How Place and Need Intersect:
How Lack of Enrollment Offices Deters Nutrition Assistance Take-Up*

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February 25, 2025

[INCOMPLETE: Do not cite or circulate.]

*We thank Anna Aizer, Joseph Doyle, Jessie Handbury, Katherine Meckel, Maya Rossin-Slater, Diane Schanzenbach, and James Ziliak for helpful comments as well as seminar participants at the Institute for Research on Poverty Summer Workshop, the University of Colorado–Denver, the National Tax Association, the Utah/BYU joint seminar series, the University of Michigan's Survey Research Center, the University of San Francisco, the Opportunity and Inclusive Growth Institute at the Federal Reserve Bank of Minneapolis. We thank Seojung Oh, Alec Erb, Kristine Thomas, and Paige Nelson for helpful research assistance. This research was supported by USDA-ERS Cooperative Agreement 58-4000-8-0049-R, the Eunice Kennedy Shriver National Institute of Child Health & Human Development of the National Institutes of Health under Award Number R01HD0938898, and a Tufts/UConn RIDGE Grant. The findings, conclusions, views, and opinions are solely those of the authors and do not necessarily represent the views of the United States government, the National Institutes of Health, and RIDGE.

1 Important Note on Disclosure

Due to the sensitive nature of the data used in this project, all results must first go through disclosure review from the organization that houses the data and the State SNAP Department. We also cannot disclose the name of the state that is the focus of the study. We will call it "State X" until disclosure review is complete. We are currently in the process of submitting and navigating this complicated and lengthy disclosure process. Unfortunately, even though the analyses of the paper are complete and known to us, we cannot include them in this draft until disclosure is finalized. In this draft, we do our best to describe the project without discussing any of the findings.

2 Introduction

The U.S. has a patchwork social safety net, with a host of programs targeting different populations, with widely varying eligibility rules and forms (cash and in-kind). 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. One other key feature of the US means-tested safety net is the need for individuals to apply to participate in these programs, and it is 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, with the causes not being fully understood. A large, older, and primarily theoretical literature in economics (Nichols and Zeckhauser, 1982) suggests that this incomplete take-up (not having all eligible persons sign up for and use programs) may be efficient, because it deters the least needy from participating. In this neo-classical view, if all participants have full information and program administrators also want to maximize participation among eligibles, incomplete take-up simply reflects that more able individuals who could manage without program access decline to sign up.

A more recent literature from public administration and behavioral science suggests two reasons that the least advantaged may not be using the programs in which they are eligible. Behavioral science suggests this incomplete take-up could be because individuals have limited capacity to manage enrollment and maintain eligibility for programs. Public administration suggests a large role for what it calls administrative burden, where federal, state and local governments are not always trying to maximize participation among eligibles (Barnes et al., 2023; Herd and Moynihan, 2018). A related literature tries to assess both whether various social programs are well targeted to the needy (e.g., Deshpande and Li, 2019; Finkelstein and Notowidigdo, 2018) and also to assess the welfare benefits of expanding take-up—either by using a social welfare function approach, or by applying another criteria such as the marginal value of public funds (Hendren and Spring-Keyser, 2020).

We, by contrast, leverage quasi-experimental variation in the presence of SNAP offices on take-up of SNAP. SNAP is a crucial part of the U.S. social safety net, providing the only broad-based universal benefits via EBT cards for low income, otherwise eligible individuals. SNAP is also large, serving 22.3 million people in May 2023, with more than $80 billion in SNAP benefits going out in fiscal year 2023. Benefits are substantial; per capita benefits in May 2023 were $177.54.[1] Yet, despite the relatively high value of this program to participants, participation among eligibles is well under 100%. Estimates from October 2019 through February of 2020 show that take-up of SNAP in normal times is well under 100%, with average take-up rates being close to 80%, but showing wide variation across

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

states, ranging from 49% in Wyoming to full take-up in a number of states.[2] These low take-up rates suggest that there are features about the US system of providing food assistance which deter eligible persons from applying. To participate in SNAP, individuals must apply to the program, typically filling out forms, submitting documentation of earnings and asset holdings, and participating in an interview, and then being deemed eligible. SNAP eligibility forms are quite lengthy, requiring extensive information on the past residence and work experience of household members, and for some, account balances for a host of assets. Eligibility requires applicants have low income or participate in SSI/TANF and some citizenship requirements, and non-elderly recipients have to recertify at least once a year. In many states, applications can be done online and interviews can be done by phone. Yet, as we discuss further below, most applications seem to be submitted in offices, and, in many states, caseworkers do more than simply interview recipients, providing assistance with completing the forms. SNAP is also somewhat unique in that recipients cycle on and off the program, unlike a program like SSI where most recipients stay on the program for long spells. This complexity and need for frequent interactions with the bureaucracy suggest that proximity to a SNAP office could be an important determinant of SNAP take-up. The size and scale of the SNAP program and our access to complete administrative SNAP data provide large samples which should enable us to detect economically meaningful effects of office openings, closings, and relocations.

Much of the existing literature on take-up, targeting, and welfare touched on above leverages randomized control trials on limited populations or tests nudges in specific settings. We instead harness a host of exogenous shocks to SNAP participation caused by changes in the costs of enrolling in the program. Specifically, we look at how take-up of SNAP responds to plausibly exogenous shocks to program access driven by SNAP administrative offices opening, closing, or relocating. We do so using unique data, combining administrative records on use of SNAP with administrative program participation for TANF, Medcaid, and Section 8/vouchers. The micro SNAP administrative data are uniquely beneficial for this research question. Publicly available micro data on SNAP use are self-reported, have limited geographic detail, and are known to suffer from severe underreporting (e.g., Meyer et al., 2015). Publicly available administrative data provide aggregate totals for SNAP participation and benefits disbursed at the county level, with no information on case characteristics beyond use of welfare and no detailed geography. With the micro SNAP data, we can accurately determine participation in SNAP according to distance from the SNAP offices. An additional advantage to using the administrative data is that it allows us to assess target-

[2] https://fns-prod.azureedge.us/sites/default/files/resource-files/snap-participation-rates-2020summary.pdf

ing by using characteristics of the individuals verified through the eligibility determination process. Thus, we can tell whether effects on those who join the program when a new office opens or drop out when a local eligibility office closes are among particularly disadvantaged participants (those with no earned income) or among those who are less mobile (elderly, disabled, or cases with children).

Almost everything we know to date about how people respond to programs is driven by changes in program rules. In our setting, we can instead leverage changes in the costs to participating in a difference-in-differences framework. Our design requires us to determine the location of SNAP offices over time. We hand collected these data from sources detailed below. With these data, we show participation in the program [results awaiting disclosure] when access to offices change. We present two way fixed effects and event study estimates, with our main results leveraging various approaches to dealing with possible treatment effect heterogeneity within group and time (e.g., De Chaisemartin and d'Haultfoeuille, 2020; Dube et al., 2023). We carefully explore the exogeneity of office locations opening and closing. We do this both by looking at how the caseloads on SNAP and their characteristics vary by whether offices open or close and by looking at how the presence of stores and people vary. This work suggests there are not systematic patterns in participation or in the population or food environment related to the opening and closing of offices.

Interestingly, among the states for which we were able to collect SNAP office locations, we find one state is unique. State X rents and routinely relocates the buildings where its government offices for SNAP are housed and provides most of the variation in our openings and closings. Talking to program officials clarified that this is deliberate as State X has offices in all of its 90-plus counties and attempts to locate them to facilitate access. Our analysis then looks at the number of participants (or participants from specific groups) within concentric circles around the locations where an office is at some time in our panel. We focus our analysis on State X for two reasons. First, State X provides roughly 80% of the identifying variation in SNAP office location changes. Second, because the timing of office changes is a function of lease length, office changes are plausibly uncorrelated with other determinants of SNAP participation. We find strong evidence that access to offices [awaiting disclosure]. Our findings are important for understanding how changes in program access link to take-up as well as how it interacts with targeting.

In section 2, we present more background on SNAP and discuss on the related literature. We next discuss our data in section 3 and our empirical strategies in section 4. Section 5 contains our results and we conclude in section 6.

3 Background

As noted above, SNAP is the backbone of the US safety net. It is the the largest of USDA's food assistance programs and the only means-tested transfer program available to nearly all low-income US households. In FY 2019, SNAP covered 35.7 million Americans (11% percent of the population) in an average month, at a cost of $60 billion. The program provided the average recipient $130 per person each month. There is considerable evidence that SNAP reduces food insecurity (e.g., McKernan et al., 2021), and poverty (e.g., Jolliffe et al., 2023), and that access to SNAP during childhood improves long-term health and self-sufficiency (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 is means-tested based on both income and asset tests and includes work requirements for some households, such as able-bodied adults without dependents (ABAWDS). Households are typically certified to participate for periods ranging from 6–12 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.

While an application and interview are required of all SNAP applicants, states have been given latitude in both how they receive applications and how they conduct interviews. To learn more about this process as well as the services provided by SNAP offices, we communicated with several urban and rural offices in each state we study. Applicants 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. The States also vary in how they administer interviews. A few states require in-person interviews, 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).[3] 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 in person instead of through the seemingly more convenient online submission portals? One possibility is that they benefit from assistance in applying for SNAP when they visit offices in person. The

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

applications are long and complex, requiring documentation of income and assets as well as child care, medical, and housing expenses. A 2000 study 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 (Bartlett et al., 2004). Cook and East (2023) find that caseworkers play an important role helping applicants navigate the application process. In a phone interview with the program administrators in State X's SNAP program, we learned that SNAP offices in State X 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 applications, SNAP offices often provide other services. In some states, SNAP offices provide resources to connect SNAP applicants to other programs such as TANF, Medicaid, WIC, and housing assistance. In other states, the SNAP office jointly administers other assistance programs (most commonly TANF).

In our design, we exploit changes in office locations to isolate the causal effects of geographic access to offices on SNAP participation. To do so, we need to argue that the location and, in particular, the timing of office location changes is not related 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 was little evidence that states systematically locate offices based on trends in local caseloads or eligible populations, and in fact given the lags with 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. Sometimes, budget cuts cause states to reduce the number of offices. For example, Idaho closed 7 of its 28 offices in May 2010. Other states relocate offices more frequently. State X purposefully only leases all SNAP offices and every 4-6 years the leases expire and offices often relocate. State X 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. The timing of these changes occur whenever the lease happens to be expiring and is thus unlikely to be related to changing economic conditions. Of course, there are many other reasons why offices close or relocate. For instance, while collecting office locations, we learned that the North Shore, Massachusetts office was moved because lightning struck the old building and caused damages. While the Framingham, Massachusetts office moved because of a town sewage back up that frequently affected the building.

3.1 Prior Literature

A large literature studies how economic conditions and policy choices affect SNAP use (e.g., Bitler and Hoynes, 2016; Ganong and Liebman, 2018; Kabbani and Wilde, 2003; Klerman and Danielson, 2011). These studies generally examine state-level policies, and find that SNAP participation is positively related to policies that reduce transaction costs, such as longer eligibility recertification periods (Homonoff and Somerville, 2021).

Research also examines the determinants of take-up in SNAP and other social assistance programs (Currie, 2006; **?**). Recently, Finkelstein and Notowidigdo (2018) find that randomly providing information about potential eligibility and assistance enrolling in SNAP increases take-up among the elderly, and assess whether these increases are welfare enhancing, introducing a new framework for including public costs of determining eligibility.

While to the best of our knowledge, no other work focuses on the effect of distance to SNAP offices on take-up or participation, an existing literature has explored the extent to which closing and opening eligibility-determination offices for other programs affects program take-up, and to some extent, assessed targeting.[4] Deshpande and Li (2019) shows that closing the Social Security Offices which determine eligibility for Supplemental Security Income and other SSA programs leads to a large, 16 percent reduction in the count of disability recipients near closing offices, with effects being larger for less-educated or more-disabled individuals. Disability applications also went down, but by slightly less. As part of a broader analysis, Rossin-Slater (2013) looks at the closing of WIC clinics in Texas with a sample of mothers with more than one birth during the study period. She finds having a WIC clinic in her zipcode during pregnancy led to about a 6 percent increase in the probability that a mother used WIC for food benefits.

This study makes several contributions to the literature. We provide the first study to directly test whether SNAP take-up is sensitive to minimum travel distances to SNAP offices and how this varies by urban or rural status of the offices. Our novel administrative records provide geocoded residential information for every SNAP participant across many states, allowing us to explore this relationship at an unprecedented level of geographic detail and precision.

[4]Work has also examined the geography of SNAP use (e.g., Goetz et al., 2004; Slack and Myers, 2012), but much of this work uses caseloads at the county level and cannot examine take-up in a nuanced fashion. Much of the other work on take-up focuses on higher levels of aggregation than the county level.

4 Data

4.1 Administrative Data Sources

To study the effect of geographic access to SNAP offices on participation, we utilize novel administrative data from State X. The data include demographic information for SNAP recipients along with benefit amounts and reported income of the case. Importantly, the state provided us with residential address information at the monthly level, which allow us to exactly pinpoint the distance between residence and SNAP offices. Previous researchers have not enjoyed this level of specificity in their measure of travel distance. Existing research has calculated distance using centroids of the aggregated geographic regions (e.g., census-tracts, counties, or zip codes) as a proxy for true residential address. To the extent that these approaches randomly mismeasure residential address, resulting estimates will be attenuated toward zero. Further problems come from survey data with reporting error in use of SNAP, which Meyer et al. (2018) have shown is widely under-reported in ways that vary with individual observables and likely unobservables and thus is likely not innocuous. We also merge these data with administrative program data for Section 8/vouchers, TANF, and Medicaid for State X to study the impact of access to SNAP offices on cross-program participation.

4.2 SNAP Office Location Data

We merge these administrative records with roughly twenty years of hand-collected information on the location of every SNAP administration office. We collect SNAP office location data using Internet Archive's "Way Back Machine" (WBM). 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.

For each state of our broader study, we create a list of SNAP offices and attempt to record the span of dates that each office was open at their given location.[5] For each state in our frame, we locate the state SNAP agency's website that displays office locations and systematically move through time using the WBM interface and record the date range

[5]Specifically, 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.

over which we observe changes in office locations, or openings or closings.[6] 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.[7] It is also worth noting that our of 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. Appendix Figure A1 shows the WBM interface for an office that changes its location across two months in Indiana.

We were able to collect SNAP office locations for roughly half of the country. In doing so, we learned that State X was an outlier in terms of the number of office changes we observed relative to the other states for which we collected data. From 2006 to 2016, State X had 118 office closings. The next two closest states had 79 and 44 closings, with most states having about 15 closings. Over the same time, State X had 109 openings. The next two closest states had 40 and 24 openings, with most states having about 10 openings. Through conversations with State-X administrators, we learned that the state leases its buildings intentionally so that offices will periodically move and allow for more equitable access to the offices over time. Importantly to our design, the timing of office changes is primarily a function of when the given lease is set to end, which is plausibly unrelated to trends in local SNAP participation. As a result, we focus our study exclusively on the impact of access to SNAP offices in State X, but we use the office location information from other states to help benchmark the prevalence of office changes more broadly.

Figure 1 plots counts of openings and closings that we manually collected over time. In general, physical access to SNAP offices is declining over time—more offices are closing than opening on average across states. 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. We plot State X office changes in panel b, and show a different pattern. In this state, most closings and openings are simply a result of relocating offices due to ending leases and not an overall pattern of removing access to offices. Figure 2 also shows the geographic spread of office changes across four example states for which we collected office location information.

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

[6]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.

[7]We also manually called offices with long date ranges where possible to get a more accurate date of change.

area. This can be seen in survey data. Appendix Figure A2 shows differences in travel distances to shop at grocery stores from the 2017 wave of the National Household Travel Survey (NHTS) which provides information on 900,000 trips taken across roughly 120,000 nationally-representative households. Driving distances for the median, 75th, and 90th percentile households located in areas with fewer than 50,000 residents are roughly 5, 11 and 22 miles, respectively. For cities with over 50,000 residents, the distribution of travel distances are nearly indistinguishable. In these cities, the median, 75th and 90th percentile travel distances are 2.5, 5 and 10 miles, respectively. Longer travel distances are clearly more common in less densely-populated areas. As a result, we might expect the effect of access to SNAP offices to similarly differ by the population density of the surrounding area. To classify whether a SNAP office location is urban or rural we use the 2010 Census Urban Area National Shapefile. We define offices as being "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 State X.

4.3 Sample Restrictions

We make a few restrictions to our final regression sample. First to avoid the COVID-19 pandemic, we only consider SNAP participation prior to 2019. Further, an important outcome for our analysis is whether the case is new to the system. Because the administrative data does not contain date-of-first receipt, we must infer this from the data. To do so, we construct a new-case measure that equals one if the case is not observed receiving SNAP for the 12 months prior to the given month. We construct the measure in this rolling manner so that cases at the beginning and end of the sample have the same criteria. We limit the regression sample by dropping the first year of data which allows us to calculate the new-case measure for all years in the sample. Our final sample includes SNAP recipients from 2005 to 2018.

We similarly restrict attention to SNAP office changes that occur between 2005 and 2018 in State X. However, 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.

4.4 Measuring Access to SNAP Offices

We conceptualize access from the perspective of the SNAP offices. Specifically, for every latitude/longitude combination for which we observe an open SNAP office at some point across the study window, we count the number of SNAP cases that reside within a given distance to the office. We perform these counts in one-mile increments from one mile away from the office all the way up to ten miles away. We aggregate these counts for every year-month combination in the administrative data so that we have the number of recipients for time periods both preceding and following an office opening or closing. In the case of overlap, we only attribute a case with it's closest office. Figure 3 shows a fictitious example for how we generate these counts for SNAP-authorized stores (the circles) in relation to SNAP offices (the diamonds).[8] Similar to as is depicted in this figure, we only attribute each case to its closest office as represented by the color of the circles and diamonds. The benefit of this approach is that each case is only counted once even if it falls within multiple office radii. Finally, the detailed administrative data also allow us to compute counts of SNAP cases with particular characteristics such as race/ethnicity, age, presence of children, and income. This detailed administrative data allow us to characterize the change in access to SNAP offices at an unprecedented level of depth and accuracy.

5 Empirical Methods

To estimate the impact of access to SNAP offices on SNAP take-up, we will exploit variation in office proximity created by SNAP offices opening, closing, or relocating. We do so in a local projections approach to difference-in-differences (LP-DID) following Dube et al. (2023). Specifically, we estimate

$$y_{i,t+h} - y_{i,t-6} = \beta_h \Delta D_{it} + \delta_t^h + \epsilon_{it}^h \qquad \text{for } h = 0, ..., H, \tag{1}$$

where i indexes SNAP office locations, t indexes calendar-year-month combinations, and h indexes event time. D_{it} is an indicator variable that equals one if the location i is treated in the given period t. We analyze openings and closings separately so the treatment event will respectively either be an office opening or closing at the given location. δ_t^h are calendar-year-month fixed effects. We cluster standard errors by SNAP office. Importantly, we estimate Equation 1 among the set of observations that are either newly treated (i.e., $\Delta D_{it} = 1$) or are clean controls. We define clean controls to be office-month combinations where the office will experience the relevant opening/closing at some point in the future, but never in the past

[8] We depict stores instead of residences in this example to protect sensitive residential information.

and not during the two years following the event (i.e., the post-event horizon). This clean control group is ideal because it does not include already-treated offices—circumventing the negative weighting issue in staggered designs. These clean controls also help to strengthen the exogeneity condition by explicitly leveraging only the timing of office changes, similar to Deshpande and Li (2019). Because they will be treated in the future, control locations share the unobservables that make them a desirable setting for an office to locate and the only difference in treatment status arises from the timing of the treatment. Recall in our setting that the timing of office changes is primarily a function of when the given office lease expires. This predetermined expiration date is unlikely to correlate with trends in unobservables that determine SNAP participation.

We separately estimate Equation 1 for the different event times h surrounding when the given office either opens or closes. Because we often only observe a range of months over which a SNAP office location change occurs, we benchmark all estimates to 6 months prior to our best guess of when the change occurred. We consider SNAP participation for the 12 months preceding an office change and the 24 months following. Intuitively, because the outcome has already been long-differenced for the given event time—obviating the need for unit fixed effects—and because the specification includes calendar fixed effects, the β_h estimates have a similar interpretation to estimates from more traditional event study designs.

This approach circumvents the negative weighting issue discussed in the new staggered-event-study-design literature (e.g., De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021) that arises from including already-treated controls. While the approach is relatively new, Dube et al. (2023) show that the linear projection approach is equivalent to the stacked regression designs published in notable papers such as Cengiz et al. (2019) and Deshpande and Li (2019). The benefits to this approach relative to stacked designs is computational speed and simplicity of the specification. LP-DID does not require the researcher to manually append datasets together for each event studied—a procedure that is prone to coding errors, is computationally expensive, and adds an unnecessary layer of complexity for the reader. LP-DID still enjoys all the benefits of the stacked design. The researcher has flexibility over how they choose the clean controls. Here, we use this flexibility to only consider other locations that will be treated at some point in the future as clean controls. A decision that lets us explicitly use the variation in treatment timing rather than location. We show robustness of our estimates to traditional two-way fixed effect designs as well as the two-step difference-in-difference technique proposed by Gardner (2022).[9]

[9]Other proposed methods would also make excellent alternatives, but releasing additional estimates through disclosure is costly and so we limit our discussion to these models.

By default the LP-DID from (1), provides a weighted average of all cohort-specific treatment effects that are a function of treatment variance and sample size—and importantly are always positive. In the language of Goodman-Bacon (2021), the LP-DID estimates a variance-weighted average treatment effect on the treated. We follow the reweighting procedure proposed by Dube et al. (2023), to instead estimate an equally-weighted average treatment effect on the treated, which is more natural to interpret.

5.1 Testing Identifying Assumptions

The two key assumptions for all difference-in-differences methods, including LP-DID, are the assumptions of no anticipation and parallel trends. 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 assumptions 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. There is also measurement error in the date of office changes that arises from how often the WayBack Machine scrapes the government webpage.[10] Even if perfectly observed, our measure of location change is simply what is posted online. If there are delays or preempting in when an office location change is posted, that would be an additional source of measurement error. In order to account for this measurement error and for the possibility that the public learns of the impending location change, we benchmark all estimates from (1) to 6 months before the event. We observe roughly 98 percent of all posted office changes within a 6-month window and it is unlikely that the public would be notified of office changes any earlier as well.

The parallel trends assumptions additionally requires that SNAP participation around newly opening or closing offices would have evolved in the same way relative to the not-yet-treated locations over the same time period. As is common in the literature, we provide evidence for this assumption by exploring differences in treatment effects during the periods leading up to an office opening or closing using (1). Again, because we are explicitly leveraging differences in timing of office changes driven by lease expiration dates, there is no reason to believe ex-ante that treated and non-yet-treated locations should be trending differently in their SNAP participation. We see that [results awaiting disclosure].

In addition to testing whether the determinants of SNAP participation are differentially trending between treated and not-yet-treated areas, we further test whether the retail

[10]For State X, we are able to observe the exact month of opening and closing for about 65 percent of events. We observe roughly 95 percent of events within a 3-month window.

landscape differs as well. To do so, we utilize data from the Store Tracking and Redemption System (STARS) from USDA. These data track the location of SNAP-authorized stores in any given month. We perform a similar aggregation exercise as discussed in Section 4, where we count the number of SNAP-authorized stores that are open within various radii during the given months surrounding an office location change. We present event-study estimates from (1) but using these store counts as the outcome in Figure [results awaiting disclosure]. We see that [results awaiting disclosure].

6 Results

6.1 The Effect of Access to SNAP Offices on Participation

First, we explore the impact of opening a SNAP office on program participation within one mile of the office. Because travel costs likely vary by the urbanicity of the area, we split estimates by whether the office is located in a urban area (specifically an Urban Cluster or Urbanized Area). We see that [results awaiting disclosure].

Changes in participation levels can be the result of either people entering/leaving the program or from current participants changing their residential location. We attempt to disentangle these two channels. To do so, we use as an outcome an indicator for whether the case was on SNAP during both the focal and preceding month and who also changed residential addresses during that time. Intuitively, this captures whether people who were already on SNAP are more or less likely to move to areas experience SNAP office location changes. This could happen for instance if SNAP offices target areas that are changing in their affordability or even if the presence of a SNAP office causally impacts the desirability of the surrounding area. We find that [results awaiting disclosure].

We next explore whether the effects that we have estimated differ by case demographics. Specifically, we explore treatment effect heterogeneity across the gender, age, race/ethnicity, presence of children, and working status for SNAP heads of household as well as for case-level measures of income. We find that [results awaiting disclosure].

Finally, we explore whether access to SNAP offices impacts cross-program participation. We are able to observe administrative data on program participation across several important dimensions of the safety net. We can observe whether people receive Medicaid, cash assistance (TANF), or if they are participating in HUD-specific programs such as public housing or housing vouchers. Access to SNAP offices could affect cross program participation in two ways. First, State X specifically tries to co-locate public program offices such as SNAP, WIC, TANF, Medicaid, Child Care, and motor vehicles, to make public programs more accessible to residents. Thus, if a SNAP office co-locates with these other programs,

cross-program participation could increase. Secondly, in State X, caseworkers are trained to provide information to SNAP applicants on other safety net programs and resources that they may be eligible to receive. We find that [results awaiting disclosure].

7 Conclusion

Even though the nominal value of SNAP is fixed across the country (with higher allotments given to Alaska and Hawaii), variation in local food prices create substantial variation in the real purchasing power of SNAP benefits (Bronchetti et al., 2018). In the rural areas of the country, long travel distances to program offices similarly create barriers to utilizing benefits, particularly for extremely low-income or elderly populations. As a result, when offices open and substantially reduce both fiscal and physical travel costs, there is scope for SNAP take-up to increase. Our finds shed light on [results awaiting disclosure].

This research has several implications for public policy. This study informs whether policies aimed at reducing physical barriers to program offices and caseworker assistance could help the geographically isolated and impoverished communities of the country. Specifically, [results awaiting disclosure].

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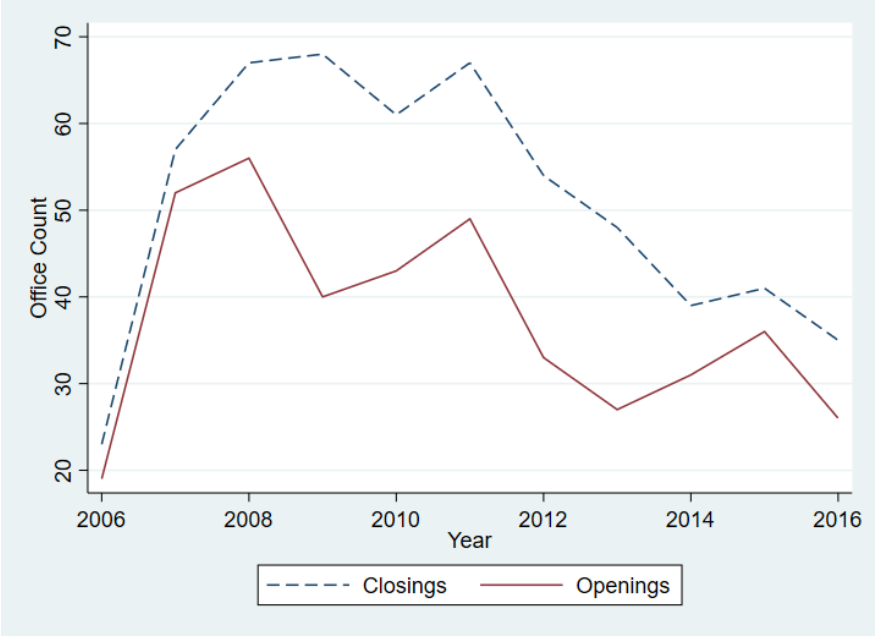
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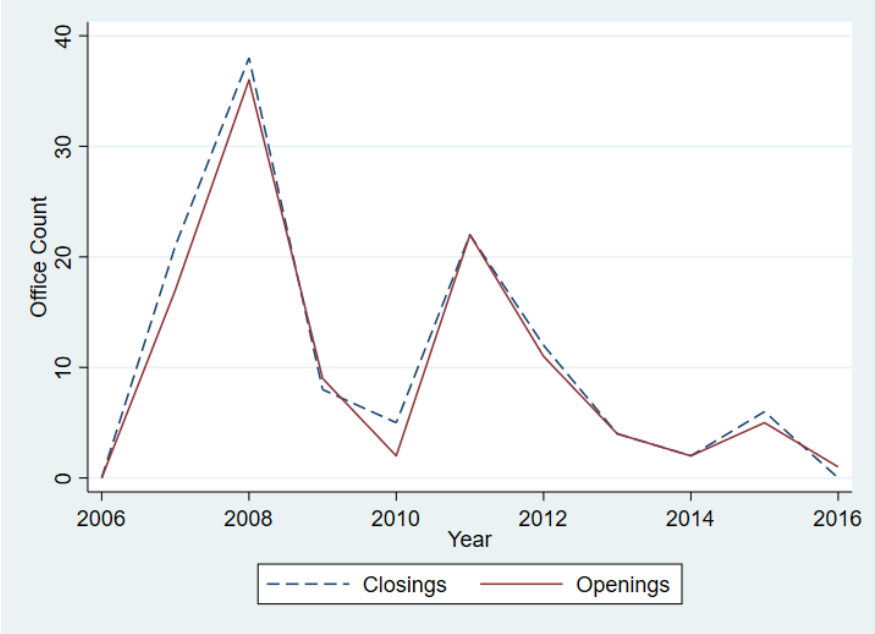
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Figure 1: SNAP Office Openings and Closings Over Time



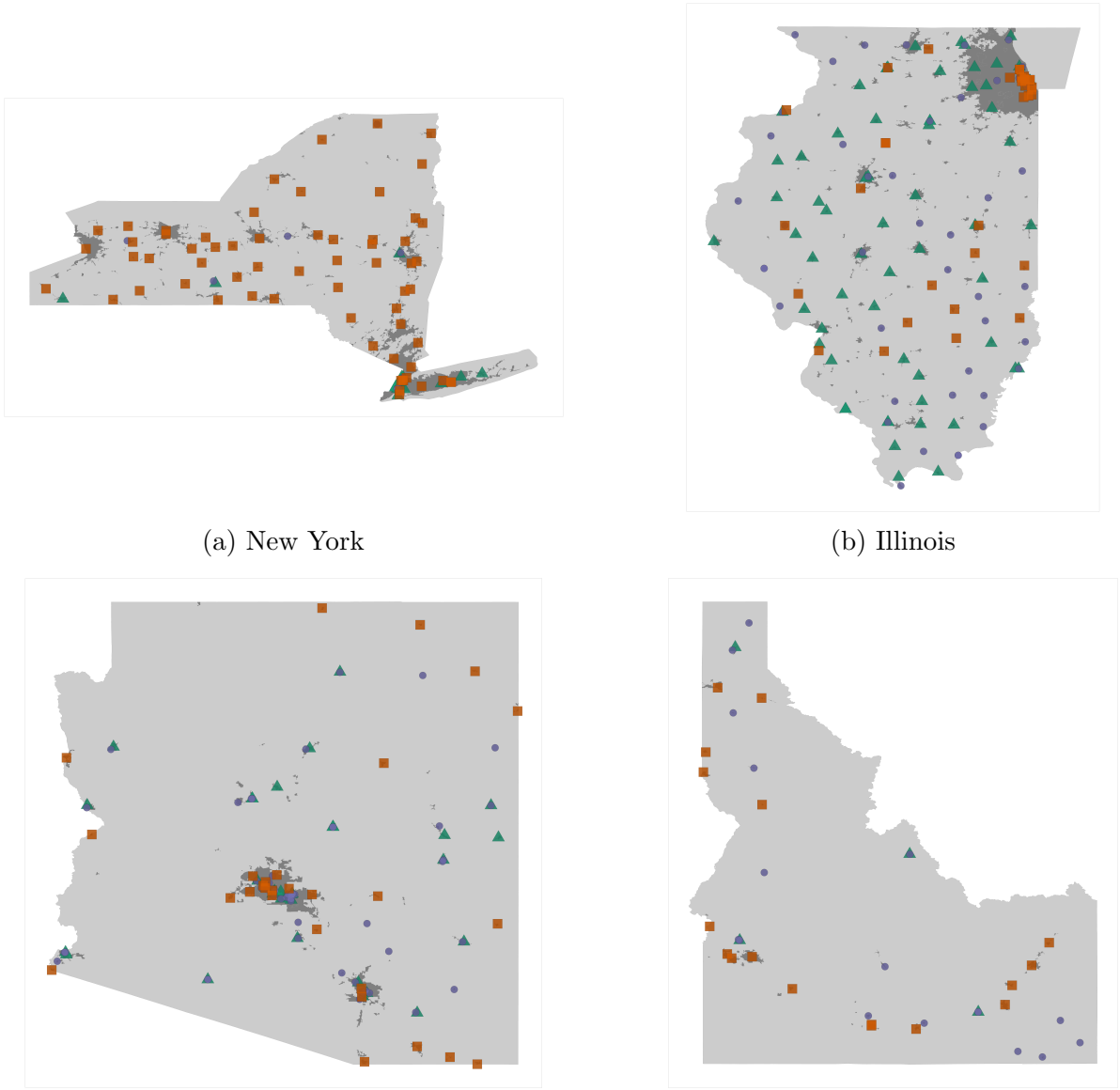
(a) All States with Collected SNAP Data



(b) State X

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 State X. "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

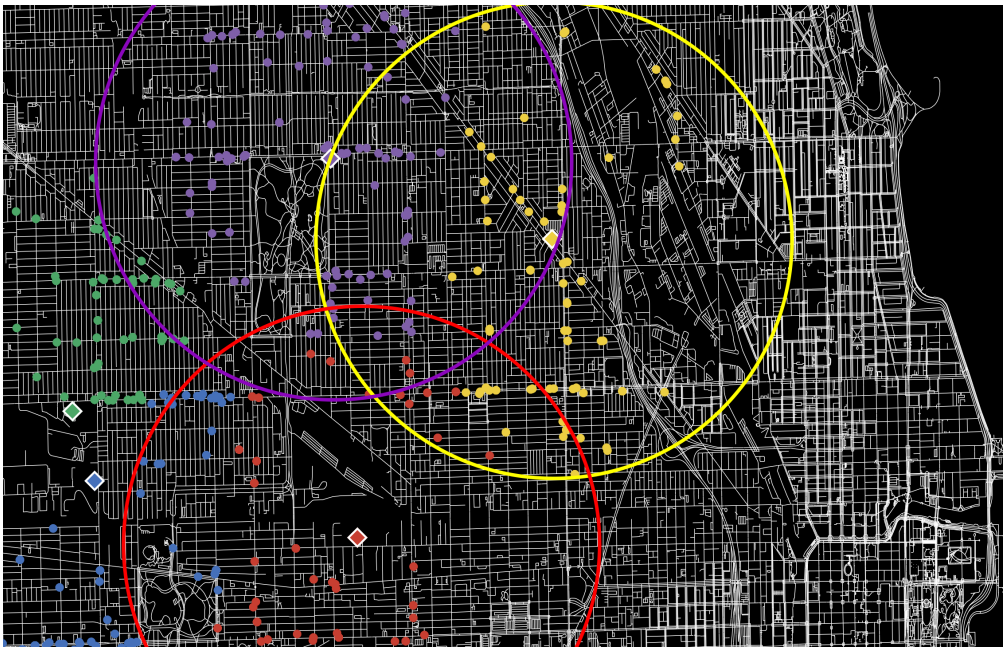
(b) Illinois

(c) Arizona

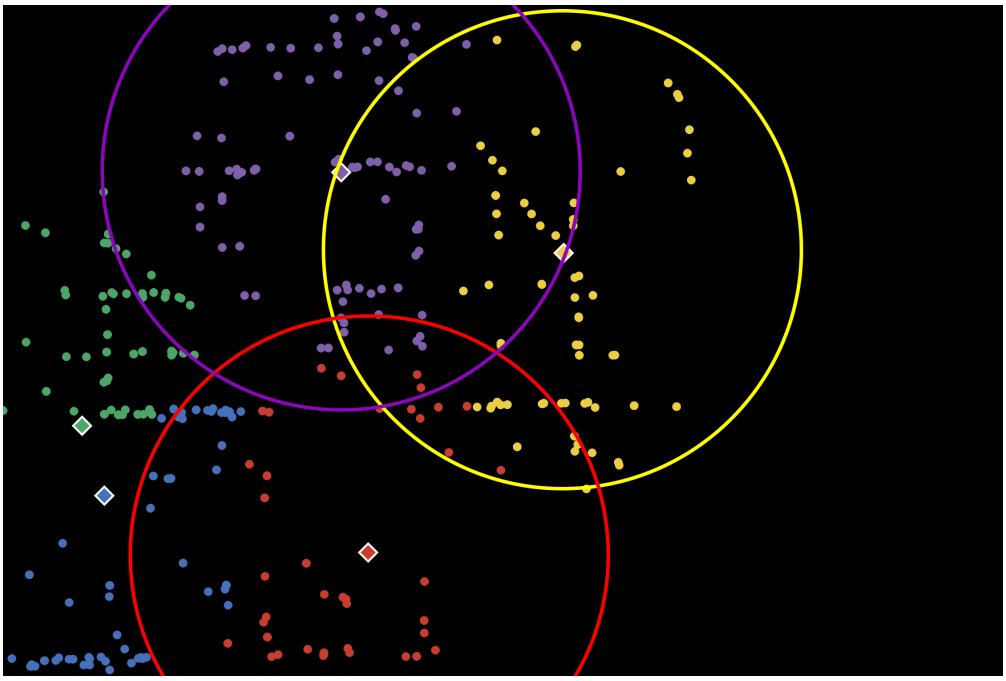
(d) Idaho

Notes: Figure displays the geography of office openings and closings for four 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.

Figure 3: Chicago SNAP Offices and Stores



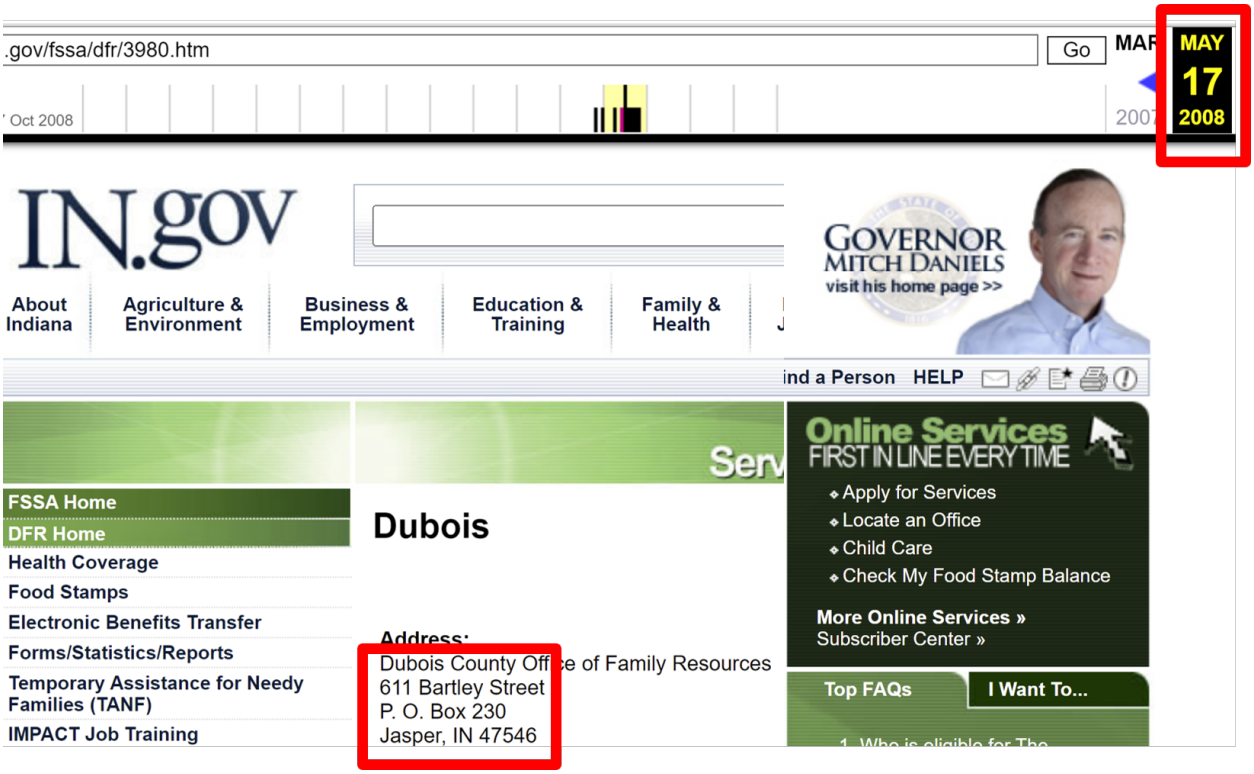
(a) With Streets



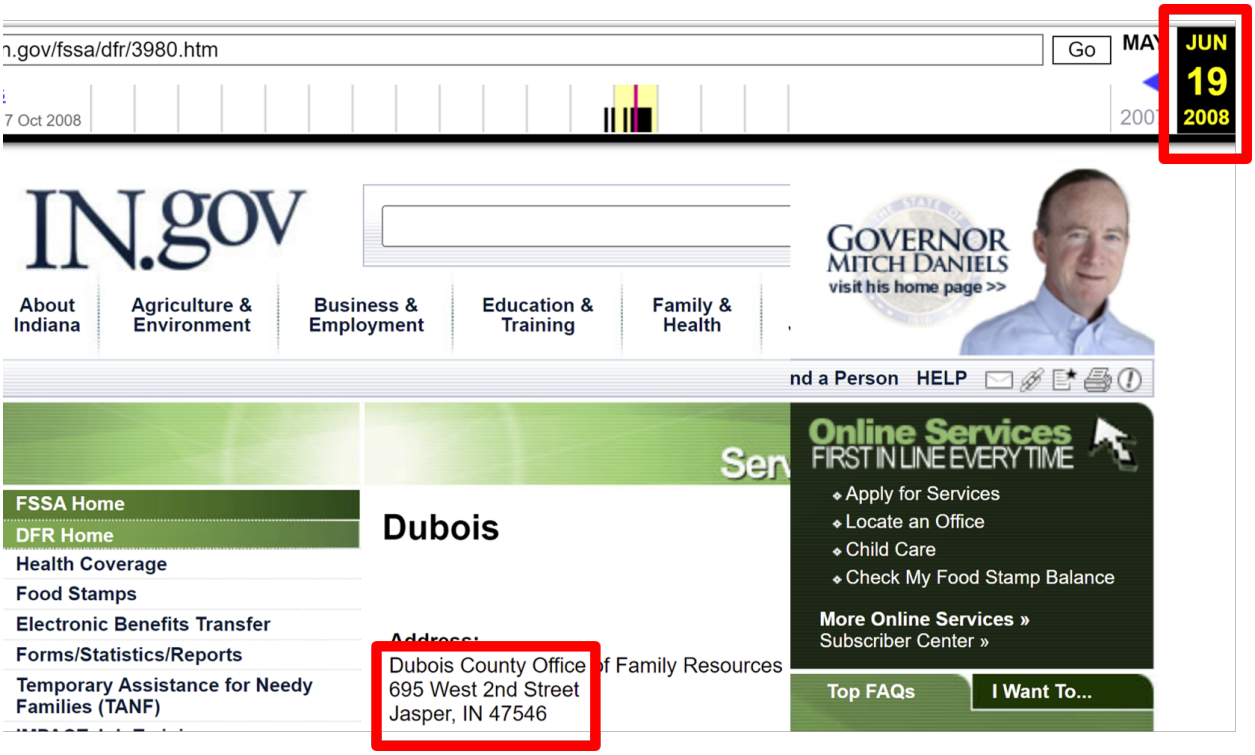
(b) Without Streets

Notes: Figure depicts a fictitious example for how we count households that are nearby a SNAP office. For data security reasons, here instead though we plot SNAP-authorized stores (dots) in relation to SNAP offices (diamonds). Panels a and b display streets or not for ease of visualization. The rings are meant to depict a given radius that is being search over. The color of the dots means they are assigned to the SNAP office of the same color. In the event that multiple radii intersect, we assign the store to its closest SNAP office.

Figure A1: Way Back Machine Web Scraping Example



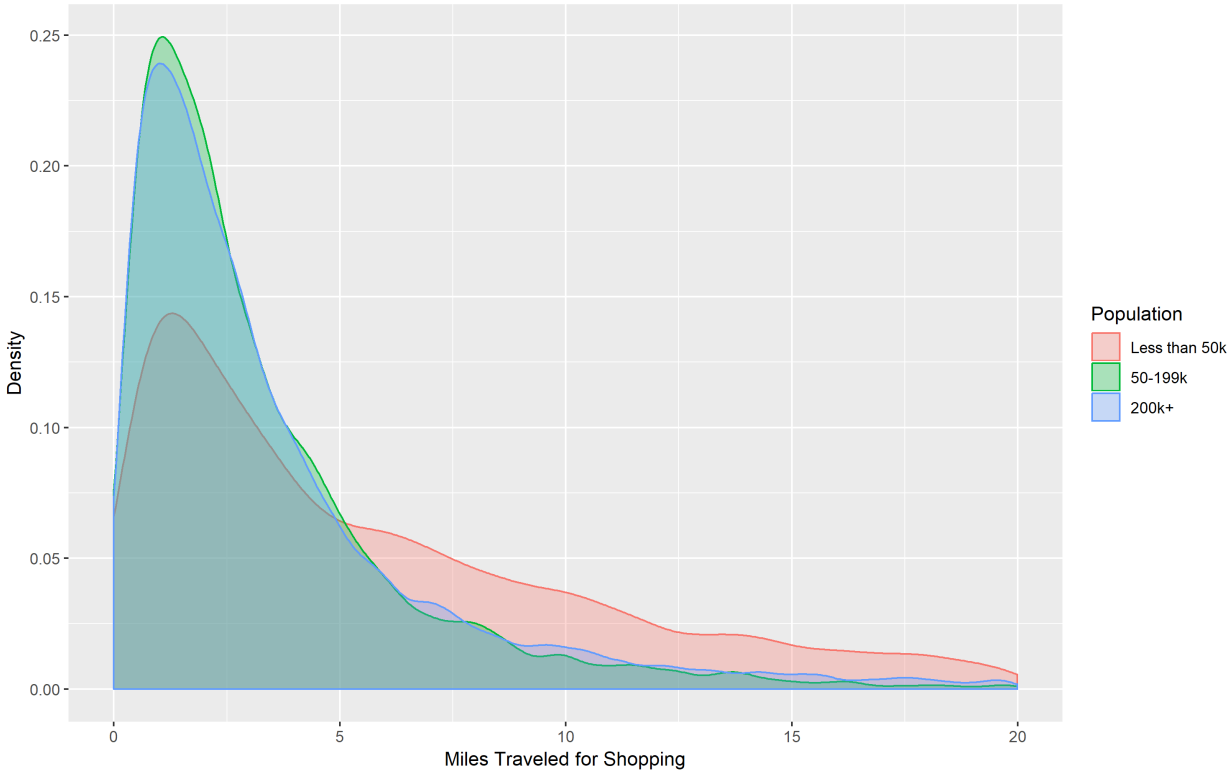
(a) Dubois County Office – Pre-Change



(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.