The Effect of Source of Income Policies on Recipients of Non-Voucher Categories of Protected Income

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Abstract

Source of income (SOI) policies prohibit landlords from discriminating against rental housing applicants based on their reported income sources. Such policies have been enacted in more than 20 states and 100 local jurisdictions. While prior research has focused on the impact of these policies on Housing Choice Voucher recipients, this paper provides the first examination of their effects on recipients of non-voucher protected income sources including SSI and welfare. Using American Community Survey data from 2006 to 2021 and a robust event-study framework, I find null effects on mobility rates following SOI policy enactment. Point estimates are uniformly close to zero with sufficient precision to rule out mobility increases exceeding 1-2 percentage points, representing relative increases of 10%-13% given baseline mobility rates of 22%. I also find no effects on average rents, household size, or crowding. These estimates suggest SOI policies are not associated with large-scale changes in housing outcomes for non-voucher protected income recipients. Because some protected income recipients may elect to avoid reporting this income on rental applications, these estimates reflect intent-to-treat effects. Under a range of plausible income disclosure rates, I cannot rule out economically meaningful effects for those renters who do report such income.

Keywords: Source of Income, Housing Discrimination, Welfare, Residential Mobility
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Declaration of Interest

The author reports there are no competing interests to declare.

List of Abbreviations Used in Manuscript

- American Community Survey (ACS)
- Consistent Public Use Microdata Area (ConsPUMA)
- Housing Choice Voucher (HCV)
- Protected Income (PI)
- Source of Income (SOI)
- Supplemental Security Income (SSI)
- Temporary Assistance for Needy Families (TANF)

1 Introduction

Landlords regularly screen rental housing applicants on the basis of income. In addition to ensuring that applicants have sufficient resources to make regular rental payments, landlords also consider the *source* of applicants' income when making lease decisions. In qualitative and survey research, some landlords express a hesitancy to extend lease offers to applicants who report income from public assistance programs (Garboden et al., 2018; Cunningham et al., 2018; Tighe et al., 2017). In response to these discriminatory practices, more than 20 states and 100 county and local jurisdictions in the United States have enacted source of income (SOI) policies. These policies prohibit landlords from screening rental housing applicants on the basis of their reported income sources. Advocates of such policies argue that they expand the range of housing opportunities available to vulnerable populations and reduce residential segregation (Poverty & Race Research Action Council, 2020). However, opponents of these policies argue that they impose administrative burdens on landlords and restrict their ability to effectively screen applications (NMHC/NAA Joint Legislative Program, 2014).

A growing literature has explored the effect of SOI policies on Housing Choice Voucher (HCV) program participants (Freeman and Li, 2014; Ellen et al., 2022). SOI policies, however, cover a wide range of other public income sources, including Social Security, Supplemental Security Income (SSI), and other public assistance programs. This paper contributes to the literature on SOI policies by examining the impact of such policies on the broader population of non-HCV protected income (PI) recipients. I focus on recipients of SSI and public assistance programs such as Temporary Assistance for Needy Families (TANF). Beneficiaries of these and other public assistance programs constitute a substantially larger population than HCV recipients. In 2022, more than 7.5 million Americans received benefits from SSI (Social Security Administration, 2023) and 2.5 million people received benefits from TANF (Brown, 2025). By contrast, roughly 5.2 million people participated in the HCV program in the same year (USAFacts, 2023). Understanding the impact of SOI policies on the broader population of PI recipients is crucial for evaluating the overall effectiveness and potential unintended consequences of these policies. These populations may face different types or degrees of rental housing discrimination and their incentives for information disclosure during the application process may differ from those of HCV participants. Structural differences in how these programs operate may also lead to differential effects of SOI policies across recipient populations. Insofar as existing rental housing discrimination may limit the housing options available to non-voucher PI recipients, the enactment of such policies may generate increases in mobility as individuals with PI move to housing that better matches their preferred mix of characteristics.

Using data from the American Community Survey (ACS), I explore the relationship between SOI policies and measures of mobility and household characteristics among renters likely to be affected by such policies in an event-study framework. Using data from 2006 to 2021, I employ several definitions for renters potentially affected by SOI policies, focusing on individuals who report income from either SSI or welfare programs.[1] I map all outcomes to the Consistent Public Use Microdata Area (ConsPUMA)-by-year level and merge these records with information on the enactment of SOI policies across jurisdictions in the United States. The resulting sample comprises data on more than 11.4 million renters receiving some form of PI in 2021, mapped to 1,078 ConsPUMAs across the United States.

I use a modern event-study estimator proposed by de Chaisemartin and D'Haultfœuille (2024) that is robust to methodological concerns about the estimation of event-study models via OLS. I show that key outcomes are not trending differentially before policy enactment in jurisdictions that eventually adopt SOI policies, supporting a causal interpretation of the research design. I report null effects across a range of specifications and subgroups on residential mobility rates following the enactment of SOI policies. I also find no effect of SOI policies on average rents, household size, or crowding.

These null effects for mobility are uniformly close to 0 and estimated with sufficient precision to rule out mobility increases of more than 1-2 percentage points. Given average mobility rates among renters with protected income of roughly 22%, this implies ruling out relative increases in mobility greater than 10% to 13%. However, because SOI disclosure behavior is not observable in traditional surveys such as the ACS, and some PI recipients may elect not

[1] While the ACS includes information on income from other sources such as retirement benefits, which in principle are protected by SOI policies, the demographic composition and propensity for experiencing SOI discrimination among households with substantial earnings from these sources likely differs dramatically from the income sources considered here. I do, however, specifically consider individuals receiving Social Security payments as part of the results presented below.

to disclose the source of their income on rental housing applications, these estimates represent intent-to-treat effects. I conduct an exercise in which I scale these upper-bound mobility estimates by a plausible range of PI disclosure rates. Here, I find that I cannot rule out economically meaningful increases in mobility for the subset of renters who do disclose PI. Finally, because HCV recipiency is not reported in the ACS, I define a sample of renters who are less likely to be HCV recipients and find a similar pattern of results.

This study contributes to the growing literature on SOI policies by being the first paper to my knowledge to explore the impact of such policies on recipients of non-HCV sources of PI. This population comprises millions of renters whose response to SOI policies may differ substantially from HCV renters. The results presented below suggest that SOI policies are not associated with large-scale changes in the mobility or rental housing characteristics of likely-affected renters with PI. However, this does *not* mean that such policies are ineffective. Instead, these results provide an upper bound on the likely effect of such policies and provide important context for policymakers aiming to address rental housing discrimination. Future research using audit study methods or administrative data linking rental applications to mobility outcomes could shed light on the disclosure decisions and subsequent rental housing outcomes of PI recipients.

In Section 2, I describe the rental housing market and SOI policies, as well as the existing academic literature on these policies. Section 3 discusses the data, identification of affected renters, policy mapping, and event-study methodology. In Section 4, I present the results and contextualize them under various assumptions about applicant disclosure behavior. Finally, Section 5 concludes.

2 Background

Applicants for rental housing are required to submit applications to prospective landlords. Such applications generally ask applicants to report their income to ensure that they can make regular rental payments. In reporting their income, applicants are often required to disclose information about the source of their income, by listing employers, providing pay stubs, etc. In addition to signaling the likely future reliability of earnings, and thus ability to pay rent, landlords view SOI as a proxy for other, unobservable characteristics such as general tenant desirability (Tighe et al., 2017; Rosen, 2020).

Different types of PI sources covered under SOI policies may elicit differing responses on the part of landlords. As a class of PI, HCVs differ from other PI categories in several important ways. First, accepting a rental application from an HCV recipient entails interacting with the local housing agency in order to receive subsidized rental payments. This process also entails inspections of rental housing units to ensure they meet program requirements (HUD, 2001). Qualitative literature suggests that for some landlords, these housing agency interactions can be time-consuming and otherwise onerous, and may prompt landlords to avoid renting to HCV holders (Greenlee, 2014). Landlords may also harbor concerns that HCV holders themselves may be undesirable tenants who may be more likely to engage in criminal activity or damage property. These perceptions have been documented across a number of qualitative and survey or field experiment-style studies (Cunningham et al., 2018; Garboden et al., 2018; Rosen, 2020).

For other protected classes of income, landlord perceptions are less well documented. In general, many of the tenant-specific concerns such as potential criminality and poor property upkeep that landlords raise regarding renting to HCV recipients also apply to other welfare program participants. In the case of disability-related benefits, the literature on discrimination against disabled applicants in rental housing process is informative. Here, Aranda (2015) finds that rental housing applicants in the United States who use a wheelchair or who are deaf or hard of hearing experience discrimination and frictions throughout the rental application process. In an international context, Fumarco (2017) and Verhaeghe et al. (2016) document discrimination against visually-impaired and blind applicants. Categories of PI such as SSDI, which signal the disability status of rental applicants, may experience similar discrimination.

A growing literature has explored the impact of SOI policies on HCV recipients and the local housing agencies which administer the HCV program. Freeman (2012) finds that SOI policies increased voucher utilization rates for local housing agencies. Freeman and Li (2014) provide suggestive evidence that SOI policies are associated with decreases in the average poverty rates in the Census tracts in which HCV recipients live. A more recent study by Ellen et al. (2022) finds that in the years following SOI policy adoption, HCV recipients who move tend to do so into neighborhoods with lower poverty rates. The research on SOI policy impacts on non-HCV populations is limited. One exception is Han (2024), who uses housing agency-level data to assess the impact of SOI policies on residents on public housing. This study finds that SOI policies are associated with a decline in the flow of new residents into public housing and a decline in the share of public housing residents that are in poverty.

3 Data and Methods

This section begins by describing the collection of information on SOI policies across the United States. I then describe the ACS data used for this study and the identification of individuals likely impacted by SOI policies. Finally, I summarize the event-study research design and estimator used to generate the results presented below.

3.1 SOI Polices Across the US

In order to record the enactment of SOI policies across jurisdictions in the United States, I use detailed records collected by Poverty & Race Research Action Council (2025) on SOI policies. This information, which has been widely cited in other recent studies on SOI policies, provides information on the enactment of SOI policies at the state, county, and city level. While these records are primarily concerned with policies covering HCVs, they also list SOI policies that either explicitly do not cover HCVs or which were later changed via amendment or judicial to remove HCV coverage. These records serve as the basis of SOI policy tables compiled by Bell et al. (2018) and Teles and Su (2022), against which the policy records for this study have been checked. For more discussion of SOI policies, and a full list of policies across jurisdictions, see Appendix Section A1.

In Figure 2, I show the annual count of enacted SOI policies across jurisdictions. In Table 1, I provide summary statistics for sample of SOI policies included in this study, with a focus on policies that were enacted during the 2006-2021 time period. Of the 145 total SOI policies enacted as of 2021 (Panel A), 100 were enacted between the 2006 and 2021 time period covered by the analysis sample describe below (Panel B). In Appendix Table A1, I list all jurisdictions with SOI policies enacted during the sample time period along with enactment years; in Appendix

Table A2, I list all jurisdictions with SOI policies enacted prior to the start of the sample period.

3.2 American Community Survey (ACS) Data

I begin by collecting data from the ACS for the years 2006 to 2021 (Ruggles et al., 2025). The analysis sample is restricted to adults ages 18 or older who rent their housing and pay cash rent. I define PI as any income received from SSI and welfare payments including TANF, General Assistance, and other welfare programs.[2] Using this definition of PI, I then define two samples of renters potentially affected by SOI policies: 1) those who report positive income from either PI source, and 2) those for whom PI accounts for 50% of more of their total reported personal income. In Table 2, I show that approximately 6% of renters report any PI, while 4% of renters have more than 50% of their income from protected sources.

HCV recipiency is not reported in the ACS and thus the samples above may include individuals who both receive PI from SSI/welfare *and* participate in the HCV program. Because the existing SOI literature has focused on HCV recipients, and this study is intended to expand the focus to the impact of these policies on the broader population of non-HCV PI recipients, I construct a sample of individuals who are potentially affected by SOI policies who also appear less likely receive vouchers. This sample is comprised of all adult renters with 1) incomes below the poverty line for whom 2) their gross rent accounts for more than 40% of their total monthly household income. In general, the HCV program requires recipients to pay 30% of their income towards monthly rent while vouchers cover the balance up to a specified limit. Families may elect to exceed this limit, but their rent burden must be 40% or less when they begin a lease (Ellen, 2020). Thus, this sample of renters includes individuals who are in poverty, and are thus potentially eligible for public assistance, but whose rent burden indicates they are less likely to receive HCVs than comparable renters in poverty with lower rent burdens.[3]

[2] While the Annual Social and Economic Supplement (ASEC) supplement of the Current Population Survey contains more detailed information on income, the ASEC lacks the sample sizes of the ACS and crucially offers less granular geographic identifiers than the ACS. The ability to observe ConsPUMAs in the ACS allows for including more SOI policy variation at the county and city levels; county identifiers are available for fewer than half of households in the IPUMS ASEC extracts.

[3] While in principle I could apply this rent burden restriction to the sample of individuals who report receiving PI, the imposition of additional restrictions on this population results in limited data availability across ConsPUMA-year cells. Additionally, by conditioning on poverty status but not on reported PI recipiency, this sample includes individuals who may have received public assistance but failed to report it to the ACS, thus expanding the scope of potentially-affected individuals relative to the primary two samples defined above.

In order to balance sample size constraints with the desire for geographic granularity, I aggregate the ACS data to the Consistent Public Use Microdata Area (ConsPUMA) by year level. These geographic units are formed by combining sets of Census-defined 2010 PUMAs that align with sets of 2000-vintage PUMAs. The 0010 version of ConsPUMAs used here are provided by IPUMS and provide the most granular geographic unit of measurement that is both available for all observations and consistently defined over the entire 2006-2021 sample period.[4] While county identifiers are available for a subset of observations in the ACS, this is generally limited to larger counties, and within-unit sample sizes are small. Because all observations in the ACS are matched to a ConsPUMA, this unit of analysis is best suited for my purposes.

For outcomes measured at the individual level, all ConsPUMA-by-year statistics are weighted using person-level weights, while household weights are used for household-level variables. In addition to measuring outcomes for likely-affected renters, I also generate a range of covariates describing the demographic composition of the overall renter population. The resulting analysis sample covers 1,078 ConsPUMAs across the years 2006 to 2021 and is summarized in Table 2.

3.3 Mapping SOI Policies to ConsPUMAs

ConsPUMAs nest cleanly within each state. When a state enacts an SOI policy in a given year, I count all ConsPUMAs within that state as treated by that policy. Mapping SOI policies enacted at the county or city level is less straightforward because the relationship between these geographic units depends on the population size of a given county or city. Larger jurisdictions may be divided into multiple ConsPUMAs, while smaller jurisdictions may be combined to form a single ConsPUMA. In order to account for these relationships, I map all jurisdictions with SOI policies to 2010-vintage PUMAs defined by the Census Bureau. Doing so allows me

[4]As of the time of writing, while ACS data is available via IPUMS through the year 2023, the `CPUMA0010` variable which identifies ConsPUMAs is only available through 2021, and thus this is the final year included in the sample. Because ConsPUMAs and 2020-vintage PUMA boundaries are not the same, attempting to extend the panel through 2023 using 2020-vintage PUMA boundaries for 2022 and 2023 presents difficulties in terms of consistently identifying treated areas over time (i.e., a jurisdiction that treats a given ConsPUMA during the 2006-2021 period may be included in a separate 2020-vintage PUMA leaving the other jurisdictions in its prior ConsPUMA potentially untreated).

to leverage population shares provided by the Missouri Census Data Center (MCDC). These shares report the fraction of each 2010-vintage PUMA's population that is located within each treated jurisdiction. I consider a 2010-PUMA treated by a given SOI policy if the jurisdiction enacting that policy accounts for 50% or more of that 2010-PUMA's population. I then use a crosswalk provided by IPUMS to map policies to the ConsPUMA level.

Finally, I create a ConsPUMA-by-year level measure of SOI policy treatment status covering the years 2006 to 2021. I consider a given ConsPUMA treated by the *first* SOI policy which is enacted within its borders at any jurisdictional level. I allow this SOI treatment measure to be non-absorbing (i.e., switch from 1 to 0) in the event in the event that a jurisdiction enacts an SOI policy which is later removed or preempted. If this removal happens within the next calendar year, as in the case of Austin and Dallas, TX, which implemented local SOI policies in 2014 and 2015 that were preempted by state legislation in 2015, I do *not* consider those areas treated by those policies. When a removal or preemption occurs more than one calendar year after the policy's initial enactment, I consider that area treated for the duration of time between enactment and removal. In Figure 1, I show the spatial distribution of SOI policies across the United States as of the end of 2024 with policies mapped to the ConsPUMA level.

3.4 Empirical Strategy

In order to estimate the effects of SOI policies on renters with protected sources of income, I use an event-study style research design. Using data aggregated to the ConsPUMA c by year y level, I consider the following general event-study specification:

$$Y_{cy} = \alpha_0 + \sum_{j=-m}^{m} \alpha_j \mathit{SOI\ Policy}_{c,t+j} + X'_{cy}\beta + \gamma_c + \tau_y + \epsilon_{cy} \tag{1}$$

Where Y_{cy} is the ConsPUMA-by-year level outcome of interest and γ_c and τ_y denote ConsPUMA and year fixed effects, respectively. $\mathit{SOI\ Policy}_{c,t+j}$ is an event-time indicator indexed by j relative to the enactment of an SOI policy in ConsPUMA c and year t. In the main results presented below, I adopt an event-time window of $m = 3$ years before and after the enactment of an SOI policy. I also include a vector of time-varying covariates, X_{cy}, including average employment and poverty rates, as well as shares of the renter population in each ConsPUMA and year that are non-white, high school graduates, and over the age of 65, respectively.

I use the robust estimator developed by de Chaisemartin and D'Haultfœuille (2024), which I refer to as the dCDH below, to generate all event-study results. This estimator addresses the potential pitfalls of estimating two-way, fixed-effects event-study and difference-in-differences models via Ordinary Least Squares (Goodman-Bacon, 2021). A substantial number of estimators have been developed in recent years to mitigate these concerns. However, the dCDH estimator is particularly well-suited to this empirical setting because it can handle binary, non-absorbing treatments. This is important because judicial rulings and state-level amendments can remove SOI policies. While I present Equation (1) to fix ideas, in practice, the dCDH estimator as with most modern comparable estimators constructs event-time estimates of group-specific difference-in-differences (DiD) comparisons across event time. For further discussion of the dCDH estimator, see Appendix Section A2.

In the event-study results presented below, I restrict the sample of treated ConsPUMAs to only those that can identify all pre- and post-treatment event-time coefficients, in addition to never-treated ConsPUMAs that do not enact SOI policies. Using the dCDH estimator, estimating 3 years of pre-treatment event-time coefficients requires 4 years of pre-treatment data because each coefficient is estimated relative to the time period immediately before a given ConsPUMA c is treated. This means the baseline event-study sample includes ConsPUMAs treated after 2010. In order to estimate 3 years of post-treatment coefficients, ConsPUMAs must be treated in or before 2019. In Table 1, I summarize time variation in the enactment of SOI policies; in Panel B, I show the number of ConsPUMAs contributing identifying variation to the baseline specification given by Equation (1) above. There are 59 SOI policies enacted between 2010 and 2019 treating 136 ConsPUMAs across 12 states; there are 508 ConsPUMAs that are not treated by an SOI policy during the entire 2006-2021 sample period.

4 Results

In Table 3, I consider the effect of SOI policies on the proportion of renters who report moving in the past year. Insofar as renters receiving PI may experience housing discrimination prior to the enactment of SOI policies, their menu of housing options pre-policy may be constrained. If such policies alleviate housing discrimination, then following their enactment we may expect increases in mobility as renters take advantage of a broader menu of available housing options and move to housing that better fits their needs and preferences.

Following the discussion in Section 3.2 above, I consider several definitions for renters potentially affected by such policies. In Column (1), the sample is restricted to adult renters who report PI from either SSI or welfare over the past year. In Column (2) of Table 3, the sample is further restricted to renters for whom such income accounts for 50% or more of their total personal income. In Panel A, I show event-study estimates following Equation (1). The p-value from a joint significance test of the pre-treatment coefficients joint difference from 0 is greater than 0.50 in both columns. This provides evidence that mobility was not trending differentially in treated as compared to control ConsPUMAs prior to SOI policy enactment and bolsters a causal interpretation of the post-treatment estimates. In the post-treatment period, I find point estimates that are uniformly close to 0 and not statistically significant; the average post-treatment effect is summarized in Panel B. In Figure 3, I show corresponding event-study graphs. Columns (1) and (2) of Table 3 suggest that SOI policies are not associated with significant increases in mobility for PI recipients. In Column (3), I consider a sample of renters who are potentially affected by SOI policies, but who are less likely to receive vouchers compared to renters with lower rent burdens, as described in Section 3.2 above. This sample is comprised of individuals who have incomes below the poverty line and rent burdens in excess of 40%. Here, as in the first two columns, I find no evidence that SOI policies are associated with significant changes in mobility for this sample of renters.

4.1 Placing Mobility Results in Context

The estimates from Table 3 suggest that SOI policies are not associated with large changes in overall mobility across likely-affected renters. In Table 4, I provide context for these results by assessing their precision and interpreting these estimates as intent-to-treat effects. In Panel A, I begin by framing these mobility results in terms of the upper bound effect sizes which I can rule out based on the precision of the post-treatment average estimates from Table 3, Panel B. By dividing the upper bound of each 95% confidence interval (CI) by the respective mobility rates within each sample, I report upper bound effect sizes in percentage terms for each sample in the final column. The precision of these mobility estimates is such that I can rule out increases in mobility larger than 13.1% in the case of individuals with any PI, 10.5% for those for whom PI accounts for 50% or more of their total income, and 5.9% for the rent-burdened, poverty-based sample.

In the second panel of Table 4, I conduct an exercise in which I scale the estimated upper bound effect sizes from Panel A by the proportion of individuals within each sample who actually disclose PI on rental applications. The motivation for this exercise is that individuals may choose to avoid disclosing their PI in an effort to avoid SOI discrimination. Thus, the estimates reported above are intent-to-treat effects; the actual effect on the treated is given by the estimated effect divided by the proportion of renters actually disclosing PI on applications. Such an exercise is necessary because no publicly available survey captures both public assistance recipiency and income disclosure on rental housing applications, and thus this exercise serves as the next-best alternative.

If I assume that only 50% of renters with any PI disclose their PI on rental applications, then the corresponding upper bound effect from Panel A, a 13.1% increase, becomes a 26.2% increase when scaling by the proportion actually disclosing income (i.e., by dividing 13.1% by 0.50). For individuals who derive a majority of their income from PI and who are thus less able to avoid reporting such income, the scaled effects are smaller. If I assume that 75% of such renters actually disclose PI receipt, then these estimates are consistent with increases in mobility for this population of up to 14%. In general this scaling exercise suggests that I cannot rule out economically and practically significant effects if renters are able to avoid disclosure of their PI status. This finding shows that while the null findings from Table 3 still apply to the broad population of likely-affected renters, there may exist a subset of renters for whom such policies increase mobility.

4.2 Household Characteristics and Alternative Sample Definitions

In Table 5, I evaluate the impact of SOI policies on housing characteristics for adult renters with PI income (the sample from Table 3, Column (1) above) who reported moving in the past year. I consider total rent including utilities, average household size, and the number of people per bedrooms. Across all three columns, I find no evidence that outcomes are trending differentially in treated as compared to control ConsPUMAs in the pre-treatment period. In the post-treatment period, I find no consistent evidence of an impact of SOI policies on any of the three reported outcomes, suggesting that SOI policies do not affect housing characteristics for renters with PI who report moving.

In Table 6, I experiment with several variations of the baseline sample definitions used in Table 3 in order to assess the robustness of the baseline estimates. In Column (1), I consider renters for whom SSI accounts for 50% or more of their income who do *not* receive other welfare income, while in Column (2), I expand the poverty-based sample to include rent-burdened individuals near poverty, with family incomes less than or equal to 150% of the poverty line. Across both columns, I find no evidence of an effect of SOI policies on mobility for either population, with statistically insignificant average post-treatment effects near 0 in both cases. In Column (3), I consider a sample of renters for whom Social Security accounts for 50% or more of their personal income who do not report any income from SSI/welfare. Income derived from Social Security is protected by SOI policies. However, the substantial differences in the structure of the Social Security program and the demographic composition of beneficiaries mean that this population may be less affected by SOI discrimination, despite having a formally PI source, than the samples considered above. Here in Column (3), I find no evidence that the mobility of renters with substantial income from Social Security is affected by SOI policies. While the pre-treatment coefficient corresponding to $t = -3$ is negative and statistically significant, the pre-treatment coefficients are jointly indistinguishable from 0. In the post-treatment period, there is likewise no clear evidence of a change in mobility.

Finally, in Appendix Table A3, I consider household- as opposed to individual-level definitions of renters potentially affected by SOI policies. This alternative specification allows me to assess the degree to which the results presented above are sensitive to sample construction, and is significant because of the potential for resource sharing across household members. Mobility at the household level is defined here as having all residents within the household report moving in the past year. I consider three samples analogous to those considered in Table 3. In

Column (1), I define households with any protected income as those in which any resident reports PI, while households with more than 50% of their total income derived from PI, summed across all residents, are included in Column (2). Finally, in Column (3), I include households in which all residents have incomes below the poverty line and rental housing burdens in excess of 40%. Across all three columns, I find no consistent evidence that SOI policies are associated with mobility at the household level, with average post-treatment effects in Panel B that are uniformly close to 0 and statistically insignificant.

5 Conclusion

This paper examines the effect of SOI policies on recipients of non-voucher PI sources such as SSI and welfare. Using ACS data from 2006 to 2021 and a robust event-study framework across 1,078 ConsPUMAs, I find uniformly null effects of SOI policies on residential mobility rates for likely-affected renters. Point estimates are close to zero with sufficient precision to rule out mobility increases exceeding 1-2 percentage points, representing relative increases of 10-13% given baseline mobility rates of approximately 22%. I also find no evidence that SOI policies affect average rents, household size, or crowding among those who move. Importantly, the results presented above are intent-to-treat effects, which suggest that SOI policies are not associated with large-scale changes in housing outcomes for the overall population of non-voucher PI recipients.

Residential mobility for the broader population of non-HCV PI recipients may be relatively less responsive to SOI policies than the HCV population, which has been the focus of prior studies of SOI policies, for a number of reasons. Landlords may perceive different categories of PI differently, with the administrative requirements of the HCV program potentially generating more systematic discrimination. Additionally, HCV recipients do not have the option to avoid disclosing their voucher as part of the rental application process. In contrast, some non-voucher PI recipients may avoid disclosing their PI recipiency in order to avoid facing SOI discrimination, which motivates the exercise conducted in Section 4.1. Under plausible disclosure rates, I cannot rule out economically meaningful mobility increases for individuals who derive the majority of their income from protected sources and thus are most likely to disclose such income on rental applications. For instance, if 75% of renters whose PI comprises over half their income disclose this information, the estimates are consistent with mobility increases of up to 14% for this subpopulation.

While SOI policies may not generate dramatic changes in mobility patterns for all PI recipients, they may still serve important anti-discrimination functions for those renters who must disclose PI sources. Future research using audit studies or administrative data linking rental applications to housing outcomes could shed light on the disclosure decisions and rental market experiences of PI recipients, providing a more complete picture of how SOI policies shape access to housing opportunities for vulnerable populations.

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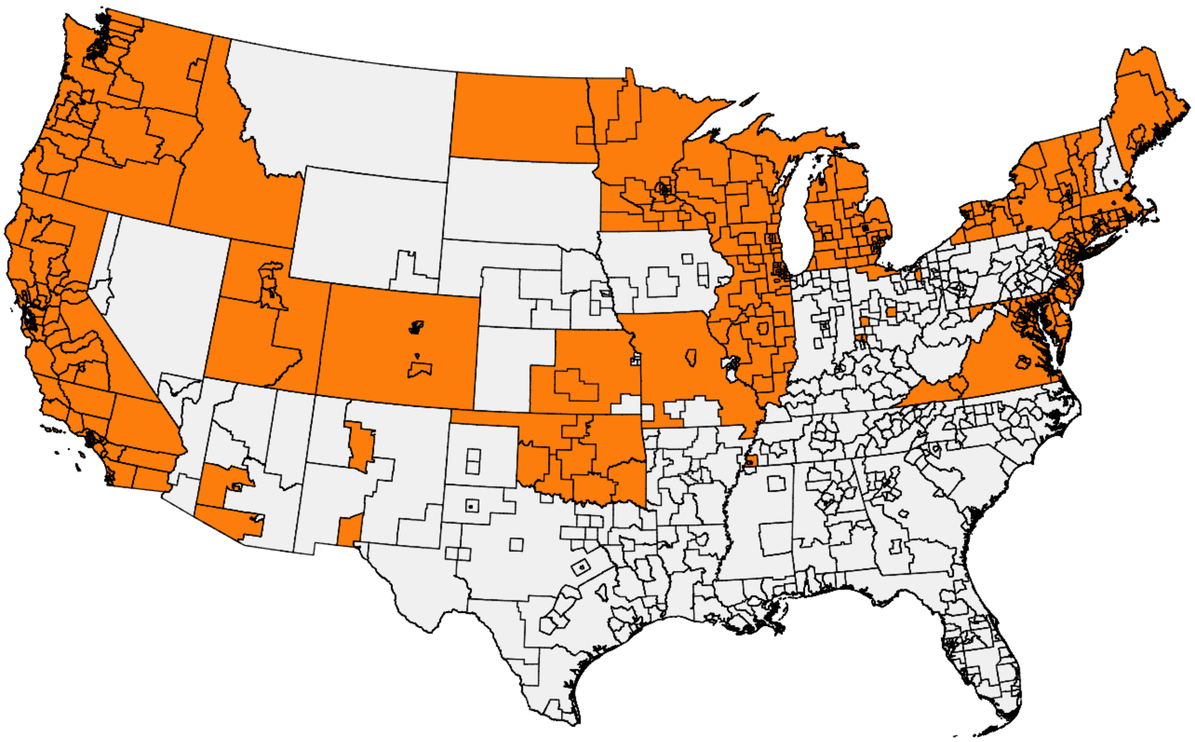
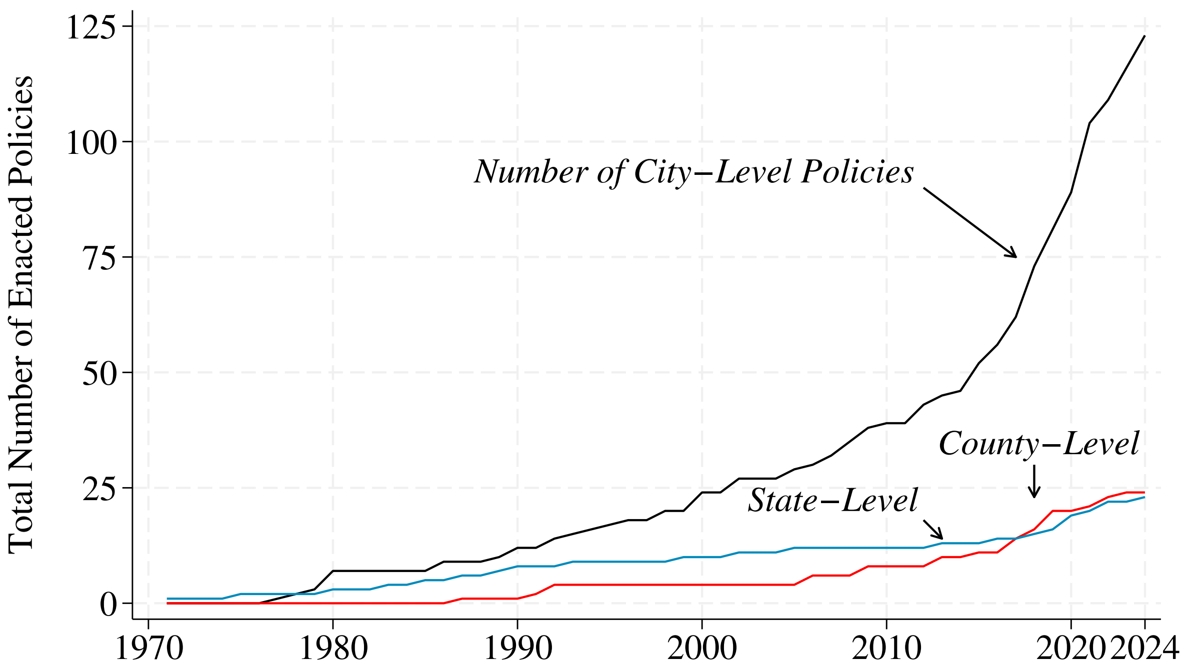
Figure 1: *SOI Policies Enacted as of 2024*



Notes: Source of income (SOI) policies enacted as of 2024 are mapped to Consistent Public Use Microdata Areas (ConsPUMAs) and plotted in orange. For a full list of SOI policies enacted at the state, county, and city levels, see Appendix Tables A1 and A2.

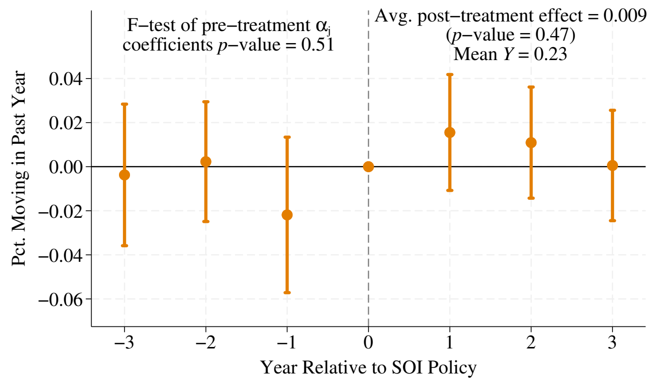
Figure 2: *SOI Policy Enactment over Time*



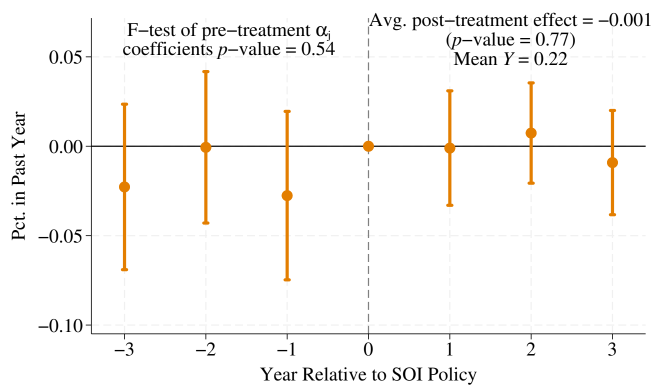
Notes: The total number of SOI policies enacted in each year is plotted separately by jurisdiction type. For a full list of SOI policies, see Appendix Tables A1 and A2.

Figure 3: *SOI Policies and Mobility for Likely-Affected Renters*

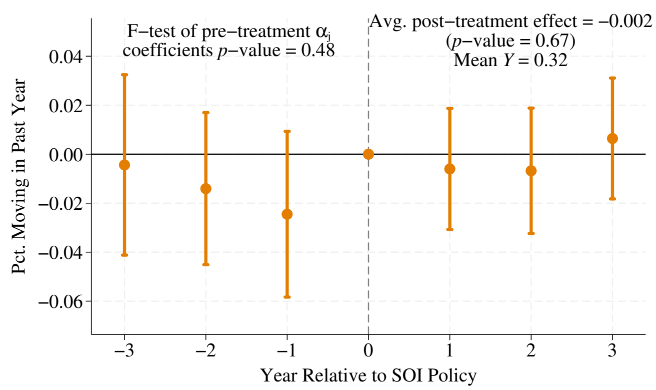
Panel A: Any Protected Income



Panel B: 50 Pct. or More of Total Income from Protected Sources



Panel C: Poverty-Based Sample



Notes: Event study estimates are reported in table format in Table 3. Columns correspond to three alternative definitions of adult renters who are potentially affected by SOI policies; for sample definitions and specification descriptions, see notes for Table 3. 95% confidence intervals are reported around each point estimate.

Table 1: *Summary Statistics for SOI Policies*

Panel A: Total Number of Policies Enacted through 2021	
State-Level	20
County-Level	21
City-Level	104
Total	145
Panel B: Number of Policies by Enactment Year	
Pre-2006	45
2006-2009	13
2010-2019	59
2020-2021	28
Panel C: Number of Policies Enacted between 2010-2019 *Switchers Contributing Identifying Variation to Equation (1)*	
State-Level	4
County-Level	12
City-Level	43
Total	59
Panel D: Policies across ConsPUMAs	
Number of Treated ConsPUMAs (2006)	318
Number of Treated ConsPUMAs (2021)	570
Total PUMAs in Sample	1,078
Panel E: Population of Likely-Affected Renters in Treated ConsPUMAs	
Estimated Total Likely-Affected Population (2006)	1,925,426
Estimated Total Likely-Affected Population (2021)	4,599,961

Notes: For a full list of SOI policies included in this study, see Appendix Tables A1 and A2. In Panel C, SOI policies enacted between 2010 and 2019 contribute identifying variation to the baseline event-study estimates corresponding to Equation (1), described in Section 3.4. In Panel E, likely-affected renters refers to the baseline sample definition from Table 3, Column (1), and includes individuals 18 or older who report protected income (PI) from SSI or welfare.

Table 2: *Summary Statistics for ConsPUMA-Level Analysis Sample*

Panel A: All Renters	**Mean**	**10th Pct.**	**90th Pct.**
Employment Rate	0.47	0.37	0.57
Poverty Rate	0.25	0.12	0.39
Percent Non-White	0.37	0.10	0.68
Fraction Over Age 65	0.08	0.04	0.12
Percent with Any Move	0.27	0.14	0.40
Percent of Renters Who Are…			
Adults with Any Protected Income	0.06	0.02	0.09
Adults with Protected Income $\geq$ 50% of Total Income	0.04	0.01	0.06
Panel B: Mobility Rates for Likely-Affected Renters			
Pct. Moved in Past Year with Any Protected Income (PI)	0.23	0.04	0.43
Pct. Moved in Past Year with PI $\geq$ 50% of Total Income	0.22	0.00	0.46

Notes: Summary statistics for the Consistent Public Use Microdata Areas (ConsPUMA)-by-year analysis sample. This sample is comprised of 1,078 ConsPUMAs covering the years 2006 to 2021 based on microdata from the American Community Survey (ACS). Sample across both panels is restricted to individuals who report living in rental housing and is collapsed to the ConsPUMA-year level using person-level weights. Protected income (PI) includes SSI and welfare payments. In Panel B, mobility rates are reported within the indicated population (e.g., in Panel B, Row 1, the proportion of adults with any PI who report moving in the past year is 0.23).

Table 3: *SOI Policies and Mobility for Likely-Affected Renters*

	(1) Any Protected Income (PI)	(2) $\geq$ 50% PI	(3) Poverty Sample
Panel A: Event Study Estimates			
$t = -3$	-0.004	-0.023	-0.004
	(0.016)	(0.024)	(0.019)
$t = -2$	0.002	-0.001	-0.014
	(0.014)	(0.022)	(0.016)
$t = -1$	-0.022	-0.028	-0.025
	(0.018)	(0.024)	(0.017)
$t = 1$	0.016	-0.001	-0.006
	(0.013)	(0.016)	(0.013)
$t = 2$	0.011	0.007	-0.007
	(0.013)	(0.014)	(0.013)
$t = 3$	0.001	-0.009	0.006
	(0.013)	(0.015)	(0.013)
Panel B: Post-Treatment Average Effect			
Source of Income Policy	0.009	-0.001	-0.002
	(0.011)	(0.012)	(0.011)
Pre-Treatment Joint Sig. Test P-Value	0.512	0.544	0.484
Observations	12,160	12,160	12,160
Mean(Outcome)	0.229	0.218	0.317

Notes: Sample is comprised of ACS data aggregated to the ConsPUMA-by-year level from 2006 to 2021 using person-level weights. Columns correspond to three alternative definitions of adult renters who are potentially affected by SOI policies. In Column (1), the sample is restricted to individuals who report receiving protected income (PI) from SSI and welfare; in Column (2) the sample is restricted to individuals for whom PI accounts for 50% or more of their total personal income; in Column (3), the sample is restricted to adults with incomes below the poverty line with rent burdens in excess of 40%. All specifications include ConsPUMA and year fixed effects and ConsPUMA-level, time-varying covariates including employment and poverty rates, and shares for the percentage of the population that are non-white, high school graduates, and over the age of 65. Standard errors clustered at the ConsPUMA level are reported in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4: *Assessing Precision of Mobility Estimates from Table 3*

Panel A: Precision of Estimates from Table 3

	Point Estimate	95% CI	Mean(Y)	U.B. Effect (%)
Any Protected Income (PI)	0.009	[-0.012, 0.030]	0.229	13.1%
≥50% PI	-0.001	[-0.025, 0.023]	0.218	10.5%
Poverty Sample	-0.002	[-0.023, 0.019]	0.317	5.9%

Panel B: Implied Effect Sizes under Alternative Income Disclosure Scenarios

	Share Disclosing PI Recipiency			
	50%	75%	90%	100%
Any Protected Income (PI)	26.2%	17.5%	14.6%	13.1%
≥50% PI	21.0%	14.0%	11.7%	10.5%
Poverty Sample	11.8%	7.9%	6.6%	5.9%

Notes: Panel A reports post-treatment average effect estimates from Panel B of Table 3 along with corresponding 95% confidence intervals (CI). The *U.B. Effect (% Change)* column reports the maximum implied effect in percentage terms, given by dividing the upper bound of the CI by the sample average of Y. In Panel B, the maximum implied effects from Panel A are scaled by the hypothetical proportion of individuals within a given sample that actually disclose their PI on rental applications. Panel B serves a simulation to account for the fact that in practice, only a subset of individuals in each sample considered above may choose to disclose protected income (PI) on their rental applications and thus directly face potential SOI discrimination. See Section 4.1 for more detail.

Table 5: *SOI Policies and Housing-Related Outcomes*

	(1) Gross Rent	(2) HH Size	(3) People per Bedroom
Panel A: Event Study Estimates			
$t = -3$	0.34	0.227	0.032
	(39.74)	(0.189)	(0.049)
$t = -2$	-37.92	-0.125	-0.021
	(36.47)	(0.106)	(0.029)
$t = -1$	-57.31*	0.076	0.007
	(33.52)	(0.137)	(0.033)
$t = 1$	-64.42*	0.017	0.017
	(36.03)	(0.134)	(0.037)
$t = 2$	-24.11	-0.211*	-0.035
	(41.63)	(0.115)	(0.035)
$t = 3$	3.35	0.022	0.034
	(37.49)	(0.126)	(0.037)
Panel B: Post-Treatment Average Effect			
Source of Income Policy	-28.39	-0.057	0.005
	(29.71)	(0.103)	(0.028)
Pre-Treatment Joint Sig. Test P-Value	0.321	0.224	0.627
Observations	12,160	12,160	12,160
Mean(Outcome)	1,063.74	2.759	0.903

Notes: Sample is comprised of ACS data aggregated to the ConsPUMA-by-year level from 2006 to 2021 using person-level weights. Sample is restricted to adults who report protected income (PI) from SSI and welfare payments, matching the sample definition from Table 3, Column (1) above. All outcome variables are conditional on moving in the past year (e.g., in Column (1), Rent is the average rental price paid by those who reported moving in the past year and who have positive PI). All specifications include ConsPUMA and year fixed effects and the time-varying covariates listed in the notes for Table 3. Standard errors clustered at the ConsPUMA level are reported in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6: *Mobility Effects of SOI Policies for Alternative Samples of Renters*

	(1) SSI $\geq$ 50%	(2) Near-Poverty (150% of PL)	(3) Social Security $\geq$50%
Panel A: Event Study Estimates			
$t = -3$	-0.003	-0.029	-0.024**
	(0.016)	(0.026)	(0.012)
$t = -2$	-0.010	-0.011	-0.016
	(0.014)	(0.026)	(0.012)
$t = -1$	-0.012	-0.037	-0.007
	(0.014)	(0.024)	(0.013)
$t = 1$	-0.003	-0.002	0.008
	(0.010)	(0.017)	(0.010)
$t = 2$	-0.009	0.014	0.007
	(0.009)	(0.017)	(0.010)
$t = 3$	0.001	-0.005	0.003
	(0.010)	(0.017)	(0.009)
Panel B: Post-Treatment Average Effect			
Source of Income Policy	-0.003	0.002	0.006
	(0.008)	(0.013)	(0.008)
Pre-Treatment Joint Sig. Test P-Value	0.796	0.412	0.194
Observations	12,160	12,160	12,160
Mean(Outcome)	0.306	0.201	0.166

Notes: Sample is comprised of ACS data aggregated to the ConsPUMA-by-year level from 2006 to 2021 using person-level weights. In Column (1), sample is restricted to individuals for whom SSI income contributes 50% or more of their total personal income who also report *no* income from other welfare payments; in Column (2) sample is restricted to rent-burdened individuals with incomes below 1.5× the poverty line; in Column (3), sample is restricted to individuals for whom Social Security payments contributes 50% or more to their total personal income who report no payments from SSI or welfare. All specifications include ConsPUMA and year fixed effects and the time-varying covariates listed in the notes for Table 3. Standard errors clustered at the ConsPUMA level are reported in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

APPENDIX

This appendix provides additional technical details and robustness checks for "The Effect of Source of Income Policies on Recipients of Non-Voucher Categories of Protected Income."

A1 Documenting Source of Income (SOI) Policies

In Appendix Table A1, I show SOI policies enacted during the time period considered by this study, 2006 to 2021, at the state, county, and city levels. In Appendix Table A2, I list all SOI policies that were enacted prior to 2006 by jurisdiction type.

A number of jurisdictions have experienced changes to their SOI policies over time. In particular, jurisdictions have added or removed Housing Choice Vouchers (HCV) as a protected source of income as a result of judicial review or subsequent amendments. For the purposes of this analysis, I consider a jurisdiction treated by an SOI policy if any such policy is enacted, regardless of the specific treatment of HCVs. As a result, my classification of a given jurisdiction's SOI policy and the timing of the enactment of that SOI policy may vary from the broader SOI literature, which focuses specifically on HCV recipients. California enacted an SOI policy in 1999, effective in 2000, which did not cover HCVs; HCV was added to this broader policy in 2019. Similarly, Delaware implemented an SOI policy in 2016 which did not cover HCVs, with a subsequent amendment adding HCVs to the policy slated to go into effect in 2026. In both cases, I consider each state treated by their initial SOI policy enactment. Minnesota's 1990 SOI policy covered HCVs until 2010, when HCV protections were removed as a result of judicial review. Several states have passed state-level legislation specifically preempting local SOI policies. The state of Texas passed such legislation in 2015, removing SOI protections in Dallas and Austin. While outside the time period considered by this study, both Florida and Iowa implemented similar legislation in 2023.

A2 Event-Study Estimator

All results produced in the main text use the robust estimator developed by de Chaisemartin and D'Haultfœuille (2024), referred to as dCDH in the discussion below. The dCDH estimator addresses concerns about the estimation of event-study and difference-in-differences style research designs via Ordinary Least Squares (OLS). In this section, I provide a brief summary of important features of the dCDH estimator.

The dCDH estimator calculates event-study effects as weighted averages of "clean" difference-in-differences comparisons. For each event-time j, the estimator compares the outcome evolution of units whose treatment changed ("switchers") to units with the same baseline treatment whose treatment has not yet changed (control units). Unlike traditional OLS event studies, which can inadvertently use already-treated units as controls for newly-treated units (leading to bias when treatment effects vary across groups or over time), the dCDH estimator explicitly compares each switching unit only to not-yet-treated units with the same baseline treatment status. This approach avoids the problematic comparisons that can contaminate OLS estimates in staggered adoption settings (Goodman-Bacon, 2021). Additionally, the estimator is designed to handle settings where treatment can switch both on and off over time, which is particularly important in this setting because SOI policies have been removed via judicial review and state legislation.

As with other event-study style estimators, credible causal inference with the dCDH approach requires an identifying assumption about the counterfactual trajectory of Y in the absence of treatment. In this setting, the identifying assumption is that ConsPUMAs with the same baseline treatment status (either with or without an SOI policy enacted) would have experienced parallel outcome trends if they had all maintained their baseline treatment status (i.e., parallel trends conditional on baseline treatment). This assumption can be assessed using a joint significance test of the pre-treatment event study estimates, which compare outcome changes of to-be-treated units with their control groups before any treatment occurs. For each event-study table, I report these pre-treatment estimates alongside a joint test of their statistical significance to evaluate the credibility of this identifying assumption.

Table A1: *SOI Policies Enacted between 2006 and 2021*

Jurisdiction Type	Jurisdiction	State	Enactment Year
State-Level	Oregon	.	2013
	Delaware	.	2016
	Washington	.	2018
	New York	.	2019
	Colorado	.	2020
	Maryland	.	2020
	Virginia	.	2020
	Rhode Island	.	2021
County-Level	Nassau	New York	2006
	King	Washington	2006
	Miami-Dade	Florida	2009
	Frederick	Maryland	2009
	Cook	Illinois	2013
	Westchester	New York	2013
	Suffolk	New York	2015
	Marin	California	2017
	Santa Clara	California	2017
	Broward	Florida	2017
	Denver	Colorado	2018
	Erie	New York	2018
	Alachua	Florida	2019
	Anne Arundel	Maryland	2019
	Baltimore	Maryland	2019
	Prince Georges	Maryland	2019
	Hillsborough	Florida	2021
City-Level	Buffalo	New York	2006
	Ripon	Wisconsin	2007
	Sun Prairie	Wisconsin	2007
	Detroit	Michigan	2008
	New York	New York	2008
	Lorain	Ohio	2008
	Harwood Heights	Illinois	2009
	Annapolis	Maryland	2009
	Wickliffe	Ohio	2009

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Jurisdiction Type	Jurisdiction	State	Enactment Year
	Cambridge	Wisconsin	2010
	Linndale	Ohio	2012
	University Heights	Ohio	2012
	Warrensville Heights	Ohio	2012
	Redmond	Washington	2012
	Royal Oak	Michigan	2013
	Kirkland	Washington	2013
	Austin	Texas	2014
	Santa Monica	California	2015
	Iowa City	Iowa	2015
	St. Louis	Missouri	2015
	South Euclid	Ohio	2015
	Pittsburgh	Pennsylvania	2015
	Vancouver	Washington	2015
	Oak Park	Illinois	2016
	Syracuse	New York	2016
	Dallas	Texas	2016
	Renton	Washington	2016
	Berkeley	California	2017
	Westland	Michigan	2017
	Minneapolis	Minnesota	2017
	Rochester	New York	2017
	Kent	Washington	2017
	Spokane	Washington	2017
	Fairfax	California	2018
	Novato	California	2018
	San Anselmo	California	2018
	San Diego	California	2018
	San Rafael	California	2018
	Woodland	California	2018
	Boulder	Colorado	2018
	Jackson	Michigan	2018
	Kentwood	Michigan	2018
	Wyoming	Michigan	2018
	Bellingham	Washington	2018

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Jurisdiction Type	Jurisdiction	State	Enactment Year
	Alameda	California	2019
	Los Angeles	California	2019
	Milpitas	California	2019
	San Jose	California	2019
	Des Moines	Iowa	2019
	Baltimore	Maryland	2019
	Clayton	Missouri	2019
	Webster Groves	Missouri	2019
	Gainesville	Florida	2020
	Naperville	Illinois	2020
	Louisville	Kentucky	2020
	Portland	Maine	2020
	Holland	Michigan	2020
	Kalamazoo	Michigan	2020
	Bexley	Ohio	2020
	Toledo	Ohio	2020
	Daytona Beach	Florida	2021
	St. Petersburg	Florida	2021
	Ferndale	Michigan	2021
	Hazel Park	Michigan	2021
	Oak Park	Michigan	2021
	Akron	Ohio	2021
	Athens	Ohio	2021
	Cleveland Heights	Ohio	2021
	Columbus	Ohio	2021
	Reynoldsburg	Ohio	2021
	Westerville	Ohio	2021
	Whitehall	Ohio	2021
	Worthington	Ohio	2021
	Yellow Springs	Ohio	2021
	Providence	Rhode Island	2021

Notes: Source of income (SOI) laws enacted between 2006 and 2021 by jurisdiction type.

Table A2: *SOI Policies Enacted Prior to 2006*

Jurisdiction Type	**Jurisdictions with SOI Policies**
State-Level	Massachusetts; Maine; Wisconsin; North Dakota; Oklahoma; Vermont; Connecticut; Minnesota; Utah; California; New Jersey; District of Columbia
County-Level	Dane, WI; Montgomery, MD; Howard, MD; Milwaukee, WI
City-Level	Madison, WI; Ann Arbor, MI; West Seneca, NY; Boston, MA; Cincinnati, OH; Philadelphia, PA; Olympia, WA; Lansing, MI; Wauwatosa, WI; Seattle, WA; Chicago, IL; Bellevue, WA; Cambridge, MA; Quincy, MA; State College, PA; Revere, MA; Wheeling, IL; Urbana, IL; San Francisco, CA; Wilmington, DE; Corte Madera, CA; East Palo Alto, CA; Marion, IA; Grand Rapids, MI; Frederick, MD; East Lansing, MI; Memphis, TN; Mill Valley, CA; Hamburg, NY

Notes: Source of income (SOI) laws enacted prior to 2006 by jurisdiction type.

Table A3: *SOI Policies and Household-Level Mobility*

	(1) Any Protected Income (PI)	(2) $\geq$ 50% PI	(3) Poverty-Sample
Panel A: Event Study Estimates			
$t = -3$	-0.015	-0.050*	-0.011
	(0.019)	(0.026)	(0.017)
$t = -2$	-0.004	-0.006	-0.017
	(0.015)	(0.027)	(0.015)
$t = -1$	-0.038**	-0.031	-0.020
	(0.019)	(0.027)	(0.016)
$t = 1$	0.015	0.011	-0.012
	(0.015)	(0.019)	(0.012)
$t = 2$	0.007	0.003	0.005
	(0.014)	(0.020)	(0.013)
$t = 3$	-0.005	-0.001	0.016
	(0.013)	(0.017)	(0.013)
Panel B: Post-Treatment Average Effect			
Source of Income Policy	0.005	0.004	0.003
	(0.011)	(0.015)	(0.010)
Pre-Treatment Joint Sig. Test P-Value	0.183	0.191	0.602
Observations	12,160	12,160	12,160
Mean(Outcome)	0.265	0.223	0.259

Notes: Mobility at the household level is defined as all residents reporting moving in the prior year. Columns correspond to three alternative definitions of households potentially affected by SOI policies. In Column (1), the sample is restricted to households which report receiving protected income (PI) from SSI and welfare payments; in Column (2), the sample is restricted to households for whom PI income accounts for 50% or more of their total household income; in Column (3), the sample is restricted to households with rent burdens in excess of 40% in which all residents have incomes below the poverty line. All specifications include ConsPUMA and year fixed effects and the time-varying covariates listed in the notes for Table 3. Standard errors clustered at the ConsPUMA level are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.