Why Leave Benefits on the Table?
Evidence from SNAP

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Abstract

Many social insurance programs require that beneficiaries periodically reverify 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 the eligible and the efficacy of these simplifications is not well established. This paper uses administrative data from the Supplemental Nutritions 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 over 2pp (13%). These facts suggest that eligible retention is very incomplete, but that ongoing simplification efforts are effective.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 it’s 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, 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 exits 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 those who lose all contact with SNAP officials, I use three distinct facts to support this conclusion:

- Administrative data from two states (Virginia and Idaho) 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). Using a reasonable set of assumptions, I find that approximately half of the cases that leave the program in the first year would be eligible for recertification.

Third, I provide quasi-experimental evidence that administrative barriers meaningfully affect retention. Throughout 2009, Michigan introduced a website that helped beneficiaries understand program requirements and track their benefits online. The website was rolled out incrementally across different groups of counties at different times through the year, providing a clean quasi-experimental design. The new interface coincides with a sharp reduction in long-term exit at recertification of over 2pp (13%), 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 it alleviates multiple barriers that have limited previous empirical progress. One existing body of work 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 drop out of SNAP lose immediate and often substantial flow benefits, and discover this quickly when they try to buy groceries. A different strand of literature
studies the effect of SNAP policy changes using state-level difference-in-difference methods over extended time horizons (Heflin and Mueser (2010); Schwabish (2012); Pomerleau (2013); Ganong and Liebman (2018)). This paper complements that work by studying the within-state roll out of a program over a short time horizon, thereby alleviating possible concerns over differential trends of endogenous state policy decisions. Finally, this paper draws from a small literature documenting a large amount of non-random under-reporting among SNAP beneficiaries in cross-sectional surveys (Meyer and Goerge (2010); Mittag and Nikolas (2015)). I rely almost exclusively on administrative panel data collected by state agencies, allowing for reliable and representative findings.

The paper will proceed as follows. In Section 2, I explain the institutional background of the SNAP program and describe my data sources. In Section 3, I use data from multiple states to document that retention in SNAP is low. Section 4 provides evidence that a large fraction of those who drop out of the program remain eligible, with reasonable simulations suggesting a fraction around one-half. Section 5 uses a quasi-experiment in Michigan to show that a modest policy change – a website to help beneficiaries track their benefits and communicate with their SNAP offices – reduced program exit by a meaningful amount. Section 6 concludes.

2 Background and Data

2.1 The SNAP Program

The Supplemental Nutrition Assistance Program (SNAP) is among the most popular 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 most 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 child support 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. Most state agencies do not independently verify beneficiary income using other agencies’ administrative records, but do require beneficiaries to submit documentation such as W-2s or paystubs 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

\footnote{The 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 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.}
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 Michi-
gan, 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 fur-
nish 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 for
verification of income (but not 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
individuals aged 18–59, (2) “parent” cases consisting of one or more adults aged 18–59 together with
one or more children aged 17 or below, 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.\(^3\)

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 2011–2012 for six other 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.
as provided by USDA’s Food and Nutrition Services (Mills et al. (2014)). These records provide comprehensive monthly data on every SNAP case that appeared at any point in fiscal year 2011. The data then follow each case in this sample through December 2012. To maintain comparability to the Michigan data, I restrict the USDA sample to households that began participating between December 2010 and September 2011 after two or more months of nonparticipation.

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. Table 1 also includes statistics on a nationally representative cross-section of all SNAP cases from USDA’s Quality Control (QC) files. This national sample looks broadly similar to the sample of new entrants in select states.

<table>
<thead>
<tr>
<th>Table 1: Sample Size of SNAP Cases</th>
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<tbody>
<tr>
<td>MI</td>
</tr>
<tr>
<td>No. Cases (1000s)</td>
</tr>
<tr>
<td>Adults (%)</td>
</tr>
<tr>
<td>Parents (%)</td>
</tr>
<tr>
<td>Elderly/Disabled (%)</td>
</tr>
<tr>
<td>Alone (%)</td>
</tr>
<tr>
<td>25% Benefits ($)</td>
</tr>
<tr>
<td>75% Benefits ($)</td>
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<tr>
<td>With Earnings (%)</td>
</tr>
</tbody>
</table>

Table shows the number and composition of cases in the Michigan sample, the USDA samples, and the national cross-section from the USDA Quality Control (QC) files.

3 Retention in SNAP

I first show that retention is low across many states’ SNAP programs. Figure 1 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.

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.
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. The right panel replicates the survival curve with re-entry (black) for each of the six states in USDA data.

Figure 1: Retention in SNAP

Left figure shows retention over two years for the longer panel of SNAP cases in Michigan, considering any case that enters between March 2005 and September 2009 that has not appeared in the sample before. The black line shows the fraction of entrants on the program in a given month since entry, while the red line shows the fraction who have exited for 1+ months. The right figure shows the equivalent of this black line, but for the sample of FNS states that entered from December 2010 through September 2011.

These rates are not uniform across types of households. Figure 2 again focuses on participation with re-entry, but splits the sample into adults, parents, and elderly/ disabled cases. Adults and parents exit at higher rates than the elderly/ disabled.

Figure 2: Retention by Case Type

Figure shows retention over two years for Michigan SNAP cases that enter between March 2005 and September 2009 that has not appeared in the sample before. The figure divides the sample into case types in the month of entry.
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 A1 replicates the figures for Michigan, but follows newly entering case heads instead of state-designated case identifiers. Re-coding of cases only slightly exaggerates the low level of retention in SNAP at the person level. After one year, just over 40% of new case heads no longer participate in SNAP, and the majority of case heads have left for one month or longer. 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 A2 shows that churn accounts for a limited amount of non-participation: the majority of cases that drop out are gone for two years or longer, as are almost 50% of case heads.

4 Eligibility ofExiting Cases

I next provide evidence that a high fraction of exits in SNAP are among eligible beneficiaries, implying that SNAP agencies face a trade-off between regularly shedding ineligible cases and retaining eligible cases. Given the eligibility criteria described above, reliably predicting eligibility at the household 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 exit the program by the one-year mark are still eligible.

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 3 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 determined ineligibility.
4.2 Likely Eligible Households Exit at High 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 that likely remained eligible has little effect on exit rates at recertification. Figure 4 shows the hazard rate of exits at recertification for Michigan households with less than $100 of UI-covered earnings in the quarter in which they recertify, and less than $100 of UI-covered earnings in the following quarter. Quarterly earnings are in real 2012 dollars using the PCE price deflator. Hazard rates of households with consistent negligible UI-covered earnings are barely higher than hazard rates of other households. Appendix Figure A3 shows hazard rates at recertification across cells of quarterly earnings in the quarter of recertification and in the following quarter. The figure displays a positive but gradual relationship between earnings and long-term exit.
4.3 Evidence from Simulations

The third piece of evidence suggesting that many who exit remain eligible come from a simulation exercise. 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 merge Michigan’s administrative data with survey data from the Survey of Income and Program Participation (SIPP). The exercise suggests that about one-half of those who are no longer on SNAP at their one-year anniversary of entry are still eligible.

To perform this simulation, 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 after 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. 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 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 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 am able to simulate eligibility for each Michigan household.

This simulation exercise is meant to provide a reasonable approximation of how many Michigan households within each cell were ineligible 12 months after their initial entry. Taking the simulation literally would require a number of heroic assumptions: that Michigan and the SIPP sample have identical cell-specific distributions of each change; that SIPP does not have meaningful measurement error; that zero SNAP cases are on the “plateau” in which they could earn more without losing benefits; and that other deductions (e.g. medical expenses) are constant across time. Even then, the simulation only gives valid estimate at high levels of aggregation, since each particular Michigan household certainly does not follow the path of their randomly matched SIPP counterpart. Nonetheless, this simulation uses large enough sample to match households on very detailed characteristics, allowing me to construct a reasonable approximation of eligibility based on the typical distribution of changes in

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6I use 10-year age bins and $1,000 quarterly earnings bins in my main simulation. I winsorize real quarterly earnings at $10,000, use the maximum age of a household member in the SIPP to stand in for the “case head”, and keep only cases and households with a head aged over 20. I also exclude about 8% of the sample with net or gross income above the pre-specified limits, who are likely in special “categorically eligible” groups with higher income limits. The data source for gross and net income provided by DHHS is unable to provide a complete record for all case-months due to data storage issues, so I take the first available record within the first 6 months after case entry. This provides data for three-quarters of entering cases.

7The sample is drawn with 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 rows. About 75% of Michigan households with the requisite data are matched to one or more SIPP households when using all seven covariate cells. A similar exercise matching only on the first and fourth quarter of income (instead of four consecutive quarters of income) captures over 95% of Michigan cases but does not change the qualitative results of the simulation.
important variables.

Table 2 compares eligibility from this simulation to actual dropout rates in the Michigan sample. The top panel reports the results of the main simulation, in which I am able to match three-quarters of Michigan cases with at least one corresponding SIPP household. The bottom panel reports the results of a second simulation which matches on coarser cells, which may decrease precision but allows me to match over 99% of Michigan cases to a SIPP household.8 In each table, the row column reports the actual rate of exit by 12 months. The second row reports the median percent ineligible for each group out of 20 simulations (the 10th highest simulation value), while the third row reports the 90-10 range of simulation (the 2nd and 18th highest values). Although almost half of cases are not present 12 months after entering, simulations suggest that around 25% of cases have actually lost eligibility, implying that almost one-half of those who exit the program remain eligible. These rates vary by case type: over 50% of exiting childless adults remain eligible, while between 40-50% of exiting parents remain eligible. Simulations from the elderly population actually suggest that the exit rates in the Michigan sample are surprisingly low. This may be due to combination of factors, including the limitations of the simulation exercise; the fact that elderly cases are typically on a 24-month recertification cycle (so some ineligible may not have exited at 12 months); and due to additional variance caused by well-known issues in how elderly household report income to surveys (Bee and Mitchell (2017)). 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.

Table 2: Eligibility Simulation

<table>
<thead>
<tr>
<th></th>
<th>More Detailed Matching</th>
<th>Less Detailed Matching</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>All</td>
<td>Adults</td>
</tr>
<tr>
<td>Absent at 12 Months (%)</td>
<td>47.4</td>
<td>50.5</td>
</tr>
<tr>
<td>Ineligible, Median (%)</td>
<td>25.0</td>
<td>23.5</td>
</tr>
<tr>
<td>Ineligible, Range (%)</td>
<td>[24.9, 25.0]</td>
<td>[23.4, 23.6]</td>
</tr>
</tbody>
</table>

The “Less Detailed Matching” procedure only matches on the first and fourth quarters of income (rather than four consecutive quarters) and groups the age of case heads into bins of 20 years (rather than 10 years).
5 Simplifying Administrative Requirements

The evidence above suggests that state SNAP agencies face a trade-off in determining their recertification policies: more frequent recertifications allow the program to shed ineligible applicants, but as many as half of the beneficiaries that agencies lose are actually still eligible. These households overwhelmingly exit because they fail to complete the detailed 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 sensed 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)). State difference-in-difference estimates, while informative, risk bias from differential trends across states or endogenous state policy responses. In contrast, this section uses a precise 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 1.7pp (over 11%). Effects are concentrated among childless adults and households with earnings.

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 frequency of errors during the reporting or recertification process. Michigan DHHS rolled out the first iteration of this system incrementally throughout the state from August 2008 through August 2009, with most rollouts occurring between March and August 2009. Figure 5 shows the landing page of the website at that time 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.\(^9\)

\(^9\)The expanded pilot counties released the online interface through late January and early February 2009. My empirical specification assigns the date of introduction as January 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.\(^{10}\) I run the following regression, with standard errors clustered at the county level.

$$\text{Hazard}_{ct} = \mu_c + \mu_t + \sum_{k=-6}^{6} \gamma_k 1(t = T + k)_{ct} + \beta X_{ct} + e_{ct}$$

In order to identify year effects in a rolling window, I restrict coefficients $\gamma_k$ to be the same for all relative years less than $-6$ and to be the same for all relative years greater than $6$. I cut out all sample months that are not included in this window for at least one county. I report all coefficients $\gamma_k$ relative to month $k = -2$, since those recertifying in the month before the roll out may have still been able to access the website before their benefits were cut off at the start of month $k = 1$. 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 these selection issues.

\(^{10}\)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 category, 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/category/earnings bin level.
5.2 Results

Although the introduction of the online interface was a modest intervention, it was helpful in providing information to beneficiaries and allowing for simpler communication with SNAP administrators. It is realistic to think that this is especially helpful in the population of SNAP recipients, many of whom have unstable living situations (and therefore have difficulty communicating by mail) or limited telephone minutes (and therefore have difficulty communicating by phone). Regression results corroborate this broad story, suggesting that the roll out of the online interface has substantial effects on the hazard rate of program exit.

Figure 6 plots the coefficients $\gamma_k$ for each year relative to the introduction of the Bridges website. The series shows no evidence of pre-trends in years before the online interface was introduced, and then shows a sudden and statistically significant downward shift in hazard rates at month $k = 0$. The downward shift persists through the end of the sample window, after which selection issues would threaten the interpretation of these coefficients.

To better understand these effects, Table 3 reports regressions that combine relative months $k = -5$ through $k = -2$ into a “pre” period and combine months $k = 2$ through $k = 5$ into a “post” period. Depending on controls, the baseline coefficients hover around $-2pp$ (13%). However, this hides substantial heterogeneity in the effect: the effect is larger for childless adults ($-3pp$) and for those with some earnings in the UI data ($-3.6pp$). The effect is similar for those on their first or subsequent SNAP spells.
Table 3: Regressions of Recertification

<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>-1.86***</td>
<td>-2.25***</td>
<td>-2.96***</td>
<td>-1.06**</td>
<td>0.02</td>
<td>-3.55***</td>
<td>-0.87</td>
</tr>
<tr>
<td></td>
<td>(0.58)</td>
<td>(0.60)</td>
<td>(0.93)</td>
<td>(0.44)</td>
<td>(1.25)</td>
<td>(0.95)</td>
<td>(0.64)</td>
</tr>
<tr>
<td>Observations</td>
<td>27,169</td>
<td>27,169</td>
<td>10,336</td>
<td>3,825</td>
<td>25,391</td>
<td>5,554</td>
<td>24,556</td>
</tr>
<tr>
<td>( R^2 )</td>
<td>0.434</td>
<td>0.455</td>
<td>0.418</td>
<td>0.211</td>
<td>0.228</td>
<td>0.389</td>
<td>0.676</td>
</tr>
<tr>
<td>Base</td>
<td>15.6</td>
<td>15.6</td>
<td>22.2</td>
<td>7.4</td>
<td>10.5</td>
<td>15.9</td>
<td>15.5</td>
</tr>
<tr>
<td>Controls</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
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<td>X</td>
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Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Finally, Appendix Table A1 and Figure A5 show the results for reporting months, in which less intensive verifications are required for cases reporting earned income. Table A1 suggests that pre/post differences are small for reporters and follow the same heterogeneity patterns as the effect on recertifiers. However, Figure A5 suggests a substantial delayed reaction in the effect and possible declines of greater than 2pp. This pattern is robust to different controls and samples used, and may reflect delayed publicity or implementation of certain features for reporters.

The online interface appears to have substantially reduced hazard rates among childless adults, who are a particularly transient and vulnerable SNAP population according to SNAP officials. It also had a larger impact on beneficiaries with more complex required procedures, such as verifying earned income and completing full recertification. It appears simplifying access to information and communication with the SNAP agency led to substantial reductions in eligible exits.

6 Conclusion

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Appendix Figures

Figure A1: Retention for Case Heads

Survival Curves for New Case Heads (Michigan)

This 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 A2: 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. The 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+ bin. The right figure shows the same statistics, but considers newly entering case heads rather than internal case identifiers.
Figure A3: Retention by Earnings

Exit for 3+ Months at Recertification

By Earnings This Quarter

By Earnings Next Quarter

Figure A4: Heterogeneity of Bridges Effect

Exits for 3+ Months at Recertification

By Case Category

By Earnings

Base Values are 21.7%, 7.7%, 9%

Base Values are 14.7%, 14.9%
Figure A5: Bridges for Reporting (Michigan)

Exit Hazard Rate (%) vs. Month of Spell

Base Value 13.5%
## Table A1: Regressions of Reporting

<table>
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<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>
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<tbody>
<tr>
<td>1(Online)</td>
<td>-1.16**</td>
<td>-0.76</td>
<td>-1.37</td>
<td>1.66</td>
<td>-1.17</td>
<td>1.87*</td>
<td>-0.48</td>
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<tr>
<td></td>
<td>(0.53)</td>
<td>(0.50)</td>
<td>(1.19)</td>
<td>(0.61)</td>
<td>(1.21)</td>
<td>(0.75)</td>
<td>(1.03)</td>
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<tr>
<td>Observations</td>
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<td>31,422</td>
<td>12,391</td>
<td>14,415</td>
<td>4,616</td>
<td>30,508</td>
<td>1,870</td>
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<td>$R^2$</td>
<td>0.248</td>
<td>0.256</td>
<td>0.283</td>
<td>0.231</td>
<td>0.135</td>
<td>0.258</td>
<td>0.245</td>
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<tr>
<td>Base</td>
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<td>13.3</td>
<td>17.7</td>
<td>10.5</td>
<td>11</td>
<td>14</td>
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</tr>
</tbody>
</table>

Robust standard errors in parentheses

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


