

RESETTLEMENT AGENCY RESOURCE STRAIN AND REFUGEE OUTCOMES: EVIDENCE FROM CATHOLIC SEX ABUSE SCANDALS

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ABSTRACT. In partnership with government agencies, voluntary organizations have provided key resources to successfully resettle millions of refugees in the United States over the past four decades. However, the structure of the U.S. resettlement program leaves refugees exposed to variability in the resources and general capacity of non-governmental partner entities to support them. In this paper, we study the effect of partner resettlement organization support 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. When evaluating mechanisms, we discuss the impact of scandals on financial and non-financial support at partner resettlement organizations, who rely on both types of resources to help refugees access public social safety net programs. With diminished resource capacity, partner entities are unable to offer comprehensive services to this vulnerable population.

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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 (Connor and 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

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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 et al., 2004; Åslund, Edin, Fredriksson and Grönqvist, 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 (CCA, 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

¹Blinken (2021)

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 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 (Gjelten, 2018). Beyond the direct finances of the Catholic Church, Church-affiliated charities have also experienced large declines in private contributions and participation (Crary, 2019; Bottan and 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 (*Bishop Accountability*, 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 et al., 2004; Åslund, Edin, Fredriksson and Grönqvist, 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). 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

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

discusses the mechanisms underlying our results, Section 7 highlights our robustness checks, and Section 8 concludes.

2. BACKGROUND

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 Nations 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.

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

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

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 (?), 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 et al. (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. DATA

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 (*Bishop Accountability*, 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.

3.2. 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 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

⁴We 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.

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 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-year 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 be 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 refugees. This may increase the risk of false positives relative using the RCR alone. Table A.4 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.

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:

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

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

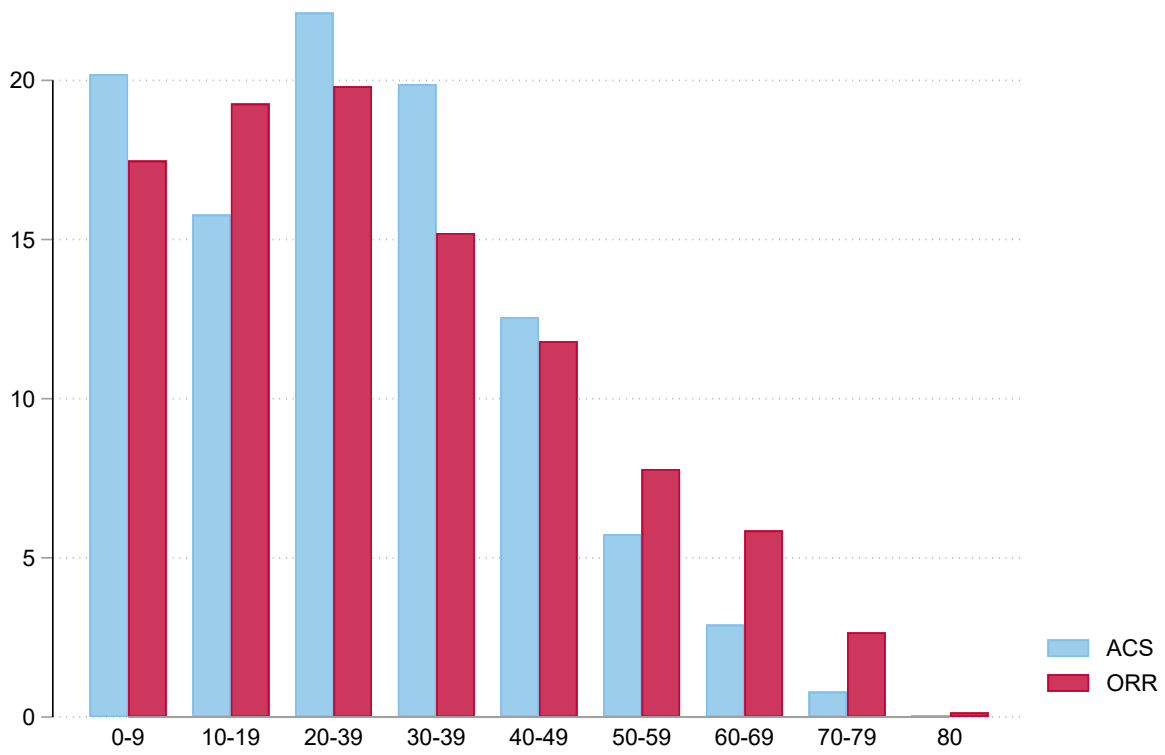
Beyond our main measures of interest, we control for year of arrival fixed effects α_τ , diocese fixed effects γ_d , and observation year fixed effects λ_t . We also control for individual characteristics X_i such as gender, age, education, English speaking ability, marital status, country of origin, and race. Finally, we include measures at the state-year level η_{st} , such

⁵Another 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.

⁶We 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.

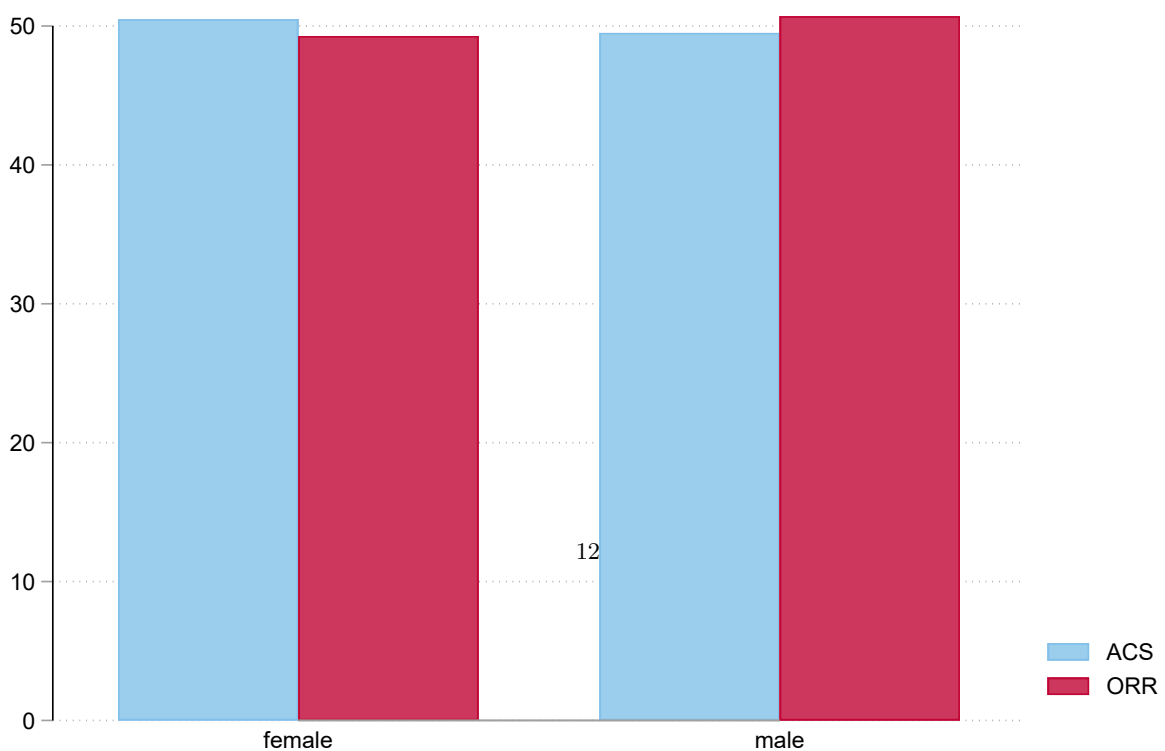
0.49

FIGURE 1. Age



0.49

FIGURE 2. Sex



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:

$$(2) \quad Y_{dti} = \beta_0 + \frac{CA}{TA_d} \beta_1 S_{-5-0,dt} + \alpha_\tau + \gamma_d + \lambda_t + X_i + \epsilon_{dti}$$

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).

5. RESULTS

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 1, 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.

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 1, 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.

⁷In 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.

TABLE 1. 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) $Welfare$ $take - up$
<i>Panel A: Raw Scandal Measure</i>						
S_{-5-0}	-0.103** (0.045)	-0.078** (0.039)	-0.021 (0.015)	-0.530 (0.586)	-0.045*** (0.016)	-0.058*** (0.012)
Observations	2,640	3,148	4,358	4,358	4,358	4,358
<i>Panel B: Affiliate-Scaled Scandal Measure</i>						
S_{-5-0}	-0.268** (0.120)	-0.175 (0.118)	-0.041 (0.041)	-0.689 (1.589)	-0.129*** (0.042)	-0.163*** (0.016)
Observations	2,617	3,122	4,323	4,323	4,323	4,323
<i>Panel C: Population-Scaled Scandal Measure</i>						
S_{-5-0}	-0.330*** (0.116)	-0.219** (0.107)	-0.056 (0.043)	-1.535 (1.655)	-0.109** (0.050)	-0.140** (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. Robust standard errors are clustered at the diocese level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

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.10.

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 A.4 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 1 and A.4 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

⁸In 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% (?). 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 (?).

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.

TABLE 2. Likely Refugees (>80% arrivals from sending country are refugees)

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

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 A.8 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 A.4. Notably however, the coefficients on SNAP take up are small and positive, contrasting with the results in other specifications.

TABLE 3. Placebo: Unlikely Refugees (<20% arrivals from sending country are refugees)

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

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.

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 4 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.

TABLE 4. Effects of Catholic Scandal Revelations on Volunteer Service

Dep. Var.:	(1)	(2)
	Overall volunteer service	Immig./Refugee assistance
<i>Panel A: Raw Scandal Measure</i>		
S_{-5-0}	-0.011 (0.023)	-0.002* (0.001)
<i>Panel B: Affiliate-Scaled Scandal Measure</i>		
S_{-5-0}	-0.006 (0.028)	-0.004*** (0.001)
<i>Panel C: Population-Scaled Scandal Measure</i>		
S_{-5-0}	0.004 (0.060)	-0.005 (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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

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.

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.

7. ROBUSTNESS

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 7 and 12. We find that the mean of the point estimates from the 500-simulation reported in A.12 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.

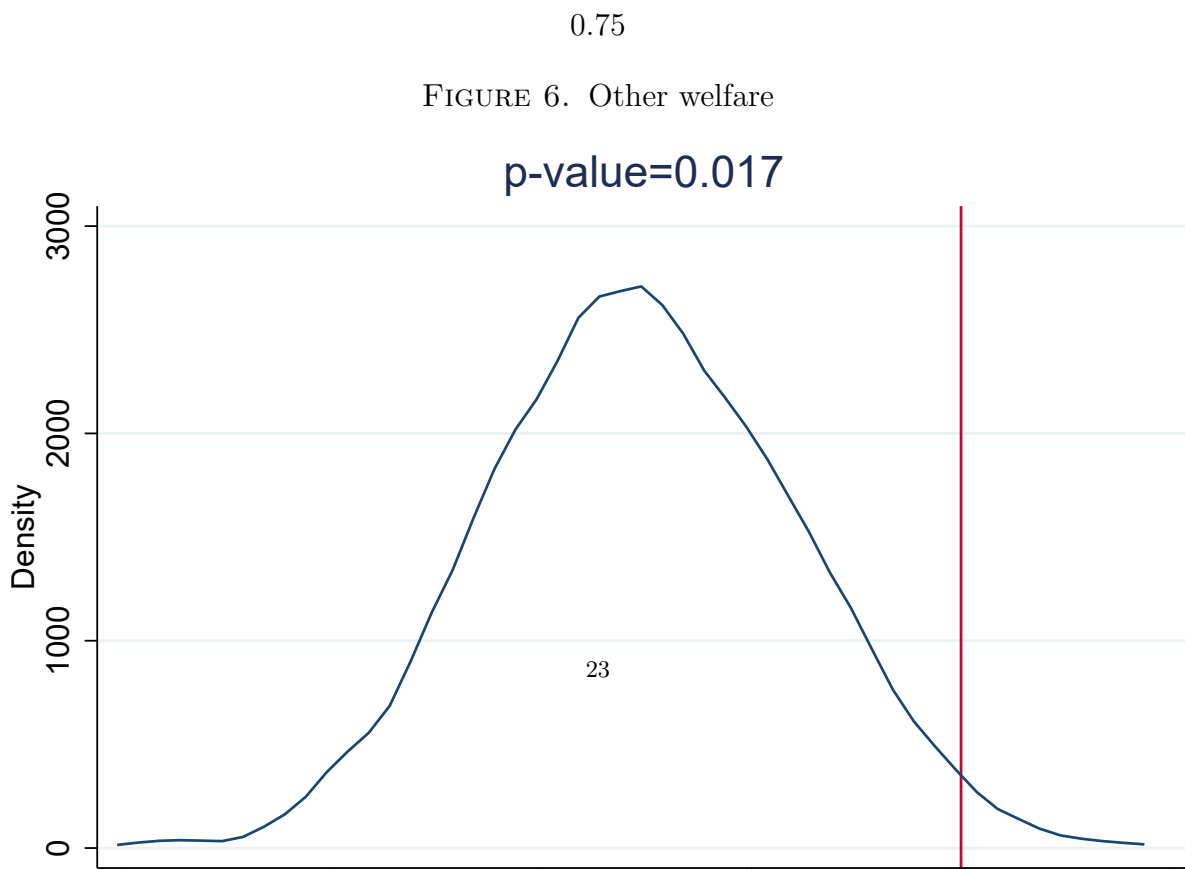
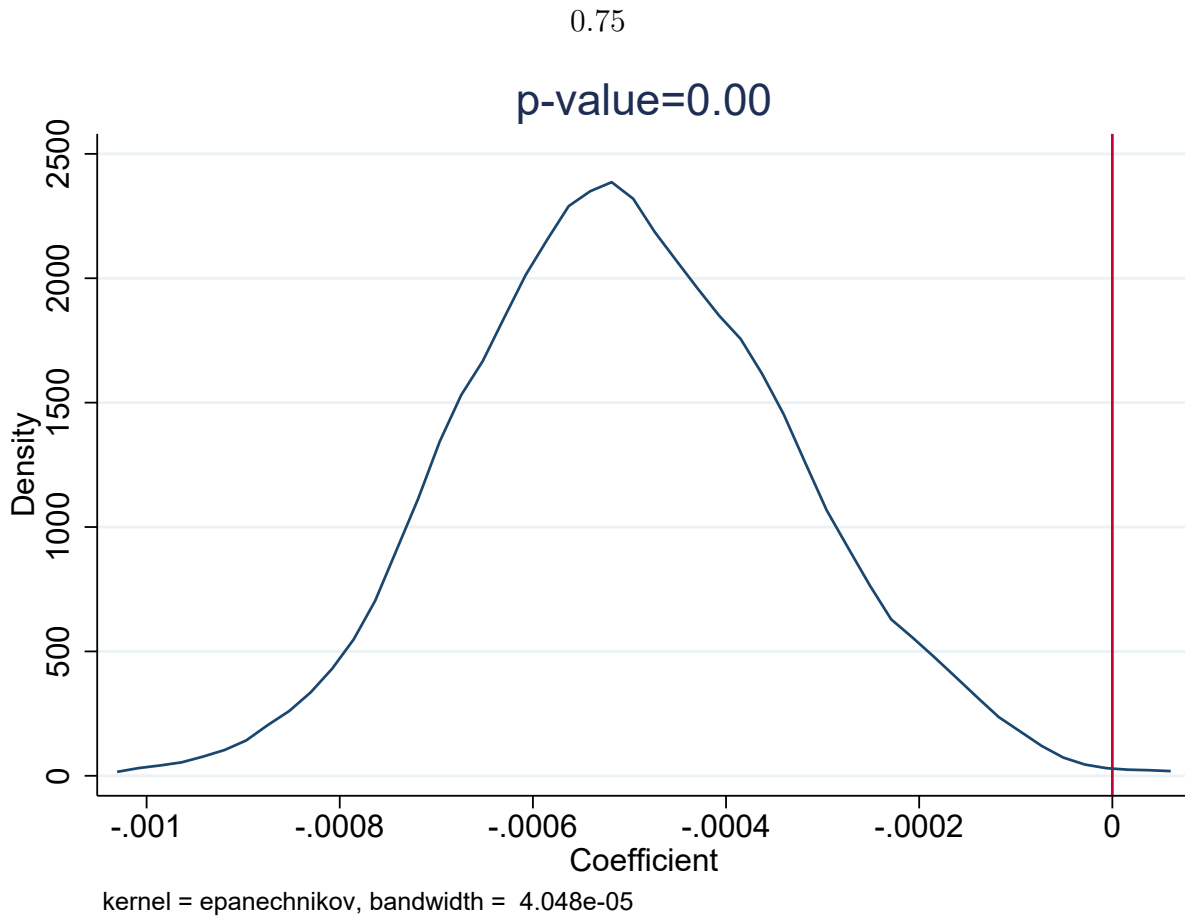
7.2. Dropping Cuban Refugees. As discussed in the data section, Cuban refugees make up 25% of refugees identified in the ACS. This group makes up the largest share of refugees when describing refugees by country of origin. To test the robustness of our results, in this section we drop Cuban refugees and rerun our primary analysis. The results are reported in Table A.13. 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.

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.

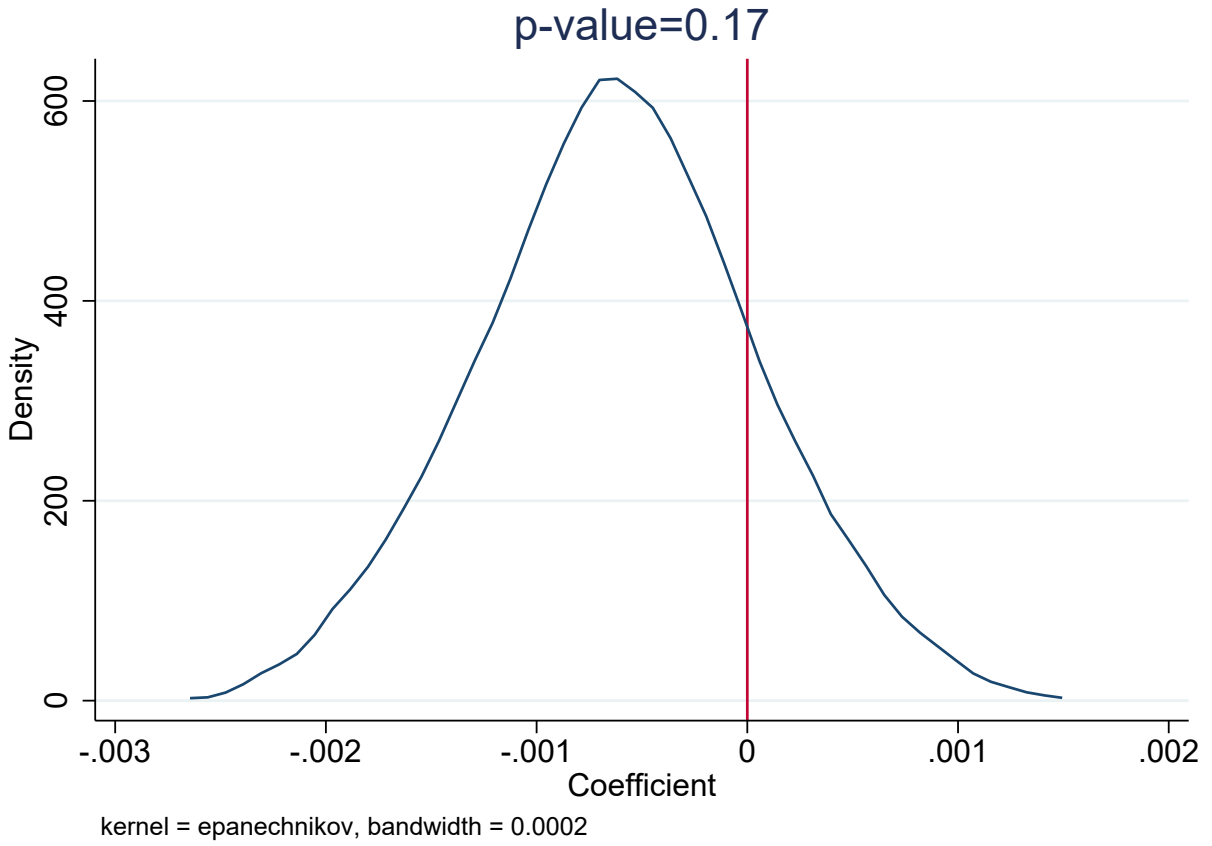
Our results suggest that resource shocks to VOLAGs lead to a reduction in federal social safety net program participation in the short run. We find that this reduction in public welfare take-up is not due to improving labor market outcomes; rather, while not conclusive, we find suggestive evidence that 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.



(a)
b

0.49

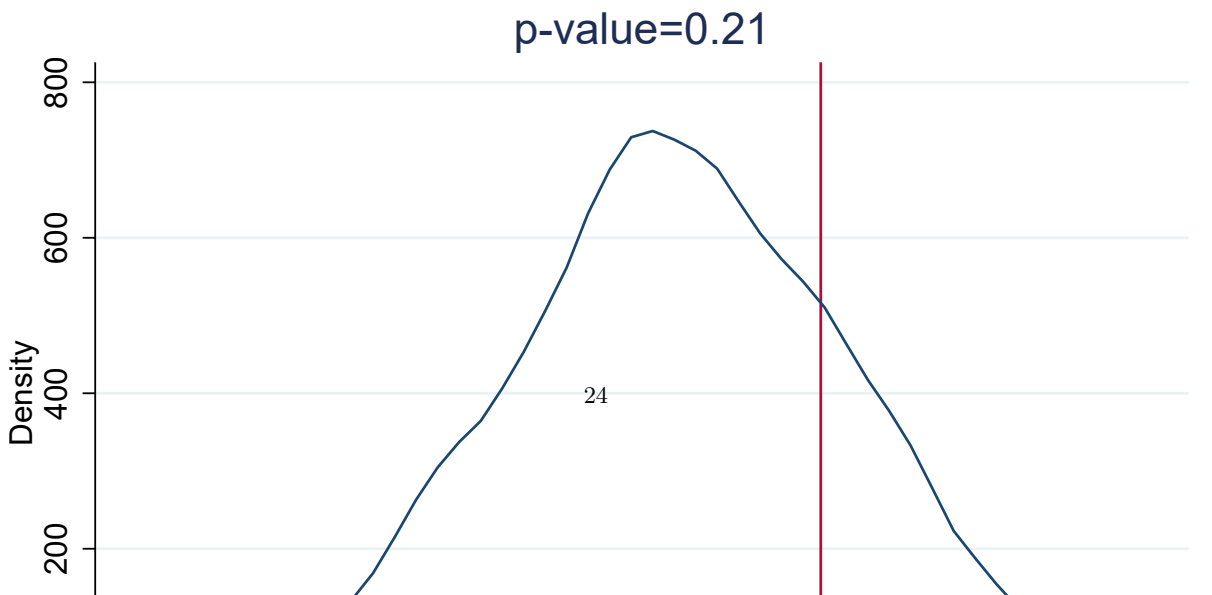
FIGURE 8. Log Wage



(a)
b

0.49

FIGURE 9. Log Income



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).

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APPENDIX A. 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

Dep. Var.:	(1) $\ln(wage)$	(2) $\ln(inc)$	(3) <i>Employed</i>	(4) <i>Hours worked</i>	(5) <i>SNAP</i>	(6) Take-up of: <i>Welfare</i>	(7) <i>Scandals</i> (0 – 5)
<i>Panel A: Full Sample</i>							
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
<i>Panel B: Employed Sample</i>							
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. Likely Refugees (>80% arrivals from sending country are refugees)

Dep. Var.:	(1) ln(<i>wage</i>)	(2) ln(<i>inc</i>)	(3) <i>Employed</i>	(4) <i>Hours worked</i>	(5) <i>SNAP take – up</i>	(6) <i>Welfare take – up</i>
<i>Panel A: Raw Scandal Measure</i>						
S_{-5-0}	1.332*** (0.438)	4.89 (0.596)	-0.207 (0.133)	-3.469 (7.439)	-0.322*** (0.116)	-0.086 (0.126)
Observations	1,094	1,654	1,654	1,654	1,654	1,654
<i>Panel B: Affiliate-Scaled Scandal Measure</i>						
S_{-5-0}	1.918 (1.1327)	1.330 (1.473)	-0.229 (0.258)	3.752 (13.354)	-1.173*** (0.299)	-0.568** (0.230)
Observations	1,093	1,262	1,650	1,650	1,650	1,650
<i>Panel C: Population-Scaled Scandal Measure</i>						
S_{-5-0}	4.203*** (1.268)	0.686 (1.413)	-0.693** (0.282)	-0.17270 (17.824)	-0.730** (0.359)	-0.101 (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 A.4. Likely Refugees (>80% arrivals from sending country are refugees), excluding Cuba

Dep. Var.:	(1) $\ln(wage)$	(2) $\ln(inc)$	(3) $Employed$	(4) $Hours$ $worked$	(5) $SNAP$ $take - up$	(6) $Welfare$ $take - up$
<i>Panel A: Raw Scandal Measure</i>						
S_{-5-0}	-0.611467 (1.757512)	-0.672270 (0.644425)	-0.538086** (0.205852)	-22.865578*** (8.192701)	-0.075326 (0.177371)	0.092363 (0.193553)
Observations	220	257	327	327	327	327
<i>Panel B: Affiliate-Scaled Scandal Measure</i>						
S_{-5-0}	-2.191077 (3.142202)	-3.043791 (3.169096)	-1.009305 (0.813555)	-44.032457 (32.877498)	-1.192608 (0.911074)	0.673455 (0.580369)
Observations	219	255	325	325	325	325
<i>Panel C: Population-Scaled Scandal Measure</i>						
S_{-5-0}	-2.783477 (9.531324)	-1.144821 (1.024621)	-1.153468*** (0.392399)	-48.442042*** (16.323531)	-0.178657 (0.324224)	0.135490 (0.372558)
Observations	220	257	327	327	327	327

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 A.5. Likely Refugees (>70% arrivals from sending country are refugees)

Dep. Var.:	(1) ln(<i>wage</i>)	(2) ln(<i>inc</i>)	(3) <i>Employed</i>	(4) <i>Hours worked</i>	(5) <i>SNAP take – up</i>	(6) <i>Welfare take – up</i>
<i>Panel A: Raw Scandal Measure</i>						
S_{-5-0}	1.249*** (0.410)	0.590 (0.494)	-0.199* (0.104)	-2.967 (5.810)	-0.266*** (0.091)	-0.075 (0.087)
Observations	1,191	1,374	1,801	1,801	1,801	1,801
<i>Panel B: Affiliate-Scaled Scandal Measure</i>						
S_{-5-0}	1.724 (1.205)	1.435 (1.343)	-0.352 (0.238)	-0.830 (12.155)	-0.808*** (0.275)	-0.360* (0.208)
Observations	1,190	1,372	1,796	1,796	1,796	1,796
<i>Panel C: Population-Scaled Scandal Measure</i>						
S_{-5-0}	4.177*** (1.136)	1.085 (1.131)	-0.581*** (0.211)	-12.097 (13.679)	-0.766*** (0.207)	-0.123 (0.210)
Observations	1,191	1,374	1,801	1,801	1,801	1,801

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 70% 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 A.6. Likely Refugees (>60% arrivals from sending country are refugees)

Dep. Var.:	(1) ln(<i>wage</i>)	(2) ln(<i>inc</i>)	(3) <i>Employed</i>	(4) <i>Hours worked</i>	(5) <i>SNAP take – up</i>	(6) <i>Welfare take – up</i>
<i>Panel A: Raw Scandal Measure</i>						
S_{-5-0}	0.346 (0.625)	0.371 (0.501)	-0.110 (0.101)	-0.245 (4.828)	-0.086 (0.119)	-0.002 (0.073)
Observations	1,347	1,552	2,029	2,029	2,029	2,029
<i>Panel B: Affiliate-Scaled Scandal Measure</i>						
S_{-5-0}	0.361 (1.452)	0.997 (1.316)	-0.129 (0.252)	7.204 (9.384)	-0.392 (0.299)	-0.175 (0.178)
Observations	1,342	1,546	2,020	2,020	2,020	2,020
<i>Panel C: Population-Scaled Scandal Measure</i>						
S_{-5-0}	0.739 (1.968)	0.654 (1.162)	-0.337 (0.248)	-4.036 (13.372)	-0.416* (0.220)	0.007 (0.165)
Observations	1,347	1,552	2,029	2,029	2,029	2,029

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 60% 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 A.7. Placebo: Unlikely Refugees (<30% arrivals from sending country are refugees)

Dep. Var.:	(1)	(2)	(3)	(4)	(5)	(6)
	$\ln(wage)$	$\ln(inc)$	<i>Employed</i>	<i>Hours worked</i>	<i>SNAP take – up</i>	<i>Welfare take – up</i>
<i>Panel A: Raw Scandal Measure</i>						
S_{-5-0}	0.104 (0.090)	0.105 (0.094)	0.001 (0.022)	0.056 (0.793)	-0.078*** (0.019)	-0.068*** (0.012)
Observations	635	785	1,159	1,159	1,159	1,159
<i>Panel B: Affiliate-Scaled Scandal Measure</i>						
S_{-5-0}	0.169 (0.200)	0.176 (0.218)	0.017 (0.061)	0.075 (2.077)	-0.176*** (0.045)	-0.169*** (0.033)
Observations	627	777	1,151	1,151	1,151	1,151
<i>Panel C: Population-Scaled Scandal Measure</i>						
S_{-5-0}	0.313 (0.272)	0.289 (0.270)	0.001 (0.064)	0.174 (2.197)	-0.219*** (0.054)	-0.185*** (0.035)
Observations	635	785	1,159	1,159	1,159	1,159

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 30% 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 A.8. Placebo: Unlikely Refugees (<20% arrivals from sending country are refugees)

Dep. Var.:	(1)	(2)	(3)	(4)	(5)	(6)
	$\ln(wage)$	$\ln(inc)$	<i>Employed</i>	<i>Hours worked</i>	<i>SNAP take – up</i>	<i>Welfare take – up</i>
<i>Panel A: Raw Scandal Measure</i>						
S_{-5-0}	0.167 (0.167)	0.302*** (0.112)	-0.016 (0.039)	-1.387 (1.347)	0.050 (0.040)	-0.044 (0.049)
Observations	360	431	674	674	674	674
<i>Panel B: Affiliate-Scaled Scandal Measure</i>						
S_{-5-0}	0.380 (0.389)	0.730*** (0.262)	-0.013 (0.102)	-2.872 (3.382)	0.128 (0.098)	-0.106 (0.125)
Observations	356	427	670	670	670	670
<i>Panel C: Population-Scaled Scandal Measure</i>						
S_{-5-0}	0.468 (0.447)	0.825*** (0.298)	-0.046 (0.113)	-4.172 (3.884)	0.134 (0.107)	-0.118 (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 A.9. Placebo: Unlikely Refugees (<10% arrivals from sending country are refugees)

Dep. Var.:	(1) $\ln(wage)$	(2) $\ln(inc)$	(3) $Employed$	(4) $Hours$ $worked$	(5) $SNAP$ $take - up$	(6) $Welfare$ $take - up$
<i>Panel A: Raw Scandal Measure</i>						
S_{-5-0}	-0.184 (0.351)	-0.159 (0.267)	-0.402*** (0.126)	-13.727* (7.023)	0.379** (0.144)	0.074 (0.051)
Observations	213	257	436	436	436	436
<i>Panel B: Affiliate-Scaled Scandal Measure</i>						
S_{-5-0}	-0.243 (0.856)	-0.221 (0.640)	-0.906*** (0.290)	-26.155* (14.152)	0.831*** (0.249)	0.198* (0.104)
Observations	211	255	434	434	434	434
<i>Panel C: Population-Scaled Scandal Measure</i>						
S_{-5-0}	-0.268 (0.880)	-0.144 (0.685)	-1.109*** (0.243)	-40.488** (16.680)	0.808** (0.330)	0.136 (0.124)
Observations	213	257	436	436	436	436

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 10% 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 A.10. Robustness of Scandal Revelation Effects on Refugee Outcomes
Using Different Scandal Revelation Time Groupings

Dep. Var.:	(1) $\ln(wage)$	(2) $\ln(inc)$	(3) <i>Employed</i>	(4) <i>Hours worked</i>	(5) <i>SNAP take – up</i>	(6) <i>Welfare take – up</i>
<i>Panel A: Raw Scandal Measure</i>						
S_{-4-0} Coef.	-0.128***	-0.106***	-0.026*	-0.867	-0.047***	-0.059***
<i>p</i> -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***
<i>p</i> -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***
<i>p</i> -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.11. Robustness of Affiliate-Scaled Scandal Measure Effects on Refugee Outcomes

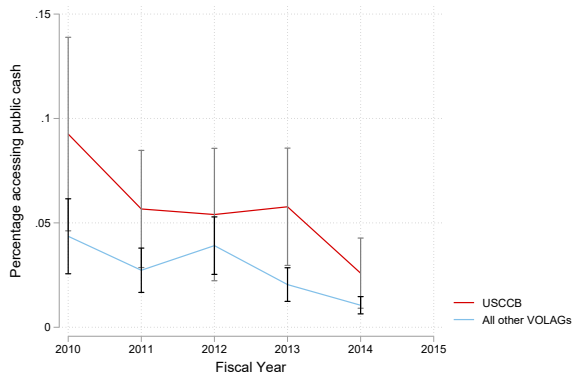
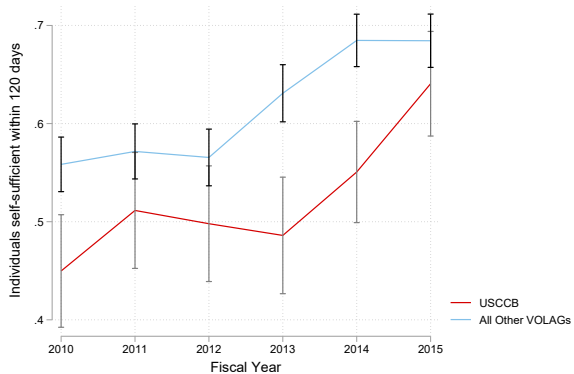
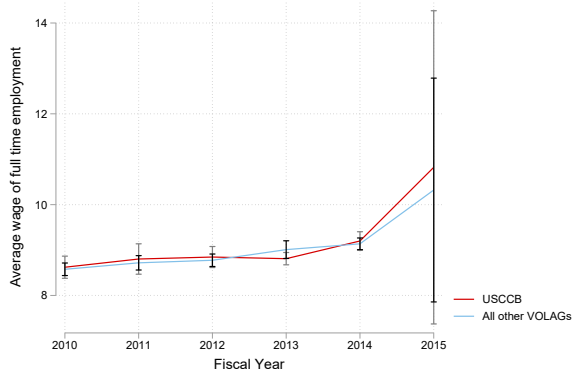
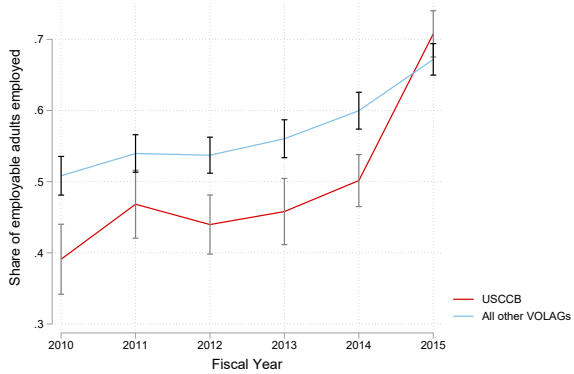
	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var.:	$\ln(wage)$	$\ln(inc)$	<i>Employed</i>	<i>Hours worked</i>	<i>SNAP take – up</i>	<i>Welfare take – up</i>
<i>Panel A: Scandals Scaled by Affiliates Listed in Matching Grant Recipient Data</i>						
S_{-5-0}	-0.247**	-0.203**	-0.031	-0.678	-0.126***	-0.122***
	(0.099)	(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.12. Simulation of Matching Refugees: Effects of Catholic Scandal Revelations on Refugee Outcomes

Dep. Var.:	(1) $\ln(wage)$	(2) $\ln(inc)$	(3) <i>Employed</i>	(4) <i>Hours worked</i>	(5) <i>SNAP take – up</i>	(6) <i>Welfare take – up</i>
<i>Raw Scandal Measure</i>						
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 USCCB clients to those settled by all other VOLAGs.

FIGURE A.1. Resettlement outcomes from Matching Grant data

TABLE A.13. Effects of Catholic Scandal Revelations on Refugee Outcomes by Dropping Cubans

Dep. Var.:	(1) $\ln(wage)$	(2) $\ln(inc)$	(3) <i>Employed</i>	(4) <i>Hours worked</i>	(5) <i>SNAP take-up</i>	(6) <i>Welfare take-up</i>
<i>Panel A: Raw Scandal Measure</i>						
S_{-5-0}	-0.137*** (0.017)	-0.101*** (0.037)	-0.024 (0.171)	-0.853*** (0.179)	-0.054*** (0.005)	-0.061*** (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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$