The Intersection of Place and Need:
How Lack of Enrollment Offices Deters Participation in the Safety Net*

Marianne Bitler, University of California-Davis, IZA, and NBER
Jason Cook, University of Utah, IZA, and CESifo
Chloe N. East, University of Colorado Boulder, NBER, and IZA
Sonya R. Porter, US Census Bureau
Laura Tiehen, Economic Research Service–USDA (Retired)

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Abstract

Take-up of means-tested transfer programs in the United States remains incomplete despite their substantial value to eligible households. We contribute to the literature on determinants of program participation by providing the first causal estimates of how proximity to Supplemental Nutrition Assistance Program (SNAP) offices affects program participation. Using administrative data on SNAP receipt in a single state linked to geocoded office locations, we exploit quasi-experimental variation from frequent office openings and closings. Event study estimates show that SNAP participation in a census tract decreases following the closure of an office by 7–9 percent over two years, with suggestive evidence of increases in participation following office openings. These effects are concentrated in urban areas and are robust to alternative specifications and tests of endogeneity in changes in office placement.

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1 Introduction

The United States has a patchwork social safety net comprised of both cash and in-kind programs that target different sub-populations. Because of the large degree of fiscal federalism in the U.S., many of the federal programs are both funded and run in part by states or other levels of government. This means that the processes for accessing these programs differ across and within states over time. Additionally, the burden of proving eligibility for these programs is put on individuals. Thus, potential applicants and those who already receive benefits must continually navigate the complex application and recertification processes in order to continue to receive benefits they are eligible for.

It is also well known that take-up of these programs—participation among those eligible—is well below 100% (Currie, 2006; Ko and Moffitt, 2022). This incomplete take-up is a puzzle of interest to policy makers and researchers focused on means-tested transfer programs. A large, older, and primarily theoretical literature in economics (Akerlof, 1978; Besley and Coate, 1992; Nichols and Zeckhauser, 1982) suggests that incomplete take-up may be socially optimal when there is a desire to target benefits and there are both more and less needy individuals who are eligible for programs. This happens in the context of situations such as cumbersome application processes by screening in needy individuals or deterring less needy individuals from participating. A growing economics literature assesses whether various social programs are well-targeted to the needy and whether interventions that provide information or assistance in enrolling or change transactions costs affect targeting (e.g., Deshpande and Li, 2019; Finkelstein and Notowidigdo, 2019).

Some more recent strands of literature challenge this neoclassical view that incomplete take-up contributes to program efficiency by screening out those who have little need for benefits. Behavioral science suggests that incomplete program take-up could be attributed to individuals having limited capacity to manage the administrative barriers associated with enrolling in assistance programs due to the significant mental burden imposed by poverty (Bertrand et al., 2004; Buttenheim et al., 2023; Mani et al., 2013). Meanwhile, the public administration literature argues that those barriers, referred to as administrative burdens, are not necessarily the unintended consequences of policy design decisions, but can sometimes be the result of strategic policy choices of federal, state and local governments (Herd and Moynihan, 2018; Barnes et al., 2023). The public administration literature further raises concerns, consistent with the behavioral science literature, that those with the greatest needs are least able to navigate administrative burden (Herd and Moynihan, 2025).

The Supplemental Nutrition Assistance Program (SNAP) is a large program, serving 42 million people (12% of the US population) monthly and almost 80% of SNAP households include either a child, an elderly individual, or an adult with a disability.[1] Benefits are meaningful for recipients; monthly per capita benefits in April 2025 were \$189, and the program paid out \$94 billion in fiscal year 2025.[2] Yet, despite the relatively high value of this program to participants, SNAP participation among those eligible is well under 100%. The most recent estimates indicate that in fiscal year 2022, 88% of eligible people received SNAP benefits nationwide and there is wide variation across demographic groups and states, ranging from 59% in Arkansas to 100% take-up in a number of states.[3]

These incomplete take-up rates suggest that there are features of SNAP design and administration that deter eligible people from applying such as time costs, lack of information about the program, or stigma. To participate in SNAP, individuals must apply to the program, which involves filling out forms, documenting earnings and assets, and participating in an interview with a caseworker, all before being deemed eligible or not. Those who already receive SNAP have to periodically recertify their eligibility through a similar process to that undergone by new applicants. Given the many aspects that matter for eligibility and benefit determination, SNAP forms can be quite lengthy. In many states, applications may be submitted online and interviews may be conducted over the phone. Yet, as we discuss further below, a large share of initial applications and recertifications are submitted in person in offices; and, in many states, caseworkers provide assistance with completing the complex processes. This program complexity and need for frequent interactions with bureaucracy suggest that the effect of SNAP office proximity could be an important determinant of SNAP participation.

In this paper, we add to the literature by being the first to estimate the effect of Supplemental Nutrition Assistance Program (SNAP) office locations changing on SNAP participation. To do so, we leverage quasi-experimental variation in the presence of SNAP offices across locations and over time. Additionally, we use unique data, combining person-level administrative records on SNAP receipt with geocoded data on SNAP office locations.

Our empirical approach exploits changes in SNAP office locations over time. We focus on Indiana because it both has offices in every county, and because it rents its SNAP offices, which means the office locations change frequently as leases expire. Talking to program officials in Indiana clarified that this is deliberate, as Indiana has offices in all of its roughly 90 counties and attempts to locate them to facilitate access. Our analysis uses census tracts to identify proximity to SNAP offices. Census tracts are defined to have between 1,000-8,000 people in them and are generally much smaller than counties. Thus, the primary identifying

[1] https://www.fns.usda.gov/research/snap/characteristics-fy23

[2] https://fns-prod.azureedge.us/sites/default/files/data-files/keydata-may-2023.pdf

[3] For more details, see https://www.fns.usda.gov/research/snap/state-participation-rates/2022

assumptions are: 1) absent the office openings and closings, SNAP participation would be on the same trend across census tracts, 2) there are no other changes occurring in these tracts that are correlated with the offices opening or closing, and 3) there are no anticipation effects.

We explore whether the total number of SNAP cases in a given tract changes if a SNAP office opens or closes in that tract using an event study approach. Because we do not observe the number of people *eligible* for SNAP by tract and month, we are not able to directly examine effects on the take-up rate, but rather focus on participation, which we have at this more granular level for the universe of participants. There is no evidence of large or significant pre-trends in SNAP cases prior to offices opening or closing, supporting the identifying assumption of parallel trends. We find strong evidence that having an office in a tract increases SNAP cases, and the opposite is true when an office closes in a tract. This effect is largely driven by those living in urban areas. Specifically, an office opening in an urban area increases the number of SNAP cases by up to 20%, though these estimates are not significant at conventional levels. Conversely, an office closing in a tract reduces the number of SNAP cases by a significant 7-9% over the next two years. This is a meaningful effect.

Since we use a staggered-treatment-timing approach with two-way fixed effects, we confirm that our results are robust to using alternative methods that account for possible treatment effect heterogeneity within groups and time (e.g., de Chaisemartin and d'Haultfoeuille, 2020). We also carefully explore the endogeneity of office locations opening and closing. We test whether there is a statistically and economically significant relationship between pre-period changes in the presence of stores in the tract or pre-period levels of demographic and economic characteristics of the census tract and the timing of office location changes in that tract. There are no meaningful pre-trends in the count of participants in each tract nor are there meaningful predictors of the treatment when controlling for changes in the number of stores of various types present before our sample. Taken together, these suggest that the timing of office location changes within tracts is plausibly exogenous.

Recent research has examined the role of administrative burdens in explaining incomplete take-up in many programs, including WIC (Bitler et al., 2003; Rossin-Slater, 2013), the Earned Income Tax Credit (Bhargava and Manoli, 2015; Iselin et al., 2023), the Social Security Disability Insurance Program (Armour, 2018; Foote et al., 2019), and the Supplemental Nutrition Assistance Program (Ganong and Liebman, 2018; Homonoff and Somerville, 2021; Giannella et al., 2024).[4] We contribute in part by bringing in granular administrative data

[4]Research has also examined barriers to participation in college financial aid (Bettinger et al., 2012) and student debt relief programs (Jacob et al., 2024).

that are a strength of our approach, since publicly available data on SNAP receipt are often self-reported, suffer from under-reporting issues (e.g., Meyer et al., 2015), and usually have limited geographic detail and small sample sizes. With the administrative records, we can accurately determine participation in SNAP as a function of whether a SNAP office opened or closed in one's census tract.

We also add to much of the existing literature on administrative burden and SNAP take-up. Previous research primarily leverages randomized control trials on limited populations or alternatively tests nudges in specific settings. We instead harness a host of exogenous shocks to SNAP participation caused by policy-driven changes in the costs of enrolling in the program. Our paper complements others that have studied the effect of offices closing on more targeted transfer programs—WIC and Social Security Disability Insurance participation (Rossin-Slater, 2013; Deshpande and Li, 2019), both because we study a more universal program and because we consider offices closing and opening. We extend existing work in progress by Cholli and Wu (2025), who study the effects of the distances to SNAP offices in Virginia and multiple program use but do not rely on variation from offices opening and closing.

2 Background

SNAP is the backbone of the US safety net. SNAP is currently the largest of USDA's food assistance programs and the only means-tested transfer program available to nearly all low-income US households. There is considerable evidence that SNAP reduces food insecurity (e.g., McKernan et al., 2021), and poverty (e.g., Jolliffe et al., 2024), and that access to SNAP during childhood improves long-term health and self-sufficiency (Bitler and Figinski, 2024; East, 2020; Hoynes et al., 2016). In addition, as an entitlement program, SNAP plays a critical role during economic downturns, protecting low-income families from their negative effects (Bitler and Hoynes, 2016) and serving as an important economic stimulus (Canning and Stacy, 2019).

SNAP eligibility is based on both income and asset tests and includes work requirements for some households, such as able-bodied adults without dependents (ABAWDS). Eligible households are typically certified to participate for periods ranging from 6 to 12 months, but seniors recertify less frequently (24 months or more) and ABAWDS have to recertify every 3 months. The SNAP application process, as determined by federal mandate, consists of submission of an application and an interview with a caseworker. The recertification process varies some from state to state, but generally consists of updating forms and supporting documentation, as well as completing another interview with a caseworker. Recertification lengths vary state to state, but are commonly 6 or 12 months.

States have latitude in both how they receive paperwork and how they conduct interviews. To learn more about this process as well as the services provided by SNAP offices in our context, we communicated with several urban and rural offices across Indiana as well as across several other states. Applicants and those recertifying eligibility can submit paperwork in person at a SNAP office, or in many states, digitally through an online portal. Online applications for SNAP first became available in January 2002 and are now offered in at least 47 states. A few states require in-person interviews for applications and recertifications, while many have been able to waive that requirement and allow interviews by phone. As of 2016, 47 states had been granted waivers to allow phone interviews at either initial application or recertification (USDA-ERS, 2018).[5] From our informal survey, we learned that despite access to digital submission options, the most common mode of application submission was in person, followed by online, and then by phone and email. One office estimated that 80% of applications were submitted in person despite there being an online portal available.

Why might applicants be choosing to submit their application or recertification in person instead of the seemingly more convenient online submission portals? One possibility is that they benefit from assistance when they visit offices in person. SNAP processes are long and complex, requiring documentation of income and assets as well as child care, medical, and housing expenses. Bartlett et al. (2004) found that applicants who were ultimately approved for benefits spent an average of 6.1 hours on the process and were required to make an average of 2.4 trips to the SNAP office. Cook and East (2023) find that caseworkers play an important role helping applicants navigate the SNAP application process. Further, James (2024) provides evidence that the managers who supervise caseworker teams in safety net program offices in Texas explain 8-10% of the variation in caseworker performance. In a phone interview with the program administrators in Indiana's SNAP program, we learned that SNAP offices in Indiana have self-help kiosks where applicants can submit applications and staff are trained to directly help applicants navigate the process.

In addition to providing help with SNAP applications and recertifications, SNAP offices often provide other services. In some states, offices can help connect individuals to other programs such as Temporary Assistance for Needy Families (TANF), Medicaid, WIC, and housing assistance. In other states, the SNAP office jointly administers other assistance programs (most commonly TANF). Because of this, we also examine spillover effects on enrollment in other safety net programs.

Our research design exploits changes in office locations across census tracts over time to

[5]https://www.ers.usda.gov/data-products/snap-policy-data-sets/

isolate the causal effects of geographic access to offices on SNAP participation. To do so, we need to demonstrate that the location and, in particular, the timing of office location changes is plausibly unrelated to trends in unobserved determinants of SNAP participation. To understand these decisions, we also solicited information about why offices changed locations in our informal survey (11 out of the 34 states we surveyed answered these questions). There is little evidence that states systematically locate offices based on trends in local caseloads or eligible populations, and given the lags in data on population characteristics, it is hard to imagine states could target eligibility trends as they are unlikely to have data on that by detailed geography. While this assumption is untestable, we provide a number of checks that show support for the idea that changes in office locations are plausibly exogenous.

As discussed above, we focus on Indiana because it purposefully leases all SNAP offices and as the leases expire every 4-6 years, the offices often relocate. Indiana attempts to locate offices in populated zipcodes and to lease office space on public bus lines and in places where they can co-locate SNAP offices with other program offices such as the DMV, WIC, child services, etc. While these decisions are intentional, what would be a threat to our identification strategy is if Indiana was choosing when to open or close offices based on trends in SNAP participation or some other factor that might also be correlated with SNAP participation. However, because Indiana changes offices frequently based on lease expirations, and chooses the precise location of the office based on where they can find a good deal on a new lease, the timing of these moves are less likely to violate our identifying assumptions. We also test directly whether we can predict office moves at the census tract level based on pre-period observable characteristics below and show we fail to do so with either pre-sample characteristics of tracts or changes before our sample begins in the number of stores in each tract.[6]

[6]The tests with pre-sample changes in the number of stores provide no evidence that these predict openings or closings. Given the limited number of openings (107) and closings (123), and the large number—9—of predictors we test in Table 1, we do find evidence that taken all together, the levels of tract level variables predict openings and to a lesser extent, closings. Note that it is the pre-trends in unobservables not level differences which are the concern for identification. We also note that a number of the characteristics enter the opening and closing regressions with the same sign, inconsistent with a simple story about unobservables driving timing. Further, the sole significant predictor is for office closings, and it suggests they are more likely to close where there are more women in the tract with less than a high school degree. This is inconsistent with offices being more likely to close where there is more need.

3 Data

3.1 SNAP and Other Program Administrative Data

Our data include administrative data housed within the Census Bureau that include information on recipients of SNAP, TANF, and Medicaid. For each recipient household, we observe demographic and income information, as well as benefit amounts. Importantly, we also observe the residential address (via the SNAP administrative records) of recipients at the monthly level, which allows us to link each SNAP recipient to their census tract using the Census Bureau's Master Address File. This level of geographic specificity is rare and a strength of our data and approach.

Our measure of treatment is whether there is an office opening or closing in the census tract. To get a better sense of how this treatment operates, we explore a "first stage" outcome, which is how an office opening or closing in a given tract impacts the average distance in miles that all of the people in that tract have to travel to get to the nearest SNAP office. This is a natural measure of the costs of going to the office, and more granular than previous work.

Our primary outcome variable is the total count of SNAP cases by tract and month. We include all SNAP cases regardless of the demographics of the case or if the case is a new SNAP recipient or not. We explore defining the tract of residence of SNAP participants in two ways. For our main approach, we focus on the current tract of residence and allow this to change over time due to migration. And, to address concerns about potentially endogenous migration, we explore an alternate specification where we consider a given household's tract of residence to be the first tract that we observe them in the SNAP administrative data and hold that tract fixed over time. There is a downside of this second measure, namely that it introduces measurement error in the location of residence of individuals and whether there are offices in their tract of residence as well as whether such offices are open or closed. As a result, the estimates using it are noisier in practice.

3.2 SNAP Office Location Data

While our main analysis focuses on office changes within Indiana, we provide context by collecting information on office locations more generally. Specifically, we hand-collect roughly twenty years information on the location of every SNAP administration office for 27 states.[7]

[7] We collected office location information for Arizona, Colorado, Connecticut, Florida, Hawaii, Iowa, Idaho, Illinois, Indiana, Kentucky, Massachusetts, Maryland, Michigan, Mississippi, Montana, North Carolina, North Dakota, Nebraska, New Jersey, New York, Oregon, South Carolina, South Dakota, Tennessee, Utah, Virginia, and Wyoming.

We exclude states that did not have SNAP recipient data available in the Census Bureau for many years in our sample period, or did not have office location data available or easily collectible. For each state in our sample, we locate the state SNAP agency's website that displays office locations and systematically move through time using the Internet Archive's "Way Back Machine" (WBM) to build the database of office openings and closings.[8] Because WBM only archives websites periodically, we have some measurement error in the date of changes. Where possible, we note the month of an office change, but where that is not possible, we capture the range of dates and assign the treatment month as the first possible month in the date range that could have experienced an office change. We also called offices with long date ranges where possible to get a more accurate date of change. It is also worth noting that our measure of office location reflects when the state agency lists the change to their site, any delay or preemptive posting to the office site will affect our measure of office location. We merge this data on office locations to the data on SNAP receipt based on census tract of residence of SNAP recipients.

Figure 1 plots counts of openings and closings. In general, more offices are closing than opening on average across all states we collected data on in panel (a). We communicated with offices as we collected data on office locations and learned that many states have moved to primarily administering SNAP via phone and online systems which allows the state to consolidate offices to reduce costs.

Figure 2 also shows the geographic spread of office changes across 3 example states. Indiana is an outlier in terms of the number of office location changes relative to the other states for which we collected data. We plot office changes in Indiana in panel b of Figure 1. From 2006 to 2016, Indiana had 143 office closings. The next two closest states had 79 and 44 closings, with most states having about 15 closings. Over the same time, Indiana had 126 openings. The next two closest states had 40 and 24 openings, with most states having about 10 openings. Given these frequent changes and the fact the changes are largely driven by plausibly exogenous lease expirations, we focus the remainder of our study exclusively on the impact of access to SNAP offices in Indiana.

An important source of treatment effect heterogeneity in our case comes from the urbanicity of the location of the SNAP office. Travel costs vary by the density of the area.

[8] Internet Archive is a non-profit organization with the goal of creating a digital library of the internet. Archiving begin in 1996 and websites are primarily collected via a real-time webcrawler that accesses and then stores information from important websites. The webcrawler periodically accesses the same sites and records the updated versions. Sites that receive more traffic are archived more frequently. Some states using dynamic maps that require user interaction. WBM does a poor job at capturing these dynamic sites and for these states, we were unable to collect office data. Appendix Figure A1 shows the WBM interface for an office that changes its location across two months in one of the 27 states for which we hand collected the data, Indiana.

Appendix Figure A2 shows differences in travel distances to shop at grocery stores from the 2017 wave of the National Household Travel Survey (NHTS). This survey provides information on 900,000 trips taken across roughly 120,000 nationally-representative households. We show the driving distance for individuals living in areas with less than 50,000 people, 50,000-199,000 people and 200,000 plus. Longer travel distances are clearly more common in less densely-populated areas.

As a result, we expect the effect of a changes in the distance to SNAP offices to be larger in magnitude per mile traveled in more urban areas where individuals do not typically travel as long as distances in other aspects of their life.[9] Our main analysis sample is urban areas only and we show how results differ in rural areas.

An alternative reason that urban and rural effects could be different is that census tracts are generally geographically larger in rural areas. However, in our first stage analysis that investigates the effect of an office opening or closing on the distance individuals in that tract have to travel to get to the nearest office, we do not find large differences in the effect on distance across rural and urban areas.

3.3 Sample Restrictions

We make several restrictions to our final regression sample, after restricting to Indiana.[10] To avoid the COVID-19 pandemic, we only consider SNAP participation prior to 2019. We further limit the sample of office changes to those where we can observe SNAP participation from the year before through the two years following each given office change (i.e., the event horizon we consider in the next section). This effectively limits our analysis to office changes occurring between 2006 and 2016 and SNAP recipients from 2005 to 2018.

Next, we drop anyone in the SNAP administrative data who are flagged as being unhoused in the residential file or who appear to reside at the SNAP office address or within one tenth of a mile away. When unhoused individuals apply for SNAP, many times the caseworker puts the SNAP office as their residential address so that mail has a place to be sent. However, when offices change location, these addresses automatically move, making it artificially appear as though case rolls are changing when office locations change. We drop anyone who is ever unhoused across our sample to avoid this mechanical address change.

[9] To classify whether a SNAP office location is urban or rural, we use the 2010 Census Urban Area National Shapefile. We define offices as "urban" if they are located within a Census-defined Urban Cluster (census tracts or blocks with between 2,500 and 50,000 residents) or an Urbanized Area (more than 50,000 residents). Across all the states for which we collected office information, roughly 72 percent are located in Urban Clusters or Urbanized Areas—81 percent in Indiana.

[10] We also note that we are restricted to observations where we can link individuals to a location and match them across time with Protected Identification Keys, or PIKS, which the Census Bureau links to individuals in both survey and frame data such as the the Master Address Files and associated MAFX files).

Finally, we limit the sample to tracts that experience the relevant office change at some point in our sample.[11] For instance, when examining office closures, we limit the sample to tracts that ever experience an office closure at some point in our sample. This final restriction is done to ensure that we are isolating variation in treatment timing. While the location of where to place offices may be endogenous (e.g., located near bus lines and other services), the timing of office changes is a function of lease expiration dates and is plausibly exogenous as a result. We further note that new offices must be placed in locations where there are buildings for rent.

4 Empirical Methods

To estimate the impact of access to SNAP offices on SNAP participation, we exploit variation in office proximity created by SNAP offices opening and closing. Specifically, we estimate

$$y_{it} = \alpha_0 + \sum_{h=-12, h\neq -1}^{h=23} D_{h(it)} \cdot \beta_h + \delta_t + \gamma_i + \epsilon_{it} \tag{1}$$

where i indexes the census tract, t indexes calendar-year-month, and h indexes event time relative to the *first* opening or closing observed in the data for the given tract. The outcome variable is the total number of SNAP cases y_{it} at the tract by time level. $D_{h(it)}$ is an indicator variable that equals one if, in the given period t, tract i is h periods away from experiencing treatment—an opening or closing for the first time in our sample. We analyze openings and closings separately, so event time will either be the first office opening or closing, respectively, in a given tract. δ_t are calendar-year-month fixed effects and γ_i are fixed effects for the tracts. We cluster standard errors by tract.

To ensure robustness to the potential negative weighting issues created by differential treatment timing across locations and heterogeneous treatment effects across timing and group (e.g., Goodman-Bacon, 2021; de Chaisemartin and d'Haultfoeuille, 2020), we employ a simplified version of the estimator proposed by de Chaisemartin and d'Haultfoeuille (2020) as our main approach. This method restricts estimation to only include comparisons of place-specific outcomes around a treatment status change, to outcomes in places that did

[11] We show the demographic and economic characteristics in 2000 of tracts in Indiana with and without an office in them in Appendix Table A1. There are some differences across these two groups of tracts in these levels. Tracts that have SNAP offices have higher percentages of individuals with a high school education or less (both females and males) and lower income on average. Tracts with and without SNAP offices are however similar in terms of race, ethnicity, household size, and employment levels. However, if these differences are fixed over time, this is not a concern for our empirical approach because we focus on changes within tracts over time using tract fixed effects. In section 4.1 below we explore whether these pre-period characteristics are related to the *timing* of office changes within tracts.

not experience changes in treatment across the same time span. In practice, this means that the treated units are tracts that experience an opening or closing within a specific time period, and the control units are tracts that do not experience either an opening or closing in the same time frame. This method assumes that not-yet-treated and recently treated tracts would have had the same trends if untreated, but does not allow for effects to vary by previous treatment. We call this the "static DCDH" approach below. Because we define treatment status to be an absorbing state, this approach does not allow for treatment effects to differ by past treatments. That is, we focus only on the first opening or closing in a given tract. In practice, this is not an overly restrictive assumption since no tract has multiple openings in our sample period, less than 1% of tracts have multiple closings, and only 2% of tracts have an opening and a closing in our sample period.

We also show robustness to using a simple two-way fixed effects estimation approach and we call this the "OLS" model in the figures. Finally, we show robustness to an approach suggested in de Chaisemartin et al. (2024) that builds on the static DCDH, but allows effects to depend on previous treatment status. Thus, the assumption about pre-trends is that, if never treated, all the units would have the same differences in effects across periods. Here, instead of considering treatment as simply the first time an office opens or closes, we augment (1) by considering treatment to be an indicator for whether the tract had an office open in the given period, and let the algorithm account for the fact that tracts can experience multiple treatments over the sample period and that treatment is not an absorbing state. We call this method the "dynamic DCDH."

We present the $\widehat{\beta_h}$'s in our main figures for $h = -12$ to $h = -1$ (year before treatment) and $h = 0$ to $h = 23$ (two years after treatment), constraining the effect for the pre-treatment year, $h = -1$, to be 0. All the figures display the effects of going from no office to an office in the tract (openings), or from an office to no office in the tract (closings).

4.1 Testing Identifying Assumptions

The key assumptions for our approach to identify causal effects of office openings and closings is there is no anticipation, there are no correlated shocks, and there are parallel trends in the key outcome variables of interest. No anticipation requires that potential SNAP applicants are not preemptively joining SNAP in anticipation of an office opening or that current SNAP applicants are not leaving the program in anticipation of a future office closing. In our context, this assumption seems plausible. While the terms of the office lease are known to the office administrators, impending office location changes are not communicated to the public until the month or two preceding the change.[12] We can test directly whether we see changes in the number of SNAP cases in the months leading up to an opening or closing using our event study approach below, and find no evidence of this.

The no correlated shocks assumption requires that there are no other changes in a given tract over time that are correlated with the office openings or closings and that would impact the number of people receiving SNAP. This would be violated, if, for example, states chose to open offices in tracts that had recently experienced mass layoffs. In practice this is unlikely to be the case since states are looking for available office space that fits their budget and their criteria largely relies on pre-existing characteristics such as proximity to a bus stop.

Finally, the parallel trends assumption requires that SNAP participation in tracts would have evolved in the same way in tracts with an opening or closing, relative to the untreated census tracts over the same time period. As is common in the literature, we provide evidence in support of this assumption by pre-trends in the outcomes of interest before an office opening or closing. Below, we show these pre-trends are small and insignificant.

As a further test of our identifying assumptions, we explore whether the year that an office opens or closes in a given tract can be predicted by tract demographic and economic characteristics in the 2000 Census, or by changes in the number of stores who accept SNAP benefits in the four years before the opening or closing.[13] As shown in Tables 1 and 2, these variables have almost no relationship with the date of the opening or closing, lending credibility to our empirical approach.

5 Results

We begin by analyzing the "first stage"—whether an office opening or closing in a tract changes the distance that residents of that tract have to travel to get to the nearest SNAP office. We measure the tract-by-time distance to nearest office by calculating the distance to the nearest SNAP office as the crow flies for each resident of the tract, and then taking the average of these individual-level distances by tract and time. In Figure 3, we plot the effect of an office opening (panel a) and closing (panel b) in urban census tracts on the distance to the nearest SNAP office, where the horizontal axis indicates months relative to the office opening or closing (which happens in month 1) and the vertical axis indicates effect

[12]However, it is important to note that our treatment variable is not leases specifically, but rather office openings and closings, which are largely driven by lease changes in this context.

[13]In Tables 1 and 2, office, Census, and store data are calculated at the year-tract level. This is then used to calculate the first year that a tract had an opening or closure take place, which is used as the outcome in columns (3) and (4). Rather than levels, the change in number of stores in the four years preceding an office opening or closure is used in Table 2.

on distance in miles.

Openings, in panel a, lead to a significant and largely persistent 2-2.5 mile decrease in distance to nearest office among residents in the tract with the office opening. Prior to this opening, the average distance was 3.3 miles so this is a large decrease in distance.

Closings, in panel b, lead to a 3-4 mile increase in distance to nearest office for the first 12 months after the closing. Again, this is a large effect since the average distance before the closing was only 0.6 miles. After 12 months, the distance falls to a little less than 2, and this is driven by other offices opening in nearby tracts over time. Given that Indiana operates one office in all but one county, it may be surprising that within-county office moves generate such meaningful changes in travel distance. However, counties in Indiana are on average 400 square miles, with the largest county being roughly 650 square miles and the smallest county being roughly 100 square miles. As a result, the average distance changes we observe are reasonable and reflect consequential changes in access to program offices.

Having established that office openings and closings do indeed change the distance people have to travel to access a SNAP office, we turn to the main effects on SNAP participation. Recall, our outcome variable here is the total number of SNAP cases in the given tract and month. Figure 4 plots the effect of openings and closings in our event study framework.

Looking at the results, it is important to note that there are no statistically significant nor are there economically meaningful pre-trends in the number of SNAP cases before the openings (panel a) or closings (panel b) providing support for our identifying assumptions. Focusing on openings, there is a clear increase in the number of SNAP cases that increases steadily over time to about 60 additional cases per month two years after the opening. If we were to take the point estimates at face value, this would imply two years after the opening, there is an increase in SNAP cases of about 20%, however, these estimates are not statistically significant at conventional levels. Since we measure total SNAP participation, this effect can be driven by both households beginning to participate in SNAP after the opening, and those who were already on SNAP before the opening participating for longer because it is now easier to recertify eligibility. Unfortunately, our data do not provide a clean way to separate these two channels.

Turning to closings, there is a statistically significant decrease in the number of SNAP cases following an office closing. This effect could be driven by cases who were participating in SNAP falling off the program because recertification has become harder, and by eligible households who would have signed up for SNAP if there was an office closer to them no longer doing so due to the further distance. After three months there are about 25 fewer cases per month and after two years there are about 35 fewer cases per month. This is a meaningful effect, before the closing, there are 373 cases participating in SNAP in a given tract-by-month, so the closing decreases participation by 7–9%. To put it into context, having a WIC office in a pregnant person's neighborhood increases WIC participation by 6% (Rossin-Slater, 2013) and closing an SSDI office reduces SSDI receipt by 16% (Deshpande and Li, 2019).

5.1 Robustness

We test the robustness of these main results in two ways. First, we use OLS and the dynamic DCDH as alternative estimation approaches in Figure A3. The results are very similar with these alternative estimation approaches.

A possible threat to identification is that there are changes in the SNAP-eligible population that are correlated with the openings and closings. This could happen for several reasons. First, if SNAP-eligible individuals move into or out of a tract in response to an office opening or closing. Second, if there is some tract-level change in, for example, economic conditions, that are correlated with the openings and closings and cause more people to become eligible. However, it is very unlikely these changes would only occur in the single tract that is experiencing an office change, at the same time the office location is changing, so we think this is unlikely to drive our results.

Further, to address the first concern about endogenous migration, we re-estimate our main results, but fix the initial tract of residence for each case. If people who are eligible for SNAP move in response to an office opening or closing, or in response to a correlated shock, and this is what is driving our main results, then fixing the tract at the initial observation would lead to different results. The results are very similar with a fixed tract as shown in Figure A4, though somewhat less precise, likely due to measurement error introduced when we fix the tract of residence (people may no longer live in the tract they were in earlier).

5.2 Spillover to Other Programs

Next, we investigate the possibility that changes in participation in SNAP may have spillover effects to other programs. This is plausible given that many states, including Indiana, have some degree of integration in their application processes for multiple programs and that participating in multiple means-tested programs at once is quite common in the U.S. A strength of our data is that we not only have administrative records for SNAP, but we also have them for TANF and Medicaid receipt. This allows us to test for program spillover effects and avoid any issues of under-reporting in survey data.

In Figure 5, we plot results from the same specification as we discussed above but with the number of TANF (panels a and b) and Medicaid (panels c and d) cases as the outcome variables. The direction of all the effects is consistent with spillover effects, although most point estimates are not significant at conventional levels. Focusing on office closings, where we do get more precise results, we find that two years after the office closes there are about 20 fewer TANF cases in the tract-by-month (and this is statistically significant), and 30 fewer Medicaid cases in the tract-by-month. Again, these effects are meaningful in magnitude; they represent a 25% decrease in TANF cases and a 4% decrease in Medicaid cases. This is important evidence that these programs do not operate in isolation, and cross-program spillovers may be important to track and understand.

5.3 Effects in Rural Areas

Our main analysis has been on urban tracts because we anticipate the effect of a change in distance to office to be larger for those in urban areas. Now, we explore results in rural and urban areas. We begin by verifying that the first stage effect on distance to nearest office is similar across rural and urban areas as shown in Figure 6. The effect on distance is slightly smaller for urban closings (panel b) than rural closings (panel d) but we cannot rule out that they are the same given overlapping confidence intervals.

Informed by this we next explore the effect on SNAP participation in Figure 7. The effects of rural openings and closings are much smaller in magnitude than the effects in urban, though they do go in the same direction. This is consistent with the idea that those in rural areas already have to travel longer distances in general for grocery shopping and other daily activities (shown in Appendix Figure A2). Thus, a change of only a couple miles may have less of impact on those in rural areas than those in urban areas and these results are consistent with this hypothesis.

6 Conclusion

Our results point to office locations being an important determinant of SNAP participation, especially in urban areas. Once an office closes within a census tract, the number of SNAP cases fall by 7-9% and this lasts for at least two years after the closure. This finding adds to a growing literature that points out that the costs of accessing program benefits has a non-trivial effect on benefit receipt among those eligible.

However, we are somewhat limited in our ability to draw strong conclusions about the impact of office openings, given issues of precision. Future work in this area should continue to consider these effects and seek approaches and data which lead to increased precision, explore the exact mechanisms through which participation is impacted, and analyze subgroups of interest.

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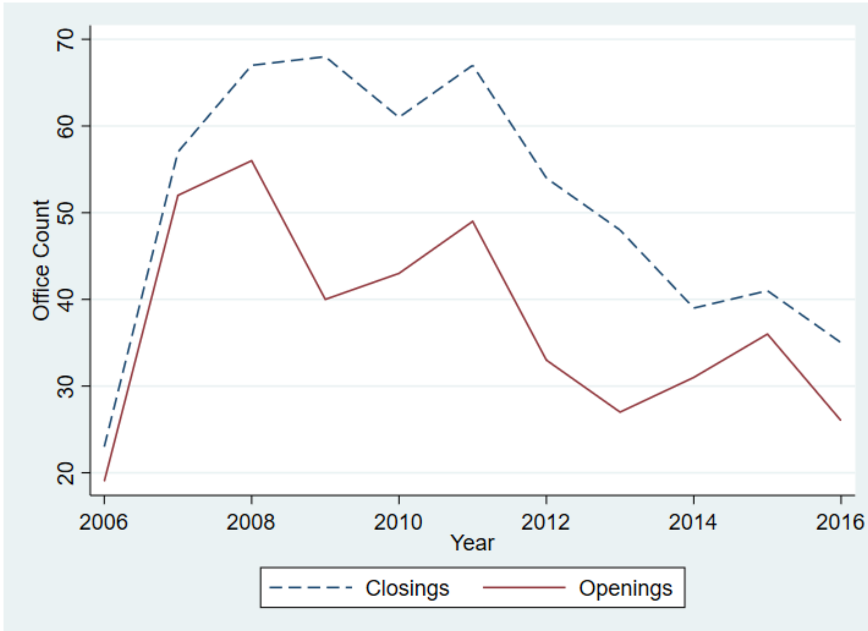
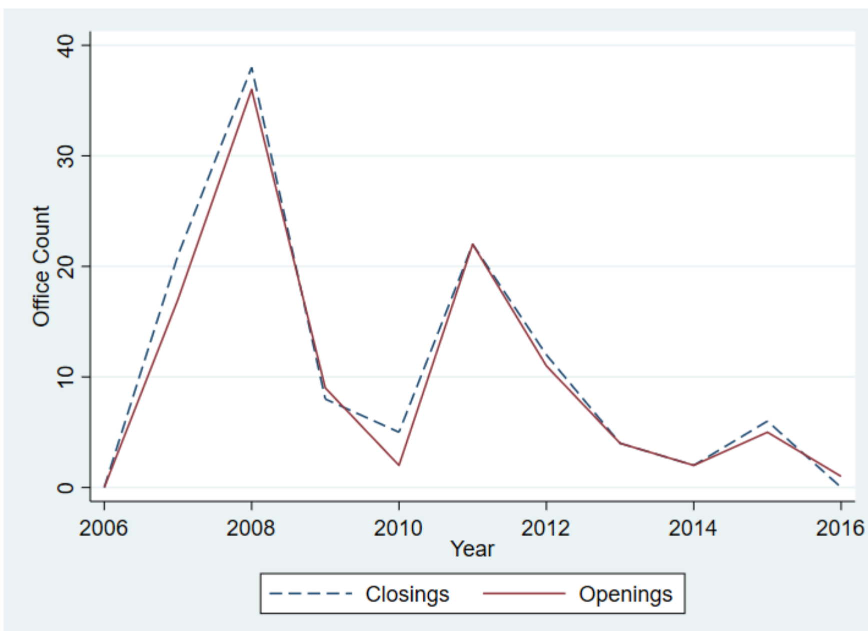
Figure 1: SNAP Office Openings and Closings Over Time



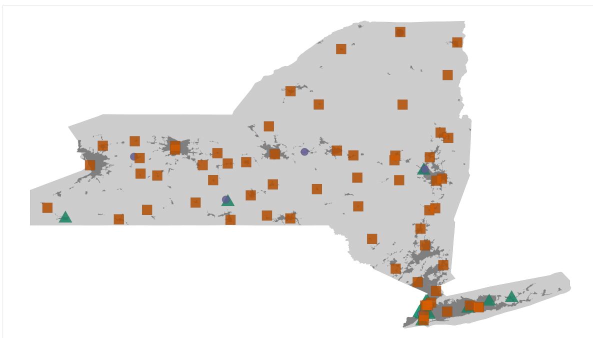
(a) All States with Collected SNAP Data



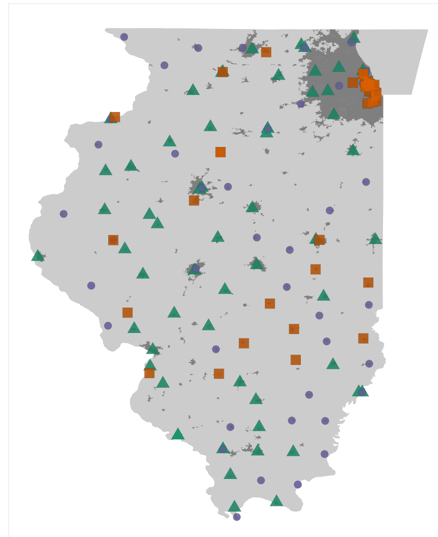
(b) Indiana

Notes: Plot shows the counts of SNAP office openings and closings that we manually collected for the following states: Arizona, Colorado, Connecticut, Florida, Hawaii, Iowa, Idaho, Illinois, Indiana, Kentucky, Massachusetts, Maryland, Michigan, Mississippi, Montana, North Carolina, North Dakota, Nebraska, New Jersey, New York, Oregon, South Carolina, South Dakota, Tennessee, Utah, Virginia, and Wyoming. Panel (b) provides the counts solely for Indiana. "Openings" refers to whether the given location has a SNAP office open during the time frame that we collected information. Similarly, "Closings" refers to locations where a SNAP office closes during the same time frame. "Stable" refers to a location with a SNAP office that remains open during the same time frame.

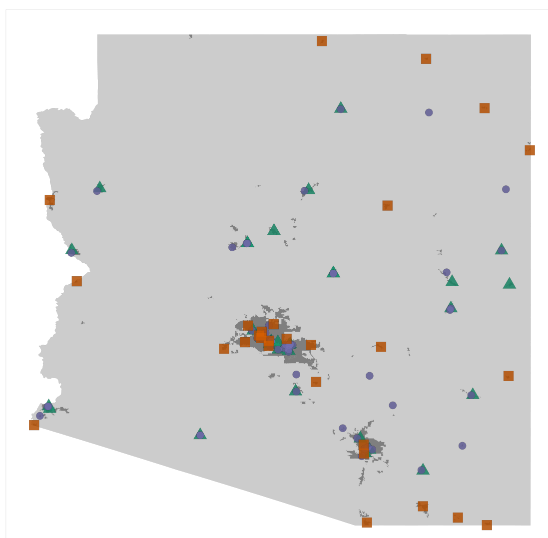
Figure 2: SNAP Office Changes for Selected States



(a) New York (2013-2018)



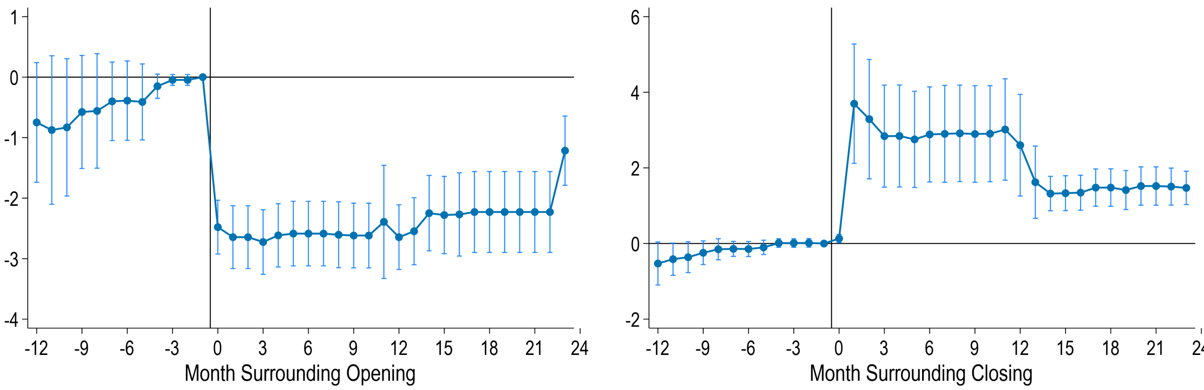
(b) Illinois (2008-2016)



(c) Arizona (2009-2018)

Notes: Figure displays the geography of office openings and closings for three example states. “openings” refers to whether the given location has a SNAP office open during the time frame that we collected information. Similarly, “Closings” refers to locations where a SNAP office closes during the same time frame. “Stable” refers to a location with a SNAP office that remains open during the same time frame. We collected office locations to match with administrative data from Census and so time periods reflect Census data availability at the time of collection.

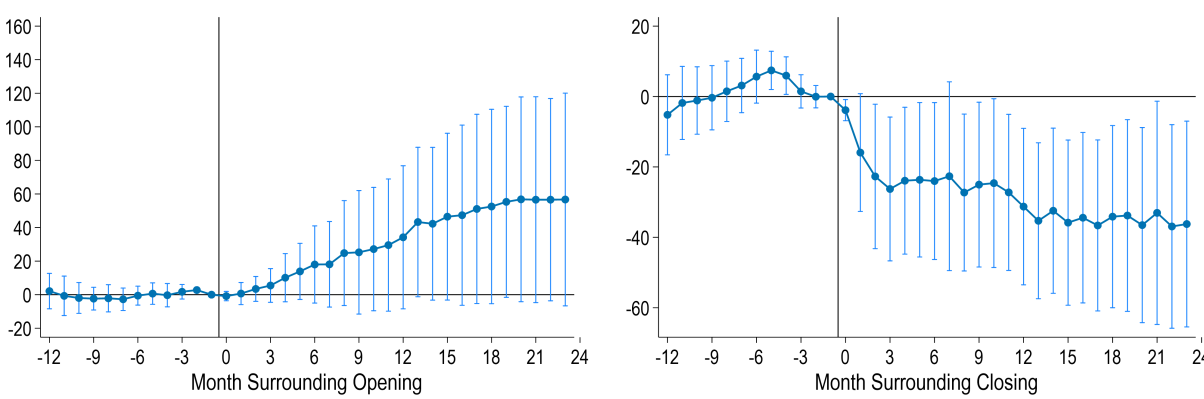
Figure 3: Effect of SNAP Office Changes on Distance to the Nearest Open Office, Urban Tracts



(a) Openings (b) Closings

Notes: This figure includes event studies from the "Static" DCDH (de Chaisemartin and d'Haultfoeuille, 2020) approach on the distance to the nearest open office, where treatment is offices either opening or closing in the given tract for the first time in our data. Baseline mean is 3.3 miles for openings and 0.61 miles for closings. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-0333.

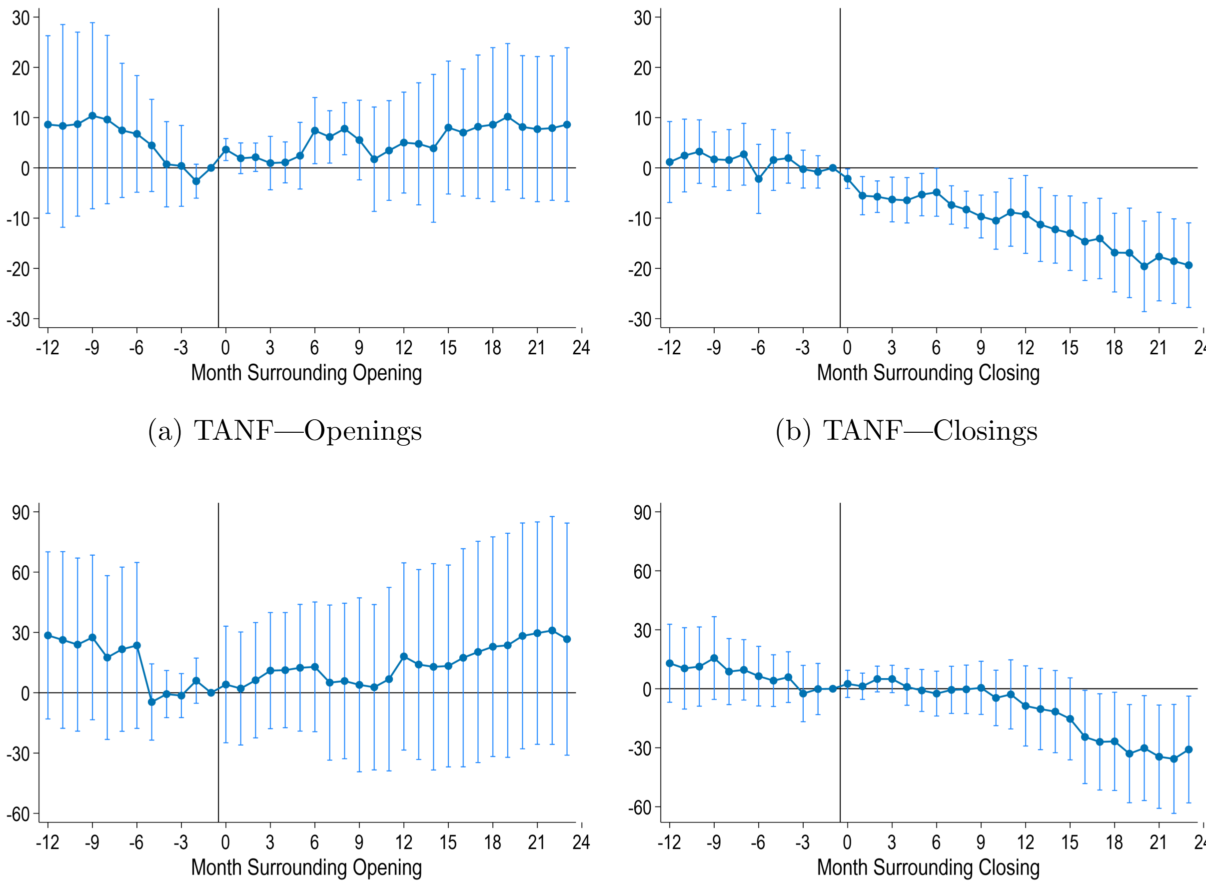
Figure 4: Effect of Office Changes on Number of Active SNAP Cases, Urban Tracts



(a) Openings (b) Closings

Notes: This figure includes event studies from the "Static" DCDH (de Chaisemartin and d'Haultfoeuille, 2020) approach on the number of active SNAP cases residing in the given tract, where treatment is offices either opening or closing in the given tract for the first time in our data. Baseline mean is 281.4 cases for openings and 373 cases for closings. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-0333.

Figure 5: Effect of Office Changes on active TANF or Medicaid cases within each tract, Urban tracts



(a) TANF—Openings

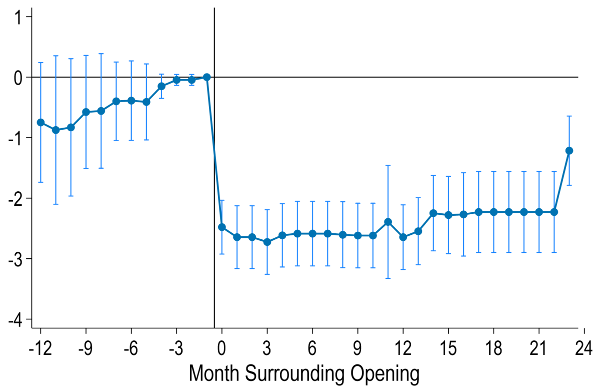
(b) TANF—Closings

(c) Medicaid—Openings

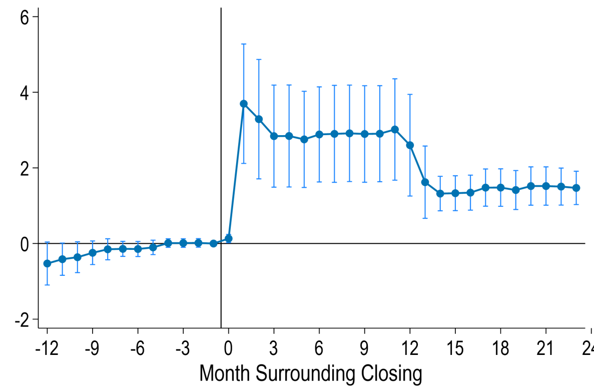
(d) Medicaid—Closings

Notes: This figure includes event studies from the "Static" DCDH (de Chaisemartin and d'Haultfoeuille, 2020) approach on the number of active TANF or Medicaid cases in the given tract, where treatment is offices either opening or closing in the given tract for the first time in our data. Baseline mean for TANF is 52.25 cases for openings and 85.1 cases for closings. Baseline mean for Medicaid is 562.6 cases for openings and 737.9 cases for closings. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-0333.

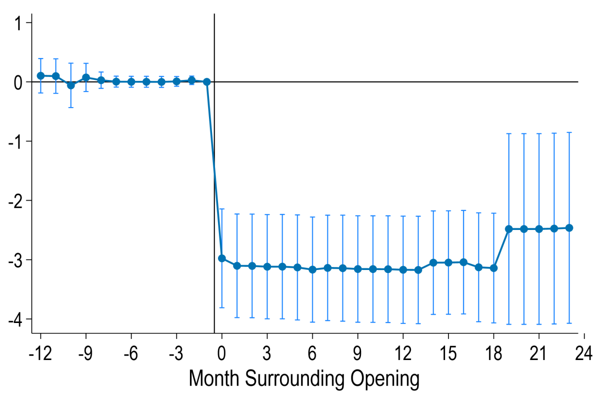
Figure 6: Effect of SNAP Office Changes on Distance to the Nearest Open Office, by Urbanicity



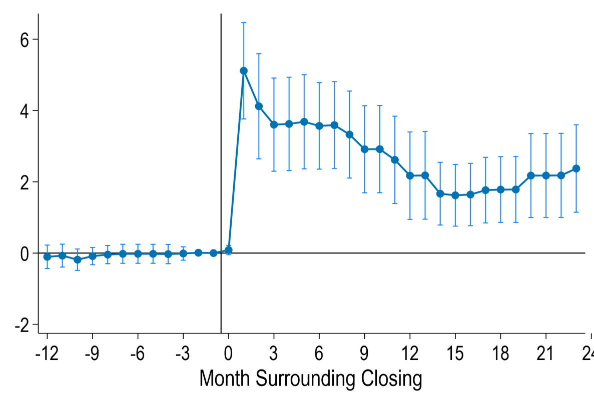
(a) Urban Openings



(b) Urban Closings



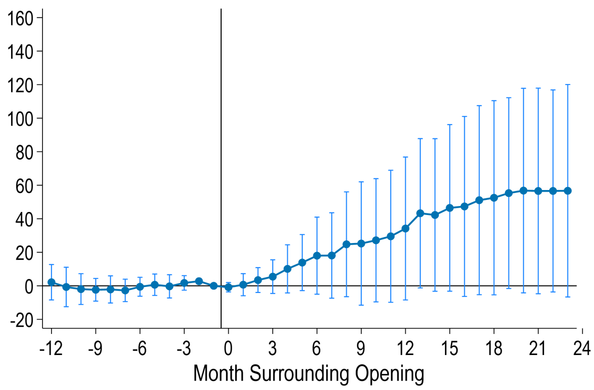
(c) Rural Openings



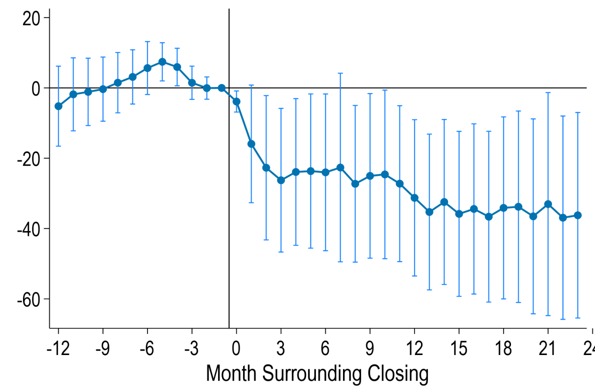
(d) Rural Closings

Notes: This figure includes event studies from the "Static" DCDH (de Chaisemartin and d'Haultfoeuille, 2020) approach on the distance to the nearest open office, where treatment is offices either opening or closing in the given tract for the first time in our data. Baseline mean for urban tracts is 3.3 miles for openings and 0.61 miles for closings. Baseline mean for rural tracts is 4.081 miles for openings and 1.461 miles for closings. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-0333.

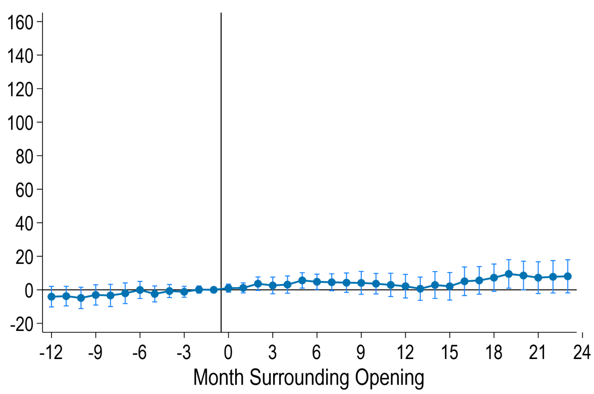
Figure 7: Effect of Office Changes on Number of Active SNAP Cases, by Urbanicity



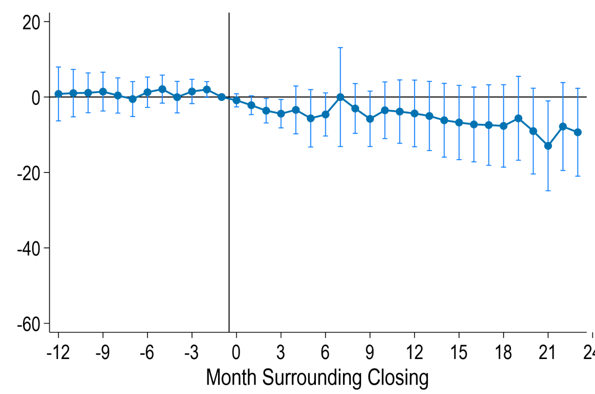
(a) Urban Openings



(b) Urban Closings



(c) Rural Openings



(d) Rural Closings

Notes: This figure includes event studies from the "Static" DCDH (de Chaisemartin and d'Haultfoeuille, 2020) approach on the number of active SNAP cases residing in the given tract, where treatment is offices either opening or closing in the given tract for the first time in our data. Baseline mean for urban tracts is 281.4 cases for openings and 373 cases for closings. Baseline mean for rural tracts is 203.8 cases for openings and 204.2 cases for closings. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-0333.

Table 1: Correlation of Tract Characteristics with Office Openings and Closures

	(1) Mean of Characteristic	(2) Standard Deviation of Characteristic	(3) Regression Estimate Year Office Opens	(4) Regression Estimate Year Office Closes
% Female < High School	62.711	9.670	-0.063	0.116**
			(0.053)	(0.043)
% Male < High School	60.816	11.781	0.000	-0.089*
			(0.044)	(0.041)
% Immigrants	2.791	4.078	0.187	0.172
			(0.106)	(0.108)
Median HH Income (1,000s)	36.218	8.486	0.000	0.021
			(0.000)	(0.062)
% Below Poverty	11.666	7.342	0.004	0.010
			(0.058)	(0.052)
% Black	4.774	12.073	0.011	0.014
			(0.024)	(0.018)
% Hispanic	3.623	6.520	-0.089	0.018
			(0.068)	(0.075)
Mean HH Size	2.399	0.196	-0.203	-0.322
			(0.343)	(0.505)
% Employed	94.178	4.471	-0.046	0.020
			(0.088)	(0.076)
N	161	161	107	123
Mean			2009.262	2009.276
F			3.272	2.295

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Baseline pre-opening and closing tract level summary statistics (columns (1) and (2)) and regression coefficients (columns (3) and (4)) predicting the relationship between the baseline tract level characteristics and the timing of the office closing and opening. Regressions weighted by population. Includes all tracts in Indiana and results are reported at the tract level. Uses the first year an office opens (column (3)) and closes (column (4)) as the outcome variable. Columns (3) and (4) are weighted by population. Columns (3) and (4) restrict to only tracts that had an office opening or closing, respectively.

Table 2: Correlation of Number of Stores with Office Openings and Closures

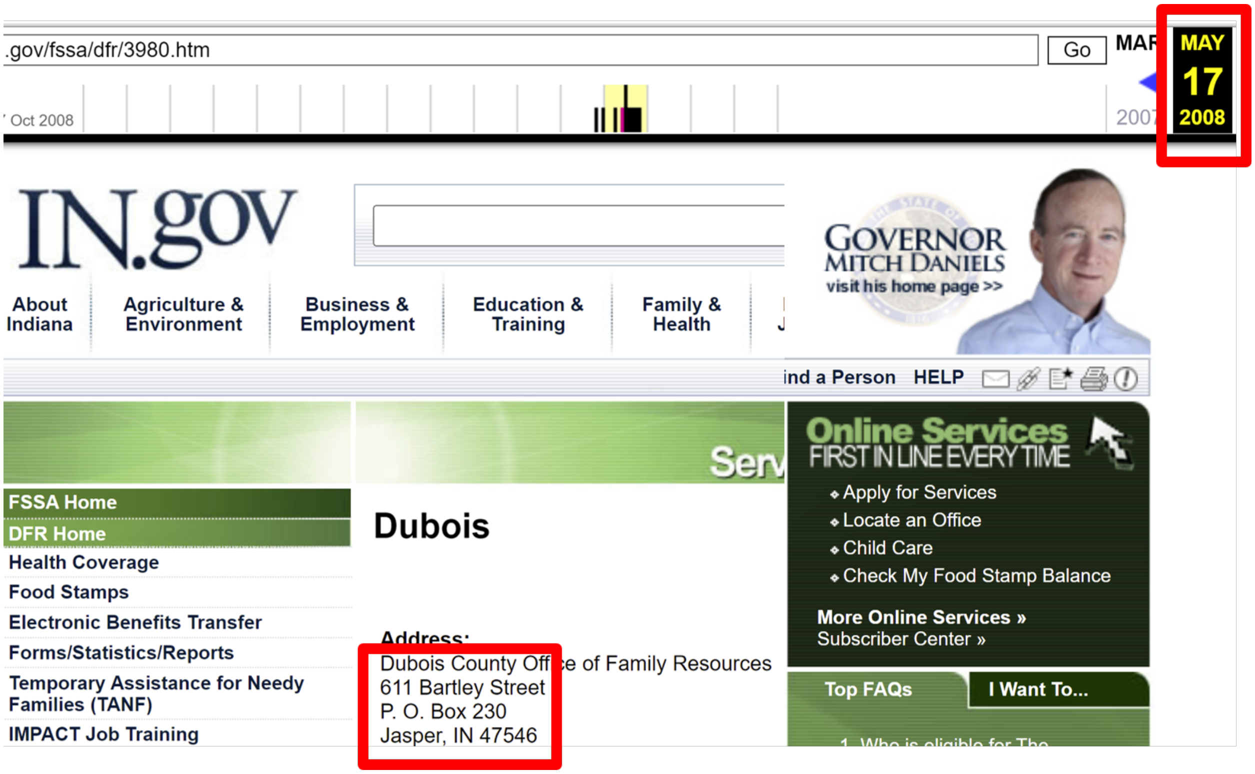
	(1) Mean of Characteristic Office Opens	(2) Standard Deviation of Characteristic Office Opens	(3) Mean of Characteristic Office Closes	(4) Standard Deviation of Characteristic Office Closes	(5) Regression Estimate Office Opens	(6) Regression Estimate Office Closes
Change in Number Superstores	0.028	0.022	0.167	0.398	0.493	0.199
					(1.878)	(0.765)
Change in Number Grocery Stores	0.167	0.311	0.507	0.668	-1.337*	-0.436
					(0.586)	(0.531)
Change in Number Convenience Stores	0.278	0.444	0.882	0.725	-0.041	0.280
					(0.330)	(0.454)
Change in Number of Other Stores	-0.056	-0.022	0.232	0.336	-2.910*	-1.388
					(1.174)	(1.016)
N	36	45	36	45	36	45
Mean					2009.642	2009.828
F					2.064	1.273

Standard errors in parentheses

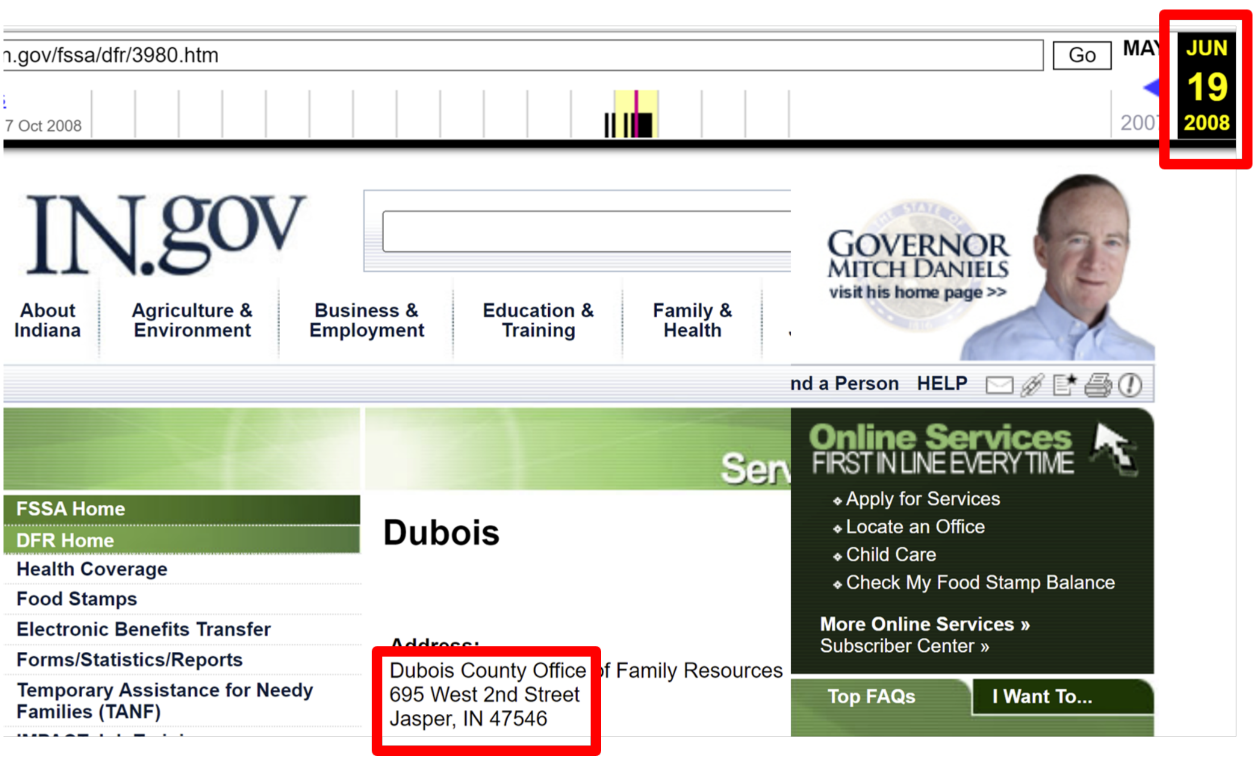
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Includes all tracts in Indiana and results are reported at the tract level from 2006-2016. Uses the first year an office opens (column (5)) and closes (column (6)) as the outcome variable. Sample size is slightly smaller due to data limitations on the store data. Columns restrict to only tracts that had an office opening or closing for their respective columns. Weighted by population.

Figure A1: Way Back Machine Web Scraping Example



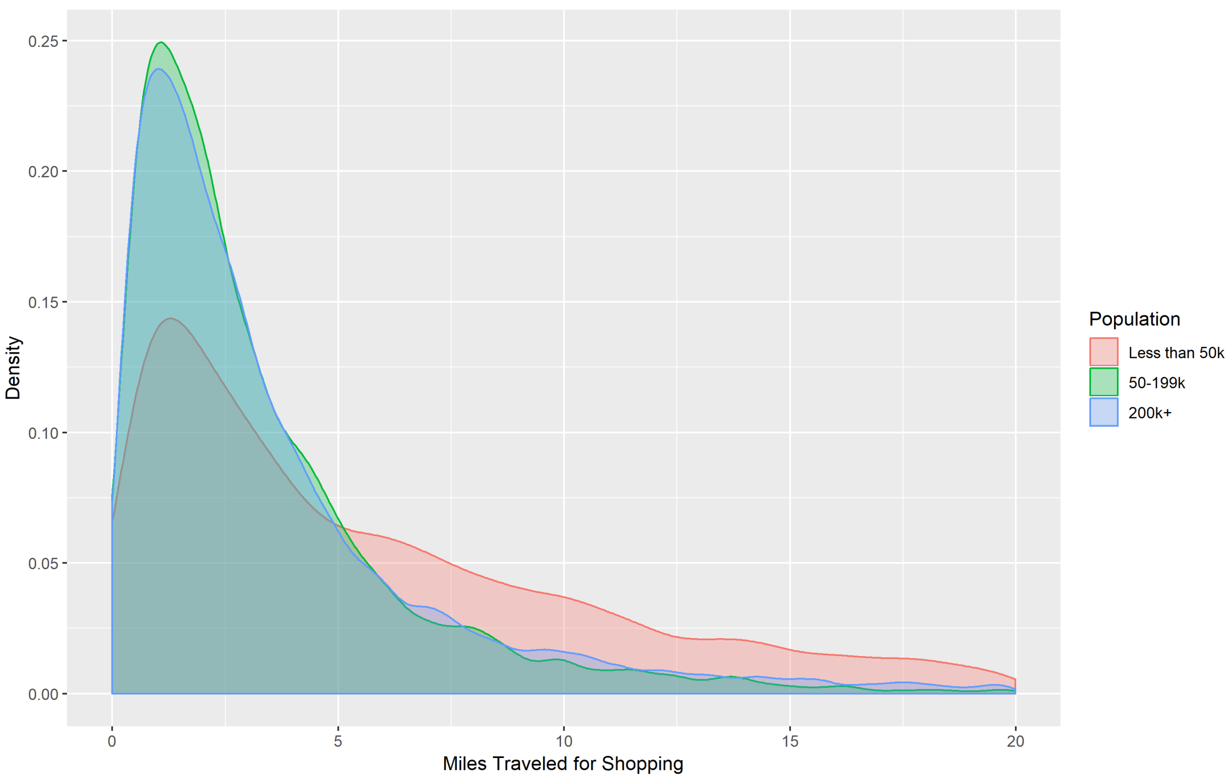
(a) Dubois County Office—Pre-Change



(b) Dubois County Office—Post-Change

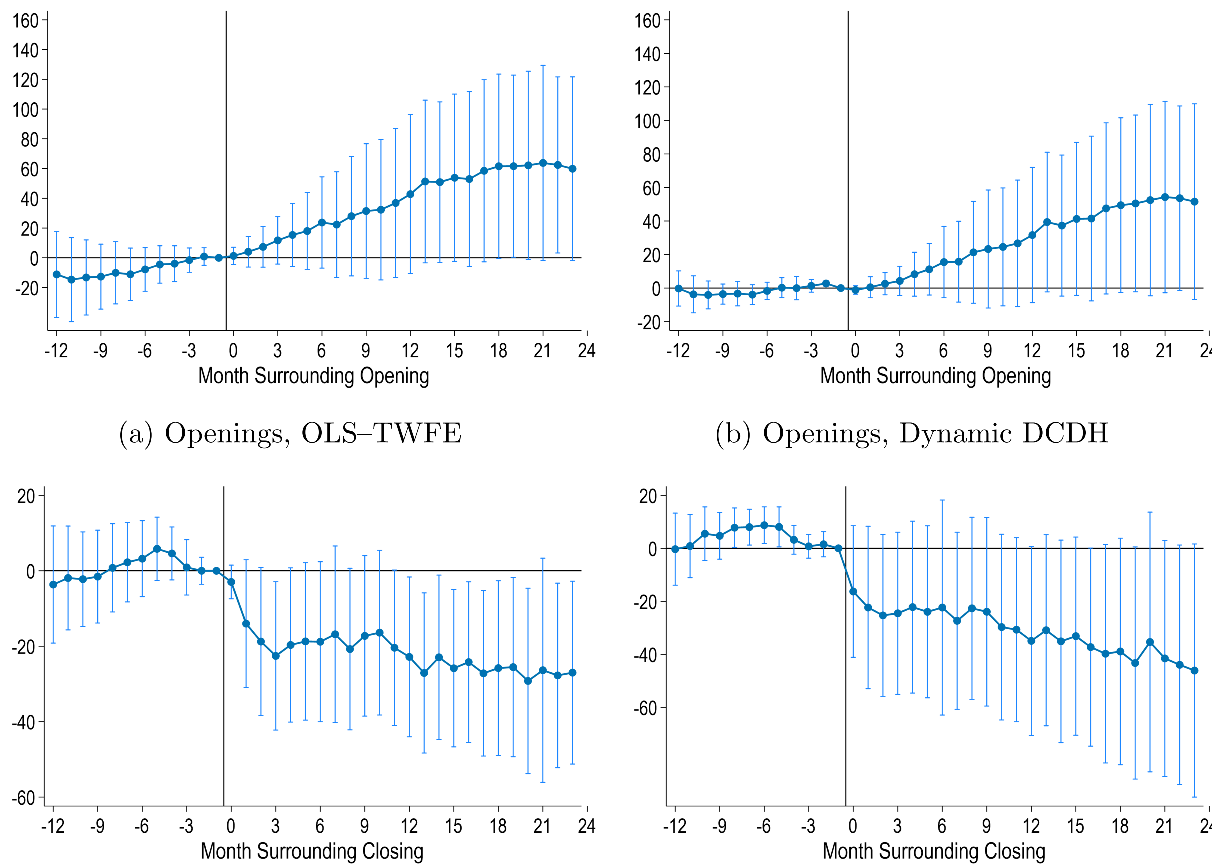
Notes: Examples of SNAP Office location changes collected from the WayBack Machine.

Figure A2: Travel Miles for Grocery Shopping



Notes: Plot shows the distribution of miles traveled for grocery shopping split by the population density where the household resides. Data come from the 2017 wave of the National Household Travel Survey.

Figure A3: Effect of SNAP Office Changes on Number of Active SNAP Cases, Urban Tracts, Alternative Estimators



(a) Openings, OLS–TWFE

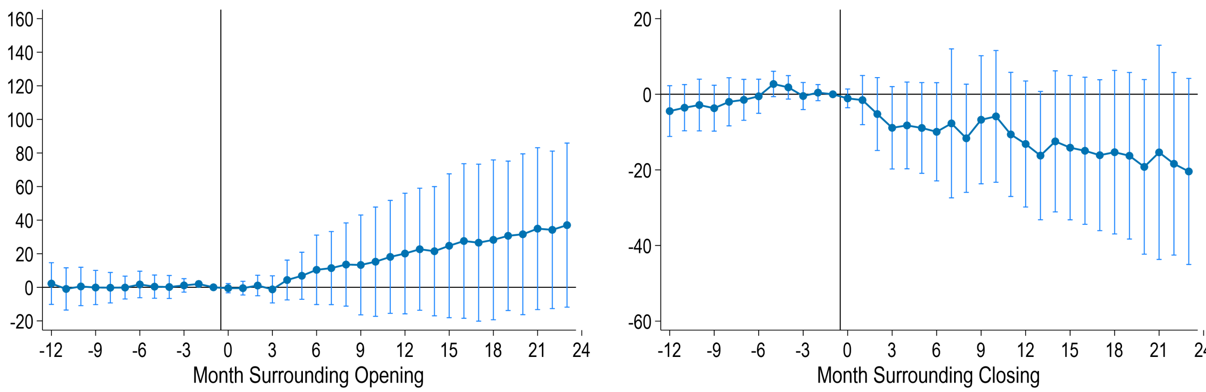
(b) Openings, Dynamic DCDH

(c) Closings, OLS–TWFE

(d) Closings, Dynamic DCDH

Notes: This figure includes event studies from OLS and the "dynamic" DCDH approach on the number of people on SNAP in the tract, where treatment is offices opening in the tract (upper figures) or closing in the tract (lower figures), in urban areas. Results from 2 methods are shown, TWFE, and de Chaisemartin et al. (2024). Baseline mean is 281.4 cases for openings and 373 for closings. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-0333.

Figure A4: Effect of SNAP Office Changes on Number of Active SNAP Cases, Urban Tracts, Initial Tract of Residence Held Fixed



(a) Openings (b) Closings

Notes: This figure includes event studies from the "dynamic" DCDH approach on the number of people on SNAP in the tract, where treatment is offices opening in the tract (left figure) or closing in the tract (right figure), in urban areas. In these plots assign tracts based on the first observed residential location in the SNAP administrative files and hold that location fixed over time. Results from 1 method are shown, de Chaisemartin et al. (2024). Baseline mean is 285.6 cases for openings and 373.4 for closings. Approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-0333.

Table A1: Summary of Indiana Tracts with No SNAP Offices Compared to Tracts with SNAP Offices

	(1) Tracts with No SNAP Office Mean	(2) Tracts with SNAP Offices Mean
% Female < High School	54.839	61.920
% Male < High School	53.138	59.620
% Immigrants	3.084	2.921
Median HH Income	44,778	36,627
% Below Poverty	9.506	11.878
% Black	8.535	6.761
% Hispanic	3.511	3.654
Mean HH Size	2.572	2.401
% Employed	95.036	94.366
Observations	1236	174

Notes: Includes all tracts in Indiana and reports characteristic means for tracts with no SNAP offices and those with SNAP offices. Tracts are determined to have a SNAP office if they have an open office within the tract at any point between 2006 and 2016. Uses baseline pre-opening and closing tract level summary statistics. Weighted by population.