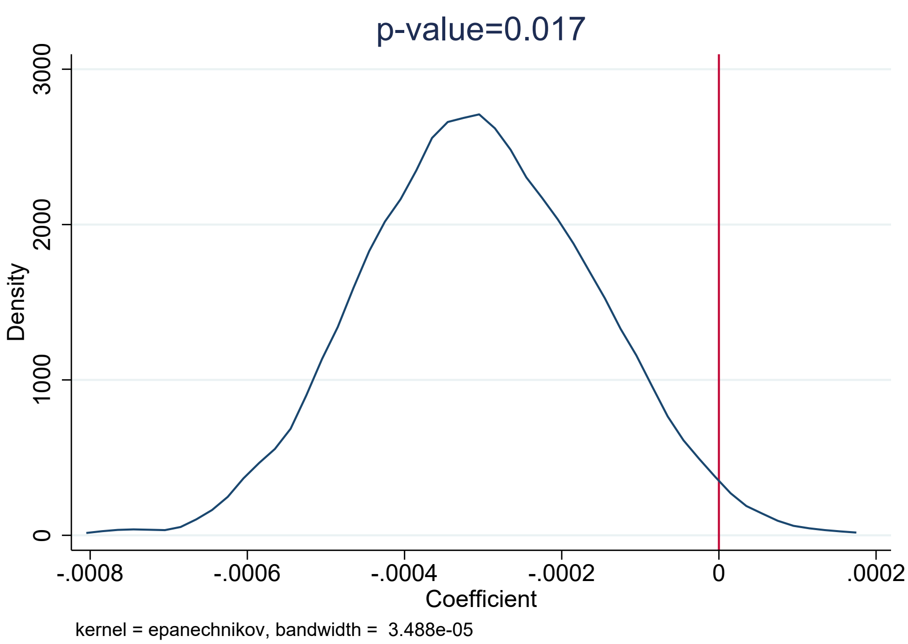
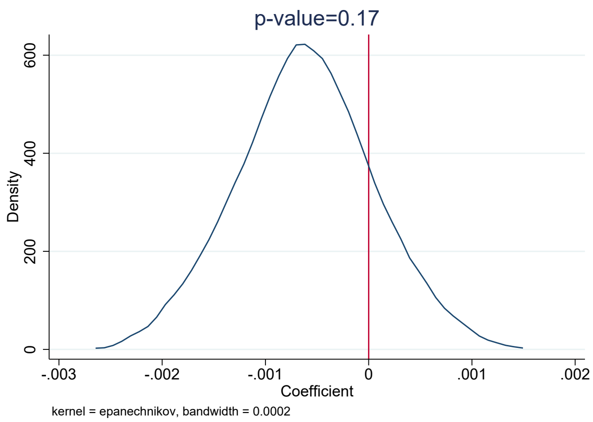
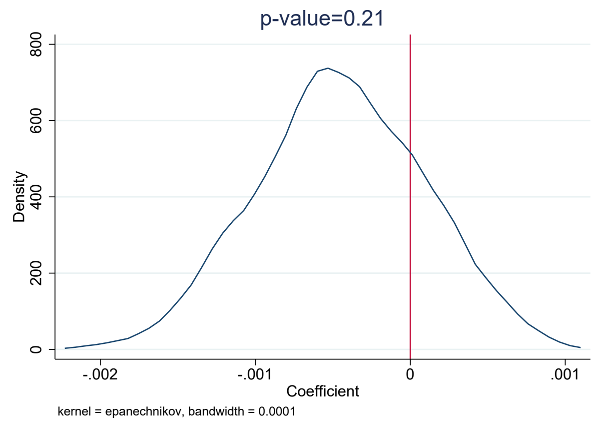


Figure 10: Coefficient distribution: labor outcomes

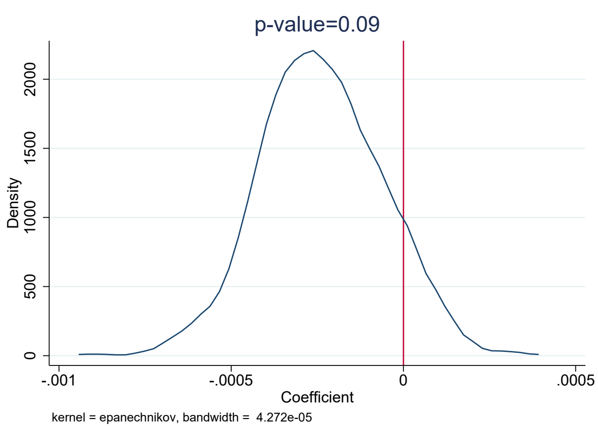
(a) Log wage



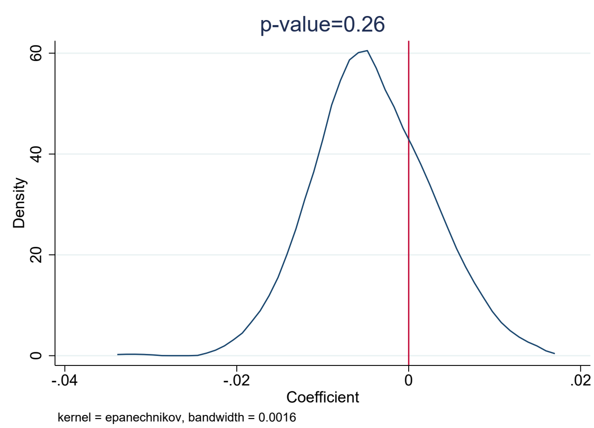
(b) Log income



(c) Employment



(d) Hours worked



Appendix A Appendix to chapter 1

A.1 Additional figures

Figure 11: Distribution of wait time across counties: 2004

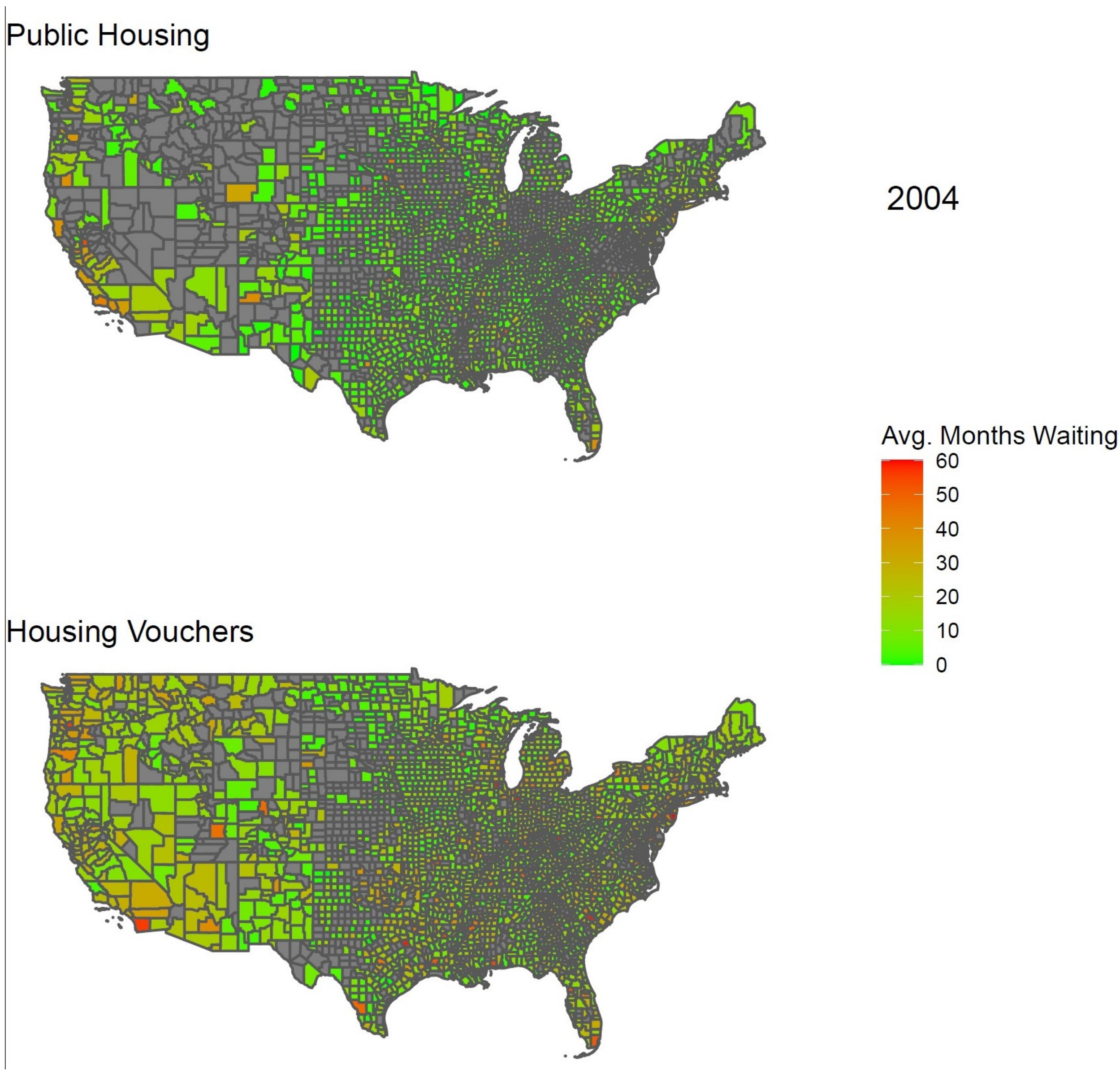


Figure 12: Distribution of wait time across counties: 2010

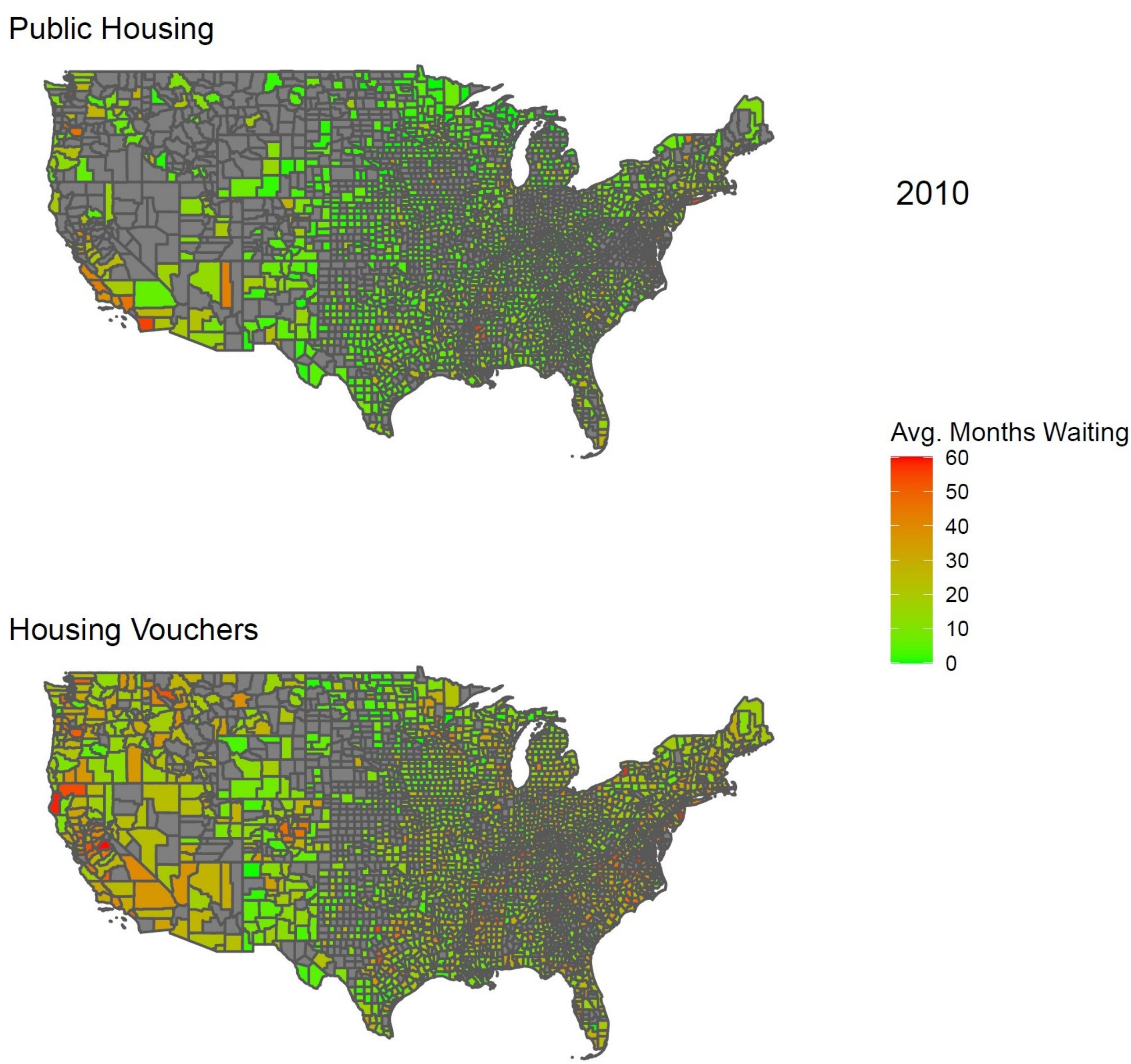
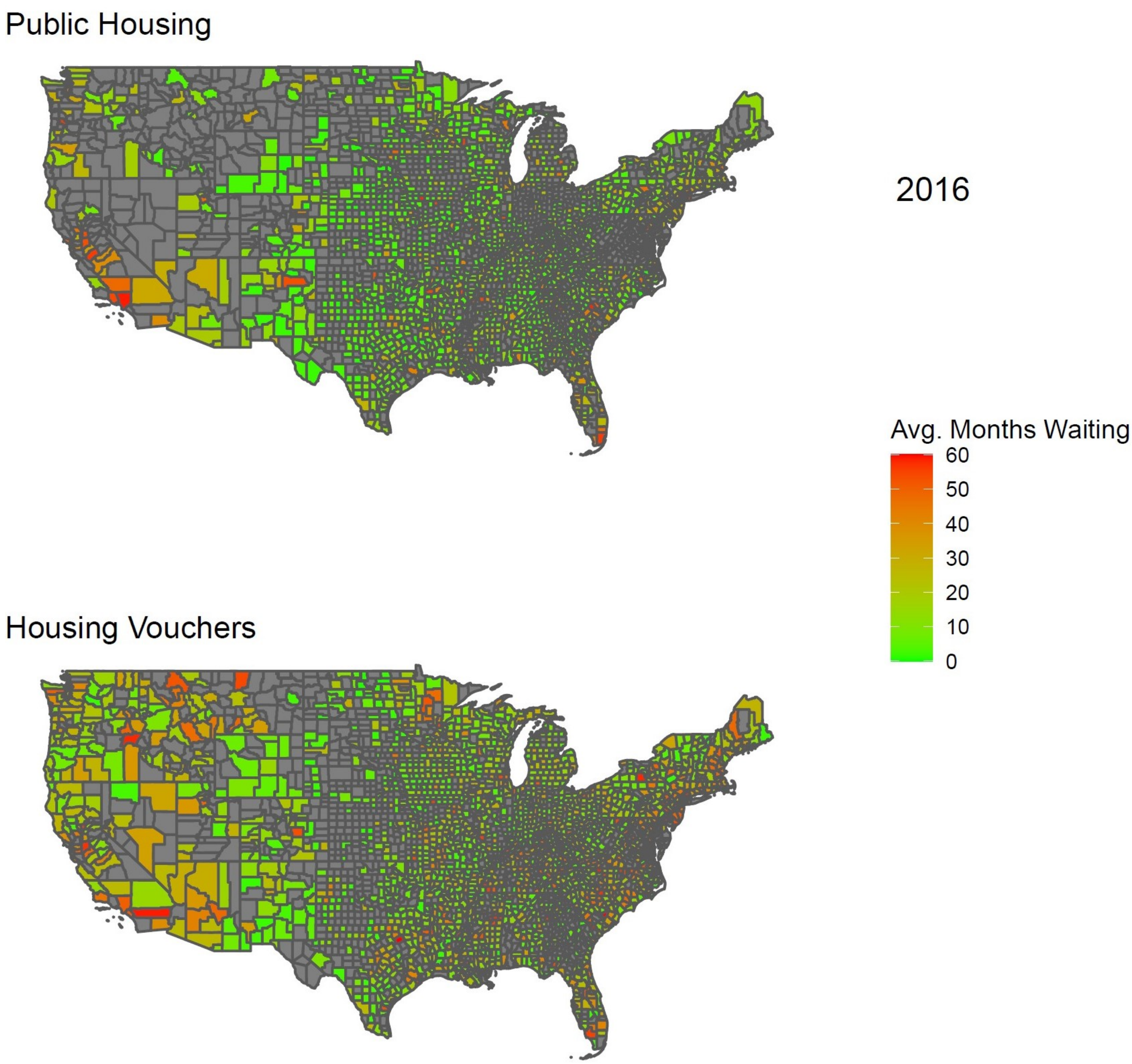


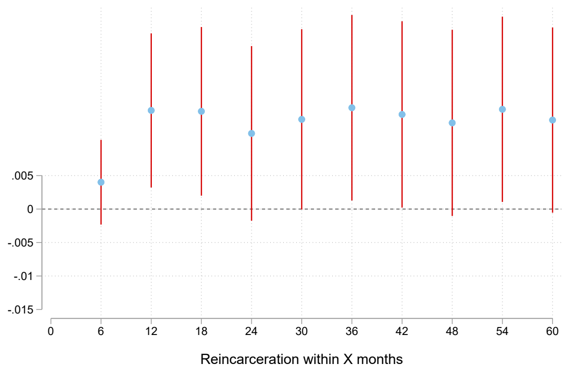
Figure 13: Distribution of wait time across counties: 2016



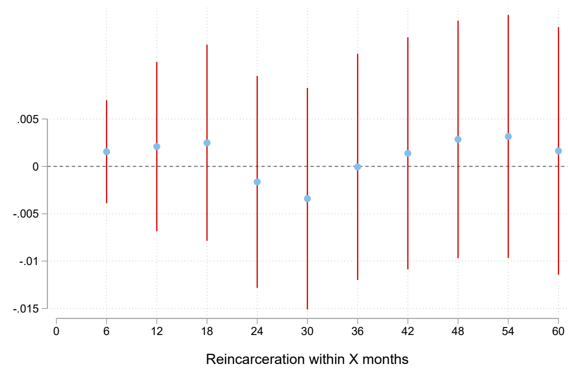
A.2 Additional results

Figure A.1: Estimated coefficient by month after release and original offense

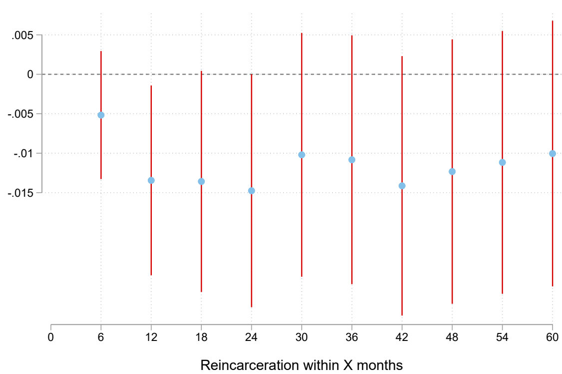
(a) Homicide



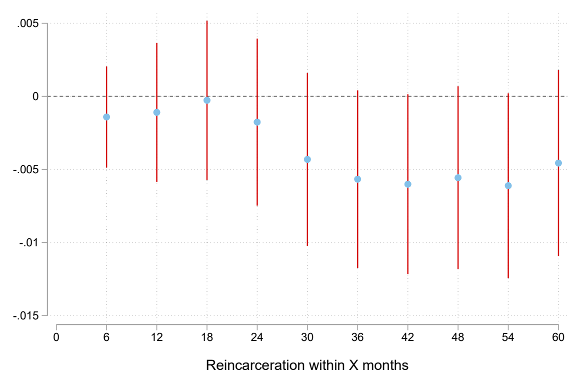
(b) Manslaughter



(c) Public order offenses



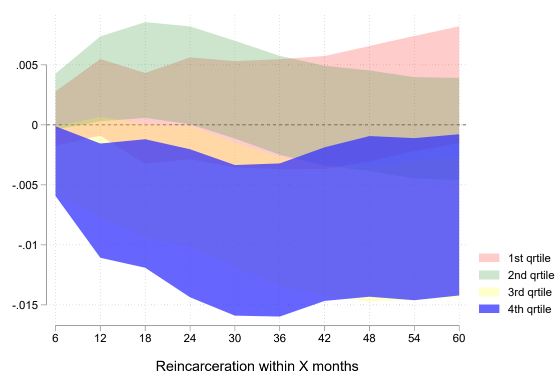
(d) White collar crimes



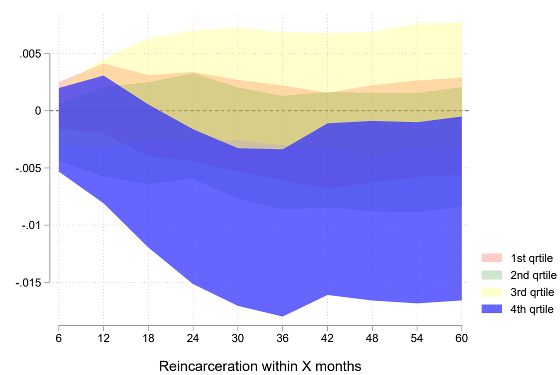
Notes: This figure displays the coefficient on being above median wait time by months since release. All regressions include year FEs, county FEs, offense dummies, and full set of county and individual controls. 90% confidence intervals displayed, calculated using standard errors clustered at the county by year level.

Figure A.2: Estimated coefficients for public housing by months after release: split by quartiles of neighborhood characteristics

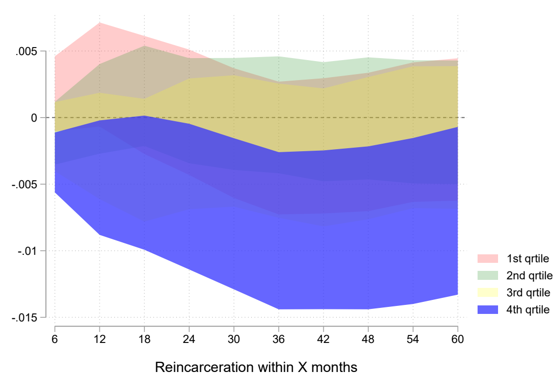
(a) Minority share in project neighborhood



(b) Share below poverty line in project neighborhood



(c) Property and violent crimes per 1000 residents



Notes: This figure displays the confidence intervals for coefficients on the "low access" treatment. Each shaded region represents estimates for separate quartiles of public housing housing characteristics experienced by released prisoners in my sample. Public housing concentration is defined as the average number of public housing units per project in a county. All regressions include year FEs, county FEs, offense dummies, and full set of county and individual controls. 90% confidence intervals displayed, calculated using standard errors clustered at the county by year level.

Appendix B Appendix to chapter 2

The primary purpose of this appendix is to report additional tables and figures. First, we provide additional details on the regression-discontinuity-based test to detect gerrymandering mention in Section 3.3.1 of the main text.

We propose an additional novel test of gerrymandering that is not without flaws, but is illustrative in this setting. Namely, we match block groups throughout the country to their nearest Congressional district boundaries for district maps that prevailed from 2011-2018 and again in Pennsylvania for the new 2018 maps. Then, we use block group demographics and the same method used to predict Democrat shares of Congressional districts in Jones and Walsh (2018) to calculate the predicted Democrat share of every block group, essentially inferring the likely share of residents of each block group that are Democrats (rather than Republicans) based on demographics and other local characteristics.

Then, we employ spatial regression discontinuity methods to detect abrupt changes at district boundaries. In particular, when districts are "packed," one form of gerrymandering, then district lines are carefully drawn to draw in as many members from one party into the district, thereby pulling those voters out of neighboring districts. Thus, "packed" districts should exhibit jumps at their boundaries in the partisan composition of block groups. With that in mind, we estimate regression discontinuity models where we identify districts that lean Democratic vs. Republican and then test whether there are jumps in the share of Democrats in block groups at the boundaries as one enters the Democratic-leaning side.

Specifically, we estimate:

$$DemShare_{bd} = \alpha + \beta_1 MajDem_d + \beta_2 MajDem_d * Dist_b + \beta_3 Dist_b + \epsilon_{bd} \tag{3}$$

where $DemShare_{bd}$ is the predicted Democrat share of Census block group b in district d. $MajDem_d$ is the main "treatment" variable, which is a dummy equal to one if district d is Majority Democrat rather than Majority Republican. $Dist_b$ is the distance of block group b from the boundary between district d and the *nearest* neighboring district. With the full interaction between $MajDem_d$ and $Dist_b$ included in the regression, β_1, the coefficient on

$MajDem_d$, identifies the difference in Dem. share of block groups just along district boundaries. If $\beta_1 > 0$, it implies that, on average throughout the state, the partisan composition of block groups abruptly turns more Democratic upon barely entering a majority Dem. district, consistent with packing. If $\beta_1 = 0$, there still will be more Democrats in majority Democrat districts, but the largest spatial changes in Democrat share do not occur at district lines, which we argue is inconsistent with packing. In our analysis, the model above is estimated within optimally-selected bandwidths in accordance with methods of Calonico, Cattaneo and Titiunik (2014).

This approach has two drawbacks. First, while we argue that it can capture "packing," it is less clear that it can capture "cracking," wherein the redistricting authorities split the opposing party's voters across a number of districts. There is no reason to expect cracking to generate abrupt jumps at district lines. Second, the test relies on the existence of neighboring majority Dem. and majority Repub. districts. In some states, all districts lean towards one party, so the test is not possible in those states.

Nonetheless, our evidence suggests that this approach meaningfully captures gerrymandering. For instance, in Table A.2, Columns 3 and 4, we show that throughout the country, jumps at district lines are larger in states that rely on their legislatures and governors to draw district lines relative to states that rely on independent commissions, consistent with existing work showing that partisan gerrymandering is less extreme in the latter (Carson and Crespin, 2004; Grainger, 2010; Edwards et al., 2017)

Our Pennsylvania results are reported in Table A.2, Columns 1 and 2. We estimate the model separately for block groups prior to 2018 redistricting and after 2018 redistricting. Results are reported in figures and tables below. Ultimately our results reveal that, in Pennsylvania before 2018, the average block group barely within a Democratic-leaning district is 6 percentage points more Democratic than a block group barely on the Republican-leaning side (Column 1, Table 2). In the district lines in 2018 in Pennsylvania, there is no significant jump at district lines in the partisan composition of block groups (Column 2, Table 2). Corresponding regression discontinuity plots are provide in Figure A.4.

Figure A.3: Map of predicted and actual gerrymandering

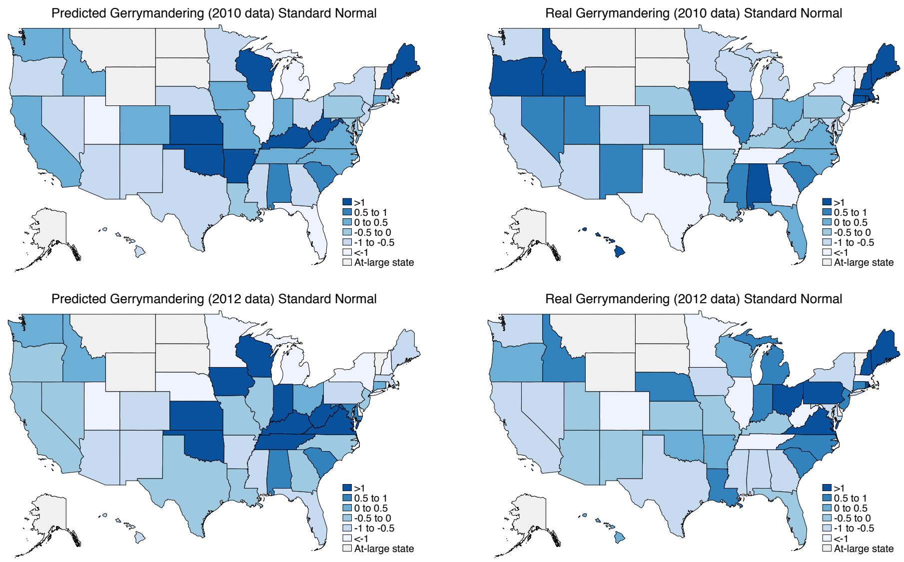
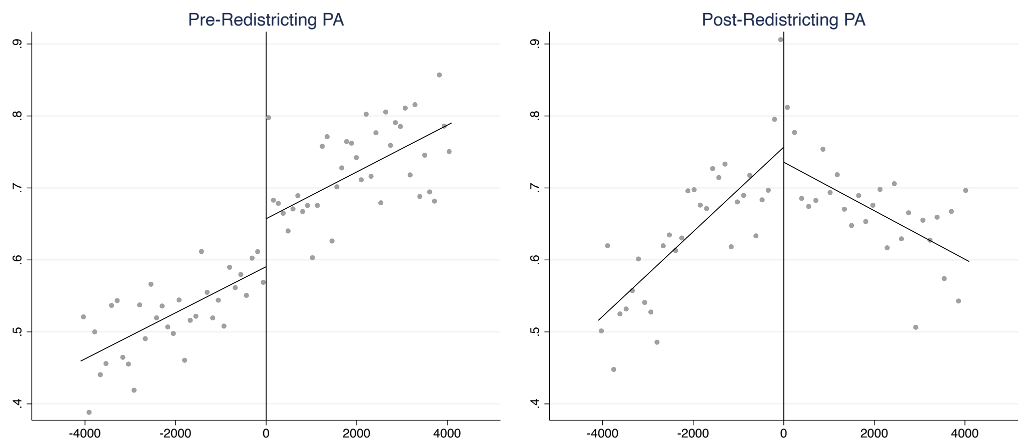


Table A.1: Predicted gerrymandering and campaign donations

	(1)	(2)	(3)
	ln(All Conts.)	ln(House Conts.)	ln(Non-House Conts.)
Pred. Gerry.	-0.041**	-0.059*	-0.052**
(std. norm.)	(0.019)	(0.035)	(0.025)
Observations	172	172	172
R-squared	0.851	0.961	0.821

Notes: Results are constructed based on a state-level panel of campaign donations and predicted gerrymandering. All contributions include all small donor (individual-level) contributions to candidates running for federal offices (President, US House, and US Senate), as well as the national committees (e.g., RNC, RSCC, RCCC); House contributions include only contributions to House candidates and the D(R)CCC; Non-House contributions include individual-level contributions to presidential or US Senate races, as well as the corresponding committees. Robust standard errors clustered at the state level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Figure A.4: Redistricting-based test to detect partisan gerrymandering, estimates from Pennsylvania before and after court-ordered redistricting



Notes: This figure is a binned scatterplot with census block groups as the unit of observation. The outcome is the predicted Democrat share of the block group. Block groups are included that are near district boundaries that divide majority Democratic and majority Republican districts. The x-axis reports distance (in meters) to the nearest district boundary, where positive numbers indicate that the block group is on the majority Democratic side; negative numbers indicate that the block group is on the majority Republican side. Proximity to zero indicates proximity to the district boundary. So that the figures are comparable, we hold constant the bandwidth, using the optimal bandwidth from the post-redistricting period, as it is the larger optimal bandwidth of the two in the estimates from Appendix Table A.2.

Table A.2: Redistricting-based test to detect partisan gerrymandering, estimates from Pennsylvania before and after court-ordered redistricting

	(1)	(2)	(3)	(4)
	PA	PA	Leg./Gov.	Non-Leg./Gov.
	Pre-2018	2018	Redist.	Redist.
RD Estimate	0.061**	-0.006	0.081***	0.038***
	(0.029)	(0.023)	(0.007)	(0.008)
Observations	2,121	2,898	35,689	26,170

Notes: This table reports regression discontinuity estimates, estimated within Calonico et al. optimal bandwidths. Census block groups are the units of observation. The outcome is the predicted Democrat share of the block group. Block groups are included that are near district boundaries that divide majority Democratic and majority Republican districts. Positive estimates imply a higher share of Democrats in census block groups at the boundary between two districts, on the side of the boundary that is majority Democratic. Columns 1 and 2 draw on data from Pennsylvania (before and after 2018 redistricting). Columns 3 and 4 draw on data from 2016, dividing the sample into states that rely on legislatures and governors for redistricting (Column 3) versus those that rely on other methods (e.g., independent redistricting commissions) (Column 4). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.3: PA results varying the size of the boundary

PANEL A: 0.5 km

	2014–2018	2016 & 2018	2014 & 2018
Post X More. Comp.	0.037***	0.020***	0.048***
	(0.010)	(0.008)	(0.011)
Observations	948,678	632,452	632,452
R-squared	0.659	0.763	0.728

PANEL B: 1 km

	2014–2018	2016 & 2018	2014 & 2018
Post X More. Comp.	0.034***	0.020*	0.043***
	(0.010)	(0.010)	(0.010)
Observations	1,946,022	1,297,348	1,297,348
R-squared	0.659	0.764	0.729

PANEL C: 3 km

	2014–2018	2016 & 2018	2014 & 2018
Post X More. Comp.	0.036***	0.026**	0.041***
	(0.009)	(0.011)	(0.009)
Observations	4,945,248	3,296,832	3,296,832
R-squared	0.660	0.765	0.730

PANEL D: 5 km

	2014–2018	2016 & 2018	2014 & 2018
Post X More. Comp.	0.033***	0.023*	0.038***
	(0.009)	(0.012)	(0.009)
Observations	6,920,598	4,613,732	4,613,732
R-squared	0.661	0.765	0.731

PANEL E: 10 km

	2014–2018	2016 & 2018	2014 & 2018
Post X More. Comp.	0.027***	0.022**	0.030***
	(0.008)	(0.009)	(0.009)
Observations	10,108,944	6,739,296	6,739,296
R-squared	0.661	0.765	0.732

Notes: All specifications include individual voter FEs, year FEs, and dummies to indicate incumbency status of the Democratic and Republican candidates. Robust standard errors (clustered at district-pair level) in parentheses ** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.4: PA results with additional controls

	(1)	(2)
	2014–2018	
Post X PA	0.039***	0.030***
	(0.007)	(0.009)
Post X PA X		0.013**
More Comp.		(0.007)
Observations	34,046,910	34,046,910
R-squared	0.679	0.679

Notes: All specifications include individual voter FEs, year FEs, and dummies to indicate incumbency status of the Democratic and Republican candidates. They also control for demographic characteristics at the district-by-post level, including the share of owner-occupied housing, vacant housing, urban, female, over 18 years of age, Hispanic, Black, Native American, Asian/Pacific Islander, and other race, as well as population. Robust standard errors (clustered at district-pair level) in parentheses ** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Appendix C Appendix to chapter 3

C.1 Additional results

Table A.1: List of nine primary VOLAGs

Organization Name	Religious Affiliation
Church World Service (CWS)	Mainline Protestant
Ethiopian Community Development Council (ECDC)	
Episcopal Migration Ministries (EMM)	Episcopal
Hebrew Immigrant Aid Society (HIAS)	Jewish
International Rescue Committee (IRC)	
US Committee for Refugees and Immigrants (USCRI)	
Lutheran Immigration and Refugee Services	Lutheran
United States Conference of Catholic Bishops (USCCB)	Catholic
World Relief Corporation (WR)	Evangelical Protestant

Note: This table provides a list of the nine major partner resettlement organizations in the United States. The religious affiliation for organizations is provided when applicable.

Table A.2: Summary statistics for study population

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. Var.:	ln($wage$)	ln(inc)	$Employed$	$Hours$ $worked$	Take-up of: $SNAP$	$Welfare$	$Scandals$ $(0-5)$
Panel A: Full Sample							
Mean	11727	13186	0.591	23.382	0.397	0.085	26.556
Std. Dev.	21093	22554	0.492	20.054	0.490	0.279	48.601
Panel B: Employed Sample							
Mean	19532	20805	1	38.789	0.361	0.060	22.949
Std. Dev.	24499	26385	-	9.556	0.481	0.237	42.217

Note: This table reports summary statistics of the main outcome variables and the main independent variable for the study population. The sample consists of likely refugees who have been in the United States for two years or less. Panel A includes 4,358 observations while Panel B includes 2,574 observations. All income measures are inflation-adjusted (2016 levels).

Table A.3: Robustness of scandal revelation effects on refugee outcomes using different scandal revelation time groupings

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var.:	ln($wage$)	ln(inc)	$Employed$	$Hours$ $worked$	$SNAP$ $take-up$	$Welfare$ $take-up$
Panel A: Raw Scandal Measure						
S_{-4-0} Coef.	-0.128***	-0.106***	-0.026*	-0.867	-0.047***	-0.059***
p-val	0.005	0.004	0.086	0.146	0.008	0.000
S_{-3-0} Coef.	-0.046	-0.034	-0.002	-0.554	-0.042***	-0.054***
p-val	0.357	0.330	0.909	0.365	0.006	0.000
S_{-2-0} Coef.	-0.055*	-0.086***	-0.012	-0.898	-0.034**	-0.035***
p-val	0.093	0.005	0.460	0.167	0.050	0.000

Note: This table reports OLS estimates and statistical significance of the effects of Catholic affiliate resource shocks, as a result of newly-disclosed sexual abuse allegations, on a series of refugee outcomes. Each cell represents a different regression. The independent variable, the number of newly-disclosed sexual abuse allegations over a fixed period of time, varies by row (0-4 years before arrival, 0-3 years before arrival, etc.). The sample consists of likely refugees who have been in the United States for two years or less. Outcomes (listed by column) include log wages, log total income, an indicator for employment status, usual hours worked, an indicator for participation in SNAP, and indicator for receiving income from welfare programs. All specifications include year of immigration, ACS year, and diocese fixed effects. All specifications also include controls for race, marital status, education, gender, age, country of origin, state unemployment rate in year of immigration, state unemployment rate in ACS year, and TANF maximum generosity benefits in the year of immigration. All income measures are inflation-adjusted. Robust standard errors are clustered at the diocese level. *** p<0.01, ** p<0.05, * p<0.1

Table A.4: Robustness of affiliate-scaled scandal measure effects on refugee outcomes

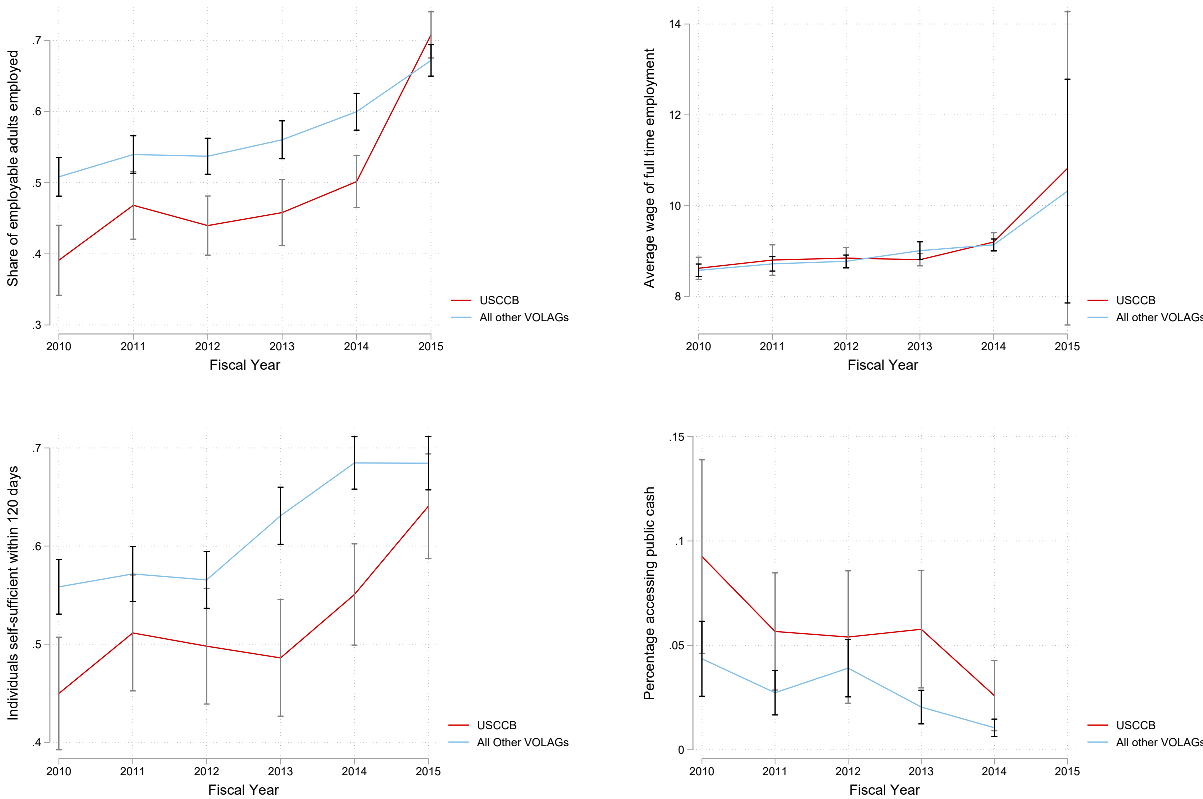
	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var.:	ln($wage$)	ln(inc)	$Employed$	$Hours$ $worked$	$SNAP$ $take-up$	$Welfare$ $take-up$
Panel A: Scandals Scaled by Affiliates Listed in Matching Grant Recipient Data						
S_{-5-0}	-0.247**	-0.203**	-0.031	-0.678	-0.126***	-0.122***
	(0.099)	(0.080)	(0.027)	(1.127)	(0.033)	(0.019)
	2,472	2,922	4,002	4,002	4,002	4,002

Note: This table reports OLS estimates of the effects of Catholic affiliate resource shocks, as a result of newly-disclosed sexual abuse allegations, on a series of refugee outcomes. The table scales the scandal measure by the number of Catholic resettlement offices in a diocese as a share of total resettlement offices across VOLAGs, as reported in 2010-2021 Matching Grant program data. The sample consists of likely refugees who have been in the United States for two years or less. Outcomes include log wages, log total income, an indicator for employment status, usual hours worked, an indicator for participation in SNAP, and indicator for receiving income from welfare programs. All specifications include year of immigration, ACS year, and diocese fixed effects. All specifications also include controls for race, marital status, education, gender, age, country of origin, state unemployment rate in year of immigration, state unemployment rate in ACS year, and TANF maximum generosity benefits in the year of immigration. All income measures are inflation-adjusted. Robust standard errors are clustered at the diocese level. *** p<0.01, ** p<0.05, * p<0.1

Table A.5: Simulation of matching refugees: effects of Catholic scandal revelations on refugee outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var.:	ln($wage$)	ln(inc)	$Employed$	$Hours$ $worked$	$SNAP$ $take-up$	$Welfare$ $take-up$
Raw Scandal Measure						
S_{-5-0}	-0.061	-0.045	-0.024*	-0.418	-0.050***	-0.031**
p-value	[0.17]	[0.21]	[0.09]	[0.26]	[0.001]	[0.017]
No. of Simulations	500	500	500	500	500	500

Note: This table reports the estimates and the associated p-value from a simulation of 500 matching exercises. *** p<0.01, ** p<0.05, * p<0.1



Notes: Figure A.1 shows outcomes for refugees enrolled in the Matching Grant program in fiscal years 2010 through 2015, comparing UCCSB clients to those settled by all other VOLAGs.

Figure A.1: Resettlement outcomes from Matching Grant data

Table A.6: Effects of Catholic scandal revelations on refugee outcomes: dropping Cubans

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var.:	ln($wage$)	ln(inc)	$Employed$	$Hours$ $worked$	$SNAP$ $take-up$	$Welfare$ $take-up$
Panel A: Raw Scandal Measure						
S_{-5-0}	-0.137***	-0.101***	-0.024	-0.853***	-0.054***	-0.061***
	(0.017)	(0.037)	(0.171)	(0.179)	(0.005)	(0.000)
Observations	2,640	3,148	4,358	4,358	4,358	4,358

Note: This table replicates the main results in the analysis after dropping Cuban refugees. *** p<0.01, ** p<0.05, * p<0.1

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