Leaving Benefits on the Table:
Evidence from SNAP

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Abstract

Many social insurance programs require that beneficiaries periodically re-verify eligibility, allowing officials to identify ineligible applicants but risking the loss of eligible applicants who do not complete these requirements. While many states have invested in simplified verification procedures to reduce that risk, the extent of exit among eligible beneficiaries and the efficacy of these simplifications is not well established. This paper uses administrative data from the Supplemental Nutrition Assistance Program (SNAP) to establish three facts. First, retention in SNAP is low, with approximately one-half of entering cases leaving in the first year. Second, qualitative evidence and quantitative simulations suggest that approximately half of those who exit in the first year remain eligible. Third, using the staggered roll-out of an online case management tool in Michigan, I find that this simplification reduced the hazard rate of long-term exit at key verification dates by almost 2pp (12%). These facts suggest that eligible retention is very incomplete, and that ongoing simplification efforts increase retention among eligible beneficiaries.¹

¹I performed much of this analysis as an employee of the W.E. Upjohn Institute for Employment Research. An earlier version of this work is currently available online as Upjohn Working Paper 18-288. I am extremely grateful to Chris O’Leary and Ken Kline for making this project possible. I would also like to thank David Autor, Amy Finkelstein and Jon Gruber for their comments; Kathryn Law at USDA for access to supplemental data sources; and Jason Page at Michigan DHHS for generously sharing his institutional expertise.
1 Introduction

While researchers have documented low take-up in many public assistance programs (Ribar (2014)), few have distinguished between initial take up and retention in transfer programs. There is reason to believe that retention is an important channel of incomplete take-up in its own right: government agencies often require periodic verifications of eligibility, and may incidentally filter out eligible beneficiaries who fail to complete these requirements. The purpose of this paper is to illustrate the importance of understanding retention in at least one major transfer program – the Supplemental Nutrition Assistance Program (SNAP) – and to test the extent to which simplifying verification procedures increases program retention.

I make three distinct contributions. First, I document that retention in SNAP is quite low. Using administrative SNAP enrollment data from seven states, I find that about one-half of new SNAP cases are not receiving benefits one year later. Exits are concentrated at points when verification is required – especially at so-called recertification months – and usually last for an extended period of time. Second, I provide evidence that many of these exits occur among households that remain eligible. While I cannot directly observe eligibility for households that lose contact with SNAP officials, I use three distinct facts to support this conclusion:

- Administrative data from two states report the reason for program exit, which is overwhelmingly the result of a failure to submit paperwork rather than confirmed ineligibility.
- Combining administrative SNAP enrollment data with Unemployment Insurance (UI) records for the state of Michigan, I find that households with and without UI-covered earned income have similar rates of program exit at recertification.
- I simulate unobserved changes in income and household size for cases in Michigan by following households with similar demographics and earnings paths in the Survey of Income and Program Participation (SIPP). Simulations suggest that approximately half of the cases that leave the program by their one-year anniversary are still eligible.

Third, I provide quasi-experimental evidence that administrative barriers meaningfully affect retention. In 2009, Michigan introduced a website that helped beneficiaries understand program requirements, communicate with SNAP officials, and track their benefits online. The website was rolled out incrementally across different groups of counties at different times through the year. The new interface coincides with a sharp reduction in long-term exit at recertification of almost 2 percentage points (12%), with larger effects for childless adults and for cases with earned income.

This paper builds on an existing body of work that studies retention in public assistance programs, but alleviates multiple barriers that have limited previous empirical progress. One existing literature finds low retention in public health insurance programs (Dick et al. (2002); Sommers (2007); Sommers and Rosenbaum (2011); Pei (2017)). These results may not apply to other transfer programs, since beneficiaries may not know that their health insurance has lapsed and can often re-enroll retroactively. In contrast, those who exit SNAP lose immediate and often substantial flow benefits, and discover this quickly when they try to buy groceries. A different strand of literature finds moderate effects of SNAP policy changes using state-level difference-in-difference methods over extended time horizons (Helfin
This paper complements that work and corroborates its broad conclusions by studying the within-state roll-out of a program over a short time horizon, thereby alleviating possible concerns over differential trends or endogenous state policy decisions. Moreover, this paper draws from a small literature documenting a large amount of non-random under-reporting among SNAP beneficiaries in surveys (Meyer and Goerge (2010); Meyer and Mittag (2015)). I rely almost exclusively on administrative panel data collected by state agencies, allowing for reliable and representative findings.

Perhaps the most closely related paper is Homonoff and Somerville (2019), who show that recertification rates are substantially lower when recertification interviews are scheduled for dates later in the calendar month. This leaves less time for beneficiaries to reschedule a missed interview before benefits expire at the end of the calendar month, leading to substantial short-term churn from the program. This work and my analysis are complementary: while they demonstrate that a certain class of administrative barriers (one-time interview deadlines) increases short-term churn disproportionately for a certain subset of beneficiaries (lower-resource cases with children), I demonstrate that a different class of administrative barriers (paperwork burdens) increase long-term drop out disproportionately for a different subset of beneficiaries (higher-income cases without children). In theory, the distinct impacts of these and other policy tools could be combined to more precisely direct benefits towards beneficiaries with specific observable characteristics.

This paper will proceed as follows. In Section 2, I explain the institutional background of the SNAP program and describe my data sources. Section 3 documents rates of SNAP retention using multiple measures across seven states. In Section 4, I present evidence that many of program exits are among cases that would still be eligible, with reasonable simulations suggesting a fraction around one-half. Section 5 demonstrates that states’ efforts to simplify the recertification process can meaningfully reduce the number of eligible beneficiaries exiting the program. I show this by documenting the effects of a new website in Michigan, which I can credibly study due to its staggered roll-out. I emphasize that this particular policy does not fully eliminate eligible exits. Rather, the fact that this modest simplification appears to have a meaningful impact suggests that the suite of similar efforts that states have undertaken are likely to increase eligible retention. Section 6 concludes.

2 Background and Data

2.1 The SNAP Program

The Supplemental Nutrition Assistance Program (SNAP) is among the most widespread social insurance programs in the United States: in July 2011, SNAP served about 15 percent of the U.S. population (Ganong and Liebman (2018)). The core aspects of the SNAP program are the same across all U.S. states. Each month, households enrolled in the program get money loaded onto an Electronic Benefits Transfer (EBT) card, which they can use to buy most food items at grocery or convenience stores. Households of a given size may receive a benefit amount up to a maximum monthly benefit that is set at the federal level for each fiscal year. However, benefits are reduced as household income rises, so

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2The SNAP program considers a household to be “a group of people who . . . buy food and prepare meals together” (fns.usda.gov/snap/facts-about-snap). However, elderly and disabled individuals are often able to split into their own
the majority households receive less than the full benefit amount. To compute benefit amounts, households may first exempt a small amount of income by claiming specific deductions (e.g., for medical expenses or rent). For each dollar of monthly unearned income in excess of those deductions, monthly SNAP benefits fall by 30 cents. For each dollar of monthly earned income in excess of those deductions, monthly SNAP benefits fall by 24 cents. If benefits have not already been reduced to zero, households are ineligible to receive benefits if their gross income (before deductions) is above 130 percent of the federal poverty line or their net income (after deductions) is above 100 percent of the federal poverty line. Typically, state agencies do not have enough administrative data to independently verify beneficiary income, so they require beneficiaries to submit documentation (e.g., paystubs or W-2s) to limit the scope of misreporting.

Every state requires that beneficiaries complete a periodic recertification to confirm that they remain eligible for benefits. While the specifics of this procedure vary by state, typically every 6 to 12 months beneficiaries must complete extensive paperwork, prove all income and deductions (e.g., paystubs, rent receipts, medical bills, etc.), and perform some type of interview. States with longer recertification periods also require mid-certification reporting with less extensive paperwork. In Michigan, for example, the Department of Health and Human Services (DHHS) mails recertification forms to each household in their eleventh month of receiving benefits, and requires that the beneficiary furnish full documentation and complete a one-hour interview by phone or in person. For adults with earned income, DHHS also sends a mid-certification reporting form in the fifth month of benefits asking for verification of any relevant changes to income or deductions. There are two notable exceptions to this schedule: those who are elderly or disabled report every 12 months and recertify every 24 months, while a small set of adults with very unstable circumstances (e.g., homeless adults) may receive recertification periods of 3-6 months at state officials’ discretion. Households must repeatedly report and recertify at these regular intervals, and any household that does not complete these requirements within a few weeks will have their benefits terminated starting the following month.

Michigan’s SNAP program is unique in two other ways that facilitate this analysis. Many states have asset tests for all beneficiaries and/or three-month time limits for nonworking childless adults under 50 ("ABAWDs" in SNAP jargon). From 2005 through 2011, Michigan’s SNAP program had neither.

2.2 Data

My primary data set consists of linked SNAP and Unemployment Insurance (UI) administrative data from the state of Michigan. The SNAP data contain payments, basic demographics, case identifiers, and individual identifiers for every program participant in every month of participation between January 2005 and November 2011. I link this file to UI quarterly wage records for every individual that participated in a SNAP case during my period. Wage records span from 2005Q1 through 2010Q3. To construct the total quarterly earnings associated with a SNAP case, I aggregate earnings from all jobs for all individuals on a SNAP case in that month.

I classify SNAP cases into three categories: (1) “adult” cases consisting entirely of nondisabled separate cases, even if they live with others. In this paper, I use “households” and “cases” as synonyms, so that SNAP households do not necessarily align with living arrangements.
individuals aged 18–59, (2) “parent” cases consisting of one or more adults aged 18–59 together with one or more children, and (3) and “elderly/disabled” cases containing individuals either aged over 60 and/or classified as disabled. Since the SNAP data do not include an indicator for disability status, I use linked administrative data from Michigan’s Medicaid program and consider a beneficiary to have a disability if they are blind, disabled, or otherwise receiving Medicaid through the Supplemental Security Income program. I infer the dates in which reporting or recertification are required using these case categories.\(^3\)

In addition to categorizing cases in this way, I divide each case’s participation history into distinct SNAP spells. I consider a SNAP spell to have started or restarted if a case begins receiving SNAP benefits after two months of nonparticipation. I consider a SNAP spell to have ended when the case stops receiving SNAP benefits for one full calendar month or longer. The decision to ignore within-month churn is helpful both for data reasons and to focus on costlier forms of program exit.

As a second source of data, I use SNAP administrative records for six other states as provided by USDA’s Food and Nutrition Services (Mills et al. (2014)).\(^4\) These records provide comprehensive monthly data on every SNAP case that entered after two or more months of non-participation from February 2010 through November 2011. The data then follow each case in this sample through December 2012.

Table 1 shows that case composition is similar across these various samples. In Michigan, about half of cases are adults and two-thirds of cases are households with one member. Benefits typically range from $150-$325 per month, and about one-third of cases include someone with UI earnings. Other states typically follow similar patterns, although earnings in these columns are from internal SNAP records.\(^5\) Table 1 also includes statistics on a nationally representative cross-section of all SNAP cases from USDA’s Quality Control (QC) files from 2005 through 2014. This national sample looks broadly similar to the sample of new entrants in these states.

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\(^3\) About 92% of cases in my sample period fit into one of these three categories. The remaining 8% consist primarily of child-only cases with their own set of rules, which I ignore for most of this analysis.

\(^4\) These states are Florida, Idaho, Illinois, Maryland, Texas, and Virginia.

\(^5\) The lower number of childless adults in Texas is likely due to the existence of work requirements in this state during the 2011 sample frame.
Table 1: Sample Size of SNAP Cases

<table>
<thead>
<tr>
<th></th>
<th>MI</th>
<th>FL</th>
<th>ID</th>
<th>IL</th>
<th>MD</th>
<th>TX</th>
<th>VA</th>
<th>QC</th>
</tr>
</thead>
<tbody>
<tr>
<td>No. Cases (1000s)</td>
<td>1,758</td>
<td>1,196</td>
<td>72</td>
<td>446</td>
<td>212</td>
<td>1,229</td>
<td>225</td>
<td>34</td>
</tr>
<tr>
<td>Adults (%)</td>
<td>46</td>
<td>52</td>
<td>41</td>
<td>43</td>
<td>32</td>
<td>23</td>
<td>43</td>
<td>43</td>
</tr>
<tr>
<td>Parents (%)</td>
<td>27</td>
<td>18</td>
<td>37</td>
<td>34</td>
<td>30</td>
<td>52</td>
<td>36</td>
<td>42</td>
</tr>
<tr>
<td>Elderly/Disabled (%)</td>
<td>20</td>
<td>27</td>
<td>18</td>
<td>20</td>
<td>35</td>
<td>18</td>
<td>17</td>
<td>15</td>
</tr>
<tr>
<td>Alone (%)</td>
<td>67</td>
<td>64</td>
<td>50</td>
<td>55</td>
<td>58</td>
<td>39</td>
<td>55</td>
<td>50</td>
</tr>
<tr>
<td>25% Benefits ($)</td>
<td>144</td>
<td>143</td>
<td>176</td>
<td>171</td>
<td>114</td>
<td>126</td>
<td>152</td>
<td>100</td>
</tr>
<tr>
<td>75% Benefits ($)</td>
<td>327</td>
<td>259</td>
<td>367</td>
<td>359</td>
<td>304</td>
<td>367</td>
<td>354</td>
<td>278</td>
</tr>
<tr>
<td>With Earnings (%)</td>
<td>33</td>
<td>26</td>
<td>38</td>
<td>21</td>
<td>27</td>
<td>39</td>
<td>30</td>
<td>28</td>
</tr>
</tbody>
</table>

Table shows the number and composition of cases in each sample. The first column counts the universe of new cases that enter the Michigan SNAP program after at least two months of non-participation within my sample window, and the summary statistics report averages over every participating month for those cases. Columns 2 through 7 report the same statistics for each state in the USDA data, following cases that enter from February 2010 through November 2011. The final column reports data from the national Quality Control files, pooling all cross-sections from fiscal years 2005-2011. Benefit amounts are in nominal terms to facilitate comparisons with Federal maximum amounts.

3 Retention in SNAP

I first show that retention is low across many states' SNAP programs. Figure 1a shows survival curves for SNAP enrollment in Michigan by following each household for 24 months after their first enrollment in my sample window. The solid black line shows the percent of households that are enrolled at each month relative to their entry month, while the dotted red line shows the percent that have continuously remained enrolled in the program since entry. In Michigan, over half of SNAP entrants exit at some point within their first year, and approximately 50 percent of SNAP entrants are not on the program at their one-year anniversary. There are some exits in the first few months since entry, owing to a mix of households self-reporting substantial changes to household structure or income (as is legally required) and some households with very unstable circumstances given special, short recertification periods. However, exits are heavily concentrated in the sixth month, when households with earnings must complete mid-certification reporting, and the twelfth month, when most households must complete recertification. Appendix Figure A1 shows an example of the paperwork required during these months. To show that this phenomenon is common across the U.S., Figure 1b replicates the survival curve with re-entry for each of the six states in USDA data. The appendix provides more detail: Figure A2 shows that retention is lowest among childless adults, while Figure A3 shows a version of Figure 1b without program re-entry. Appendix A shows that these retention rates are consistent with less complete measures that I derive from public USDA Quality Control files.

Note that Figure 1b covers a shorter time horizon, due to the shorter window available in the USDA data.
There are two ways in which these figures may over- or under-emphasize the instability of SNAP participation. First, households that leave may return under a new case identifier, either because of the coding practice at the state agency or because of changes in their household structure. Appendix Figure A4 replicates the figures for Michigan, but follows newly entering case heads instead of state-designated case identifiers. This adjustment reduces the estimate of program exit by a few percentage points, but does not change any qualitative conclusions. Second, most of these figures show participation regardless of short-term exits, and therefore miss substantial “churn” in which cases leave and re-enter the program for brief periods. Appendix Figure A5 shows that churn exists but does not characterize most exits: the majority of cases that drop out for at least one month are gone for two years or longer.

4 Eligibility of Exiting Cases

I next provide evidence that a high fraction of exits in SNAP are among eligible beneficiaries. Reliably measuring eligibility at the case level would require accurate, linked data on many disparate sources of income and expenses. To my knowledge, no such data source exists. However, using three pieces of evidence, I estimate that approximately one-half of those who have exited the program by the one-year mark are still eligible.

I first show that exits are associated with missed deadlines rather than deemed ineligibility. This is suggestive of eligible cases exiting the program due to administrative barriers, but is also theoretically consistent with cases knowing that they are ineligible and therefore deciding not to recertify. To address whether cases that drop out appear ineligible based on observed attributes, I next show that program exit is comparable for those with and without UI-covered earnings. Finally, I attempt to construct a reasonable estimate of the fraction of exiting cases that are eligible by simulating unobserved characteristics with matched survey data.
4.1 Most Exits Are Due to Missed Deadlines

Administrative records suggest that case closure is overwhelmingly due to missing deadlines rather than being deemed ineligible. Two states in my USDA sample, Virginia and Idaho, include indicators for the reason each case was closed. I convert these codes into reasons implying missed deadlines, reasons implying ineligibility, and other or missing reasons. For each month of a SNAP spell, I plot the hazard rate of subsequently exiting SNAP for three months or longer by reason. Figure 2 shows these figures for both states. Exits are concentrated around reporting and recertification periods, and the vast majority of cases cite missed deadlines rather than deemed ineligibility. This fact does not imply that all those who miss deadlines are ineligible, but it is consistent with a potentially large number of eligible exits.

Figure 2: Reasons for SNAP Exit

Figures show the hazard rate of long-term case exit in each month of a case spell for Virginia (left) and Idaho (right). Both samples consider all new spells that began between February 2010 and November 2011 after two or more months of inactivity. The bars are color-coded to represent the recorded reason for case closure: a missed deadline (red), deemed ineligibility (green), another reason such as voluntary withdrawal (gray), or missing data (white). Depending on state rules, a small number of cases may be required to submit paperwork between traditional verification dates, explaining the small fraction of cases that miss a deadline in VA during these months.

4.2 Cases with No Earnings Exit at Similar Rates

As a second piece of evidence for frequent exits among eligible beneficiaries, I show that restricting my sample of Michigan SNAP beneficiaries to those without UI-covered earnings has little effect on exit rates at recertification. A null hypothesis that all exits are due to ineligibility would predict that those with low income almost always remain on the program, while those with high income typically exit. Figure 3 shows the hazard rate of exit at recertification for Michigan households with less than $100 of UI-covered earnings in the quarter in which they must recertify, and less than $100 of UI-covered earnings in the following quarter. Hazard rates of households with consistent negligible UI-covered earnings are barely higher than hazard rates of other households.

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7Quarterly earnings are normalized to real 2012 dollars using the PCE price deflator.
To provide more detail, Appendix Figures A6 and A7 show hazard rates across cells of quarterly earnings or predicted benefit amounts (derived from quarterly earnings). These figures show a moderate relationship between exits and earnings or predicted benefits, although without further simulations it is not clear how much of this is due to eligibility or the value of benefits.

4.3 Evidence from Simulations

The third piece of evidence suggesting that many who exit remain eligible comes from a simulation exercise. The goal of this simulation is to estimate what fraction of new entrants are ineligible for SNAP benefits one year later, allowing me to estimate the fraction of exits that remain eligible at this point. Given the various income sources, possible deductions, and special cases involved in the eligibility determination, there exist no data sources that can definitively establish eligibility once a household has lost contact with SNAP administrators.

In order to obtain a broadly reasonable estimate of the fraction of exits that remain eligible, I categorize all newly-entering Michigan SNAP cases into cells based on seven variables: the number of case members, an indicator for the existence of children on the case, bins for the age of the household head, and bins for UI-covered quarterly earned income in each of the four calendar quarters since entry. The administrative data include a full record of monthly benefit amounts, and through a special data pull I am able to observe DHHS records for gross and net income for most cases.8 This allows me to approximate the amount of additional real earned and unearned income that a case would have to receive in order to become ineligible, either due to the implicit tax on benefits, net income passing

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8I use 10-year age bins and $1,000 quarterly earnings bins. I keep only cases and households with a head aged over 20 and with fewer than seven members. The data source for gross and net income provided by DHHS is unable to provide a complete record for all case-months, so I take the first available record within the first 6 months after case entry. This provides starting gross and net income fields for three-quarters of entering cases in the Michigan sample, with no clear signs of selection.
100% of the Federal Poverty Line (FPL), or gross income passing 130% of the FPL. For each Michigan case, I then randomly select a household from the 2004 and 2008 Survey of Income and Program Participation (SIPP) that has the same seven cell values, including the full path of real household earned income over four calendar quarters. I compute each SIPP household’s 12-month change in real earned income, unearned income, and household size, and mark households that report changing states of residence. By using the changes reported by these SIPP respondents “as if” they were the changes of the matched Michigan households, I construct a reasonable simulation of eligibility for each Michigan case.\footnote{The sample is drawn using national SIPP weights and with replacement. To increase the sample size of the SIPP, I separate sequential, complete years of a given household’s responses into different observations. I separate elderly subsets of each household into separate simulated households, and I use the age of the oldest household member to stand in for the age of the SNAP case head. I choose not to use the SNAP participations questions in the SIPP in any part of the simulation, given well-known issues in self-reporting SNAP benefits to surveys (Meyer and Goerge (2010); Meyer and Mittag (2015)).}

This simulation exercise is meant to provide a reasonable approximation of how many Michigan households are ineligible 12 months after their initial entry. Taking the simulation as an exact measure would require a number of heroic assumptions: that Michigan and the SIPP sample have identical cell-specific distributions for income and household changes; that SIPP does not have meaningful measurement error; that no SNAP cases have sufficient deductions to increase income without losing benefits; and that deductions are constant across time. There are various reasons that these estimates of the fraction ineligible may be slightly too high (e.g., SIPP measurement error) or slightly too low (e.g., cases with significant changes in deductions such as medical expenses or rent).\footnote{In a typical year in either Michigan or USDA states, over one-quarter of cases have benefit amounts at or slightly above the Federal maximum. This causes my simulation to overestimate the fraction ineligible, because in reality these individuals can gain some income before their benefits are reduced.} This exercise is \textit{not} an ideal substitute for complete administrative records, and is meant to provide suggestive evidence on whether unobserved factors can easily explain the dropout rates in administrative data.

Table 2 compares simulated eligibility to actual dropout rates among the Michigan households in the simulation. The top panel reports the results of the main simulation, in which I match 80% of Michigan cases to at least one SIPP household. The bottom panel reports the results of a second simulation which matches on coarser cells, allowing me to include data from the 2001 SIPP and matching 99% of Michigan cases to at least one SIPP household. While the former simulation uses more precise matching, the latter covers more of the Michigan sample.\footnote{The “Less Detailed Matching” procedure only matches on the first and third quarters of income (rather than four consecutive quarters). This allows me to use respondents from the 2001, 2004, and 2008 SIPP in the simulation, since the 2001 SIPP followed households for fewer than 4 consecutive quarters.} For each subsample, the first row reports the percent of matched Michigan cases that are not in the program at 12 months since entry. The second row reports the median percent ineligible for each group out of 1000 simulations, and the third row reports the 10th and 90th percentile range of these 1000 simulations. Although almost half of cases are not present 12 months after entering, simulations suggest that just over 25% of cases have actually lost eligibility. This implies that almost one-half of those who exit the program remain eligible. This estimate is reasonably consistent across simulations, and when considering only childless adults or parents.

Simulations from the elderly population appear problematic, suggesting that about 10% of elderly Michigan cases are ineligible but remain on the program one year after entry. While this serves as a
reminder that the simulation exercise is suggestive rather than definitive, there are four reasons why this discrepancy for elderly cases does not necessarily undermine the estimates for other cases. First, many ineligible elderly cases may remain on the program at 12 months, since (unlike adult or parent cases) elderly cases have a 24 month recertification period. Second, take up of SNAP benefits is especially low for the elderly, so actual SNAP beneficiaries may be highly selected on characteristics relative to the SIPP matched sample (Finkelstein and Notowidigdo (2019)). Third, since a high fraction of elderly households have zero UI-covered earnings, the match between administrative and SIPP data provides a worse approximation of socioeconomic status relative to the match for younger households. Finally, there exists substantial noise in how elderly households report income to surveys (Bee and Mitchell (2017)). While the simulation for elderly households highlights valid and important limitations of the simulation exercise, it does not invalidate the exercise as a whole.

Overall, typical changes in income and household structure derived from the SIPP cannot get close to explaining the extent of exit among non-elderly Michigan SNAP participants. This suggests that a high fraction of exiting households remain eligible.

Table 2: Eligibility Simulation

<table>
<thead>
<tr>
<th>More Detailed Matching</th>
<th>All</th>
<th>Adults</th>
<th>Parents</th>
<th>Elderly</th>
</tr>
</thead>
<tbody>
<tr>
<td>Absent at 12 Months (%)</td>
<td>47.80</td>
<td>51.17</td>
<td>40.80</td>
<td>31.87</td>
</tr>
<tr>
<td>Ineligible, Median (%)</td>
<td>25.44</td>
<td>24.38</td>
<td>19.89</td>
<td>43.69</td>
</tr>
<tr>
<td>Ineligible, Range (%)</td>
<td>[25.38, 25.50]</td>
<td>[24.30, 24.45]</td>
<td>[19.78, 20.01]</td>
<td>[43.47, 43.92]</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Less Detailed Matching</th>
<th>All</th>
<th>Adults</th>
<th>Parents</th>
<th>Elderly</th>
</tr>
</thead>
<tbody>
<tr>
<td>Absent at 12 Months (%)</td>
<td>50.26</td>
<td>53.57</td>
<td>45.68</td>
<td>32.72</td>
</tr>
<tr>
<td>Ineligible, Median (%)</td>
<td>27.94</td>
<td>26.80</td>
<td>25.95</td>
<td>43.47</td>
</tr>
<tr>
<td>Ineligible, Range (%)</td>
<td>[27.88, 28.00]</td>
<td>[26.72, 26.87]</td>
<td>[25.84, 26.06]</td>
<td>[43.24, 43.70]</td>
</tr>
</tbody>
</table>

Table shows the results of a simulation exercise. The sample consists of SNAP cases that enter Michigan’s sample between March 2005 and September 2010, have a case head aged 20 or over at entry, have six or fewer members, and have values for gross and net income which are non-missing and available within the first six months of entry. The first row shows the percent of these new cases that are not on the program in the 13th month after their entry date. The second row shows the median value of 100 simulations in which I estimate the fraction of entering cases that have become ineligible as of one year since entry. In particular, I match each Michigan case to a households from the 2004 and 2008 Survey of Income and Program Participation (SIPP) with the same path of earnings for the four calendar quarters since SNAP entry in $1,000 increments; the same age of the household head in 10 year increments; the number of household members; and the presence of children. Households are drawn from the SIPP with weights and with replacement, splitting calendar years and elderly household members into separate rows. The third row reports the 10th and 90th percentiles of these 1000 simulations for each column. The lower panel performs the same exercise, but matches on coarser covariates (earnings bins in quarters 0 and 3 since entry, 10 year age bins, number of members, and the presence of children) and adds the 2001 SIPP, which decreases theoretical precision but raises the match rate from about 80% to over 99%.

5 Simplifying Administrative Requirements

The evidence suggests that state SNAP agencies face a trade-off in setting recertification policies: more frequent recertifications allow the program to shed ineligible applicants, but as many as half of the cases that exit are actually still eligible. These households overwhelmingly exit because they fail to complete the requirements of recertification, which include collecting pay stubs and receipts, attending
an interview, and communicating with the state agency when paperwork is missing or lost. While state agencies have not been able to precisely estimate eligible attrition, many have recognized this trade-off and taken steps to simplify the recertification process. From 2001 to 2011, states implemented simpler paperwork (45 states), call centers (25 states), combined SNAP-SSI applications (14 states), telephone interviews (47 states), and online application options (30 states) (Ganong and Liebman (2018)). In economic terms, states facing the trade-off of shedding ineligible beneficiaries and keeping eligible beneficiaries are using simplification to expand this production possibilities frontier. To inform these efforts, this section uses a county-level quasi-experiment to estimate the effect of Michigan’s online interface on SNAP retention. I find evidence that this modest intervention had a substantial impact on SNAP retention, reducing exits at recertification by almost 2pp (13%). While different policies will have different quantitative impacts, this quasi-experiment suggests that simplifying administrative requirements does meaningfully reduce eligible exits.

5.1 Roll Out of the “Bridges” Online Interface

Today, the “Bridges” online case management system allows users to apply for SNAP, recertify, check the status of their benefits, change personal information, find their case worker’s contact information, or read letters from DHHS at any time on the Internet. The ability to perform these tasks online at any time may substantially reduce the complexity and logistical burden of tracking and recertifying benefits, especially among populations without stable mailing addresses or with limited telephone minutes. Michigan DHHS rolled out the first iteration of this system incrementally throughout the state from August 2008 through August 2009, with most roll-outs occurring between March and August 2009. Figure 4 shows the landing page of the website in 2009 and the schedule of the rollout, which occurred in different sections of the state in sequence: most of western Michigan had online capabilities in March, rural northern Michigan in May, southeast Michigan in June, and Wayne County (Detroit) in August.12

12The expanded pilot counties released the online interface through late January and early February 2009. My empirical specification assigns the date of introduction as February 2009 for these counties.
I study the effect of Bridges on the hazard rate of long-term exit (3+ months) among all cases that are scheduled to recertify. In particular, I denote each county by $c$, each calendar month by $t$, the month the local website introduction by $T$, and the month relative to local website introduction by $k$. I designate a series of controls $X_{ct}$, which include a rich set of unemployment rate and UI earnings controls as well as basic demographic controls.\footnote{My main specification controls for the unemployment rate in a case’s county of service in 1pp increments (from BLS Local Area Unemployment Statistics), quarterly earnings of everyone on the case in real 2012 $\$1,000$ increments, dummy variables for case type, and linear controls for benefit amounts, age of case head, case size, percent of case heads that are white, and percent of case heads that are female. I run regressions on data collapsed to the county/month/case type/earnings bin.} I run the following regression, with standard errors clustered at the county level:

$$\text{Hazard}_{ct} = \mu_c + \mu_t + \sum_{k=-5}^{5} \gamma_k 1(t = T + k)_{ct} + \beta X_{ct} + e_{ct}$$

In order to identify year effects precisely, I restrict coefficients $\gamma_k$ to be the same for all relative months less than $-5$ and to be the same for all relative months greater than $5$. A rolling window of 5 months around the entry of the website is not an arbitrary threshold, since many households facing recertification more than 6 months after the website was introduced would have had access to the website during a previous reporting month. A window of 5 months avoids this selection issue. I omit all sample months that are not included in this rolling 5-month window for at least one county.

### 5.2 Regression Results

Regression results suggest the online interface has substantial effects on the hazard rate of program exit. Figure 5 plots the coefficients $\gamma_k$ for each year relative to the introduction of the Bridges website.
The series shows no clear of pre-trends in months before the online interface was introduced, and then shows a sharp and statistically significant downward shift in hazard rates at month $k = 0$. The magnitude of this drop is approximately 2 percentage points (13%).

Figure 5: Recertification Regression

![Figure 5: Recertification Regression](image)

Figure shows the regression coefficients for the impact of the Bridges website on exit for 3+ months at recertification. Each point represents the coefficient on months since the introduction of the website, using the month before introduction as the excluded group, and bands show 95% confidence intervals clustered by SNAP service county. Regression controls include month fixed effects, county fixed effects, case type fixed effects, fixed effects for real 2012 quarterly earnings in $1,000 bins, fixed effects for the unemployment rate of service counties in each month in 1pp increments, and linear controls for a handful of additional covariates (benefit amounts, age of case head, case size, percent of case heads who are white, percent of case heads who are female).

To obtain a single point estimate of this effect, Table 3 reports regressions that combine relative months $k = -5$ through $k = -2$ into a “pre” period and combine relative months $k = 2$ through $k = 5$ into a “post” period. The baseline coefficient with controls is $-1.82pp$ (12%) and is highly significant. However, this point estimate hides substantial heterogeneity. The impact of the website appears to be largest for childless adults, especially relative to elderly or disabled individuals ($-2.19$ vs $0.39$, $z = -1.73$, $p = 0.084$). Moreover, the impact of the website appears to be larger for individuals with some earnings in the UI data relative to those with no earnings ($-3.20$ vs $-0.55$, $z = -2.56$, $p = 0.010$).\[^{14}\] This former finding corroborates anecdotal evidence: according to SNAP officials, childless adults on SNAP often have unstable mailing addresses and therefore stand to benefit more from the ability to track benefits online. The finding that the website almost exclusively affects cases with some earnings is potentially surprising. This may be due to the website alleviating particular challenges for earners (e.g., paystubs getting lost in the mail) or due to earners having a lower tolerance for any recertification barrier (e.g., because they have busier schedules). The impact of the website is similar for those on their first or subsequent SNAP spells, suggesting that previous experience with the SNAP program plays a limited role. Appendix Figure A1 shows that selecting controls via post-Lasso yields similar results (Belloni et al. (2014)).\[^{15}\]

\[^{14}\]These numbers come from the calculation $z = \frac{\hat{\beta}_1 - \hat{\beta}_2}{\sqrt{\sigma^2_1 + \sigma^2_2}}$.

\[^{15}\]The two main differences between the baseline and post-Lasso results are the slightly larger main estimate when
Table 3: Regressions of Exit (3+ Months) at Recertification on Availability of Online Interface

<table>
<thead>
<tr>
<th></th>
<th>Adults</th>
<th>Parents</th>
<th>Eld/Dis</th>
<th>Earn</th>
<th>No Earn</th>
<th>First</th>
<th>Not First</th>
</tr>
</thead>
<tbody>
<tr>
<td>1(Online)</td>
<td>-1.48***</td>
<td>-1.82***</td>
<td>-2.19***</td>
<td>-1.09***</td>
<td>0.39</td>
<td>-3.20***</td>
<td>-1.77***</td>
</tr>
<tr>
<td></td>
<td>(0.51)</td>
<td>(0.51)</td>
<td>(0.82)</td>
<td>(0.41)</td>
<td>(1.25)</td>
<td>(0.89)</td>
<td>(0.53)</td>
</tr>
<tr>
<td>Observations</td>
<td>27,169</td>
<td>27,169</td>
<td>10,336</td>
<td>13,008</td>
<td>3,825</td>
<td>25,391</td>
<td>5,554</td>
</tr>
<tr>
<td>R²</td>
<td>0.435</td>
<td>0.455</td>
<td>0.418</td>
<td>0.211</td>
<td>0.228</td>
<td>0.389</td>
<td>0.677</td>
</tr>
<tr>
<td>Base</td>
<td>15.8</td>
<td>15.8</td>
<td>22.1</td>
<td>8.1</td>
<td>10.2</td>
<td>16.6</td>
<td>15.5</td>
</tr>
<tr>
<td>Full Controls</td>
<td>.</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table shows the “post” coefficients for a the impact of the Michigan website roll out on exit at recertification, as well as standard errors clustered by SNAP service county. The first column shows the coefficient with dummies for county, time, case type, county unemployment rate, and case earnings bin dummies. The second column adds linear controls for benefit amount, age of case head, case size, percent of cases that are white, and percent of cases that are female. Columns 3 through 5 subdivide this regression by case type. Columns 6 and 7 split the sample based on whether the case earned over $100 in real quarterly UI-covered earnings in the month before recertification, while columns 8 and 9 split each case's first spell in the sample window from subsequent spells.

Additionally, Appendix Tables A2 and Figure A9 show the results for reporting months, in which less intensive verifications are required for cases reporting earned income. Table A2 suggests that pre/post differences are small for reporters and follow similar heterogeneity patterns as the effect on recertifiers. However, Figure A9 suggests a delayed reaction followed by a substantial decline in dropout rates. This qualitative pattern is robust to different controls and samples used, and may reflect delayed publicity or implementation of certain features for reporting cases.

The online interface appears to have substantially reduced hazard rates, especially among the more transient population of childless adults and those who had to verify earnings. It appears that modest simplifications in access to information and communication with the SNAP agency can lead to substantial reductions in eligible exits.

6 Conclusion

This paper illustrates the importance of understanding retention in at least one major social insurance program, and suggests that simplifying recertification procedures meaningfully reduces exits among eligible beneficiaries. I establish three facts. First, retention in SNAP is low, with about one-half of newly entering cases exiting exiting within the year. Second, a substantial fraction of those who exit remain eligible. Simulations using Michigan administrative data and the SIPP suggest that about one-half of cases that have exited one year after entry are still eligible. Third, efforts to simplify recertification procedures appear to meaningfully reduce exits. I study the staggered roll out of an online interface in Michigan, and find that even this modest intervention reduced the hazard rate of using post-Lasso (−2.58pp) and the large effect for chronic SNAP users when using post-Lasso (−4.75pp). Note that post-Lasso regressions use uncollapsed data, resulting in higher nominal sample sizes.

Unreported results suggest that the new interface did not affect the fraction of clearly ineligible beneficiaries remaining on the program, as the fraction of recertifying cases earning above 200% of the Federal Poverty Line did not change when the website rolled out. The pre- and post-means of this fraud measure, respectively, are 0.49% and 0.73% (z = 0.202, p = 0.84).
long-term exit by almost 2 pp (12%). Effects are concentrated among childless adults, who may have the most difficulty communicating with SNAP officials by mail or phone, and among those with earnings, who must submit additional paperwork regarding their earnings to recertify. These findings suggest that retention is an important dimension of take up in its own right, and that efforts to increase take up by simplifying recertification procedures can increase retention.
References


Figure A1: Example of Required Paperwork

Figure shows an example of the paperwork required to remain eligible for Michigan's SNAP program. This particular form is for mid-certification reporting for individuals with a 12-month recertification period in Michigan. Initial applications and recertification forms are more extensive but have a similar format.
Figure A2: Retention by Case Type

Survival Curves (Michigan)
By Category (with Re-Entry)

Figure shows retention over two years for Michigan SNAP cases that enter between March 2005 and September 2009 and have not appeared in the sample before. The figure divides the sample by case type in the month of entry. "Adults" consist of cases where all members are marked as adults in the Michigan data and are aged 59 or younger. "Parents" consist of cases with one or more adults aged 59 or younger and one or more children. "Elderly/Disabled" consist of cases with at least one adult who is aged over 60, is receiving Medicaid through Supplemental Security Income, or is receiving Medicaid as an aged, blind, or disabled (ABD) beneficiary.

Figure A3: Retention in SNAP (FNS States)

Survival Curves, No Re-Entry

Figure shows the spell length in the sample of FNS states for all new case spells that entered from December 2010 through September 2011. Since a spell is defined to end after one or more months of non-participation in SNAP, this is comparable to the survival curves of Michigan’s new SNAP cases without re-entry.
Figure A4: Retention for Case Heads

Survival Curves for New Case Heads in Michigan

Figure shows retention over two years for Michigan SNAP case heads that enter between March 2005 and September 2009 and have not appeared in the sample before. The left panel shows overall survival curves with and without re-entry, while the right panel shows survival curves with re-entry by case type in the month of entry.

Figure A5: Gaps in Participation

Gaps in Michigan SNAP Participation
Spells Ending Before July 2009

A gap of 25+ months includes cases that do not return in the sample window. Left figure shows the histogram of the number of months between a case leaving the SNAP records and re-entering the SNAP records. Cases that never return within my sample window are in the 24+ months bin. Right figure shows the same statistics, but considers newly entering case heads rather than internal case identifiers.
Figure A6: Retention by Earnings

Exit for 3+ Months at Recertification

By Earnings This Quarter

![Graph](image1)

By Earnings Next Quarter

![Graph](image2)

Figure shows hazard rates of long-term exit by earnings in the quarter of recertification (left) or the following quarter (right). Colors designate case types in the month before recertification is due, and marker size indicates the relative size of each category within case types.

Figure A7: Retention by Predicted Grant Amount

Exit for 3+ Months at Recertification

By Predicted Grant Amount

![Graph](image3)

Figure shows hazard rates of long-term exit by earnings across bins of predicted monthly benefits. Predicted monthly benefits are estimated as the starting benefit amount minus 0.24*(Quarterly Earnings Change)/3, where negative predicted monthly benefits are censored at zero. Colors designate case types in the month before recertification is due, and marker size indicates the relative size of each category within case types.
Figure A8: Heterogeneity of Bridges Effect

Figure shows the regression coefficients for the impact of the Bridges roll out on exit for 3+ months at recertification, split by case type (left) and the presence of UI-covered quarterly earnings on the case (right). Each point represents the coefficient on months since the introduction of the website, using the month before introduction as the excluded group, and bands show 95% confidence intervals clustered by SNAP service county. Regression controls include month fixed effects, county fixed effects, case type fixed effects, fixed effects for real 2012 quarterly earnings in $1,000 bins, fixed effects for the unemployment rate of service counties in each month in 1pp increments, and linear controls for a handful of additional covariates (benefit amounts, age of case head, case size, percent of case heads who are white, percent of case heads who are female).

Figure A9: Bridges for Reporting (Michigan)

Figure shows the regression coefficients for the impact of the Bridges roll out on exit for 3+ months at mid-certification reporting. Each point represents the coefficient on months since the introduction of the website, using the month before introduction as the excluded group, and bands show 95% confidence intervals clustered by SNAP service county. Regression controls include month fixed effects, county fixed effects, case type fixed effects, fixed effects for real 2012 quarterly earnings in $1,000 bins, fixed effects for the unemployment rate of service counties in each month in 1pp increments, and linear controls for a handful of additional covariates (benefit amounts, age of case head, case size, percent of case heads who are white, percent of case heads who are female).
Appendix Tables

### Table A1: Post-Lasso Regression on Recertifiers

<table>
<thead>
<tr>
<th>Adults</th>
<th>Parents</th>
<th>Eld/Dis</th>
<th>Earn</th>
<th>No Earn</th>
<th>First</th>
<th>Not First</th>
</tr>
</thead>
<tbody>
<tr>
<td>1(Online)</td>
<td>-2.58***</td>
<td>-2.10**</td>
<td>-2.18***</td>
<td>-1.30</td>
<td>-4.07***</td>
<td>-1.89***</td>
</tr>
<tr>
<td>(0.61)</td>
<td>(1.07)</td>
<td>(0.76)</td>
<td>(1.43)</td>
<td>(1.17)</td>
<td>(0.65)</td>
<td>(0.57)</td>
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<tr>
<td>Observations</td>
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<td>87,320</td>
<td>63,885</td>
<td>15,542</td>
<td>45,891</td>
<td>120,856</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.354</td>
<td>0.257</td>
<td>0.139</td>
<td>0.153</td>
<td>0.298</td>
<td>0.608</td>
</tr>
<tr>
<td>Base</td>
<td>13.4</td>
<td>18.1</td>
<td>10.4</td>
<td>10.5</td>
<td>14.1</td>
<td>10.4</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table shows the "post" coefficients for the impact of the Michigan website roll out on exit at recertification, as well as standard errors clustered by SNAP service county. All columns report individual-level regressions, unlike the collapsed regressions in the body of the text, and control for bins of benefit amount, age of case head, case size, percent of cases that are white, and percent of cases that are female as chosen by post-Lasso methods. The penalty parameter on Lasso estimation is chosen using five-fold cross-validation. The regressions additionally control for the full set of month and county dummies. Columns 2 through 4 subdivide this regression by case type. Columns 5 and 6 split the sample based on whether the case earned over $100 in real quarterly UI-covered earnings, while columns 7 and 8 split each case’s first spell in the sample window from subsequent spells. Note that Lasso covariate selection is done separately for each regression, so that heterogeneous effects do not mechanically average to the main effect.

### Table A2: Regression on Reporters

<table>
<thead>
<tr>
<th>Adults</th>
<th>Parents</th>
<th>Eld/Dis</th>
<th>Earn</th>
<th>No Earn</th>
<th>First</th>
<th>Not First</th>
</tr>
</thead>
<tbody>
<tr>
<td>1(Online)</td>
<td>-1.35***</td>
<td>-0.97**</td>
<td>-2.21**</td>
<td>-0.25</td>
<td>1.69</td>
<td>-1.53**</td>
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<td>(0.47)</td>
<td>(0.44)</td>
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<td>(0.61)</td>
<td>(1.12)</td>
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<td>Observations</td>
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<td>31,422</td>
<td>12,391</td>
<td>14,415</td>
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</tr>
<tr>
<td>$R^2$</td>
<td>0.248</td>
<td>0.256</td>
<td>0.283</td>
<td>0.231</td>
<td>0.134</td>
<td>0.258</td>
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<tr>
<td>Base</td>
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<td>18.1</td>
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<td>10.5</td>
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<td>X</td>
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<td>X</td>
<td>X</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table shows the "post" coefficients for the impact of the Michigan website roll out on exit at reporting, as well as standard errors clustered by SNAP service county. The first column shows the coefficient with dummies for county, time, case type, county unemployment rate, and case earnings bin dummies. The second column adds linear controls for benefit amount, age of case head, case size, percent of cases that are white, and percent of cases that are female. Column 3 through 5 subdivide this regression by case type. Columns 6 and 7 split the sample based on whether the case earned over $100 in real quarterly UI-covered earnings, while columns 8 and 9 split each case’s first spell in the sample window from subsequent spells.
Appendix A: Consistency with Quality Control Files

While SNAP survival curves are only available at the case level when using administrative data, it is helpful to corroborate that the high rates of exit I observe in the administrative data are consistent with their closest analogs in public data sources. To check this, I use the variable `actntype` in the cross-sectional SNAP Quality Control files to identify cases that are on their first recertification cycle. I consider households with recertification cycles of 6 or 12 months. I denote the (weighted) number of cases on their first recertification cycle in month $t$ as $entry_t$. I also compute the total (weighted) number of cases on the program in month $t$ and in either $t+6$ or $t+12$. For cases on a six month recertification cycle, I compute the number that exited the program using the identity $total_{t+6} - total_t = entry_t - exit_t$, and likewise for cases on a twelve month cycle. Figure 10 reports exit hazards $\frac{exit_t}{total_t}$ in percent terms, averaged over fiscal years to reduce noise. Consistent with the findings in the administrative data, the QC files suggests that just under half of cases fail to recertify.

Figure 10: Cross-Section Retention from QC File

![Figure shows the turnover inferred from repeated cross-sections in the USDA Quality Control files. To infer turnover for cases with a six-month recertification window, I compute the (weighted) change in national caseloads from each month $t$ to month $t+6$. I then compute the (weighted) number of cases in month $t+6$ that are on their first recertification, calling this quantity $entry_t$. I the compute $exit_t = \frac{entry_t - (total_{t+6} - total_t)}{total_t}$, and plot the weighted average of this value for each fiscal year in red. For cases with a twelve-month recertification window, I perform the analogous exercise and plot the weighted average for each fiscal year in green.]