Multiple Safety Net Program Participation: Incidence and Impacts from Reducing Administrative Burdens*

Neil A. Cholli†
Cornell University

Derek Wu‡
University of Virginia

This draft: January 2025
PRELIMINARY: PLEASE DO NOT CIRCULATE

Abstract

Multiple program participation among low-income households with children is a critical yet understudied aspect of the U.S. social safety net. Leveraging high-quality administrative records from Virginia, we find that half of program recipients participated in multiple programs despite significant gaps in take-up persisting among eligible populations. We evaluate the causal impact of a technological reform that streamlined applications for SNAP, Medicaid, and TANF via a centralized online platform. Introducing a novel framework that decomposes the reform's effects into "fixed" and "variable" administrative burdens by distance to field office, we employ complementary research designs to identify the impacts of reducing each burden type. The policy reform markedly increased multiple program participation, almost entirely among households with children, and targeted those with increased work histories and fewer criminal offenses. A partial identification analysis of recipient types reveals that a meaningful share of multiple program participation occurred on the intensive margin. Collectively, our results suggest that reductions in administrative burdens to multiple programs can benefit low-income working parents who were previously leaving benefits on the table.

*We are grateful to the Washington Center for Equitable Growth for financial support. Cholli thanks the Klarman Fellowship at Cornell University and Wu thanks the Bankard Fund for Political Economy at the University of Virginia for support. We thank Doug Miller, Michael Lovenheim, Zhuan Pei, and seminar and conference attendees at Cornell University, the 2024 National Tax Association Annual Meeting, and the Virginia Longitudinal Data System (VLDS) Research Forum for helpful comments, as well as Aline Jesus Rafi, Will Goldschmidt, and Tamra Arant for help with data acquisition. We also thank Dylan Craig for excellent research assistance.

†Email: nac85@cornell.edu
‡Email: derek.wu@virginia.edu

1 Introduction

One of the defining features of the U.S. social safety net for low-income households is its tapestry of cash and in-kind benefit programs. Many of the most important means-tested transfers, ranging from in-kind assistance for food (Supplemental Nutrition Assistance Program or SNAP) and medical bills (Medicaid) to cash assistance (Temporary Aid for Needy Families or TANF), are administered by state agencies, often with overlapping eligibility criteria. Thus, an understanding of the modern-day safety net would be incomplete without considering the interplay between various programs. However, the extent and nature of multiple program participation remain underexamined, particularly among households with children, who are among the most common beneficiaries of the safety net (Currie 2006b).

A growing concern of safety net programs is the administrative burdens imposed on households through complex enrollment and recertification processes (Currie 2006a; Herd and Moynihan 2018). For particularly disadvantaged households eligible for multiple programs, these burdens may compound and limit take-up, preventing access to critical resources for family and child well-being. Nevertheless, reducing administrative burdens may cause relatively less needy households to participate in a single program (Nichols and Zeckhauser 1982). Theoretically, it is ambiguous whether or not reducing burdens improves the overall targeting of the social safety net.

Despite this policy relevance, we have limited understanding of how many families receive multiple programs and how they differ from those receiving one program. Moreover, there is scarce empirical evidence on the role of administrative burdens in hindering multiple program participation and its implications for targeting efficiency. This paper aims to fill these gaps. We leverage high-quality administrative records from Virginia to examine the incidence of multiple safety net program participation and analyze the impacts of a major policy reform that simplified enrollment for multiple programs. To the best of our knowledge, we are among the first to empirically examine the consequences of reducing burdens on multiple program participation and the composition of multiple program recipients.

We begin by documenting several key descriptive facts about multiple program participation in SNAP, TANF, and Medicaid. First, approximately half of all program recipients participated in two or more programs, with the vast majority (80%) of multiple program recipients residing in households with children. Second, we document striking evidence of incomplete take-up, with 10–60% of various subgroups not participating in multiple programs despite being inferred to be eligible based on receipt of another program. Third, we show that transitions into and out of programs frequently occur *jointly*, highlighting the interconnected nature of participation dynamics. Finally, we find that participation rates decline

with distance from a county field office, suggesting that travel costs and other location-based barriers play an important role in shaping access to multiple programs.

Motivated by these facts, we examine the causal effects of an important policy change called CommonHelp that reduced administrative burdens for various programs in Virginia. As a novel state-administered technological reform, CommonHelp streamlined application and recertification processes for various safety net programs onto a centralized, user-friendly online platform. It was an important antecedent to contemporary policy movements that seek to ameliorate administrative burdens by streamlining access to multiple safety net programs. By 2023, 29 states have implemented online platforms that integrate applications for SNAP, Medicaid, and TANF (Code for America 2024). State policymakers are increasingly recognizing the importance of improving access to multiple programs, with a recent report by the National Governors Association highlights the promise of "leverag[ing]. . . information management technology" to "improve access and streamline delivery to beneficiaries" (2022, p. 2). Thus, our focus on CommonHelp and burdens to *multiple* program is particularly policy relevant and has promise to inform other state policies around the country.

Our study introduces a novel framework for decomposing the effects of reducing administrative burdens into two distinct types: "variable" and "fixed" burdens. Variable burdens are associated with geographic distance, as zip codes farther from field offices face higher travel costs and information frictions. In contrast, fixed burdens are unrelated to geographic distance and may include factors such as office congestion and the stigma associated with in-person visits. We employ complementary research designs to separately identify the impacts of reducing each type of burden. To estimate the effects of reducing variable burdens, we employ a difference-in-differences design comparing participation around CommonHelp between zip codes with varying distances to field offices. For fixed burdens, we use a regression discontinuity design around the time of treatment among zip codes in close proximity to their field office. Importantly, our framework can be adopted to analyze any policy change that simultaneously impacts a entire population (a setting lacking an obvious control group), so long as there are heterogeneous effects among different subpopulations (e.g., by distance to office).

We estimate that CommonHelp significantly increased program participation. Focusing first on the reduction in variable burdens, participation in any program increased by 2.3% for every 10 miles the zip code was located farther from a field office. In other words, relative to residents living next to their field offices, CommonHelp caused 2% more residents living 10 miles away from their field office to participate in at least one program. Effects occur immediately after CommonHelp's launch and jump again after a subsequent improvement to the online platform. A considerable share of these gains came from a 2.3% enrollment

increase per 10 miles in two or more programs, which was entirely concentrated in households with children.

Turning next to the impacts of reducing fixed burdens, our regression discontinuity estimates show that CommonHelp caused an immediate 3.5% jump in multiple program receipt among zip codes within close proximity to local offices. This effect is again wholly attributable to households with children. Conversely, enrollment in single programs *fell* by 1.5% around the launch of CommonHelp, which again was concentrated among households with children who likely transitioned from a single program to multiple programs. Bringing together estimates from the combination of reducing fixed and variable burdens, we estimate that CommonHelp brought in 140,000 recipients onto program rolls—the vast majority of whom were enrollees in multiple programs.

Given this compelling evidence that CommonHelp reduced administrative burdens for simultaneously accessing multiple programs, we next investigate the reform's impacts on the *composition* of multiple program recipients—focusing exclusively on households with children. We first examine targeting based on pre-determined observable characteristics, coupling baseline demographics with linked earnings and crime records to assess a more comprehensive set of variables that proxy for labor market skill and socioeconomic disadvantage. In general, we find that CommonHelp brought in individuals characterized by relatively large earnings potential (with more stable labor market histories) and fewer criminal histories. We also rigorously analyze the types of "compliers" brought into program participation as a result of CommonHelp. We invoke a monotonicity condition within an econometric framework of multiple unordered treatments to develop a methodology that bounds the shares of recipients who were induced into specific program bundles along the extensive and intensive margins. Our evidence suggests that CommonHelp reached those who would have had only partial attachment to the social safety net, providing new insights into targeting efficiency.

This paper contributes to several strands of literature. First, our paper adds to the descriptive literature documenting rates of single versus multiple program participation. Prior studies examining the prevalence of enrollment in multiple programs have largely relied on national surveys like the Survey of Income and Program Participation (Bartfeld 2013; Edelstein et al. 2014; Moffitt 2016; Macartney and Ghertner 2023). While these surveys provide useful data, they suffer from well-documented issues such as under-reporting of income sources (Meyer et al. 2015), leading to challenges in accurately estimating program receipt. We produce new statistics that shed light on the prevalence of multiple program participation for various demographic groups, which contribute to better understanding the social safety net's collective success at reaching different population groups.

Our causal estimates also contribute to a growing body of research examining administrative burdens and their role in limiting access to safety net programs. Prior work has primarily focused on individual programs, such as SNAP (e.g., Finkelstein and Notowidigdo 2019; Gray 2019; Homonoff and Somerville 2021) and Medicaid (e.g., Herd et al. 2013; Heinrich et al. 2021; Arbogast et al. 2024). In particular, previous studies have demonstrated that geographic distance to field offices matters for enrollment in a variety of programs, including Social Security Disability Insurance (Deshpande and Li 2019), the Special Supplemental Nutrition Program for Women, Infants, and Children (Rossin-Slater 2013), and social assistance in Canada (Hicks 2024). While these studies provide valuable insights, they often overlook the broader context that the social safety net is comprised of multiple programs (Baicker et al. 2014; Wu and Meyer 2023; Schmidt et al. 2024). Little is known about how reducing administrative burdens affects the ability of eligible families to enroll in multiple programs simultaneously. Administrative burdens may also have compounding effects for families eligible for multiple programs, who may be particularly disadvantaged and even less equipped to overcome burdens (Christensen et al. 2020).

Finally, our targeting analysis complements the econometric literature related to multiple unordered treatments, which generate various response or "complier" types with different fallback options (Kirkeboen et al. 2016; Heckman and Pinto 2018; Mountjoy 2022; Pinto 2022). We utilize additional econometric assumptions to bound the share of multiple program recipients who responded to CommmonHelp along the intensive versus extensive margins. Our bounding methodology complements efforts by Borusyak (2015), who focuses on partially identifying the share of treatment recipients whose outcomes are affected by a treatment, and Kline and Tartari (2016), who bound different types of behavioral labor supply responses within a structural model.

Our paper proceeds as follows. Section 2 summarizes the safety net programs and policy changes we study. Section 3 describes the administrative data employed in our analyses. Section 4 illustrates descriptive statistics on single and multiple program participation, which help motivate our subsequent analyses. Section 5 develops our conceptual framework for "variable" and "fixed" burdens and describes our research designs for identifying the causal impacts of CommonHelp. Section 6 reports CommonHelp's effects on various program bundles. Section 7 describes effects on targeting efficiency based on prior earnings, crime, and household characteristics. Section 8 bounds the shares of intensive- and extensive-margin responders among multiple program recipients. Section 9 concludes.

2 Institutional Context

2.1 Safety Net Programs

The three social safety net programs we study are the Supplemental Nutrition Assistance Program (SNAP), Medicaid, and Temporary Aid for Needy Families (TANF). These are three of the most important means-tested transfers in the U.S., with a collective cost of approximately $1 trillion in 2023. While all three are federal programs, they are administered by the individual states—meaning that eligibility rules and enrollment procedures often differ across states. Table 1 summarizes the eligibility criteria of these three programs in Virginia, focusing on 2012 (the year during which CommonHelp was launched).

SNAP (formerly the Food Stamps Program) is an in-kind transfer program specifically for food and beverage purchases. Recipients receive money via an Electronic Benefits Transfer card, which can be used at authorized grocery stores; in this way, SNAP may be viewed as a "cash-like" benefit program. In contrast to the other programs, SNAP eligibility is almost strictly based on household income and assets and is therefore broadly available regardless of age, presence of children, or medical condition. Thus, able-bodied adults without dependents (ABAWDs) who meet income and asset limits are eligible for SNAP, although they faced work requirements in Virginia starting in 2013.

Medicaid is a public health insurance program that covers medical expenses for low-income individuals. Under federal guidelines, Medicaid eligibility is limited to children, pregnant women, certain parents, seniors, and people with disabilities. Virginia also administers a supplementary healthcare program called the Family Access to Medical Insurance Security Plan (FAMIS), which provides free insurance for children ages 18 and below and whose family income falls under 205% of the federal poverty line (FPL). In our paper, we use "Medicaid" as an umbrella term for assistance provided under either Medicaid or FAMIS. Virginia did not expand Medicaid to low-income ABAWDs until 2019, when it also expanded income limits to other eligible groups.

In contrast to the other two in-kind programs, TANF is a cash assistance program targeting very low-income families with children and mandates work requirements for all parents. Given its stringent income limits—about 50% of the FPL—this program is far less widespread than SNAP and Medicaid and typically targets single-parent households. At the same time, TANF recipients are "categorically eligible" for both SNAP and Medicaid and typically participate in all three programs. More generally, during our time period of interest (before the 2019 Medicaid expansion), the only groups eligible for multiple programs tended to be low-income families with children and the elderly and disabled. Low-income ABAWDs were instead only eligible for SNAP, while "near-poor" children were eligible only for Medicaid.

Table 1: Overview of SNAP, Medicaid, and TANF in Virginia (2012)

	SNAP	Medicaid	TANF
Demographic elig.	All families	Children ages 0–18; parents, caregivers, and pregnant women; elderly; disabled*	Families with cohabiting children ages 0–17 or with children age 18 enrolled in school
Income limits	Gross income $\leq$ 130% FPL (no limit for elderly or disabled); net income $\leq$ 100% FPL**	Children and pregnant women $\leq$ 200% FPL; elderly and disabled $\leq$ 80% FPL; parents $\leq$ 33% FPL***	Gross income $\leq$ 43% FPL
Asset limits	Non-elderly/non-disabled $\leq$ \$2,000; Elderly or disabled $\leq$ \$3,250	\$2,000 excluding home, personal belongings	\$1,000 excluding home, one vehicle
Work requirements	N/A****	N/A	Yes
Time limits	N/A	N/A	6 years

*ABAWDs became eligible for Medicaid after January 2019
**Gross income limits for SNAP increased to 200% and asset limits were eliminated after July 2021
***Income limits for Medicaid increased to 138% for elderly/disabled, parents, and ABAWDs after January 2019
****ABAWDs ages 18–49 faced work requirements for SNAP after October 2013

2.2 Administrative Burdens Before and After CommonHelp

Enrollment in SNAP, Medicaid, and TANF involves going through an initial application process and then recertifying eligibility at periodic intervals. Yet, families often face barriers to enrolling in these and other programs. For example, information about program eligibility and availability may be hard to come by. And even if families learn that they are eligible, they might face substantial burdens associated with the application process—from filling out complicated paperwork to having to physically travel to a local welfare office to conduct face-to-face interviews. Verifying eligibility often requires providing numerous supplemental documents, such as proof of identity, proof of residence, and earnings records. Applicants may also face psychological costs like stigma.

Prior to October 2012, applicants in Virginia had to complete a lengthy 20-page application form where they elected to apply to SNAP, TANF, and Medicaid, as well as separate recertification forms for each program to continue receiving benefits.[1] All forms were required to be completed on paper and submitted in person or by mail or fax to their assigned local Department of Social Services (LDSS) office. Critically, LDSS office assignments are

1. At the same time, Virginia was ahead of the curve in enabling clients to apply for multiple programs on a single form. In most states, individuals could apply for SNAP and TANF using a single application, while a separate application was required for Medicaid.

based on the applicant's *county of residence*. Typically, there is one LDSS office per county. Given the complexity of the application process, clients often sought in-person guidance from caseworkers at their LDSS office to learn which programs they may be eligible for (in light of information frictions) and to navigate the hassle costs associated with submitting their application. As a result, these application processes may have been particularly burdensome for those living far from their assigned county office.

Motivated to reduce administrative burdens and improve caseworker efficiency in LDSS offices, Virginia launched CommonHelp in October 2012 to streamline application processes through a centralized, online platform. CommonHelp provided several advantages relative to the previous paper application method. First, CommonHelp offered a quick eligibility screening tool, allowing prospective applicants to input basic information about their finances and household structure to find out which combinations of programs they would be likely eligible for. Second, it provided a personalized, user-friendly portal to apply and recertify for multiple programs from the convenience of one's own home (or a preferred location with internet). Applicants could select the programs they wished to apply for and, based on their selection, were guided through a simplified, step-by-step application process. These data would be transmitted to the household's assigned LDSS office and would be subsequently reviewed by caseworkers for approval. In 2015, CommonHelp was further enhanced to allow applicants to submit digital copies of supplemental documents (e.g., proof of address) through the portal. This effectively allowed applicants to complete the entire application and recertification processes remotely, although the paper application method continued to exist.

In short, CommonHelp made it easier for households to apply for multiple programs without the need for in-person visits. The platform could be accessed 24 hours, seven days a week, providing applicants and recertifiers greater flexibility to apply to programs without being constrained to their assigned LDSS office's business hours. This reduced barriers to accessing safety net programs for eligible households—particularly for households geographically more distant to their offices. While prior work has found that automating caseworker assistance can increase administrative burdens (Wu and Meyer 2023), a key difference here is that CommonHelp "automated" the application portal rather than caseworker help. It was also introduced on top of, rather than in lieu of, existing in-person assistance.

3 Data

We leverage anonymized, longitudinal administrative microdata from Virginia between 2007–2022. The core data come from the Virginia Department of Social Services (VDSS), which

administers SNAP, TANF, and Medicaid.[2] The VDSS records provide monthly records on program participation for the universe of recipients. Critically, since we link individuals across monthly SNAP, Medicaid, and TANF records, we are able to observe if they participate in multiple programs simultaneously. This permits us to examine participation in individual programs as well as bundles of programs, allowing for an in-depth analysis of the interplay between these programs and their responsiveness to policy changes.

Each individual is linked to a program-specific case number. Individuals who are members of the same program case (e.g., a single mother and her child on TANF) share the same case number. To construct "households," we combine individuals across multiple program cases in the same year-month. This key innovation allows us to proxy for "households" even in the absence of a master relationship file or census, although it is possible that some household members are excluded from a case and non-cohabiting members are included in a case. This is particularly useful for linking child-only cases (e.g., for TANF and Medicaid) to their parents if their all individuals are part of the same case for another programs (e.g., SNAP). VDSS records also include basic demographic characteristics of each individual such as age and gender. These characteristics, coupled with the above methodology for creating households, permit us to infer household characteristics such as one- or two-parent households, household size, and presence of children.

Critically, VDSS records report the zip code and county of residence of each program recipient.[3] As we illustrate later, geographic proximity to one's county field office is strongly correlated with administrative burdens and program take-up. This will also serve as a key source of variation in our identification strategy. In several of our empirical analyses, we aggregate program participation at the residential zip–county level in monthly panels. We Note that zip codes sometimes lie in multiple counties and are thus served by multiple LDSS offices. Our geographic unit of analysis will therefore be zip-county pairs; we refer to these as "zip codes" for simplicity.[4]

We link the VDSS records with additional institutional and administrative datasets. First, for analyses aggregated at the zip-county level, we bring in a complete frame of zip codes from the Department of Housing and Urban Development (HUD) United States Postal

2. VDSS technically administers four other programs: child support, child care assistance, foster care, and refugee assistance. With the exception of child support, the other programs are very small (see Appendix Figure A.1). We also exclude child support from our analysis because—even though it is administered by VDSS—it is ultimately a private transfer rather than a public benefit program.

3. In less than 5% of panels, individuals temporarily appear in two distinct case numbers, which is typically associated with more than one zip code and county. To resolve conflicts in geographic units, we prioritize using geographic units associated with the case number appearing after multiple cases.

4. There are a few cases where a zip code lies in more than one county, but both counties happen to be served by the same LDSS office. In these cases, zip-county pairs are aggregated back up to zip codes.

Service ZIP Code Crosswalk Files. This enables us to bring in zip codes for which there may be no program recipients in a given year-month. We also collect the historical addresses of LDSS offices between 2007–2022 using records from the Wayback Machine Internet Archive. These institutional data help generate our key source of variation: the (Euclidean) distance between each zip-county pair's centroid and its assigned LDSS office.[5]

Next, we link VDSS program microdata to individual-level administrative records from the Virginia Employment Commission (VEC), the Virginia Criminal Sentencing Commission (VCSC), and the Department of Juvenile Justice (DJJ). VEC records contain quarterly earnings data and annual Unemployment Insurance (UI) receipt records, which allow us to proxy for work ability and measure prior work history. VCSC and DJJ records capture each individual's interactions with the criminal justice system, including the timing and type of offenses and incarceration sentence. By linking these data sources together, we retrieve an enriched set of characteristics that will be useful for assessing the characteristics of program recipients as part of our descriptive and targeting analyses.

To the best of our knowledge, this will be the first work in economics to analyze this unique set of linked administrative data. Recent work by Gray et al. (2023) uses a subset of these data to specifically focus on SNAP and employment in Virginia.

4 Four Facts about Multiple Program Receipt

In this section, we provide some descriptive facts to illustrate the incidence and dynamics of multiple program participation, before providing evidence of spatial heterogeneity in administrative burdens. These results help to motivate and shape our subsequent analyses examining the impacts of changing administrative burdens.

Fact 1: Approximately half of all program recipients participate in multiple programs, who are in turn concentrated in households with children.

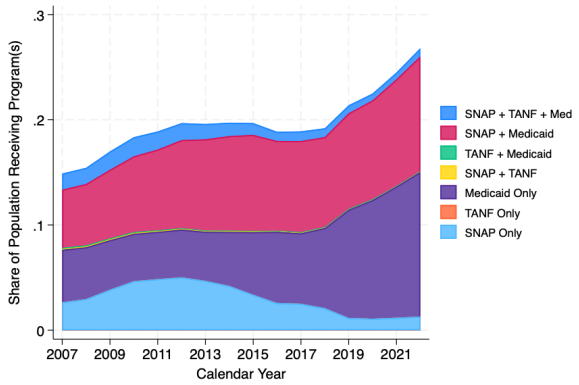
Figure 1, Panel (a), begins by illustrating trends in the share of the population in Virginia participating in individual programs and combinations of programs.[6] Between 2007 and 2022, the population share receiving any of the three programs increased from 15% to 26%; roughly half of this increase was driven by Virginia's expansion of Medicaid in 2019. While seven bundles are technically possible out of the combination of SNAP, TANF, and Medicaid,

5. We also calculate driving and transit distances using Google API, which we utilize in robustness checks.

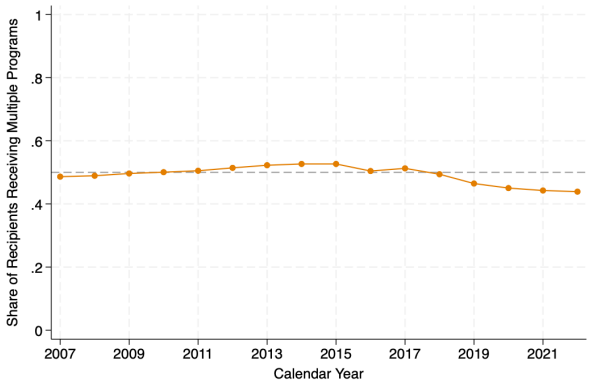
6. We rely on the American Community Survey (ACS) to estimate population-level denominators for the recipient shares.

Figure 1: Trends in Single and Multiple Program Participation

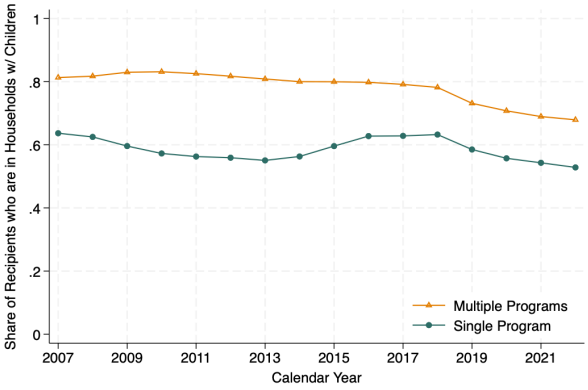
(a) Participation in Bundles of Programs



(b) Share of Recipients with Multiple Programs



(c) Recipients in Households with Children



Notes: This figure reports program participation trends in each program bundle over time. Panel (a) reports shares of the population receiving each program bundle. Population estimates are drawn from the American Community Survey. Panel (b) reports the share of program recipients participating in multiple (two or more) programs. Panel (c) illustrates the share of individuals in households with children among single and multiple program recipients.

only four are taken up at a non-trivial rate: SNAP only, Medicaid only, SNAP/Medicaid, and SNAP/Medicaid/TANF. This is due to the overwhelming majority of TANF recipients being enrolled in all three programs, as can be seen in Appendix Figure A.2.

Panel (b) shows that approximately half of all program recipients participate in multiple (i.e., two or more) programs. This fraction has remained largely steady over time, though it increased slightly during the early- to mid-2010s before declining more recently as a result of Medicaid expansion. Strikingly, Panel (c) shows that the vast majority of multiple program recipients are individuals who are in households with children, with this share hovering at around 80% for the most of the time period analyzed . Prior to 2019, the remaining recipients of multiple program were likely elderly or disabled individuals. In contrast, approximately

60% of single program recipients are individuals in households with children. This share, while lower, is still sufficiently high to wonder whether or not some of these single program recipients may in fact be eligible for multiple programs.

Fact 2: The take-up of multiple programs is incomplete.

A pervasive challenge in estimating program take-up is calculating the size of the eligible population when we only have microdata on recipients. Existing studies ofen rely on survey datasets such as the ACS and CPS to impute eligibility counts, but these imputations have shortcomings due to measurement error in surveys, misaligned reference periods, etc. We take a different approach by examining take-up rates among specific subpopulations who are inferred to be eligible based on their receipt of *another* program. This "revealed-eligibility" approach has been used in prior studies (see, e.g., Schmidt et al. 2024) and circumvents thorny issues with estimating the full eligible population.

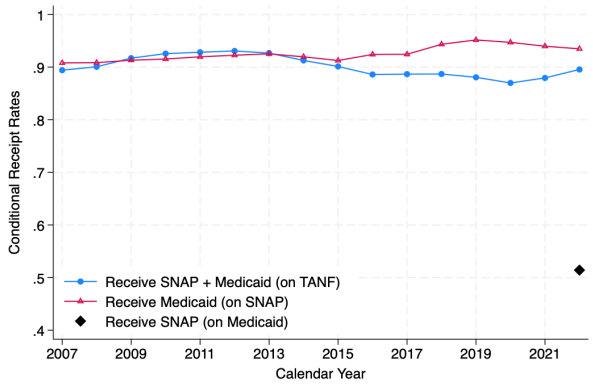
We find persistent evidence of incomplete take-up over time.[7] Examining first TANF recipients (who are categorically eligible for all three programs), Panels (a) and (b) of Figure 2 show that roughly 10% of child and adult recipients do not participate in other programs. Among children on SNAP, we find that 5–10% do not enroll in Medicaid despite being eligible. And for adults on SNAP, approximately 15% do not enroll in Medicaid even after all low-income adults became eligible after the 2019 expansion. Perhaps most notably, after income thresholds were raised to 200% and asset tests were eliminated for SNAP in 2021, 42% of children and 57% of adults on Medicaid still remained unenrolled in SNAP. These levels of incomplete take-up are all the more striking given that SNAP, Medicaid, and TANF are bundled into a common application in Virginia.

Finally, Panel (c) of Figure 2 compares the distribution of quarterly household wages (linked from VEC records) for single versus multiple program recipients, focusing solely on households with children. Given that income eligibility requirements are stricter for multiple programs, we should expect recipients of multiple programs to have lower incomes than recipients of single programs. However, we find the opposite result: single program recipients have average household wages that are 12% lower than those of multiple program recipients, with more than 60% of single program recipients having no wages at all (compared to 50% of multiple program recipients). Note that this provides indirect evidence of incomplete take-up, as wages are a subset of total income and these patterns may arise from

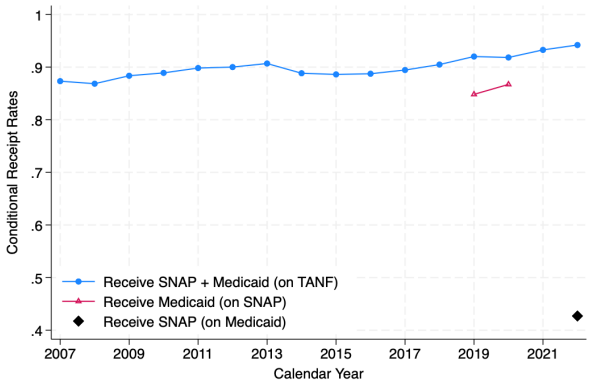
7. Note that this analysis examines annualized program participation, which provides a lower bound on the degree of incomplete take-up. We use annual data rather than monthly data to avoid conflating incomplete take-up with delays in transitioning into steady-state program bundles.

Figure 2: Evidence of Incomplete Take-Up of Multiple Programs

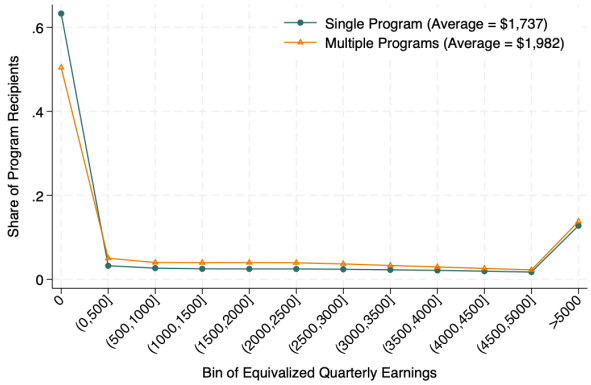
(a) Conditional Take-Up Rates: Ages 0–17



(b) Conditional Take-Up Rates: Ages 18–64



(c) Wages for Single vs Multiple Program Recipients with Children



Notes: Panels (a) and (b) plot conditional participation rates of different programs among children and adults who are inferred to be eligible based on their participation in other programs. Some lines are broken or possess a single data point due to changing eligibility criteria over time. Individuals on TANF (blue lines) are categorically eligible for both SNAP and Medicaid and children on SNAP (red line, Panel (a)) are eligible for Medicaid throughout the time period. On January 1st, 2019, Medicaid expansion increased the income eligibility threshold for adults above SNAP (red line, Panel (b)); on July 1st, 2021, SNAP expanded its income eligibility threshold beyond Medicaid (green dots). Panel (c) compares the distribution of quarterly household wages for single and multiple program recipients among households with children between 2007-22. Household earnings are equivalized using the NAS equivalence scale.

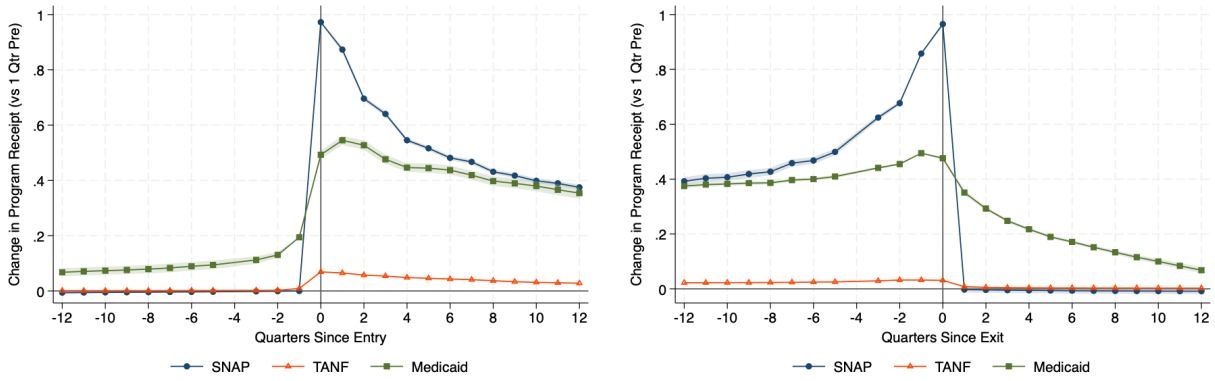
take-up of single and multiple programs being concentrated among lower- and higher-income eligible groups (respectively). Nevertheless, these patterns suggest that—based on observable characteristics—a substantial fraction of single program recipients appear to be leaving additional benefits on the table.

Fact 3: Dynamic interactions in program take-up are prevalent, with individuals

Figure 3: Multiple Program Interactions Around Program Entry and Exit

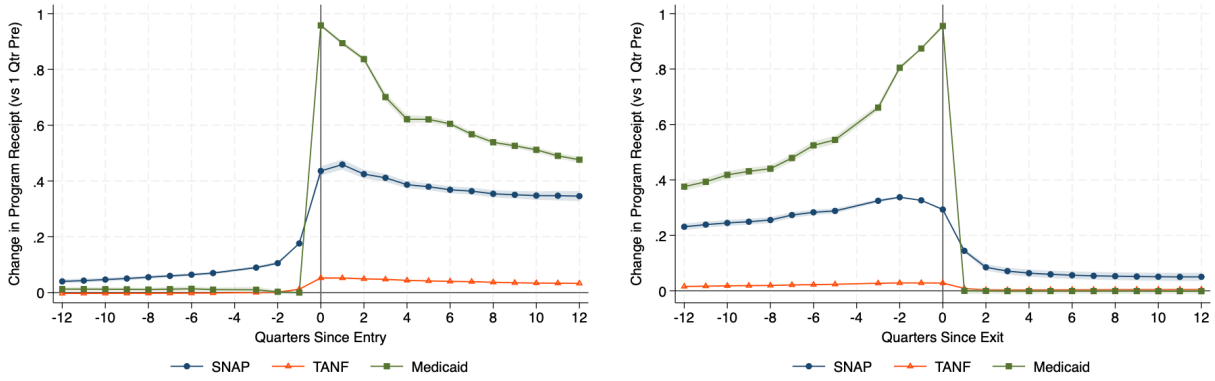
(a) SNAP entry

(b) SNAP exit



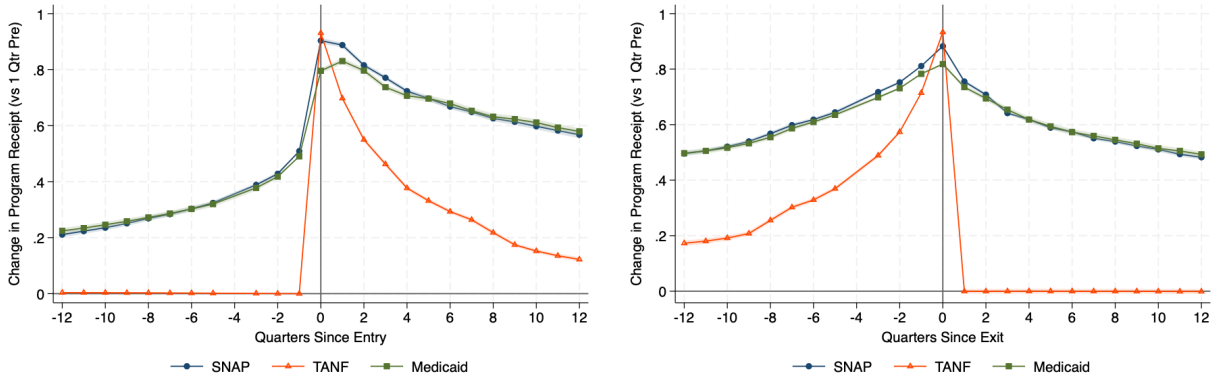
(c) Medicaid entry

(d) Medicaid exit



(e) TANF entry

(f) TANF exit



Notes: These figures plot event-study estimates of each program receipt on program entrants and exiters, centered around the quarter of entry and exit. Left panels condition on individuals who enter a given program and did not receive the program in the prior 12 quarters. Right panels condition on individuals who exit a given program and did not receive the program in the subsequent 12 quarters. Our sample consists of those entering or exiting between January 2010-December 2019, so that we can observe at least 12 quarters of participation data before and after the event. Shaded regions are 95% confidence intervals.

often entering and exiting multiple programs simultaneously.

While trends in aggregate take-up are interesting in their own right, they miss richer dynamics present among individual-level participation trajectories. Figure 3 estimates the receipt of SNAP, TANF, and Medicaid centered around entry into and exit out of a given program. When estimating dynamics around program entry (left panels), we condition on the population who did *not* receive a given program for three years prior to entry; for program exit (right panels), we condition on those who did *not* receive a given program for three years after exit. While these event-study estimates do not represent causal effects, they do illuminate how entry and exit around one program interact with entry and exit around *other* programs. Notably, there are clear spikes in program participation at the point of entry or exit for *all three* programs, suggesting that individuals simultaneously enroll or drop out of programs in bundles. While Figure 3 focuses on interactions for all entrants and exiters, Appendix Figure A.3 shows that the interactions are even stronger for recipients with children.

Focusing first on the entry margin, we find that program interactions are perhaps strongest for TANF. At the point of entry into TANF, Panel (e) shows that about 80% and 90% of recipients are also enrolled in Medicaid and SNAP, respectively—with enrollment in these two programs increasing over the months leading up to TANF entry . Moreover, while TANF receipt itself tends to be short-lived, the reliance on other programs is persistent. Three years after first enrolling in TANF, nearly 60% remain on SNAP and Medicaid, compared to only 10% still on TANF. Among those receiving SNAP for the first time, Panel (a) shows that more than half are also enrolled in Medicaid at the point of entry (compared to only 20% in the quarter before entry) and less than 10% are enrolled in TANF. For Medicaid, the interactions are less strong but still pronounced. Panel (e) shows that more than 40% of Medicaid recipients at the point of entry are enrolled in SNAP (sharply increasing from less than 20% in the prior quarter), while 5–10% are enrolled in TANF.

On the exit margin, we continue to see inflection points in the receipt of other programs around exit out of a given program. Panel (f) shows that those exiting TANF experience a steady, though not sharp, decline in the receipt of SNAP and Medicaid. Indeed, half of TANF exiters continue to receive these other programs three years after exit. Those exiting SNAP also experience a steadier decline in Medicaid receipt (Panel (b)), while those exiting Medicaid tend to experience a more rapid decline in SNAP receipt (Panel (d)). Both SNAP and Medicaid exiters also tend to exit TANF at the same time, although baseline rates of TANF receipt were already very low.

Fact 4: Those living farther from their county field office participate less in mul-

tiple programs, suggesting that administrative burdens are spatially distributed.

As alluded to in Section 2.2, prospective applicants residing in zip codes farther from their assigned LDSS office may face greater administrative burdens than more proximal residents. Figure 4 provides suggestive evidence for this hypothesis by illustrating the association between zip-level per capita program participation and distance to office over the 2007-22 time period. These binscatters control for a variety of zip-level covariates, to account for potential confounding factors that may be associated with both receipt and distance (such as spatial differences in the share of the eligible population and local economic conditions). Appendix Figure A.4 shows binscatters without any controls.

Panel (a) starts by focusing on multiple program receipt. The solid line shows that, on average, every additional mile of distance is associated with a 0.26 percentage point decline in per capita receipt of a single program, holding constant other covariates. Given a standard error of 0.001, this gradient is statistically significant at the 95% level. Panel (b) shows that the receipt-distance gradient for single programs, while negative, is weaker. Specifically, single program receipt declines at 0.16 percentage points per mile, although this is not statistically significant at conventional levels. Recall that for many households, programs shared overlapping eligibility criteria and that, prior to CommonHelp, applications to all three programs were bundled in the *same* paper form. This suggests that more distant residents faced larger information frictions that may have been compounded for those eligible for multiple programs.

This figure aligns with prior work demonstrating that distance to such offices matters for enrollment in a variety of programs, such as Social Security Disability Insurance (Deshpande and Li 2019), the Special Supplemental Nutrition Program for Women, Infants, and Children (Rossin-Slater 2013), and social assistance in Canada (Hicks 2024). We will exploit this spatial variation in our empirical strategy. In contrast to these papers, we examine the impact of *reducing* administrative burdens borne from distance to offices on program participation and targeting, rather than the impacts of distance per se.

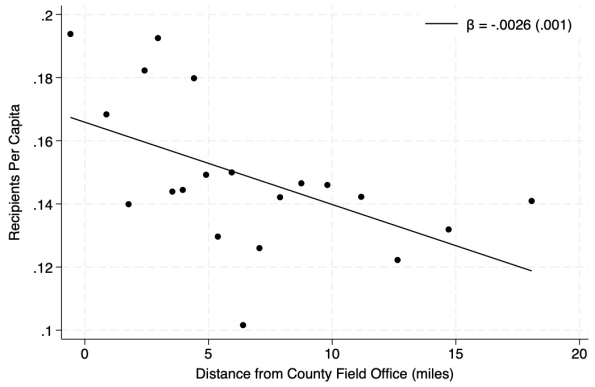
5 Empirical Strategy

5.1 Conceptual Framework: CommonHelp's Impacts on Administrative Burden Types

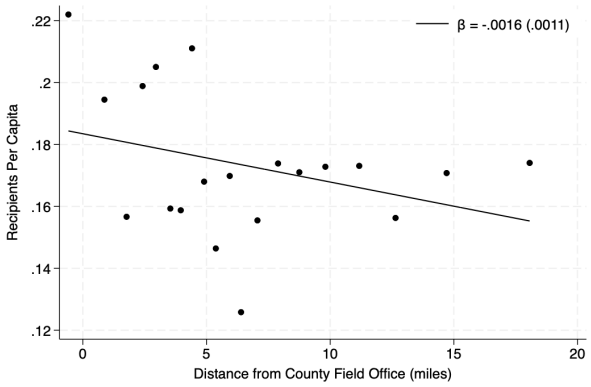
We begin by developing a simple framework to distinguish the different administrative burdens impacted by CommonHelp that promoted program receipt. Our framework motivates

Figure 4: Zip-level Participation Rates Decrease in Distance to County Field Office

(a) Multiple Program Receipt



(b) Single Program Receipt



Notes: This figure illustrates the association between zip-level single and multiple program participation rates and zip code distance to their assigned county field office using monthly data between January 2007 and December 2022. Participation rates are expressed in per capita terms using population estimates of zip code tabulation areas (ZCTAs) from the 5-year American Community Survey (ACS), scaled to the zip-county level using the HUD-USPS crosswalk. Binscatters are weighted by population and control for ZCTA-level poverty rate, zip-level population share of Blacks and Hispanics, and county fixed effects. ZCTA covariates are drawn from 5-year ACS estimates.

different research designs to identify impacts along each burden type; together, they allow us to assess CommonHelp's *total* effect on the population. Ignoring one burden type may severely understate the policy's total effect. Our framework can be applied to evaluate any setting involving a treatment that universally and simultaneously affected a population (with no available or well-defined untreated group) but generated heterogeneous effects among different subpopulation groups (e.g., geographic areas).

We conceptualize that CommonHelp increased program receipt by reducing two broad types of administrative burdens: "variable burdens" and "fixed burdens." We define "variable burdens" as costs to program participation that broadly depend on geographic distance to field office. Their existence is suggested by the negative association between zip code distance and multiple program receipt highlighted in Fact 4. Prior to CommonHelp, residents of more distant zip codes likely faced more variable burdens from hassle and financial costs of traveling to their field office for in-person application assistance and submitting applications and supplemental documents. These residents also likely faced greater information frictions about their eligibility for different programs and hassle costs from navigating the application process due to limited access to in-person assistance. Variable burdens may also capture costs or barriers associated with the composition of populations living in more distant areas. For example, variable burdens would reflect any differences in the salience of social stigma among prospective applicants among more versus less distant zip codes.

Since CommonHelp reduced the need for in-person field office visits and reduced information frictions, we expect CommonHelp delivered a larger "treatment dosage" to more distant zip codes that had endured more variable burdens. We thus expect program participation to grow faster in more remote areas. As a visual aid, Figure 5 illustrates stylized examples of CommonHelp's dynamic treatment effects on caseload growth for two zip codes with differing distances to field office. Assuming CommonHelp only reduced variable burdens, then CommonHelp would generate positive effects only in the distant zip code, as portrayed by the increasing blue lines and flat red lines in the top panels. Panel (a) focuses on the case where individuals were immediately aware of CommonHelp's developments and faced no learning costs in utilizing the platform, resulting in an instantaneous increase in program receipt by β after its launch and further by β^{15} after the 2015 upgrade. In this case—and if the difference in counterfactual log recipient counts between the distant versus close zip code is stable over time—then a difference-in-differences (DD) strategy would identify the total effects of CommonHelp's launch and upgrade. This is illustrated by the full blue shaded region in Panel (a). If instead learning costs were present or if CommonHelp spurred snowballing peer effects in program participation (Dahl et al. 2014), then effects on the distant zip code would grow over time; see Panel (b). Here, an event-study DD specification would appropriately capture these treatment effect dynamics. Section 5.2 discusses this research design in depth.

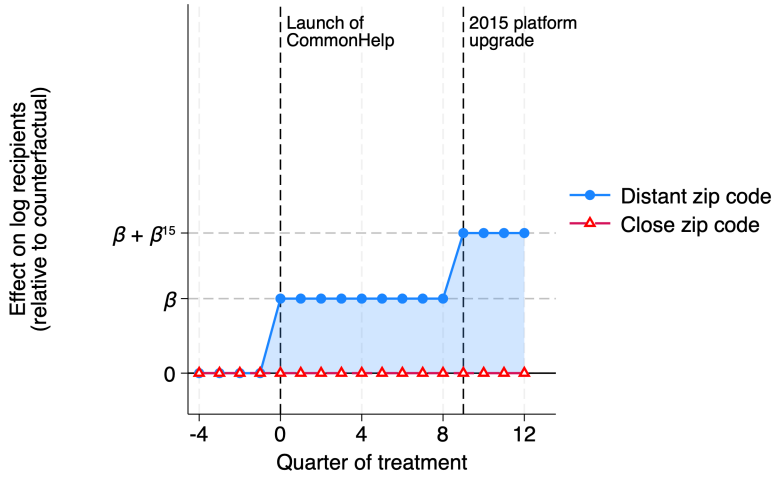
The second broad type of administrative burden reduced by CommonHelp is "fixed burdens," which we define as costs to program receipt that are unrelated to geographic distance. For example, by permitting virtual submission of applications, CommonHelp may have mitigated social stigma associated with public, in-person office visits and caseworker interactions for all prospective applicants. Moreover, substitution away from paper to online applications may reduce office congestion for in-person visitors, potentially increasing program receipt among paper applicants as well.

If CommonHelp also reduced fixed burdens, then we would expect program receipt to increase even in zip codes close to their field office. The bottom panels of Figure 5 visualize this scenario with the red lines increasing by β^{fb} and $\beta^{fb,15}$ around CommonHelp's launch and upgrade. Critically, the DD strategy described earlier would *fail to identify* these effects since it purges the effects on program receipt experienced by close zip codes, leading to an underestimate of the policy's total effects on recipient growth. This motivates an alternative identification strategy.

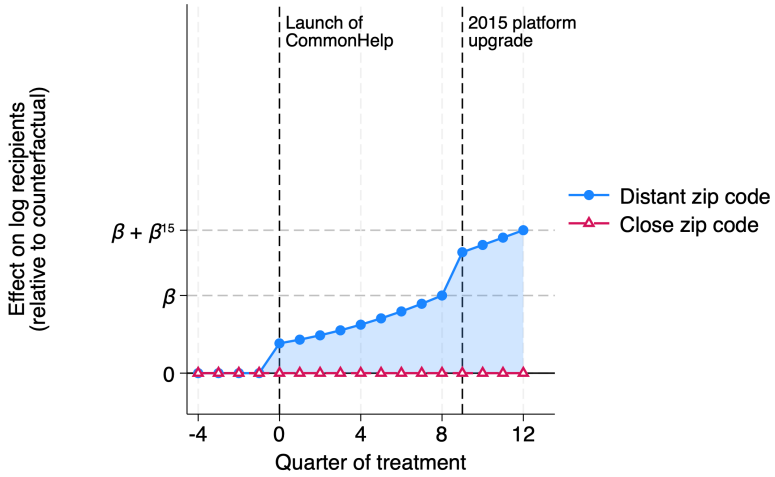
To identify the effects generated by reduced fixed burdens, we pursue a regression discontinuity (RD) design among zip codes proximal to their field office. If CommonHelp's reductions in fixed burdens generate static effects in program receipt—and under appro-

Figure 5: Expected impacts of CommonHelp on program receipt under different assumptions

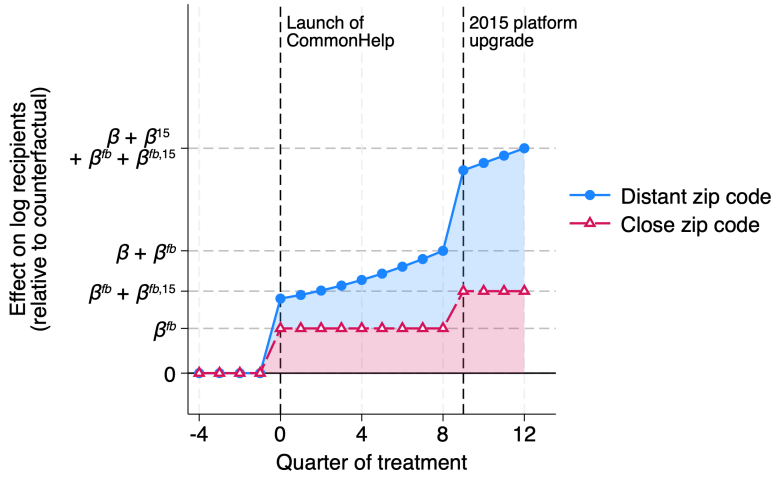
(a) Reduction in variable burdens only: Static effects



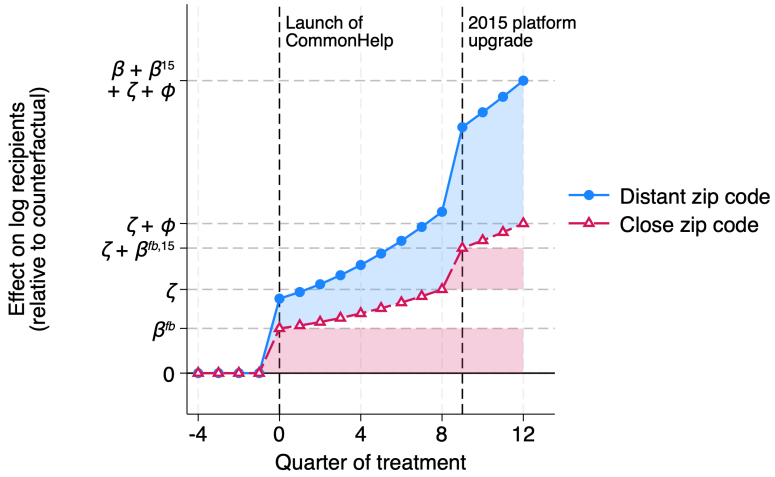
(b) Reduction in variable burdens only: Dynamic effects



(c) Including reduction in fixed burdens: Static effects



(d) Including reduction in fixed burdens: Dynamic effects



Notes: This figure illustrates hypothetical scenarios of CommonHelp's impact on program receipt. The blue and red lines show CommonHelp's causal effect on receipt in zip codes with different field office distances over time. The blue and red shaded areas represent the effects identifiable from difference-in-differences and regression discontinuity designs, respectively. See main text for details.

priate identification assumptions—an RD design around CommonHelp's launch and 2015 upgrade would identify impacts from reducing fixed burdens, denoted β^{fb} and $\beta^{fb,15}$. This is represented by the full red shaded region below the red line in Panel (c). These effects, coupled with the DD design, would successfully identify the total effects of CommonHelp on both zip codes. Nevertheless, it is plausible that CommonHelp's reduction in fixed burdens also evolves over time. For example, if there is increasing substitution from paper to online applications over time, the number of field office visitors would decline over time, resulting in decreasing office congestion and social stigma. Panel (d) illustrates this scenario by allowing the red line to grow after CommonHelp's launch from β^{fb} to ζ, and further increase from $\beta^{fb,15}$ to ϕ after the 2015 upgrade. Since the RD designs zero in on the contemporaneous impacts of the platform's launch and upgrade (β^{fb} and $\beta^{fb,15}$), we hypothesize they identify a *lower-bound* on the CommonHelp's effects via fixed burdens ($\beta^{fb} < \zeta$; $\beta^{fb,15} < \phi$). This is depicted by the partially-full shaded red region below the red line. Section 5.3 describes the RD design in more detail.

In sum, our conceptual framework demonstrates that the identified parameters from the DD and RD designs—$\beta + \beta^{15}$ and $\beta^{fb} + \beta^{fb,15}$, respectively—provide ingredients for quantifying CommonHelp's total effect on recipient growth. Specifically, if β, β^{15} were expressed as linear per-mile growth parameters and D_z denoted zip code z's distance from field office,[8] then the total growth experienced by z after CommonHelp's launch and upgrade is

$$g_z \equiv (\beta + \beta^{15}) \cdot D_z + (\beta^{fb} + \beta^{fb,15}). \tag{1}$$

Appropriately scaling zip-level program recipient counts by g_z and summing over all zip codes delivers a lower bound of the number of program recipients brought in by CommonHelp in the entire Virginian population.

Having distinguished the types of burdens that motivate our research designs, we turn to describing the empirical DD and RD models and their identification assumptions.

5.2 Identifying Effects of Reducing Variable Burdens

Our difference-in-differences (DD) design compares changes in program participation between zip codes located farther from field offices (where variable burdens were higher pre-reform) and comparison zip codes located closer to offices before and after CommonHelp's launch in October 2012. More formally, letting z index zip-county pairs, t index calendar year-month, and $q(t)$ denote t's quarter, we estimate the following continuous DD event-

8. In the simple case of Figure 5 involving only two zip codes, the close and far zip codes are assumed to have distances $D_{\text{close}} = 0$ and $D_{\text{far}} > 0$ and the DD parameters β, β^{15} are rescaled by $1/D_{\text{far}}$.

study specification:

$$\log(Y_{zt}) = \sum_{\substack{-12 \leq k \leq 12, \\ k \neq -1}} \beta_k \cdot (D_z \times \mathbb{1}\{q(t) = k\}) + \delta_z + \lambda_t + \varepsilon_{zt} \tag{2}$$

Here, Y_{zt} is the number of recipients of a given combination of program for zip z and year-month t, D_z measures zip z's distance from its assigned LDSS office, and $\mathbb{1}\{q(t) = k\}$ indicates if year-month t belongs to event quarter k. Zip- and month-level fixed effects are denoted by δ_z and λ_t. Our main estimates focus on a window of 12 quarters before and after CommonHelp's launch—i.e., October 2009 through December 2015.

Throughout our empirical analysis, our primary outcomes of interest are log counts of any program receipt, multiple program receipt, and single program receipt.[9] In light of Fact 1, we also distinguish impacts on counts of recipients in households with versus without children. Given the nature of CommonHelp and eligibility criteria, we expect particularly pronounced effects in multiple program receipt among households with children.

Our main coefficients of interest in Equation (2) are $\{\beta_k \,|\, k \geq 0\}$, which measure CommonHelp's marginal effect (per mile distance) on program receipt growth in k quarters after CommonHelp's launch, with $k = -1$ (immediately preceding CommonHelp's launch) being the omitted quarter. Based on our two-way fixed effect specification, β_k represents a positively-weighted average of marginally reducing administrative burdens (in terms of eliminating the additional hassle cost incurred from an additional mile of distance) across all zip codes (Callaway et al. 2024). A key advantage of this event-study specification is that it estimates the dynamic impacts of CommonHelp, capturing gradually growing impacts like the example shown in Figure 5, Panel (b).

Our identifying assumption is that, absent the reform, farther and closer zip codes would have evolved in parallel in program participation absent any changes in variable burdens.[10] This assumption may be violated if business cycles differentially affect zip codes with varying distances to field office. In particular, one may be wary that the Great Recession of 2007–09 may have affected the growth trajectories of program recipients in more or less distant zip codes. We probe these threats of identification in our robustness checks below. Moreover, the parameters $\{\beta_k \,|\, k < 0\}$ constitute our pre-trends test to help validate our parallel trends

9. Our estimation sample excludes individuals receiving TANF only, TANF and SNAP only, or TANF and Medicaid only given (i) the tiny shares of these program bundles found in Figure 1 and (ii) that TANF receipt almost always occurs simultaneously with SNAP and Medicaid, as seen in Figure 3, Panels (e) and (f). Thus, we only consider five program bundles: no program; SNAP only; Medicaid only; SNAP and Medicaid; and SNAP, Medicaid, and TANF.

10. This is a "strong" parallel trends assumption (Callaway et al. 2024). This assumes away that treatment effects may vary across zip codes with different distances due to unobserved factors correlated with distance.

assumption. In particular, if there are no differential trends in program enrollment between more or less distant zip codes prior to CommonHelp, we expect $\beta_k = 0$ for $k < 0$.

When reporting impacts in tables, we also consider the following simplified pre-post analog of Equation (2):

$$\log(Y_{zt}) = \beta \cdot (D_z \times Post_t) + \beta^{15} \cdot (D_z \times Post15_t) + \delta_z + \lambda_t + \boldsymbol{X}_{zt}\boldsymbol{\gamma} + \varepsilon_{zt}, \tag{3}$$

where $Post_t \equiv \mathbb{1}\{t \geq \text{October 2012}\}$ and $Post15_t \equiv \mathbb{1}\{t \geq \text{January 2015}\}$ indicate the post-launch and post-upgrade periods. The coefficients β and β^{15} map to the parameters labeled in the top panels of Figure 5 and summarize the overall effects of CommonHelp's launch and upgrade on recipient growth from reducing variable burdens. To gain precision, we also include a vector of time-varying zip-level characteristics $\boldsymbol{X}_{zt}$ including log population size, poverty rate, unemployment rate, labor force participation rates, and population shares of Blacks and women; nevertheless, all results hold without controls.

While we prefer employing a continuous "treatment" variable to exploit all variation in distance across zip codes, we can alternatively employ a binarized DD design where "treated" and "control" zip codes fall above or below a certain distance threshold. As robustness checks, we estimate the effects of CommonHelp on zips with a distance of at least 5, 7, and 9 miles away. These threshold choices are partly motivated by the kinks in participation rates observed in Figure 4 as well as meaningful cuts of the distribution of zip-to-office distances; 5 and 9 miles roughly correspond to the 25th and 50th percentiles.

Note that we employ logarithmized recipient counts (setting a lower bound of zero). This specification is appropriate since recipient counts of zip codes with differing population sizes do not increase by the same absolute level over time. By employing a log-linear specification, we require that zip codes grow at the same proportional rate. Thus, DD estimator expresses CommonHelp's effects as percentage changes. Nevertheless, given potential concerns with this approach (Chen and Roth 2024), we will alternatively estimate a Poisson specification as a robustness check. We also restrict our sample to zip codes with at least five recipients on average prior to CommonHelp. This prevents zip codes with very few or no initial recipients that experience a small increase in recipient counts after CommonHelp to disproportionately inflate our effect estimates. For this same reason, all estimates weigh zip codes by their mean recipient counts prior to CommonHelp. Standard errors are clustered at the county level to account for serial correlation within counties over time.

5.3 Identifying Effects of Reducing Fixed Burdens

We pursue a regression discontinuity (RD) strategy to analyze how zip-level program enrollment changed around the dates of CommonHelp's launch and 2015 upgrade among zip codes within very close proximity to their field office. Specifically, if our running variable t is normalized to zero at CommonHelp's launch or 2015 upgrade and we restrict our analysis to zip codes with distance below some threshold $D_z < \bar{d}$, our RD specification is

$$\log(Y_{zt}) = \beta^{fb} \cdot \mathbb{1}\{t \geq 0\} + f_0(t) \cdot \mathbb{1}\{t < 0\} + f_1(t) \cdot \mathbb{1}\{t \geq 0\} + \epsilon_{zt}, \tag{4}$$

where $f_0(t)$, $f_1(t)$ are smooth functions of log program recipients on either side of the date cutoff. Here, β^{fb} is the impact of CommonHelp's launch on program enrollment growth due to its initial reduction of fixed burdens; we denote $\beta^{fb,15}$ as the analogous parameter around the upgrade. These map precisely to the $\beta^{fb}, \beta^{fb,15}$ parameters labeled in the bottom panels of Figure 5. As discussed earlier, β^{fb} and $\beta^{fb,15}$ capture CommonHelp's *immediate* impact from reducing fixed burdens. If CommonHelp causes further declines in fixed burdens over time (as in Figure 5, Panel (d)), then these provide a lower bound on the policy's causal effects via fixed burdens.

Our identifying assumption is that, absent CommonHelp and its upgrade, program caseloads would have smoothly evolved around these dates. Usual challenges to identification include cyclicality of program enrollment within calendar years and confounding changes in composition of the population or economic environment that generate discontinuous changes in the counterfactual program caseloads. To probe these threats, we conduct falsification exercises by testing for analogous discontinuities in previous calendar years and assessing covariate balance around the cutoffs.

All of our main RD analyses employ a distance threshold of $\bar{d} = 0.5$ miles, which is just 5.03% of the mean zip code distance to field office. This limits our analysis sample to just 28 zip codes in 25 counties. Restricting our sample to such a short distance respects our goal of identifying CommonHelp's impacts from reducing fixed burdens only; larger distance thresholds poses a risk of also picking up impacts driven by variable burdens. We specify $f_0(\cdot)$ and $f_1(\cdot)$ as quadratic functions of t to respect seasonality of program receipt within calendar years. We limit our attention to a 12-month window around CommonHelp's launch and upgrade and employ a triangular kernel to weigh observations closer to each respective data (in addition to zip codes' mean enrollment levels prior to CommonHelp). We also conduct a battery of robustness checks with alternative modeling choices to ensure our results are not driven by our preferred choice of specification. As usual, standard errors are clustered at the county level.

6 Impacts on Safety Net Program Participation

6.1 Effect Estimates from Reduced Variable Burdens

Visual Evidence. We begin by presenting visual evidence that CommonHelp impacted participation in social safety net programs in zip codes that were relatively farther from their assigned LDSS office. The left panels of Figure 7 illustrate raw trends in the log number of program recipients between zip codes that were more versus less than 7 miles away from their office.[11] Panel (a) presents trends in the overall number of program recipients; Panels (c) and (e) focus on multiple and single program recipients, respectively. Log counts are normalized to zero on September 2012 (the month prior to CommonHelp) to help facilitate comparisons. In the years leading up to CommonHelp, farther and closer zip codes possess strikingly concordant trends—even in months with sharp fluctuations (e.g., May 2010)—in all three panels. These trends diverge, however, almost immediately after CommonHelp's introduction. This break signals that differences in trends after October 2012 can likely be attributed to CommonHelp and not another time-varying shock. Differences between farther and closer zips continue to widen, particularly after 2015. This growth is especially striking for single program recipients.

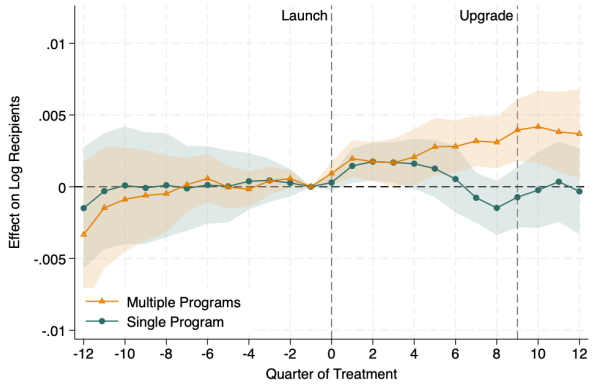
Event-study Estimates. The right panels of Figure 7 report quarterly event-study estimates from the continuous DD specification which fully exploits variation in zip-to-office distances. Panel (b) shows that the immediate average impact of CommonHelp was 0.1% per mile and grew to 0.2% per mile after eight quarters. Following the 2015 upgrade of the platform, effect estimates spike upward to 0.4% per mile. This corresponds to an average increase of about 3.8 recipients per zip per mile. Though these effect estimates are expressed in small magnitudes, they represent meaningful increases in the number of recipients. In aggregate, this amounts to a total of about 31,800 recipients ($0.4\%/\text{zip/mile} \times \sum_z D_z Y_{z,-1}$). In population terms, we estimate that CommonHelp increased the share of the population receiving SNAP, Medicaid, or TANF by 2.6%. Reassuringly, there are no pre-trends and several of the estimates leading up to CommonHelp are precise nulls, giving greater confidence in the parallel trends assumption.

Panels (d) and (f) focus on CommonHelp's dynamic impacts on multiple and single program receipt; Appendix Figure A.6 plots estimates for individual programs and different combinations of program bundles. Caseloads grew in all programs, with increased enrollment in SNAP by 0.25% (1.4 recipients) per mile, Medicaid by 0.2% (1.6 recipients) per mile, and TANF by 0.4% (0.2 recipients) per mile. We find that a significant share of program growth

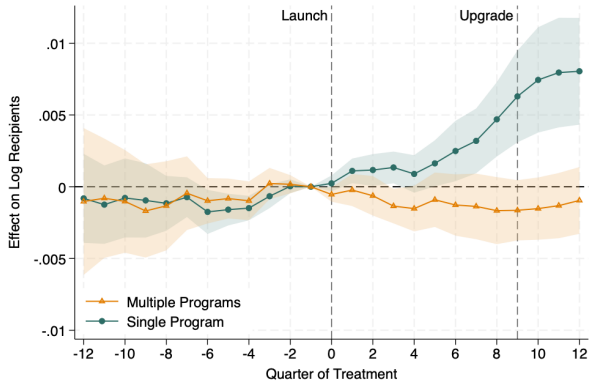
11. Appendix Figure A.5 illustrate raw trends using 5 and 9 miles as distance thresholds.

Figure 6: Event Study Estimates for Households with and without Children

(a) Households with Children



(b) Households without Children



Notes: This figure illustrates the association between zip-level single and multiple program participation rates and zip code distance to their assigned county field office using monthly data between January 2007 and December 2022. Participation rates are expressed in per capita terms using population estimates of zip code tabulation areas (ZCTAs) from the 5-year American Community Survey (ACS), scaled to the zip-county level using the HUD-USPS crosswalk. Binscatters are weighted by population and control for ZCTA-level poverty rate, zip-level population share of Blacks and Hispanics, and county fixed effects. ZCTA covariates are drawn from 5-year ACS estimates.

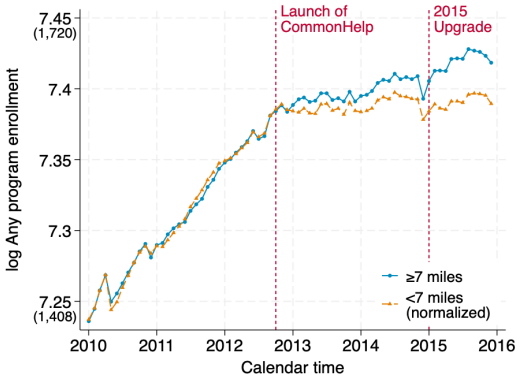
is driven by *multiple* program participation. Single program participation increased by 0.21% (1.06 recipients) per mile; multiple program participation increased by 0.25% (1.02 recipients) per mile. In aggregate, CommonHelp caused nearly 9,000 more multiple program recipients. These results provide compelling evidence that CommonHelp reduced administrative burdens for simultaneously accessing multiple programs.

Interestingly, the event-study estimates reveal that CommonHelp's 2015 upgrade generated a particularly strong uptick in single program participation. Given that multiple program recipients are deemed to be "more deserving" than single program recipients based strictly on program eligibility rules set by state and federal law, this suggests that online submission of documentation reduced targeting efficiency. Section 7 rigorously examines CommonHelp's targeting impacts for single and multiple program receipt from alternative normative perspectives.

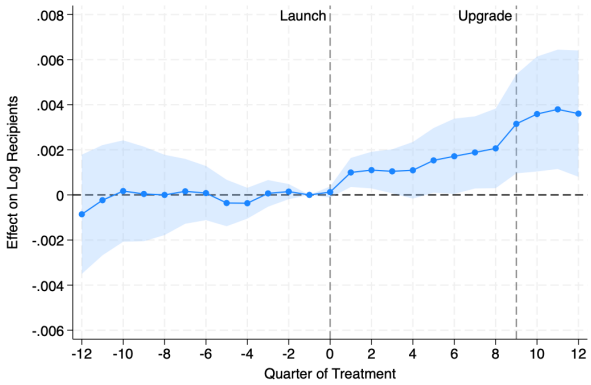
Figure 6 reveals striking heterogeneity in CommonHelp's impact when comparing households with and dwithout children. Among households with children in Panel (a), the increase in multiple program participation materialized immediately after CommonHelp's launch and remained stable. In contrast, single program participation for this group initially increased as well but subsequently decreased to pre-treatment levels, suggesting potential substitution from single to multiple programs down the line. For households without children in Panel (b), the pattern is markedly different—single program participation increased dramatically,

Figure 7: Raw trends and event-study estimates before and after CommonHelp's launch

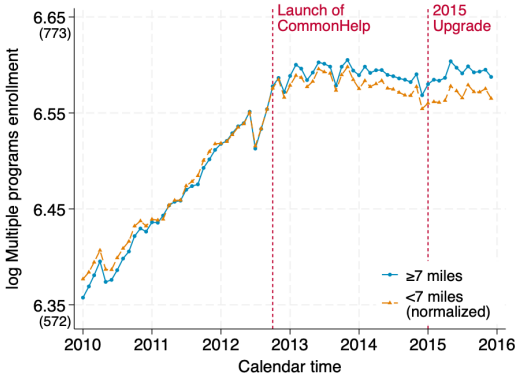
(a) Any program: Raw trends



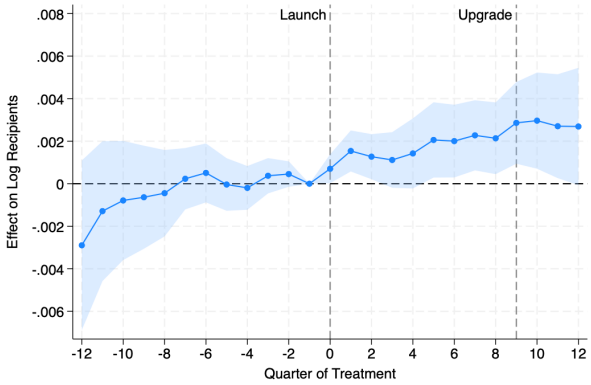
(b) Any program: Event study



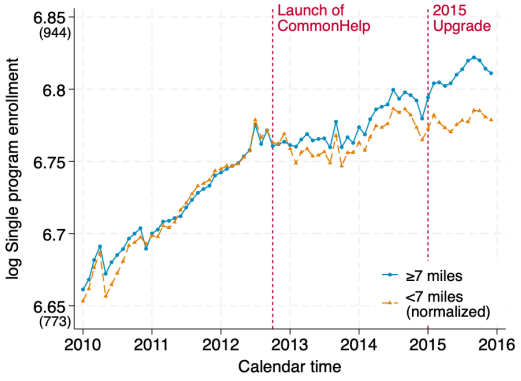
(c) Multiple programs: Raw trends



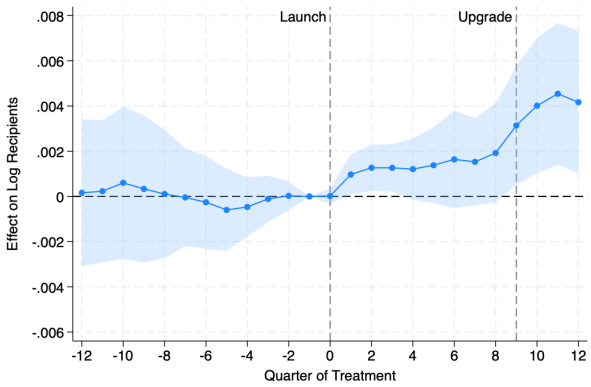
(d) Multiple programs: Event study



(e) Single program: Raw trends



(f) Single program: Event study



Notes: Left panels plot raw monthly trends in log program enrollment for the average zip code based on their distance to their assign county field office. The level of zip codes less than 7 miles away are normalized to the level of zip codes above 7 miles away in September 2012. Right panels illustrate estimates of $\{\beta_k\}$ from Equation (2). Shaded regions are 95% confidence intervals based on robust standard errors clustered at the county level. In all panels, zip codes are weighted by their average program recipient count between October 2009–September 2012.

Table 2: CommonHelp's effects of reducing variable burdens on program receipt

	(1)	(2)	(3)
	All	With children	No children
A. Any program			
Overall effect (%/10 miles)	2.254**	1.785*	2.933***
	(0.889)	(0.929)	(0.939)
Launch effect (%/10 miles)	1.300	1.091	1.688**
	(0.802)	(0.899)	(0.818)
Upgrade effect (%/10 miles)	3.104***	2.260**	4.049***
	(1.023)	(1.023)	(0.916)
Outcome mean (count/zip)	837.2	545.6	236.8
B. Multiple programs			
Overall effect (%/10 miles)	2.238**	3.066**	−0.290
	(1.116)	(1.338)	(0.957)
Launch effect (%/10 miles)	1.602	2.231*	−0.322
	(1.050)	(1.263)	(0.886)
Upgrade effect (%/10 miles)	2.071**	2.718***	0.103
	(0.927)	(1.039)	(0.922)
Outcome mean (count/zip)	384.7	286.5	70.0
C. Single program			
Overall effect (%/10 miles)	2.359***	0.213	4.406***
	(0.884)	(0.943)	(1.075)
Launch effect (%/10 miles)	1.274*	0.137	2.609***
	(0.718)	(0.817)	(0.919)
Upgrade effect (%/10 miles)	3.531***	0.245	5.846***
	(1.158)	(1.285)	(1.185)
Outcome mean (count/zip)	442.3	249.6	166.6
Number of zip codes	1,430	1,430	1,430
Number of counties	122	122	122

Notes: This table reports estimates of β and β^{15} based on a pre-post analog of Equation (2), expressed in percentages. Outcome means are calculated the average zip-level number of recipients in the quarter prior to CommonHelp. Standard errors are clustered at the county level and reported in parentheses. Stars indicate statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

while multiple program participation remained largely flat. These divergent patterns align with program eligibility rules during this period, as households with children constituted the majority of those eligible for multiple programs, while households without children generally qualified only for individuals programs like SNAP.

Summary Estimates. Table 2 summarizes the difference-in-differences estimates by reporting "time-averaged" effects corresponding to β and β^{15} from Equation (3), as well as a

pooled effect averaged over all twelve quarters after CommonHelp's launch. Overall, CommonHelp increased enrollment in any program by 2.3% for a zip code 10 miles away from its field office. These effects stem from enrollment increases in both multiple (2.3% per 10 miles) and single programs (2.4% per 10 miles), with the effects of the 2015 upgrade tending to outweigh the impacts of the initial launch.

Among those in households with children, CommonHelp's effects were concentrated in multiple program participation, which increased by 3.1% per 10 miles for this group. In contrast, single program participation among these households remained essentially unchanged, increasing by a statistically insignificant 0.2% per 10 miles. For those in households without children, we see the reverse pattern - CommonHelp's effects manifested primarily through increased participation in single programs, rising by 4.4% per 10 miles after treatment. Enrollment in multiple programs decline by a statistically insignificant 0.3% per 10 miles for this group.

Robustness Checks. To probe whether our results capture the causal impacts of CommonHelp, we conduct a battery of robustness checks.

The first three robustness analyses examine threats to our parallel trends assumption that underpins our identification strategy. First, Appendix Figure A.7 replicates our main DD analysis using zip-level caseloads unemployment insurance (UI)—a program not part of CommonHelp—as an outcome and find null effects. The absence of effects on UI receipt indicates that our main results are not driven by diverging economic trends or business cycles between further versus closer zip codes. Second, we re-estimated Equation (2) controlling for time-varying zip code characteristics drawn from the American Community Survey, including population size, poverty rate, labor force participation, and unemployment rate, and find nearly identical effect estimates; see Appendix Figure A.8.[12] This is unsurprising given Appendix Figure A.10 show there are remarkably parallel trends in zip-level covariates between further and closer zip codes. Third, Appendix Figure A.11 showcases flat and statistically insignificant pre-trends when we extend our pre-treatment horizon to include the 2007–09 Great Recession. Collectively, these checks reassure that exposure to economic shocks are not spatially correlated with distance to field office and do not confound our identification of CommonHelp's causal effects.

We also find that our results are robust to alternative subsamples and measurements. Our results remain consistent when excluding the most distant zip codes, adjusting measurement of distance of zip codes containing field offices, and indicating that the closest or furthest zip

12. American Community Survey (ACS) estimates are available for Census-generated zip code tabulation areas (ZCTAs). We employ zip-to-ZCTA crosswalks to merge ACS data to our zip codes.

codes do not drive our treatment effect estimates. All results continue to hold when using binarized treatment variables based on distance thresholds, inverse hyperbolic sine functions, or Poisson specifications à la pseudo-maximum likelihood estimation. These alternative transformation and estimation methods guard against concerns of the interpretation "log–like" transformations (Chen and Roth 2024). Together, these checks reinforce the validity of the causal interpretation of the results.

6.2 Impacts from Reduced Fixed Burdens

Visual Evidence. The left panels of Figure 8 illustrate our RD design that identifies the effects of CommonHelp's launch and 2015 upgrade on multiple program participation due to fixed burdens. This figure limits our sample to the 28 zip codes that lie within just $\bar{d} = 0.5$ miles of their field office, which are likely only susceptible to fixed burdens. The black dots are raw mean monthly zip-level log multiple program recipient counts that visualize the underlying noise in the data. The red lines are triangular kernel-weighted quadratic fits separately estimated on each side of the cutoff dates. These illustrate the trends in the data and magnitude of discontinuities.

Multiple program appears jumps to the right of the cutoffs by roughly 1.8–4.1%. This suggests that CommonHelp's launch and upgrade immediately impacted multiple program receipt in close zip codes. To check if these discontinuities are an artifact of seasonality patterns, the right panels replicate the same analysis one year *prior* to the launch and upgrade dates.[13] In both cases, program receipt evolves relatively smoothly around the cutoffs. This provides reassurance that the trend breaks illustrated in the left panels represent the genuine impacts from the policy changes.

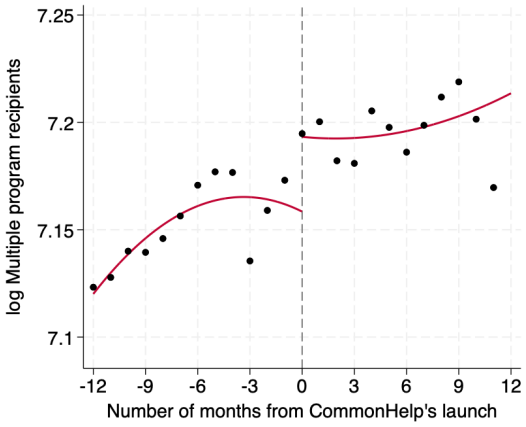
Appendix Figures A.12–A.15 provide additional graphical results for any and single program receipt as well as among households with and without children. For the sake of brevity, we discuss these outcomes in our formal regression results.

Regression Discontinuity Estimates. Table 3 reports RD estimates on the effects on any program, multiple programs, and single program caseload growth. Our results indicate that CommonHelp tended to promoted program receipt in general, but particularly in multiple programs. Focusing on Column (1), CommonHelp' increased overall program receipt in the population by 0.58%, a marginally significant effect; this impact is driven by a large and statistically-significant 3.48% gain in multiple program receipt. Meanwhile, CommonHelp's

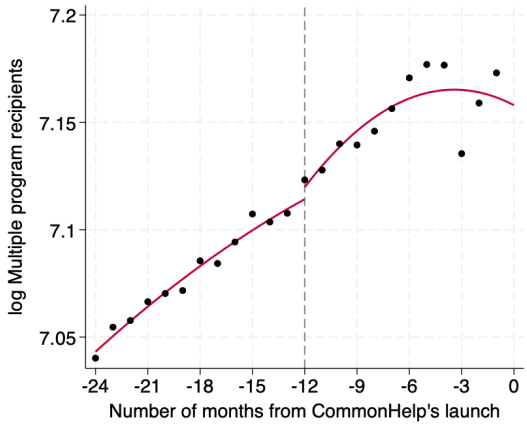
13. January 2014 is one year before CommonHelp's upgrade but falls in the post-launch period. Appendix Figure A.16 provides analogous plots around January 2012—the latest comparable date prior to CommonHelp's launch. Reassuringly, we find negligible trend breaks just like in January 2014.

Figure 8: Visual discontinuity evidence of CommonHelp's impacts on multiple program receipt from reducing fixed burdens

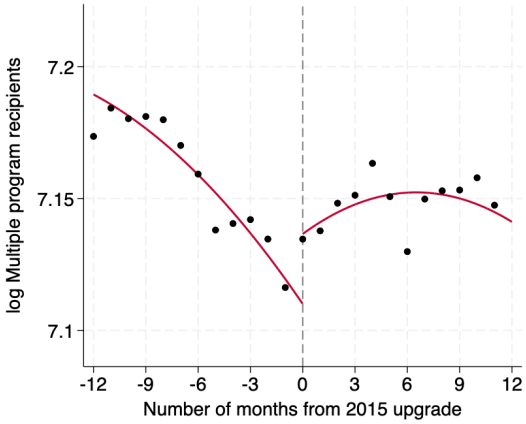
(a) Receipt around CommonHelp's launch



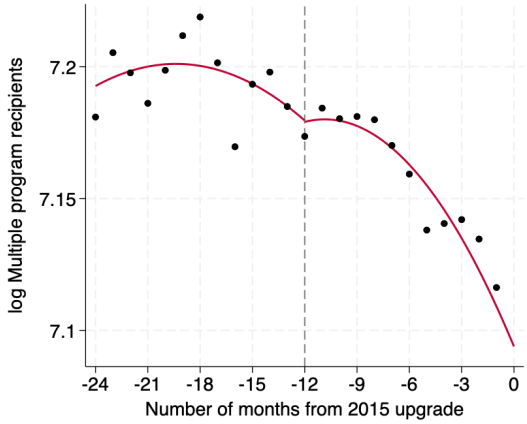
(b) Falsification: One year before launch



(c) Receipt around 2015 upgrade



(d) Falsification: One year before upgrade



Notes: These figures plot raw monthly enrollment (black dots) and best quadratic fits (red lines) around different thresholds among zip codes within $\bar{d} = 0.5$ mile of their field office. The left panels focus on the launch and upgrade of CommonHelp; right panels examine trends one year prior to these events. All quadratic fits employ triangular kernels.

2015 upgrade did not significantly change overall program receipt, but continued to significantly amplify multiple program receipt. In contrast, Panel C reveals that CommonHelp's reductions in fixed burdens caused a *decline* in *single* program receipt after both the launch and upgrade. This provides suggestive evidence that CommonHelp induced intensive-margin responses in safety net participation: individuals who would have received a single program, absent CommonHelp, instead opted into *multiple* programs simultaneously.

Column (3) reports that CommonHelp's impacts from reducing fixed burdens were concentrated among households with children. CommonHelp's launch and upgrade generated 1.8–4.1% increase in multiple program receipt growth; correspondingly, single program

Table 3: CommonHelp's effects on program receipt from reducing fixed burdens

	(1)	(2)	(3)	(4)	(5)	(6)
	All		With children		No children	
	Main	Falsif.	Main	Falsif.	Main	Falsif.
A. Any program						
Launch estimate (%)	0.581*	0.071	1.320**	0.568	−0.834	−0.783
	(0.334)	(0.274)	(0.600)	(0.631)	(1.012)	(0.612)
Upgrade estimate (%)	−0.114	−1.054	−1.529	−2.011	3.107***	−0.871
	(0.818)	(0.817)	(0.945)	(1.802)	(0.977)	(0.970)
Outcome mean (count/zip)	1,162.4	1,103.0	789.2	757.4	373.2	345.5
B. Multiple programs						
Launch estimate (%)	3.484***	0.560	4.067***	1.082*	0.628	−0.044
	(0.803)	(0.398)	(0.867)	(0.607)	(0.813)	(1.086)
Upgrade estimate (%)	2.656**	−0.052	1.849	−0.454	4.437**	−0.563
	(1.142)	(1.291)	(1.166)	(1.840)	(1.749)	(1.342)
Outcome mean (count/zip)	675.6	651.2	395.1	387.2	280.5	264.0
C. Single program						
Launch estimate (%)	−1.538**	−0.283	−1.894*	1.039	−1.596	−0.900
	(0.720)	(0.430)	(1.001)	(1.877)	(1.519)	(1.088)
Upgrade estimate (%)	−2.028**	−1.607**	−4.188***	−1.943*	2.791**	−1.176
	(0.982)	(0.671)	(1.237)	(1.030)	(1.241)	(0.973)
Outcome mean (count/zip)	475.9	440.4	383.3	359.0	92.6	81.4
Number of zip codes	28	28	28	28	28	28
Number of counties	25	25	25	25	25	25

Notes: Odd-numbered columns report estimates of β^{fb} and $\beta^{fb,15}$ from Equation (4). Even-numbered columns report analogous estimates using the same calendar month cutoff dates in the year prior to CommonHelp as falsification tests. All regressions employ quadratic polynomials with triangular kernels within a 12-month window of each cutoff date, restrict sample to zip codes within $\bar{d} = 0.5$ miles of field office, and weight zip-level observations by their outcome means measured prior to CommonHelp. Outcome means are zip-level counts in the year prior to the cutoff date. Standard errors are clustered at the county level and reported in parentheses. Stars indicate statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

caseloads among these households significantly declined. This suggests that fixed burdens from office congestion and social stigma may pose as significant barriers to low-income households with children, who are typically eligible for multiple programs. Interestingly, Column (5) finds the 2015 upgrade led to large, significant growth in multiple program receipt among households with no children—which may be driven by elderly or those with disabilities, who would particularly benefit from a fully remote application and supplemental document submission processes; nevertheless, note the baseline means of multiple program receipt for this group is significantly smaller than households with children.

Robustness Checks. The even-numbered columns of Table 3 report falsification tests that replicate the RD analysis in the prior calendar year. Effects on any program receipt (Panels A) are noisy and statistically insignificant in the year prior to the launch and upgrade for all population groups, providing reassurance that our aggregate effect estimates are not driven by cyclicality in program participation. There are a few statistically significant effect estimates for multiple and single program receipt growth (Panels B and C), but their magnitudes are significantly smaller than the main estimates. To provide greater reassurance, Appendix Table A.1 reports difference-in-discontinuity estimates that formally test for differences between the main and falsification estimates. Our primary qualitative findings continue to hold.

Appendix Table A.2 reports a battery of specification checks to validate the robustness of our main results. We obtain qualitatively similar effect magnitudes when we additionally weigh zip-level observations based on their proximity to field office and restrict our bandwidth to a 6-month window around the launch and upgrade dates. Our results also largely hold when employing a local linear specification for the full population and among households with children, but estimates are noisy and insignificant among households without children. As illustrated in Figure 8, program receipt is highly cyclical in nature, indicating that linear approximations of these time trends is not appropriate.

Appendix Figure A.17 verifies that our RD results are quite robust to choice of distance threshold $\bar{d}$ that defines our sample inclusion criteria. Estimates around CommonHelp's launch are highly stable; selecting distance thresholds closer to 2 miles tends to attenuate effect estimates of the upgrade. To ensure our results are unconfounded by CommonHelp's variable burden impacts, we prefer our 0.5 mile thresholds.[14]

Finally, it is worth emphasizing that while these RD results focus on a highly selected sample—zip codes within 0.5 miles of their field office—our variable-fixed burdens framework allows us to extend these results to *all* zip codes. This is because our DD analysis identifies the effects of CommonHelp from reducing variable burdens that exist above and beyond fixed burdens for further zip codes. As an additional check, Appendix Figure A.18 plots RD estimates for zip codes by various distance bins. This provides a nonparametric means of estimating the *total* immediate impacts of CommonHelp on multiple program receipt, capturing effects from reducing both fixed and variable burdens borne from distance. The RD estimates are roughly linearly in distance bins. In fact, the RD estimate-distance gradients are nearly identical to our $t = 0$ event-study estimate from our DD analysis. This further validates our identification strategies.

14. Shorter distance thresholds lead to unstable and highly imprecise estimates.

Table 4: Total effects of CommonHelp on the population

	(1)	(2)	(3)	(4)	(5)	(6)
	Any		Multiple		Single	
	Effect	(S.E.)/ Share	Effect	(S.E.)/ Share	Effect	(S.E.)/ Share
A. Regression effect estimates						
β: Variable, launch (%/10 miles)	1.300	(0.802)	1.602	(1.050)	1.274*	(0.718)
β^{15}: Variable, upgrade (%/10 miles)	3.104***	(1.023)	2.071**	(0.927)	3.531***	(1.158)
β^{fb}: Fixed, launch (%)	0.581*	(0.334)	3.484***	(0.803)	−1.538**	(0.720)
$\beta^{fb,15}$: Fixed, upgrade (%)	−0.114	(0.818)	2.656**	(1.142)	−2.028**	(0.982)
B. Effects on number and composition of recipients, by choice of total growth parameter						
$g_z = \beta \cdot D_z$	31,128	0.830	17,560	0.468	16,203	0.432
$g_z = \beta \cdot D_z + \beta^{fb}$	52,450	1.398	75,810	2.021	–13,742	−0.366
$g_z = (\beta + \beta^{15}) \cdot D_z$	121,520	2.756	40,411	0.916	78,926	1.790
$g_z = (\beta + \beta^{15}) \cdot D_z + (\beta^{fb} + \beta^{fb,15})$	**140,924**	3.196	**140,575**	3.188	**−10,378**	−0.235
Outcome mean (count/zip)	837.2		384.7		442.3	
Number of zip codes	1,430		1,430		1,430	
Number of counties	122		122		122	
Share of recipients (%)	100.0		47.0		52.0	

Notes: Panel A summarizes the main regression results on CommonHelp's effects on zip-level caseload growth from reducing variable and fixed burdens (from Tables 2 and 3). The outcome mean expresses the average zip-level number of recipients in the quarter prior to CommonHelp. Standard errors are clustered at the county level and reported in parentheses. Odd-numbered columns of Panel B report estimates of the effects of CommonHelp on the number of program recipients ΔY (defined by Equation (5) under different choices of the total growth parameter. Bold-faced estimates highlight the total effect estimates from CommonHelp's launch and upgrade via variable and fixed burdens. Even-numbered columns report percentage-point impacts on the shares of recipients among the full population of recipients in the post-launch period. Stars indicate statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. **(Note: Standard errors for Panel B's estimates are in progress.)**

6.3 Summarizing CommonHelp's Total Effects

Having estimated CommonHelp's effects on variable and fixed burdens, we now turn to assessing CommonHelp's total effects on the population. Table 4, Panel A summarizes our primary regression estimates from our DD and RD regression models. These parameters can be utilized to estimate a lower bound of CommonHelp's causal effect on the total zip-level growth rate in program receipt g_z, after the launch and 2015 upgrade, à la Equation (1). For the average zip code (mean distance $D_z = 9.95$ miles), we estimate that CommonHelp increased any program participation by at least 1.87% after its launch and by at least 4.84% after its upgrade.[15]

CommonHelp had particularly prominent positive effects on multiple program participa-

15. $\mathbb{E}[\hat{\beta} \cdot D_z + \hat{\beta}^{fb}] = 0.130\%/\text{mile} \times 9.95 \text{ miles} + 0.581\%$ and $\mathbb{E}[(\hat{\beta} + \hat{\beta}^{15}) \cdot D_z + (\hat{\beta}^{fb} + \hat{\beta}^{fb,15})] = (0.130 + 0.3104)\%/\text{mile} \times 9.95 \text{ miles} + (0.581\% - 0.114\%)$.

tion along both variable and fixed burdens. For the average zip code after the platform's upgrade, we estimate that multiple program receipt grew by at least 9.79%. In contrast, CommonHelp appeared to *promote* greater single program receipt via variable burdens but *discourage* receipt via fixed burdens. This suggests that reductions in office congestion and social stigma may have promoted substitution from single to multiple program receipt for residents living in close proximity to their field office. Relative to multiple program receipt, these competing forces result in a smaller lower bound on average total single program caseload growth of 1.21%. The heterogeneity in the total effects on multiple versus single program receipt align with the nature of the CommonHelp. Streamlining multiple applications and re-certification processes onto an centralized online platform successfully encouraged multiple program receipt.

How many individuals were impacted by CommonHelp? Appendix B.1 shows that we can convert estimates of total growth effect g_z, which represent *percent* changes at the *zip*-level, into the number of program recipients as a result of CommonHelp via

$$\Delta Y \equiv \sum_{z \in \mathcal{Z}} \left(1 - \frac{1}{1+g_z}\right) \cdot \bar{Y}_{z,post}, \tag{5}$$

where $\bar{Y}_{z,post}$ is zip z's mean number of recipients of p after CommonHelp. Table 4, Panel B reports estimates of ΔY for different choices of g_z in the odd-numbered columns. Overall, we calculate that when accounting for impacts on both variable *and* fixed burdens from its launch and upgrade, CommonHelp brought in over 140,000 recipients onto SNAP, Medicaid, or TANF between October 2012–December 2015. Remarkably, we find that essentially *all* of this growth is explained by multiple program recipients. Reduction in variable and fixed burdens induced by the platform's upgrade explains a significant share of this caseload growth. The even-numbered columns report the percentage-point impacts on the *composition* of program recipients after CommonHelp by normalizing ΔY by the total post-CommonHelp recipient counts: $\sum_{z \in \mathcal{Z}} \bar{Y}_{z,post}$. The additional 140,000 recipients corresponds to roughly a 3.2% growth in recipient caseloads, driven by a 3.2 percentage-point increase in the share of multiple program recipients (base share: 47.0%).

To highlight the importance of Section 5.1's conceptual framework, it is useful to examine the sensitivity of these effect estimates to ignoring either variable or fixed burdens. The first and third rows of Panel B, Column (1) find that excluding impacts via fixed burdens would underestimate the overall number of recipients by roughly 20,000 individuals, both after the launch and upgrade periods. Columns (3) and (5) reveal this generates severe upward and downward biases in estimated multiple and single program recipient counts, respectively; in fact, it results in a sign flip for single program recipients. By extension, excluding impacts via

variable burdens results in opposite biased lower bound estimates. The magnitude of these biases underscore the importance of accounting for CommonHelp's impacts on different types of administrative burdens through complementary research designs to accurately evaluate the policy's aggregate effects on the population.

7 Impacts on Targeting Efficiency

Did CommonHelp reach multiple program enrollees who come from relatively more or less disadvantaged backgrounds? Answering this question is central to understanding whether reducing administrative burdens to multiple programs through technology can improve targeting efficiency of the social safety net as a whole. In addition to the zip-level empirical specification in Equation (1), we will also rely on an individual-level regression specification to analyze how CommonHelp affected the *composition* of program recipients. The variation we exploit is the same, although our DD model is now run at the level of family i residing in zip $z(i)$. Specifically, we will estimate DD regressions using repeated monthly cross-sections of recipients in the 12-quarter window before and after CommonHelp's launch. For some fixed, predetermined characteristic C_{it} in cross-section sample t, we estimate

$$C_{it} = \gamma(D_{z(i)} \times PostCH_t) + \delta_{z(i)} + \lambda_t + \theta_1 Age_i + \theta_2 Age_i^2 + \epsilon_{it}, \tag{6}$$

where we control for a quadratic of i's age Age_i to account for how the measurement of C_{it} may be influenced by basic life-cycle dynamics.

Our coefficient of interest here is γ, which represents CommonHelp's marginal causal impact on the composition of recipients measured by characteristic C_{it} per mile distance. For example, to assess how many families with prior work history were brought into multiple programs, we would estimate Equation (6) using repeated cross-sections of recipients on two or more programs and γ would capture the percentage point change of families on multiple programs with prior work history. This enables us to assess whether CommonHelp reduced burdens for relatively more disadvantaged families to participate in multiple safety net programs.

Our analysis involves estimating Equation (6) using a variety of socioeconomic background characteristics measured prior to CommonHelp. These characteristics are constructed by linking the population of program recipients to earnings and crime records. One notable strength of our approach is that we leverage a long time horizon of data. Our earnings measures are calculated over a 3-year window leading up to CommonHelp's launch, while our crime outcomes are measured over 14 years prior to CommonHelp. This enables us to

Table 5: CommonHelp's targeting effects among households with children

	(1)	(2)	(3)
	Effect	(S.E.)	Mean
A. Employment history characteristics, 2009–2011			
Any employment (> \$0, p.p.)	1.083	(0.780)	0.651
Fraction of quarters employed	0.009**	(0.004)	0.401
Mean quarterly wage earnings	75.836***	(17.619)	1,602.7
S.D. quarterly wage earnings	27.632*	(15.929)	1,060.0
S.D. earnings among quarters with > \$0	3.193	(38.262)	1,478.0
Any crime committed (p.p.)	−0.229	(0.188)	0.083
B. Criminal history characteristics, 1998–2011			
Number of crimes committed	−0.003	(0.003)	0.105
Any violent crime committed (p.p.)	−0.114*	(0.059)	0.011
Any property crime committed (p.p.)	0.007	(0.115)	0.046
Any drug crime committed (p.p.)	−0.131	(0.123)	0.027
C. Employment & criminal history interactions			
Not employed, no crime committed (p.p.)	−0.892	(0.742)	0.320
Not employed, crime committed (p.p.)	−0.191	(0.130)	0.029
Employed, no crime committed (p.p.)	1.121	(0.731)	0.597
Employed, crime committed (p.p.)	−0.039	(0.152)	0.054
D. Household characteristics			
Single-parent household (p.p.)	0.364	(0.494)	0.563
Two-parent household (p.p.)	−0.452	(0.439)	0.388
Any children, ages 0–4 (p.p.)	0.070	(0.339)	0.439
Any children, ages 5–9 (p.p.)	−0.123	(0.360)	0.292
Any children, ages 10–17 (p.p.)	0.063	(0.237)	0.267

Notes: This table reports estimates of γ from Equation (6) for various characteristics. Means are measured in the quarter prior to CommonHelp. Standard errors are clustered at the county level and reported in parentheses. Stars indicate statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

more precisely proxy different dimensions of well-being or potential self-sufficiency or ability (Nichols and Zeckhauser 1982). Since our heterogeneity analysis found that CommonHelp primarily affected multiple program participation among households with children, we restrict our targeting analysis to this subpopulation.

Table 5, Panel A reports targeting estimates based on employment histories.[16] Across the board, CommonHelp increased the share of welfare recipients with more robust labor market histories. For example, we find that the percent of recipients who worked anytime in the year prior to CommonHelp increased by about 1.1 percentage points per mile (from a base mean of 0.65). Interestingly, we find that those induced into multiple program receipt tend to have relatively more extensive labor market histories than those induced into single

16. We restrict any longitudinal measures to lie between ages 18–64 only.

program receipt. Quarterly wages among recipients measured in the three years leading up to CommonHelp significantly increased by about $76 per mile (from a base of $1,603 per quarter). We also find that CommonHelp tended to target individuals that have experienced greater earnings volatility, although this seems driven by extensive rather than intensive margin volatility.

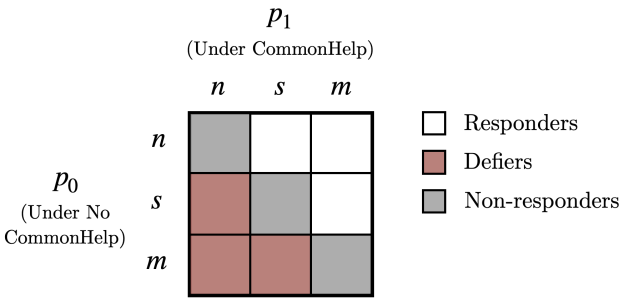
Panel B reveals that CommonHelp reduced the share of multiple program recipients with criminal histories, particularly for violent crimes (decreasing by 0.114 percentage points per mile on a base mean of 0.011). This suggests that the reform was particularly effective at reaching families without criminal justice system involvement. Panel C considers a novel dimension of economic opportunities: interactions between labor market participation and crime. Here, we find that CommonHelp *decreased* the probability that multiple program recipients had criminal offenses but no labor market history, although this is marginally insignificant. The results in Panel D indicate statistically insignificant changes in household composition, measured based on either the number of parents or the age of children. Together, these results suggest that CommonHelp's targeting effects operated primarily through increased labor market and decreased criminal histories rather than through changes in family structure.

8 Understanding the Types of Recipient Responders

The previous analyses examined how CommonHelp affected the number of multiple program recipients and their composition based on employment, criminal history, and household characteristics. Nonetheless, it is silent on how many of them would have participated in no programs (i.e., lie on the *extensive* margin of any program participation) versus a single program (i.e., lie on the *intensive* margin) in a counterfactual world without CommonHelp. To do this, we pursue a partial identification approach to bound the shares of different "responder types" impacted by CommonHelp (Borusyak 2015; Kline and Tartari 2016). Answering this question is highly policy relevant. It sheds light on whether CommonHelp reached those who would have had no attachment to the social safety net or those who would have been partially attached. This provides insights into the targeted populations through which technological reforms like CommonHelp may close the incomplete take-up of multiple programs described in Fact 2.

This analysis provides a novel dimension to targeting analysis. While Section 7—and much of the safety net literature—focuses on targeting efficiency based on *observed* and pre-determined characteristics (e.g., prior earnings), this section aims to delineate the composition of multiple program recipients based on *unobserved* and *contemporaneously counter-*

Figure 9: Permissible responder types under the monotonicity assumption



Notes: See main text for details.

factual behavior. This is particularly relevant for interventions that directly affect take-up of multiple programs (e.g., CommonHelp) or primarily targets a given program but indirectly generate cross-program spillover effects, implying the existence of distinct responder types. In this respect, this analysis respects the coexistence of multiple programs that comprise the modern social safety net.

To help facilitate discussion, we begin by introducing a potential outcomes framework for multiple program participation. Let P_{it} denote individual i's type of program receipt. Let the set of possible outcomes be $\mathcal{P} = \{n, s, m\}$, where n denotes no program receipt, s denotes single program receipt, and m denotes multiple program receipt. For each possible type of program receipt $p \in \mathcal{P}$, define $P_{p,it} \equiv \mathbb{1}\{P_{it} = p\}$. Furthermore, let $P_{it}(k)$ be individual i's potential receipt measured in period t in the counterfactual state $k \in \{0, 1\}$ that indicates the presence of CommonHelp; define $P_{p,it}(k)$, $k \in \{0, 1\}$ analogously. Thus, realized outcomes are $P_{p,it} = P_{p,it}(0) \times (1 - Post_t) + P_{p,it}(1) \times Post_t$.

To make progress, we invoke the following monotonicity assumption: $P_{it}(0) \subseteq P_{it}(1)$, for each individual i and time t. Figure 9 visualizes the content of this assumption. The white and gray squares indicate the permissible latent types of responders. Specifically, mono-

tonicity restricts behavioral responses to CommonHelp to be one of the following groups:[17]

$$(P_{it}(0), P_{it}(1)) = \begin{cases} (n,n) & \text{“no program non-responders”} \\ (n,s) & \text{“single program responders”} \\ (n,m) & \text{“extensive-margin multiple program responders”} \\ (s,s) & \text{“single program non-responders”} \\ (s,m) & \text{“intensive-margin multiple program responders”} \\ (m,m) & \text{“multiple program non-responders”} \end{cases}$$

Thus, our monotonicity assumption rules out the presence of $(P_{it}(0), P_{it}(1)) = (p_0, p_1)$ s.t. $p_1 \subset p_0$, or “defiers.” In other words, monotonicity assumes that CommonHelp does not cause any individual to reduce the number of programs they receive. This is a strong assumption. Monotonicity could be violated if CommonHelp causes some individuals to substitute away from seeking in-person assistance in field offices that would have lead to receipt of more programs. For example, it is possible that some caseworkers offer high-quality, personalized assistance or employ categorical eligibility rules (i.e., screen for TANF eligibility first to determine SNAP and Medicaid eligibility) that increase the probability of multiple program receipt than through using CommonHelp alone. Nevertheless, it is reassuring that none of our multiple program receipt estimates from the DD and RD designs are statistically significantly negative, indicating that if such violations exist, they do not play a large role.

The set of latent subpopulation groups is $\mathcal{G} = \{(n,n), (n,s), (n,m), (s,s), (s,m), (m,m)\}$. Define the share of latent group (p_0, p_1) as

$$\omega_{(p_0,p_1)} \equiv \Pr(P_{it}(0) = p_0, P_{it}(1) = p_1).$$

Note that $\omega_{(p_0,p_1)} \geq 0$, $\sum_{(p_0,p_1)\in\mathcal{G}} \omega_{(p_0,p_1)} = 1$. Our aim is to (partially) identify the shares of the three types of latent groups that responded to CommonHelp: $\omega_{(n,s)}$, $\omega_{(n,m)}$, and $\omega_{(s,m)}$.

For simplification, we define our baseline population as *recipients* in the post-CommonHelp period. This expresses $\{\omega_{(p_0,p_1)}\}$ as shares of the *recipient population* and eliminates no program non-responders from consideration ($\omega_{(n,n)} = 0$). This allows us to directly map the share estimates reported in the even-numbered columns of Table 4, Panel B to this analysis. Extending Equation (1), denote the percentage-point effect on receipt of program $p \in \mathcal{P}$

17. These behavioral response groups are analogous to the “never takers,” “compliers,” and “always takers” commonly used in the literature employing instrumental variables (IV) (Angrist et al. 1996). We do not use this taxonomy to emphasize we are not employing an IV framework, but rather a simple “treatment” (CommonHelp) and multinomial “outcome” (program receipt types) framework.

after CommonHelp as

$$\tilde{\beta}_p \equiv \frac{\Delta Y_p}{\sum_{z \in \mathcal{Z}} \bar{Y}_{z,post}}. \tag{7}$$

To make progress toward identification, we must map our already-identified parameters $\tilde{\beta}_p$ to these underlying shares of responders. Appendix B.2 shows that CommonHelp's percentage-point effect on program receipt may be expressed as

$$\tilde{\beta}_p = \mathbb{E}[P_{p,it}(1) - P_{p,it}(0) \mid Post_t = 1] = \sum_{p_0 \in \mathcal{P} \text{ s.t. } p_0 \subset p} \omega_{(p_0,p)} - \sum_{p_1 \in \mathcal{P} \text{ s.t. } p \subset p_1} \omega_{(p,p_1)}.$$

In particular, $\tilde{\beta}_s = \omega_{(n,s)} - \omega_{(s,m)}$ and $\tilde{\beta}_m = \omega_{(n,m)} + \omega_{(s,m)}$. Notice that $\tilde{\beta}_s$ may be negative if there are more intensive-margin multiple program responders than single program extensive-margin responders.

Note that these two parameters are linear combinations of the three distinct responder-type shares $\omega_{(n,s)}$, $\omega_{(n,m)}$, $\omega_{(s,m)}$. They are thus insufficient to secure point identification; at best, we can develop bounds on each responder share.[18] Fortunately, we can potentially tighten these bounds by utilizing the adding-up constraint $\sum_{(p_0,p_1) \in \mathcal{G}} \omega_{(p_0,p_1)} = 1$ and the share of single program non-responders $\omega_{(s,s)}$, which is equivalent to the share of recipients of s under no CommonHelp counterfactual less the share of intensive-margin multiple program responders $\omega_{(s,m)}$:

$$\omega_{(s,s)} = \frac{\sum_{z \in \mathcal{Z}} \bar{Y}_{s,z,post}(0)}{\sum_{z \in \mathcal{Z}} \bar{Y}_{z,post}} - \omega_{(s,m)} = \underbrace{\frac{\sum_{z \in \mathcal{Z}} \frac{1}{1+g_{z,s}} \cdot \bar{Y}_{s,z,post}}{\sum_{z \in \mathcal{Z}} \bar{Y}_{z,post}}}_{\equiv\, \theta_{(s,s)}} - \omega_{(s,m)}.$$

Given $\omega_{(p_0,p_1)} \geq 0$ and the aforementioned linear relationships between $\{\omega_{(p_0,p_1)}\}$, we derive the following bounds on each responder-type share:

$$\begin{aligned} \max\{0,\ \tilde{\beta}_s\} \ \leq\ \omega_{(n,s)} \ &\leq\ \min\{1 + \tilde{\beta}_s,\ \tilde{\beta}_s + \tilde{\beta}_m\} \\ 0 \ \leq\ \omega_{(n,m)} \ &\leq\ \min\{\tilde{\beta}_m,\ \tilde{\beta}_s + \tilde{\beta}_m\} \\ \max\{0,\ -\tilde{\beta}_s\} \ \leq\ \omega_{(s,m)} \ &\leq\ \min\{1 - \tilde{\beta}_s,\ \tilde{\beta}_m,\ \theta_{(s,s)}\} \end{aligned} \tag{8}$$

Note that depending on the sign of $\tilde{\beta}_s$, either $\omega_{(n,s)}$ or $\omega_{(s,m)}$ will achieve an informative (strictly positive) lower bound.

18. Note that point identification is not possible even if we considered a broader population that included non-recipients. In particular, the percentage-point effect on non-recipients would be $\tilde{\beta}_n = -\omega_{(n,s)} - \omega_{(s,m)}$, which is linearly dependent to the other parameters ($\tilde{\beta}_n = -\tilde{\beta}_s - \tilde{\beta}_m$). Similarly, the percentage-point effect on any recipients, which captures all extensive-margin responders, provides no additional content for identification: $\tilde{\beta}_{\text{any}} = \omega_{(n,s)} + \omega_{(n,m)} = \tilde{\beta}_s + \tilde{\beta}_m$.

Table 6: Estimated ranges of responder-type shares (p.p.)

	(1)	(2)	(3)
	$\omega_{(n,s)}$	$\omega_{(n,m)}$	$\omega_{(s,m)}$
Launch effects only	[0, 1.655]	[0, 1.655]	[0.366, 2.021]
Assuming $\omega_{(n,m)} = 0$	1.655	0	2.021
Assuming $\omega_{(n,s)} = 0$	0	1.655	0.366
Launch + upgrade effects	[0, 2.953]	[0, 2.953]	[0.235, 3.188]
Assuming $\omega_{(n,m)} = 0$	2.953	0	3.188
Assuming $\omega_{(n,s)} = 0$	0	2.953	0.235

Notes: This table reports the bounds defined in Equation (8) based on the estimates reported in the even-numbered columns of Table 4, Panel B. Each row also reports values of each share under different assumptions.

Table 6 utilizes the percentage-point estimates on recipient shares from Table 4, Panel B to estimate bounds on the underlying responder-type shares $\omega_{(n,s)}$, $\omega_{(n,m)}$, and $\omega_{(s,m)}$. Since we find an overall negative effect estimate on single program recipient shares, we achieve an informative lower bound on intensive-margin multiple program responders. In other words, we can definitively conclude that CommonHelp induced individuals to participate in *additional* safety net programs than they would have otherwise received. The share of these responders ranges between 0.366–2.021 percentage points from the launch and between 0.235–3.188 when including effects from the 2015 upgrade. This implies that intensive-margin multiple program recipients represent between 18–55% (launch only) and 7–52% (including upgrade) of all recipients who responded to CommonHelp.[19] This reveals that technological platforms like CommonHelp can help close the incomplete take-up rates among existing safety net recipients illustrated in Figure 2.

9 Conclusion

The coexistence of multiple programs is a fundamental feature of the modern U.S. social safety net. This paper examines the incidence of multiple program participation and the impacts of reducing administrative burdens on participation using administrative data from Virginia. Our analysis contributes to an active literature that has largely focused on the take-up and targeting of individual safety net programs.

Our descriptive analyses showcase that receipt of multiple programs is popular and often

19. These ranges come from dividing the estimates from Table 6, Column (3) under different assumptions of $\omega_{(n,m)}, \omega_{(n,s)}$ by their row sums; e.g., the lower bound range of intensive-margin multiple program responders from the launch is $0.366 \div (0.366 + 1.655) \times 100\% \approx 18\%$.

occurs jointly: approximately half of all recipients participate in multiple programs, with 80% of multiple program recipients residing in households with children. Moreover, transitions into and out of programs frequently occur in bundles. Yet, persistent gaps remain, with anywhere from 10 to 60% of existing recipients not enrolling in additional programs for which they qualify. Lower rates of multiple program receipt are particularly prevalent in zip codes more geographically distant from field offices.

We introduce a novel framework that decomposes administrative burdens into "variable burden" that increase with distance from field offices and "fixed burdens" that are unrelated to geographic distance. Through complementary research designs, we find that the CommonHelp technological reform successfully reduced both types of burdens. For every ten miles a zip code was located farther from a field office, the reform increased overall program participation by 2%. Among zip codes very close to their field office, CommonHelp's reduction in fixed burdens led to an immediate 3.5% increase in program receipt. Crucially, a substantial portion of these gains came from increased multiple program participation, which were almost entirely concentrated among households with children. Coupling these estimated impacts of reducing variable and fixed burdens, we find that CommonHelp generated more than 140,000 multiple program recipients over a three year period. We also find that CommonHelp brought in multiple program participants with more robust labor market histories and fewer criminal offenses, along with compliers who would otherwise have been partially attached to the safety net.

These findings carry important policy implications. They suggest that technological platforms can successfully reduce both distance-related and distance-invariant barriers for accessing safety net programs. Our framework for separating these burden types provides a novel lens for evaluating reforms that affect administrative burdens. By considering the coexistence of multiple programs and the multifaceted nature of burdens, this study contributes to a more complete understanding of how barriers to access affect low-income households and the effectiveness of the social safety net as a whole.

References

Angrist, Joshua D, Guido W Imbens, and Donald B Rubin. 1996. "Identification of causal effects using instrumental variables." *Journal of the American statistical Association* 91 (434): 444–455.

Arbogast, Iris, Anna Chorniy, and Janet Currie. 2024. "Administrative Burdens and Child Medicaid and CHIP Enrollments." *American Journal of Health Economics* 10 (2): 237–271.

Baicker, Katherine, Amy Finkelstein, Jae Song, and Sarah Taubman. 2014. "The Impact of Medicaid on Labor Market Activity and Program Participation: Evidence from the Oregon Health Insurance Experiment." *American Economic Review* 104 (5): 322–328.

Bartfeld, Judith. 2013. *SNAP and the School Meal Programs.* Technical report DP 2013-08. University of Kentucky Center for Poverty Research Discussion Paper Series.

Borusyak, Kirill. 2015. *Bounding the Population Shares Affected by Treatments.* Working Paper.

Callaway, Brantly, Andrew Goodman-Bacon, and Pedro H.C. Sant'Anna. 2024. *Difference-in-Differences with a Continuous Treatment.* Working paper. National Bureau of Economic Research.

Chen, Jiafeng, and Jonathan Roth. 2024. "Logs with Zeros? Some Problems and Solutions." *Quarterly Journal of Economics* 139 (2): 891–936.

Christensen, Julian, Lene Aarøe, Martin Baekgaard, Pamela Herd, and Donald P. Moynihan. 2020. "Human Capital and Administrative Burden: The Role of Cognitive Resources in Citizen-State Interactions." *Public Administration Review* 80 (1): 127–136.

Code for America. 2024. *Benefits Enrollment Field Guide.* Accessed: December 2, 2024. https://codeforamerica.org/explore/benefits-enrollment-field-guide/?att=online&j=VA&app=102&dev=d#explore.

Currie, Janet. 2006a. "Public Policy and the Income Distribution." Chap. The Take-Up of Social Benefits. Russell Sage Foundation.

———. 2006b. *The Invisible Safety Net: Protecting the Nation's Poor Children and Families.* Princeton: Princeton University Press.

Dahl, Gordon B, Katrine V Løken, and Magne Mogstad. 2014. "Peer effects in program participation." *American Economic Review* 104 (7): 2049–2074.

Deshpande, Manasi, and Yue Li. 2019. "Who is screened out? Application costs and the targeting of disability programs." *American Economic Journal: Economic Policy* 11 (4): 213–248.

Edelstein, Sara, Michael R. Pergamit, and Caroline Ratcliffe. 2014. *Characteristics of Families Receiving Multiple Public Benefits.* Technical report. Washington, DC: Urban Institute.

Finkelstein, Amy, and Matthew J. Notowidigdo. 2019. "Take-Up and Targeting: Experimental Evidence from SNAP." *Quarterly Journal of Economics* 134 (3): 1505–1556.

Gray, Colin. 2019. "Leaving Benefits on the Table: Evidence from SNAP." *Journal of Public Economics* 179:1–15.

Gray, Colin, Adam Leive, Elena Prager, Kelsey Pukelis, and Mary Zaki. 2023. "Employed in a SNAP? The Impact of Work Requirements on Program Participation and Labor Supply." *American Economic Journal: Economic Policy* 15 (1): 306–341.

Heckman, James J., and Rodrigo Pinto. 2018. "Unordered Monotonicity." *Econometrica* 86 (1): 1–35.

Heinrich, Carolyn J., Sayil Camacho, Sarah Clark Henderson, Monica Hernandez, and Ela Joshi. 2021. "Consequences of Administrative Burden for Social Safety Nets that Support the Healthy Development of Children." *Journal of Policy Analysis and Management* 41 (1): 11–44.

Herd, Pamela, Thomas DeLeire, Hope Harvey, and Donald P. Moynihan. 2013. "Shifting Administrative Burden to the State: The Case of Medicaid Take-Up." *Public Administration Review* 73 (S1): S69–S81.

Herd, Pamela, and Donald Moynihan. 2018. *Administrative Burden: Policymaking by Other Means.* New York: Russell Sage Foundation.

Hicks, Jeffrey. 2024. *The Effects of Field Office Closures on Social Assistance Take-Up and Targeting.* Working Paper.

Homonoff, Tatiana, and Jason Somerville. 2021. "Program Recertification Costs: Evidence from SNAP." *American Economic Journal: Economic Policy* 13 (4): 271–298.

Kirkeboen, Lars J, Edwin Leuven, and Magne Mogstad. 2016. "Field of study, earnings, and self-selection." *The Quarterly Journal of Economics* 131 (3): 1057–1111.

Kline, Patrick, and Melissa Tartari. 2016. "Bounding the Labor Supply Responses to a Randomized Welfare Experiment: A Revealed Preference Approach." *American Economic Review* 106 (4): 972–1014.

Macartney, Suzanne, and Robin Ghertner. 2023. *Participation in the U.S. Social Safety Net: Multiple Programs, 2019.* Technical report. April: Department of Health and Human Services, ASPE Office of Human Services Policy.

Meyer, Bruce D., Wallace K.C. Mok, and James X. Sullivan. 2015. "Household Surveys in Crisis." *Journal of Economic Perspectives* 29 (4): 199–226.

Moffitt, Robert. 2016. "Multiple Program Participation and the SNAP Program." In *SNAP Matters: How Food Stamps Affect Health and Well-Being,* edited by Craig Gundersen, James P. Ziliak, Judith Bartfeld, and Timothy Smeeding. Stanford University Press.

Mountjoy, Jack. 2022. "Community colleges and upward mobility." *American Economic Review* 112 (8): 2580–2630.

National Governors Association. 2022. *Advancing Economic Mobility for Low-Income Families: Policy Options for Governors.* Accessed: 2024-10-22. https://www.nga.org/wp-content/uploads/2022/07/NGA-Economic-Mobility-Report-Final.pdf.

Nichols, Albert L, and Richard J Zeckhauser. 1982. "Targeting transfers through restrictions on recipients." *The American Economic Review* 72 (2): 372–377.

Pinto, Rodrigo. 2022. *Beyond Intention-to-Treat: Using the Incentives of Moving to Opportunity to Identify Neighborhood Effects.* Working Paper.

Rossin-Slater, Maya. 2013. "WIC in Your Neighborhood: New Evidence on the Impacts of Geographic Access to Clinics." *Journal of Public Economics* 102 (C): 51–69.

Schmidt, Lucie, Lara Shore-Sheppard, and Tara Watson. 2024. "Safety-Net Program Spillovers and Administrative Burden." *National Tax Journal* 77 (1): 199–221.

Wu, Derek, and Bruce D Meyer. 2023. *Certification and Recertification in Welfare Programs: What Happens When Automation Goes Wrong?* Working paper.

Online Appendix for "Multiple Safety Net Program Participation: Incidence and Impacts from Reducing Administrative Burdens"

Neil A. Cholli and Derek Wu

January 2025

Table of Contents

A Supplemental Figures and Tables **1**

B Technical Appendix **23**

B.1 Converting Regression Parameters to Percentage Point Effects 23

B.2 Mapping Percentage-Point Effects to Latent Responder Shares 23

A Supplemental Figures and Tables

Figure A.1: Trends in Receipt of VDSS Programs

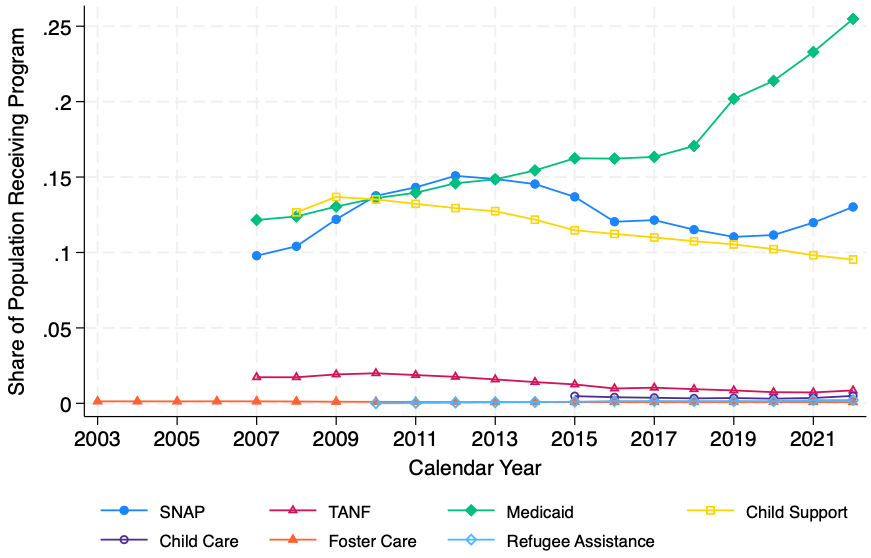
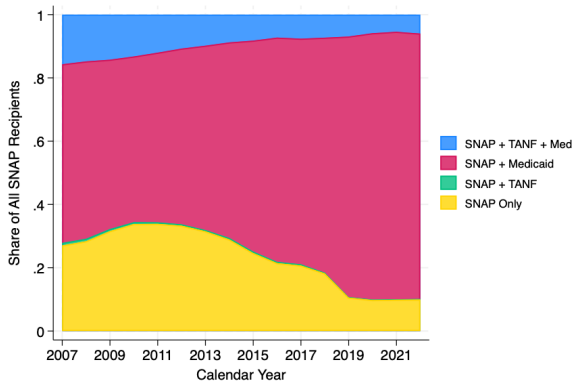
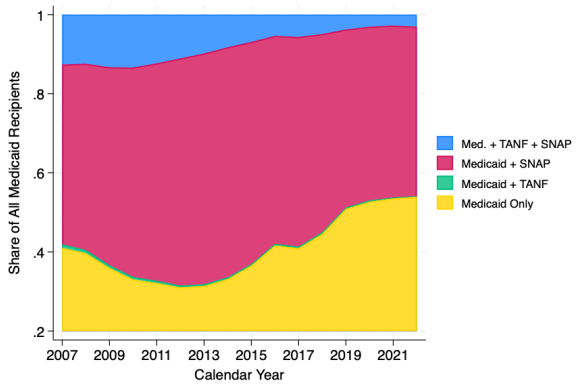


Figure A.2: Participation Conditional on Receipt of Given Program

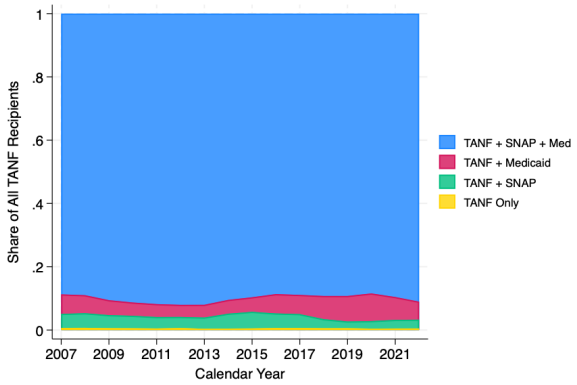
(a) Conditional on SNAP Receipt



(b) Conditional on Medicaid Receipt



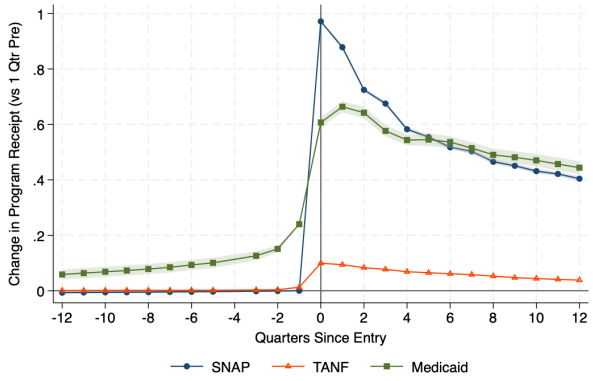
(c) Conditional on TANF Receipt



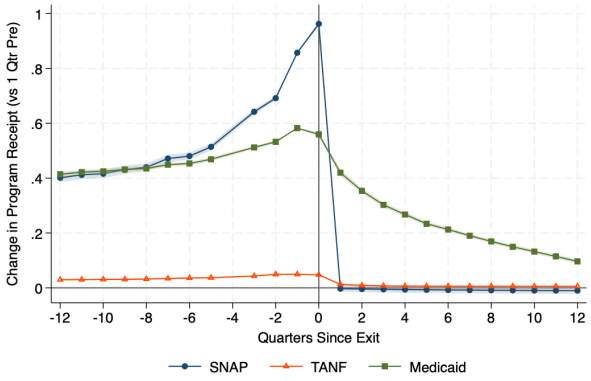
Notes: This figure reports shares of the population receiving each program bundle conditional on receiving a given program.

Figure A.3: Multiple Program Interactions Around Program Entry and Exit for Households with Children

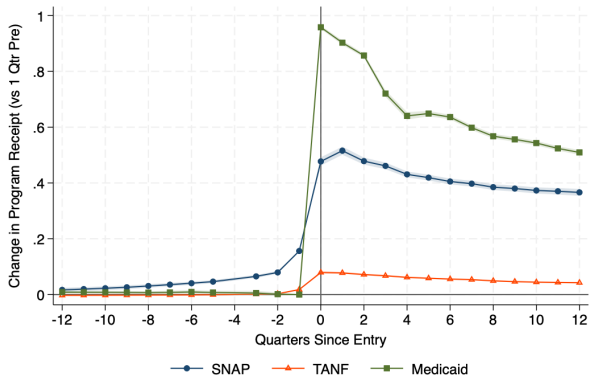
(a) SNAP entry



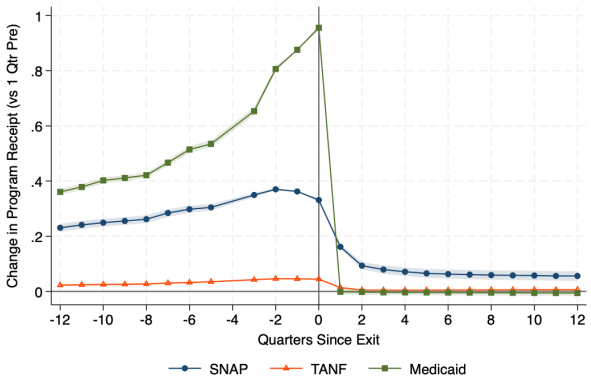
(b) SNAP exit



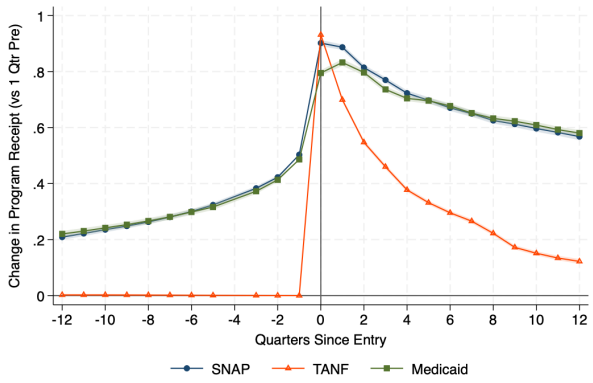
(c) Medicaid entry



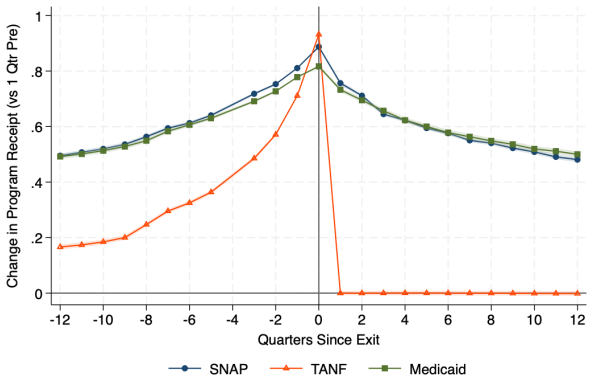
(d) Medicaid exit



(e) TANF entry



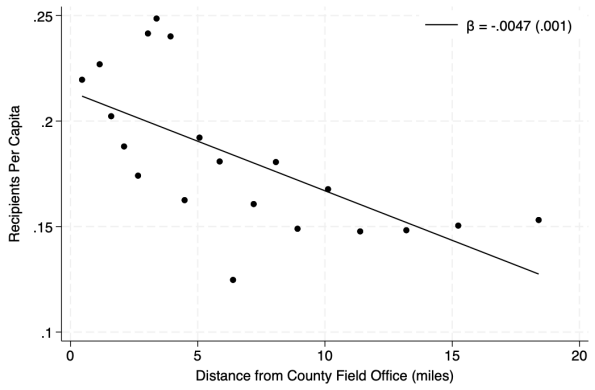
(f) TANF exit



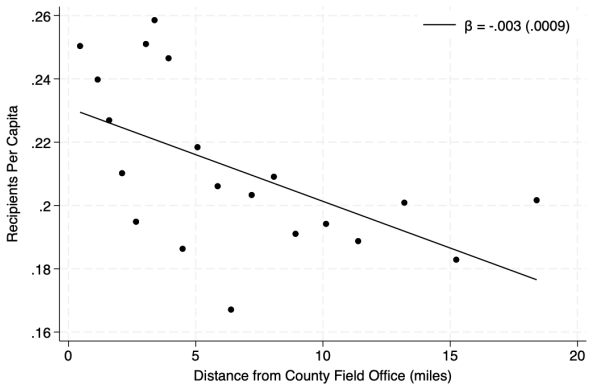
Notes: These figures plot event-study estimates of each program receipt on program entrants and exiters, focusing on individuals in households with children. Left panels condition on individuals who enter a given program and did not receive the program in the prior 12 quarters. Right panels condition on individuals who exit a given program and did not receive the program in the subsequent 12 quarters. Our sample consists of those entering or exiting between January 2010-December 2019, so that we can observe at least 12 quarters of participation data before and after the event. Shaded regions are 95% confidence intervals.

Figure A.4: Zip-level Participation Rates Decrease in Distance to County Field Office (No Controls)

(a) Multiple Program Receipt



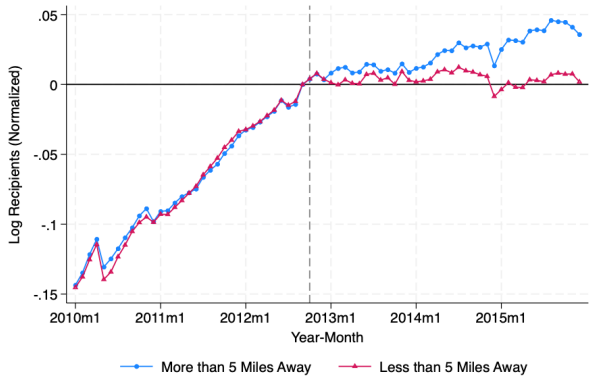
(b) Single Program Receipt



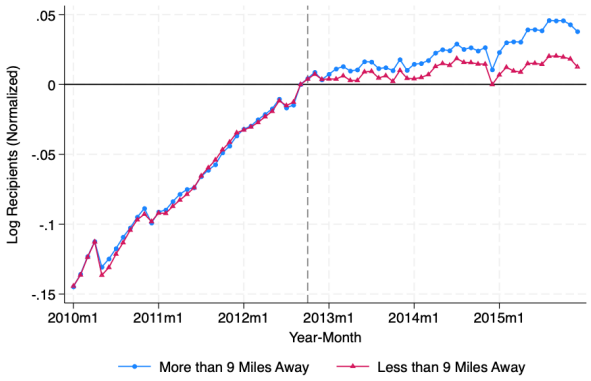
Notes: This figure illustrates the association between zip-level single and multiple program participation rates and zip code distance to their assigned county field office using monthly data between January 2007 and December 2022. Participation rates are expressed in per capita terms using population estimates of zip code tabulation areas (ZCTAs) from the 5-year American Community Survey (ACS), scaled to the zip-county level using the HUD-USPS crosswalk. Binscatters are weighted by population and do not include any controls.

Figure A.5: Raw trends using alternative distance thresholds

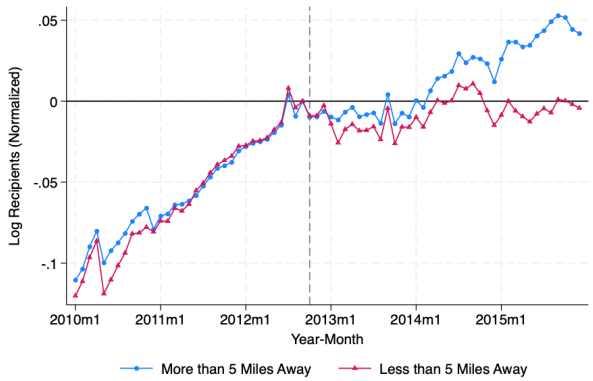
(a) Any program, 5 miles



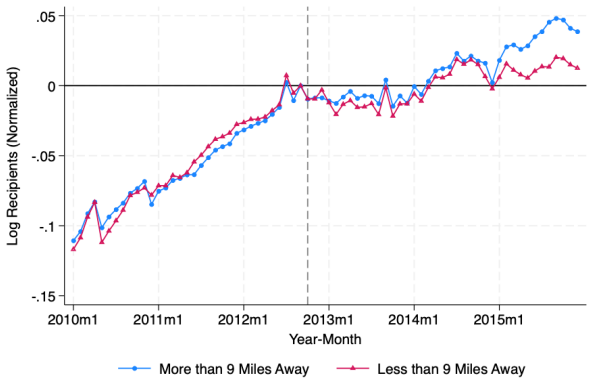
(b) Any program, 9 miles



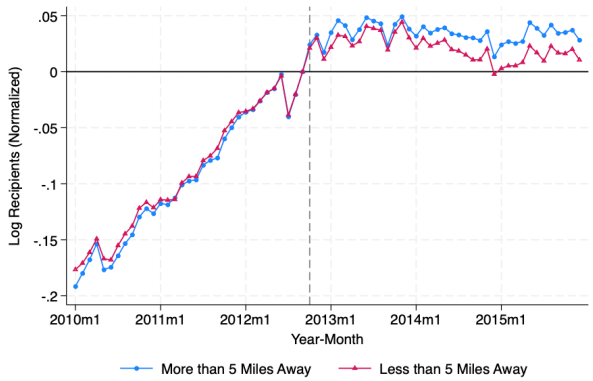
(c) One program, 5 miles



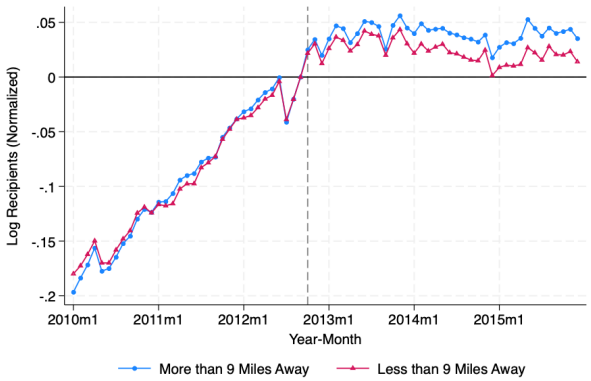
(d) One program, 9 miles



(e) Multiple programs, 5 miles



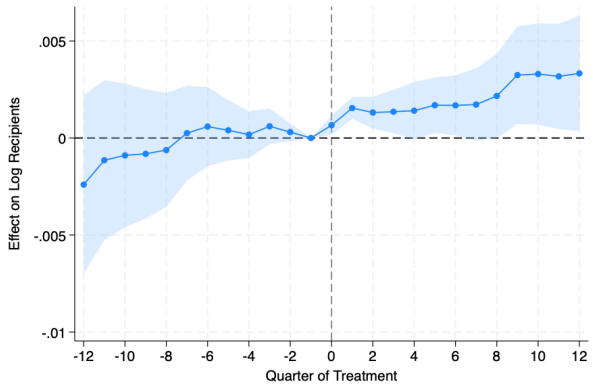
(f) Multiple programs, 9 miles



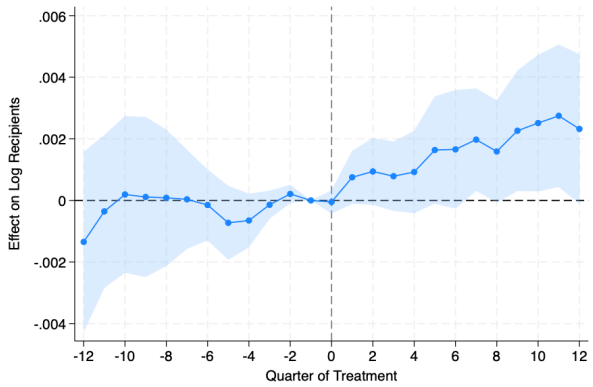
Notes: This figure plots raw monthly trends in log program enrollment for the average zip code based on their distance to their assign county field office. Levels are normalized to zero in September 2012. Zip codes are weighted by their average program recipient count between October 2010–September 2012.

Figure A.6: Event-study estimatess of CommonHelp's effect on participation in individual programs and combinations of programs

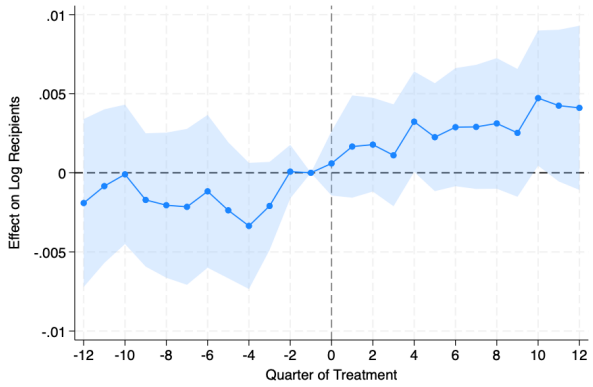
(a) SNAP



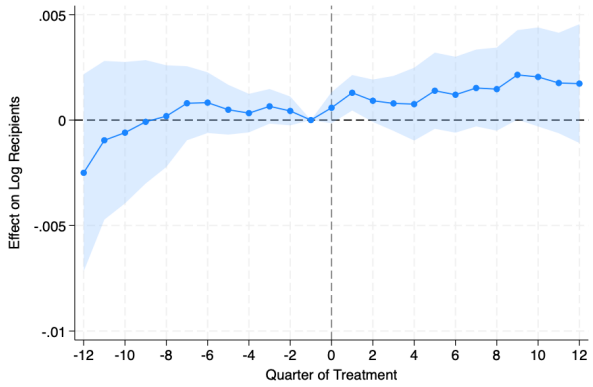
(b) Medicaid



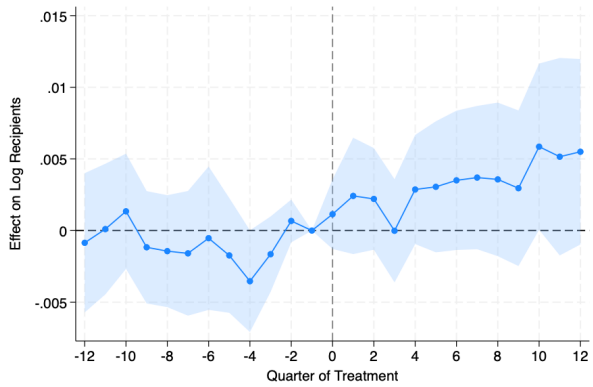
(c) TANF



(d) SNAP and Medicaid



(e) SNAP, Medicaid, and TANF



Notes: These plots illustrate estimates of $\{\beta_k\}$ from Equation (2). Shaded regions are 95% confidence intervals based on robust standard errors clustered at the county level.

Figure A.7: Falsification check: Effect on unemployment insurance claims

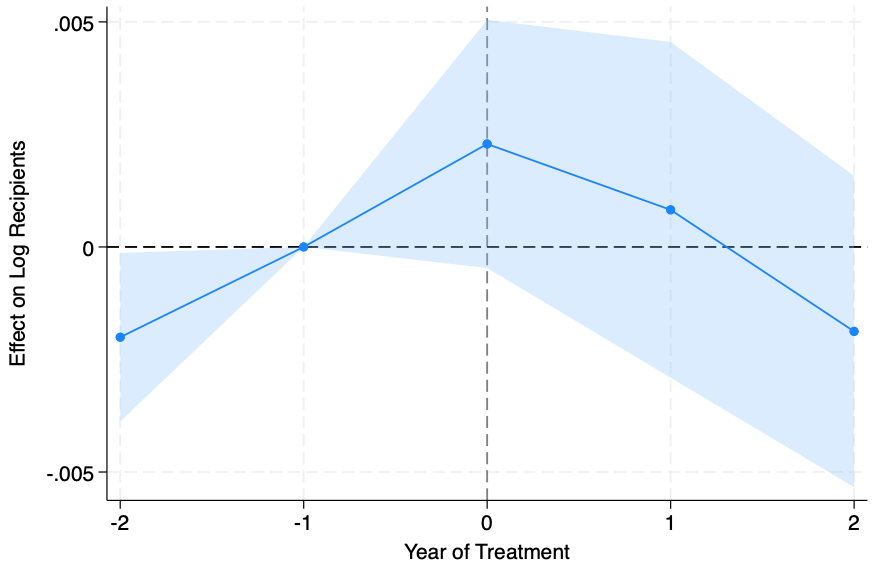


Figure A.8: Robustness checks: Reduction in Variable Inclusion to controls

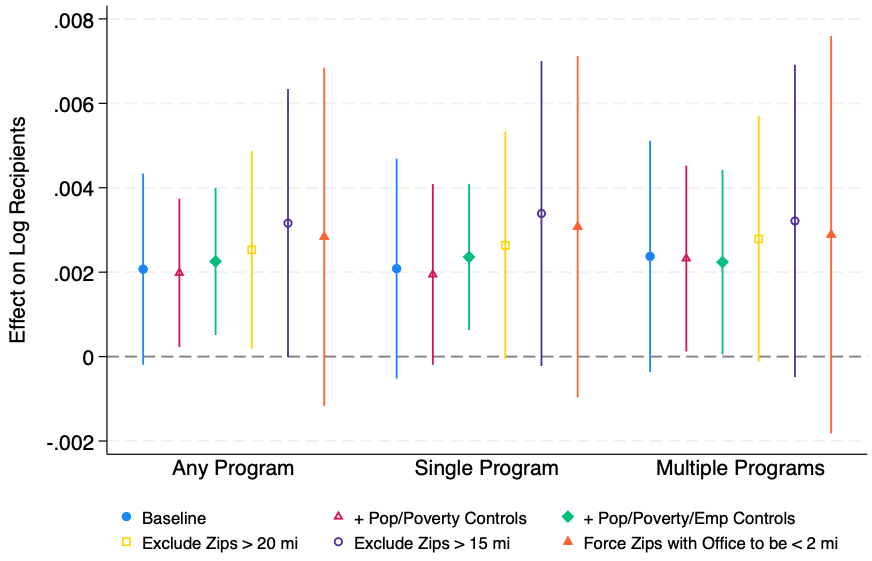
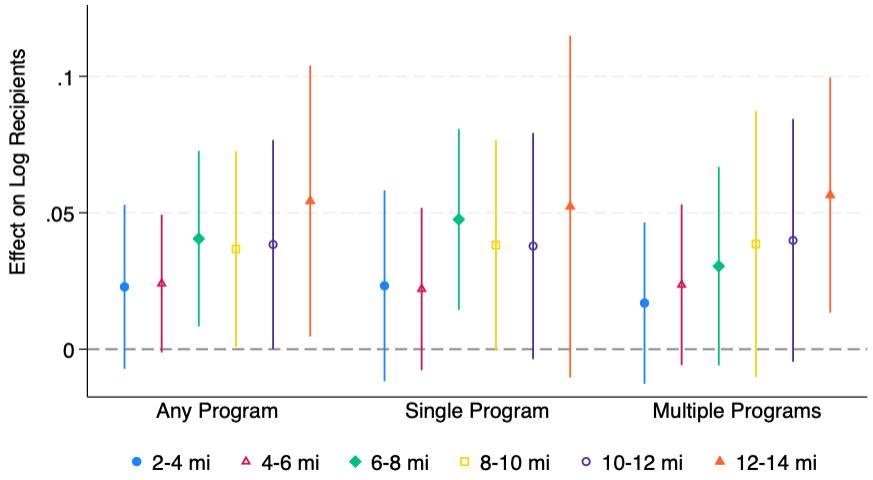


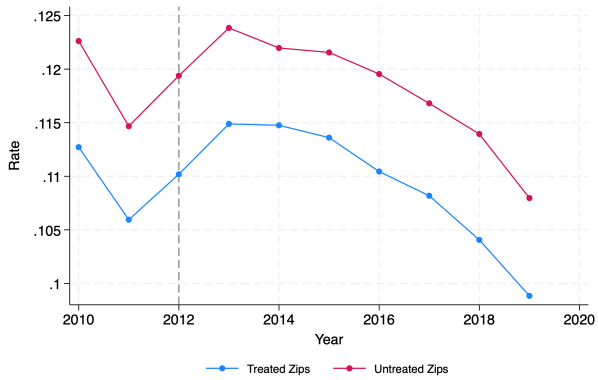
Figure A.9: CommonHelp's impact on program receipt increases in distance to field office



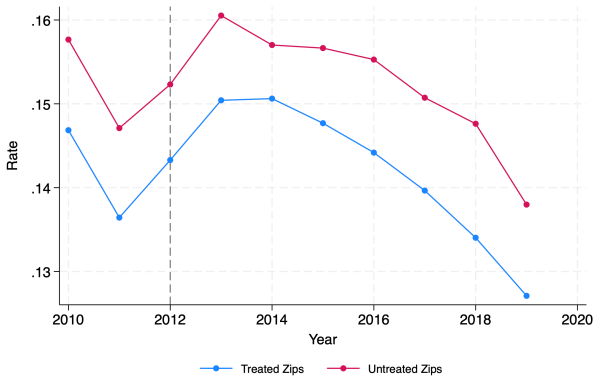
Notes: This figure reports estimates of β based on a pre-post version of Equation (2) in which D_z is replaced with $\mathbb{1}\{D_z \in [d_0, d_1)\}$ that is estimated using zip codes with $D_z < 2$ or $D_z \in [d_0, d_1)$ for different choices of d_0, d_1. Vertical lines are 95% confidence intervals based on robust standard errors clustered at the county level.

Figure A.10: Zip-level covariate trends between zip codes with distance to field office below vs. above 7 miles

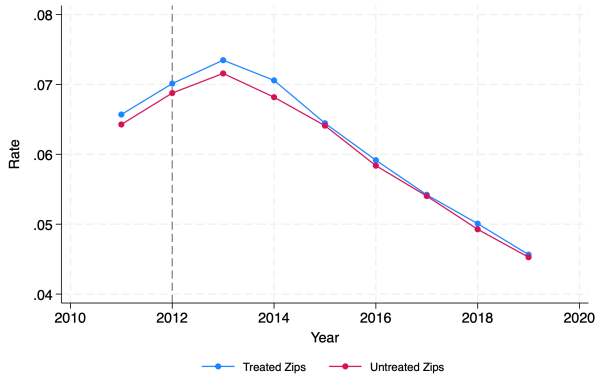
(a) Poverty rates



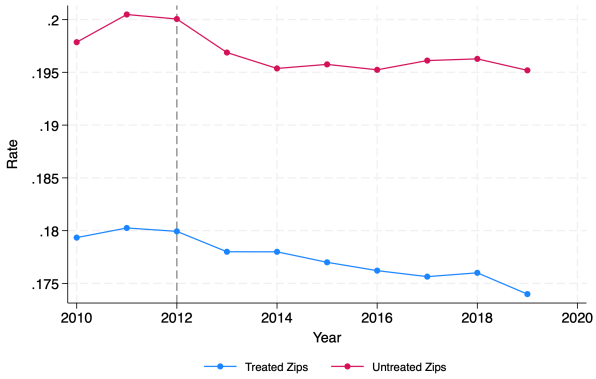
(b) Child poverty rates



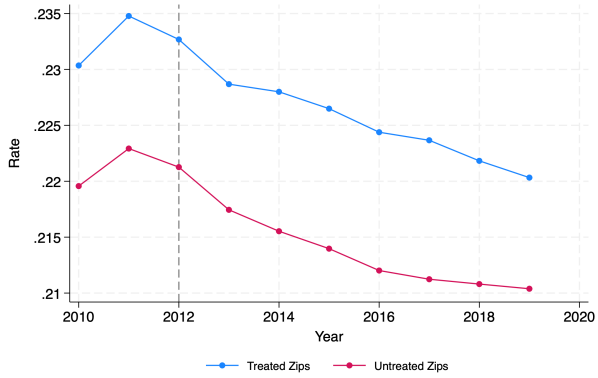
(c) Unemployment rate



(d) Share of Black non-Hispanic population



(e) Share of population age 0–17



(f) Share of population age 65+

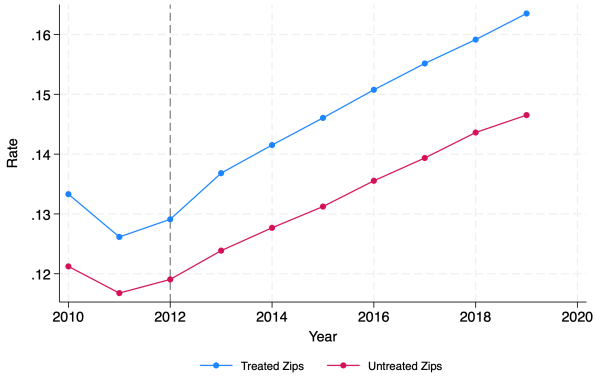


Figure A.11: Event-study estimates with extended pre-trends: Any program receipt

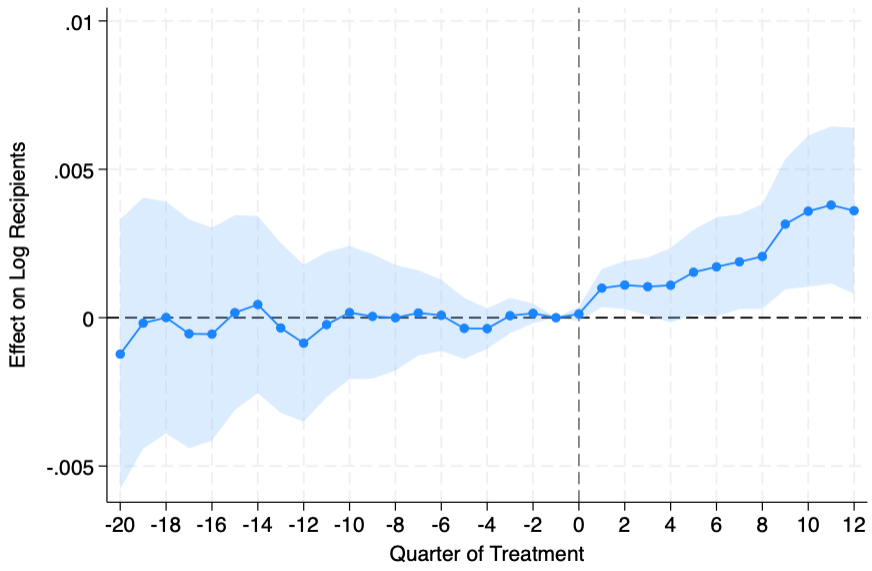
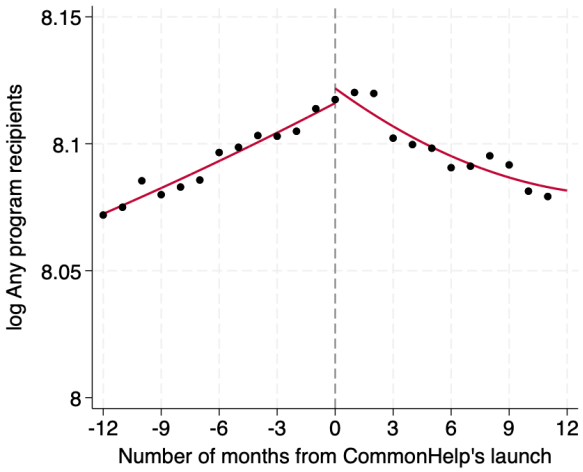
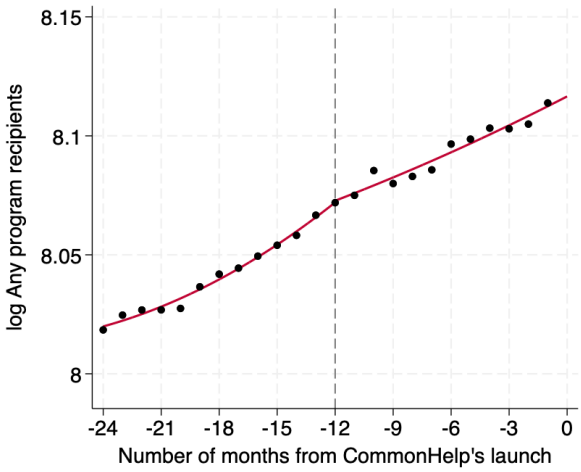


Figure A.12: Visual discontinuity evidence: Any program receipt

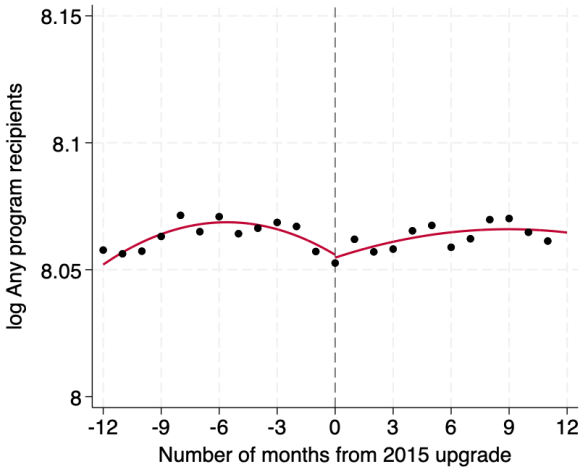
(a) Receipt around CommonHelp's launch



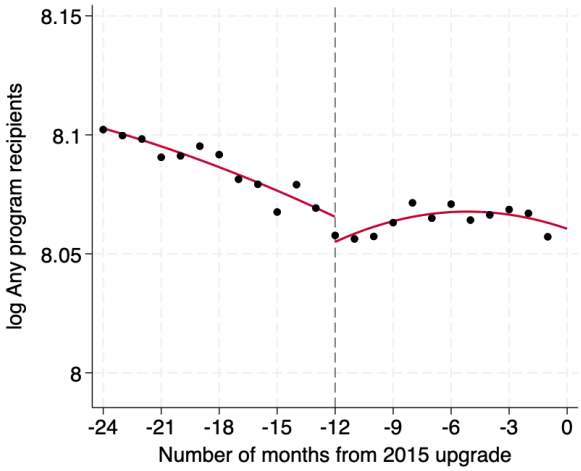
(b) Falsification: One year before launch



(c) Receipt around 2015 upgrade



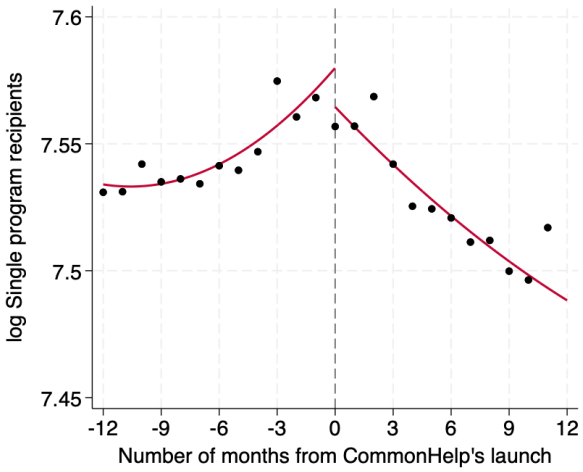
(d) Falsification: One year before upgrade



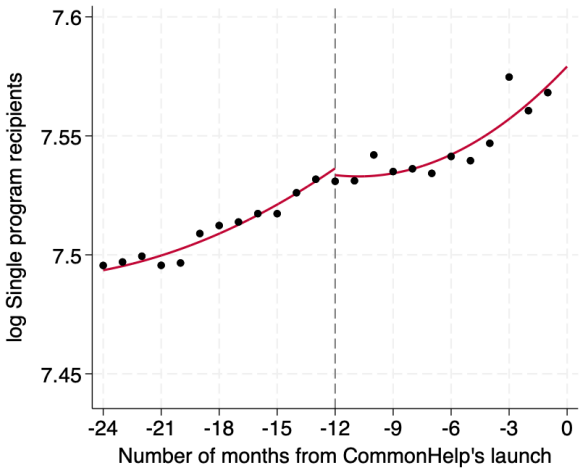
Notes: See notes to Figure 8 for details.

Figure A.13: Visual discontinuity evidence: Single program receipt

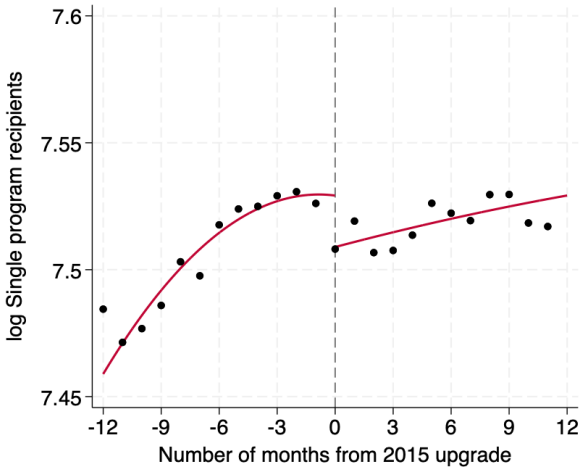
(a) Receipt around CommonHelp's launch



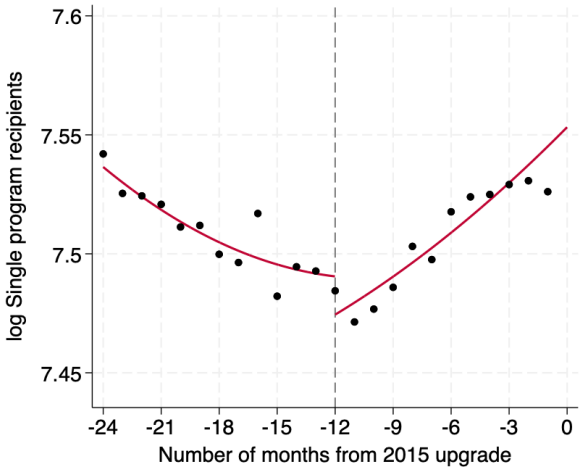
(b) Falsification: One year before launch



(c) Receipt around 2015 upgrade



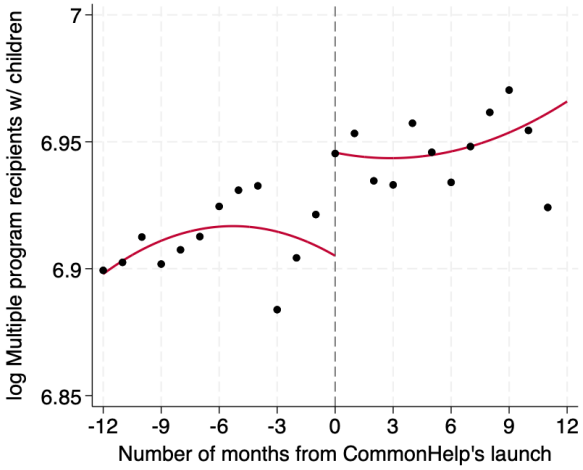
(d) Falsification: One year before upgrade



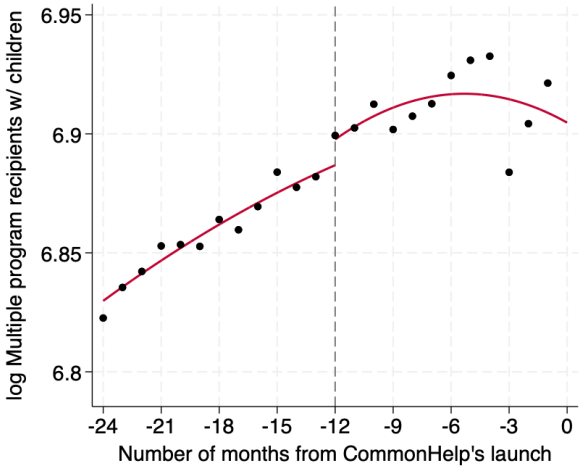
Notes: See notes to Figure 8 for details.

Figure A.14: Visual discontinuity evidence: Multiple program receipt among households with children

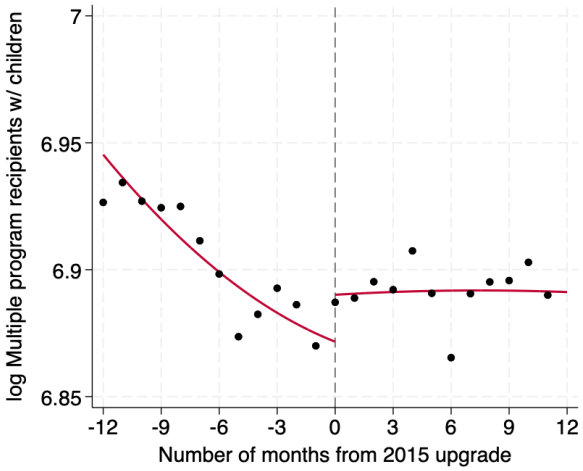
(a) Receipt around CommonHelp's launch



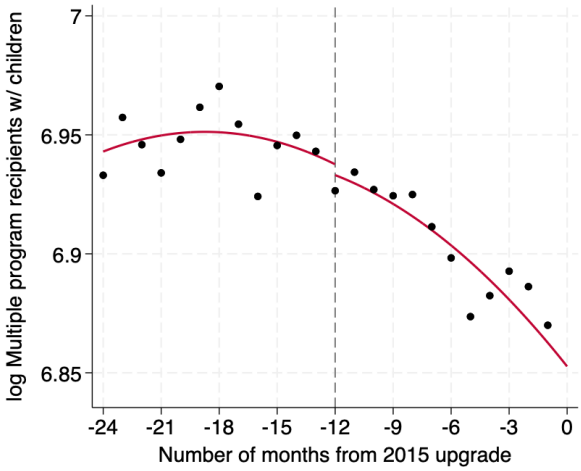
(b) Falsification: One year before launch



(c) Receipt around 2015 upgrade



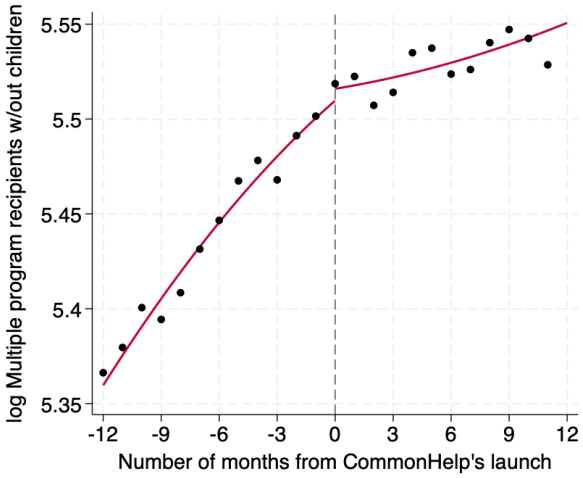
(d) Falsification: One year before upgrade



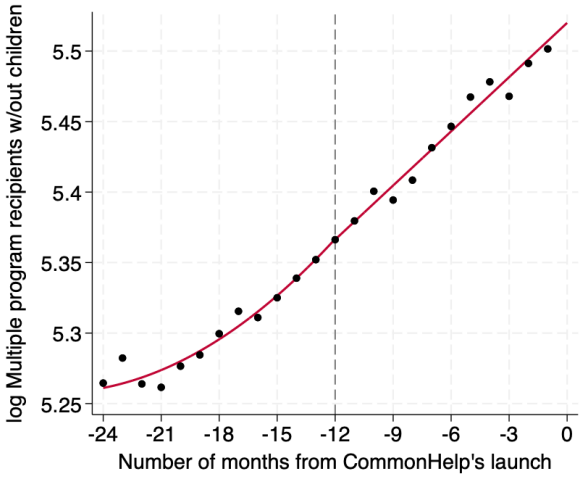
Notes: See notes to Figure 8 for details.

Figure A.15: Visual discontinuity evidence: Multiple program receipt among households without children

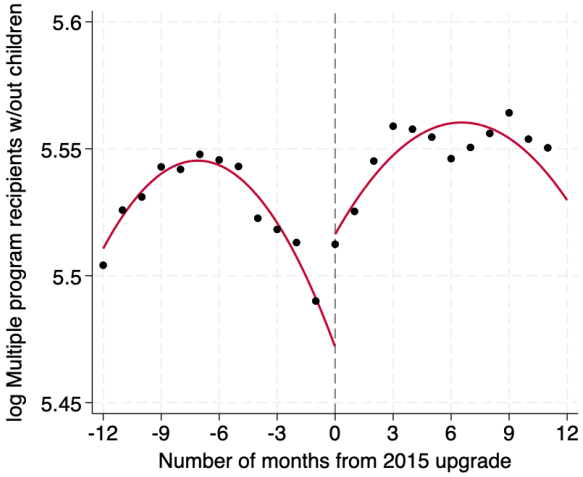
(a) Receipt around CommonHelp's launch



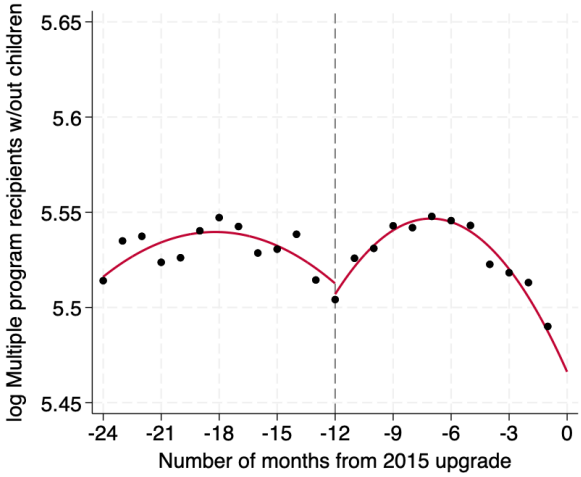
(b) Falsification: One year before launch



(c) Receipt around 2015 upgrade



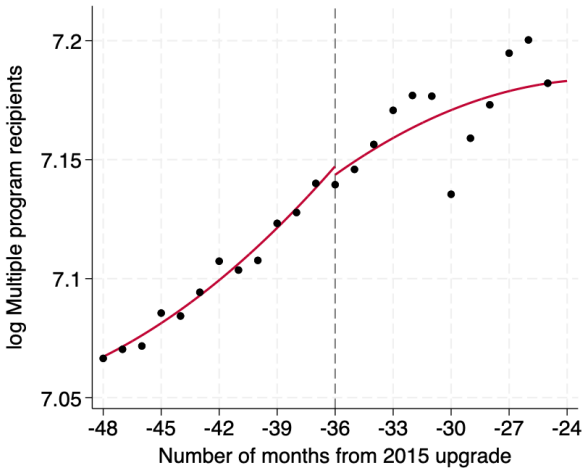
(d) Falsification: One year before upgrade



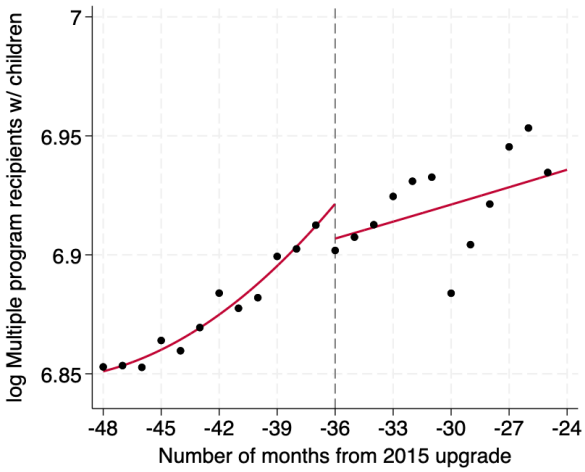
Notes: See notes to Figure 8 for details.

Figure A.16: Additional falsification tests: Receipt around three years before 2015 upgrade

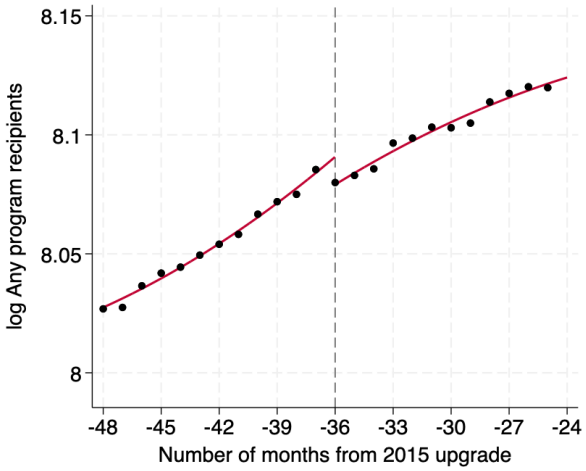
(a) Multiple program



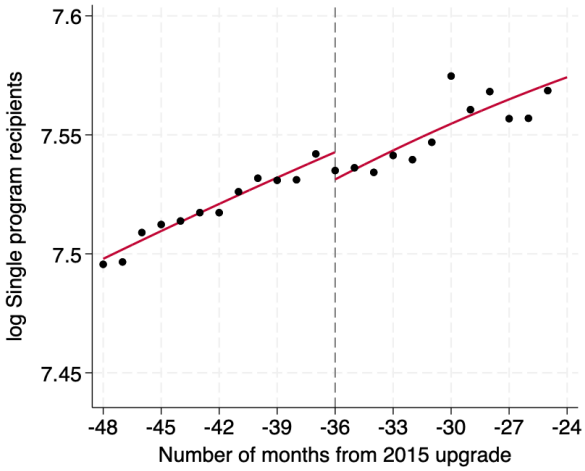
(b) Multiple program, with children



(c) Any program



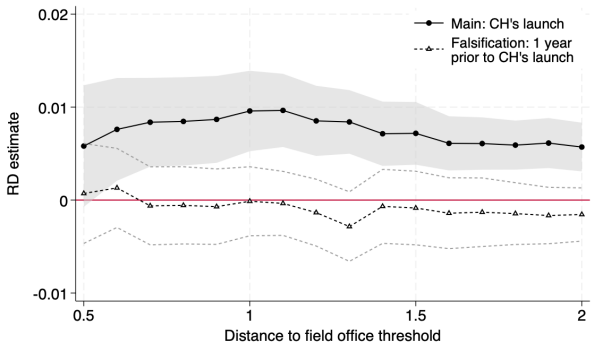
(d) One program



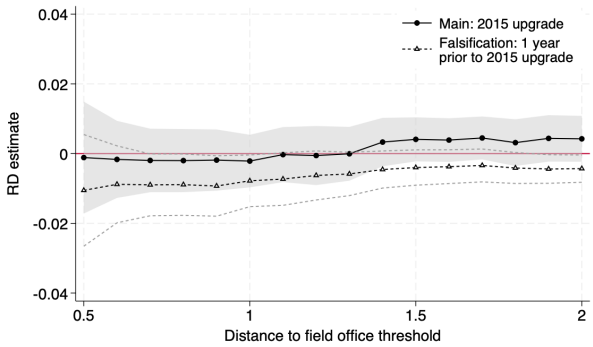
Notes: See notes to Figure 8 for details.

Figure A.17: CommonHelp's effects from reducing fixed burdens for alternative distance to field office thresholds

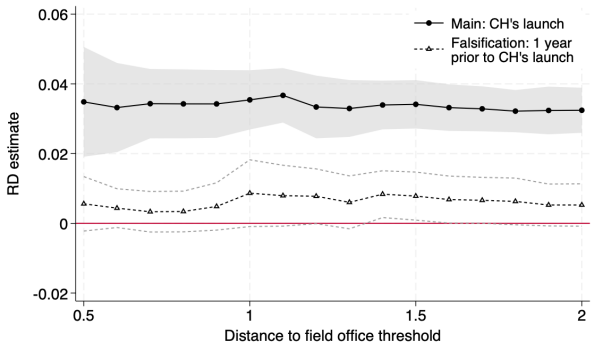
(a) Any program: Launch



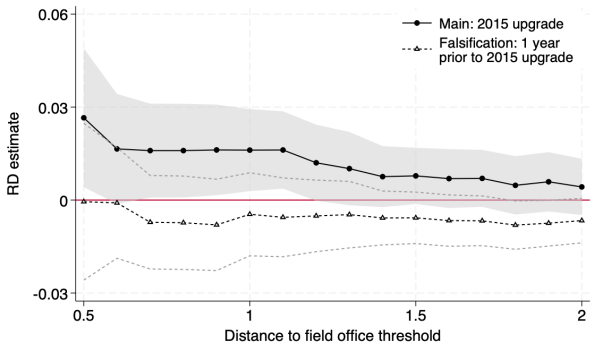
(b) Any program: Upgrade



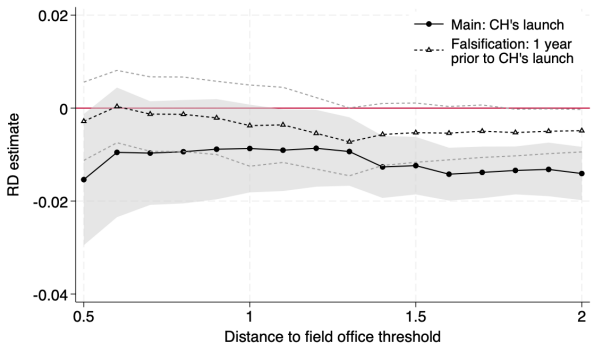
(c) Multiple programs: Launch



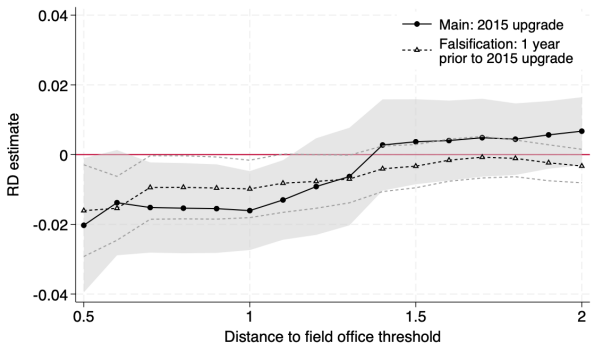
(d) Multiple programs: Upgrade



(e) Single program: Launch



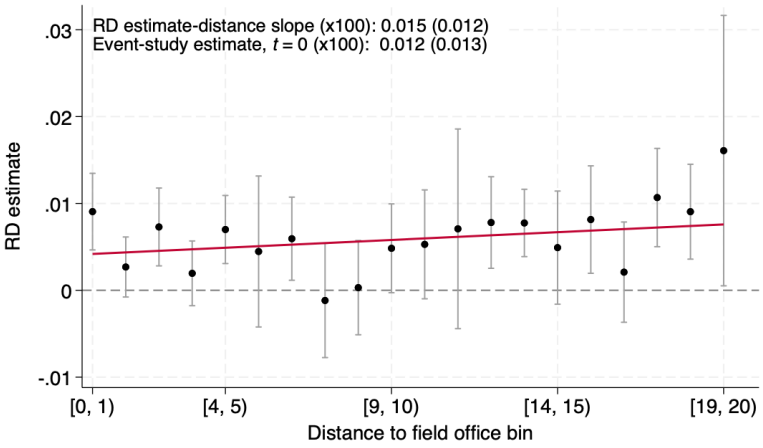
(f) Single program: Upgrade



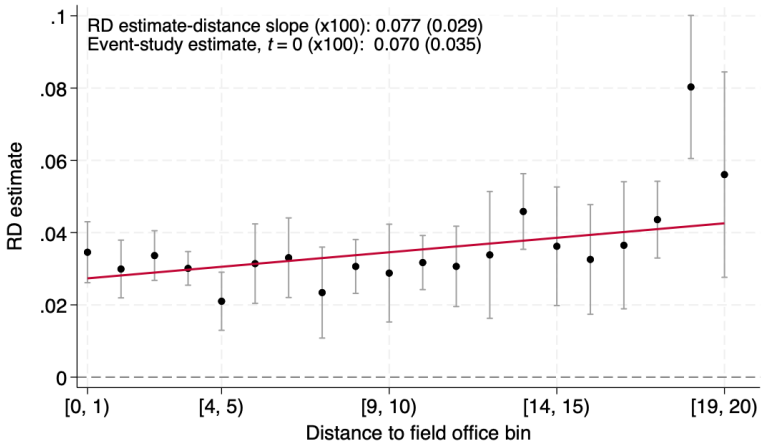
Notes: This figure plots estimates of β^{fb} and $\beta^{fb,15}$ from Equation (4) for different choices of distance thresholds $\bar{d}$. All analyses employ triangular kernels. Shaded areas and short dashed lines represent 95% confidence intervals based on robust standard errors clustered at the county level.

Figure A.18: RD estimates by distance bin: CommonHelp's immediate impacts from reducing both variable and fixed burdens

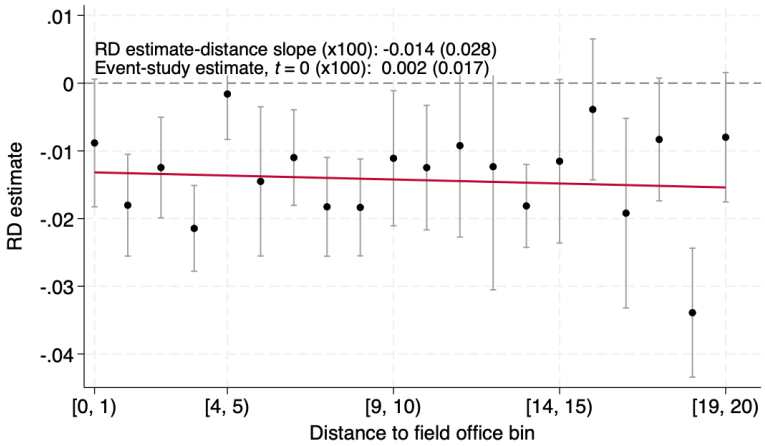
(a) Any program receipt



(b) Multiple program receipt



(c) Single program receipt



Notes: This figure plots estimates of $\beta_{fb,0}$ from Equation (4) among zip codes lying in different distance bins. All regressions employ triangular kernels. The red line is the best linear fit of the estimates, weighting estimates by their inverse squared standard errors. Its slope estimate is reported in the text box along with the estimate of β_0 from Equation (2) for comparison; standard errors are in parentheses. Gray vertical capped lines are 95% confidence intervals based on robust standard errors clustered at the county level.

Table A.1: Robustness check on RD estimates: Difference-in-discontinuity estimates

	(1)	(2)	(3)	(4)	(5)	(6)
	All		With children		No children	
	Launch	Upgrade	Launch	Upgrade	Launch	Upgrade
A. Any program						
Main estimate (%)	0.581*	−0.114	1.320**	−1.529	−0.834	3.107***
	(0.334)	(0.818)	(0.600)	(0.945)	(1.012)	(0.977)
Falsification test (%)	0.071	−1.054	0.568	−2.011	−0.783	−0.871
	(0.274)	(0.817)	(0.631)	(1.802)	(0.612)	(0.970)
Difference (%)	0.510	0.940	0.752	0.482	−0.051	3.978***
	(0.516)	(0.712)	(0.745)	(1.427)	(0.650)	(1.082)
Outcome mean (county/zip)	1,162.4	1,152.1	789.2	805.9	373.2	346.2
C. Multiple programs						
Main estimate (%)	3.484***	2.656**	4.067***	1.849	0.628	4.437**
	(0.803)	(1.142)	(0.867)	(1.166)	(0.813)	(1.749)
Falsification test (%)	0.560	−0.052	1.082*	−0.454	−0.044	−0.563
	(0.398)	(1.291)	(0.607)	(1.840)	(1.086)	(1.342)
Difference (%)	2.924***	2.708*	2.985**	2.303	0.672	5.000**
	(0.890)	(1.423)	(1.121)	(1.581)	(1.410)	(2.165)
Outcome mean (county/zip)	475.9	485.3	383.3	385.9	92.6	99.4
C. Single programs						
Main estimate (%)	−1.538**	−2.028**	−1.894*	−4.188***	−1.596	2.791**
	(0.720)	(0.982)	(1.001)	(1.237)	(1.519)	(1.241)
Falsification test (%)	−0.283	−1.607**	1.039	−1.943*	−0.900	−1.176
	(0.430)	(0.671)	(1.877)	(1.030)	(1.088)	(0.973)
Difference (%)	−1.255	−0.421	−2.933	−2.245	−0.696	3.967***
	(0.861)	(1.070)	(1.896)	(1.467)	(0.677)	(1.240)
Outcome mean (county/zip)	675.6	657.1	395.1	410.4	280.5	246.7
Number of zip codes	28	28	28	28	28	28
Number of counties	25	25	25	25	25	25

Notes: The first rows of each panel report estimates from Equation (4). Columns (1), (3), and (5) report estimates of β^{fb}; Columns (2), (4), and (6) report estimates of $\beta^{fb,15}$. The second rows report analogous estimates using the same calendar month cutoff dates prior to CommonHelp as falsification tests. Columns (1), (3), and (5) report estimates one year prior to CommonHelp's launch and upgrade. The third rows report difference-in-discontinuity estimates generated by differencing the estimates between the first and second rows. The fourth rows report outcome means in the 12 months prior to the launch and upgrade. All regressions employ quadratic polynomials with triangular kernels within a 12-month window of each cutoff date, restrict sample to zip codes within $\bar{d} = 0.5$ mile of field office, and weigh zip-level observations by their outcome means measured between October 2010–September 2012.

Table A.2: Robustness checks on RD estimates: Bandwidth and specification checks

(a) All recipients

	(1)	(2)	(3)	(4)
	Main	Distance kernel	6-month bandwidth	Local linear
A. Any program				
Launch estimate (%)	0.581*	0.637	−0.070	0.009
	(0.334)	(0.684)	(0.402)	(0.345)
Upgrade estimate (%)	−0.114	1.212	0.393	−0.502
	(0.818)	(0.966)	(1.033)	(0.856)
Outcome mean (count/zip)	1,162.4	1,162.4	1,175.3	1,162.4
B. Multiple programs				
Launch estimate (%)	3.484***	2.753*	0.557	0.480
	(0.803)	(1.526)	(1.227)	(1.099)
Upgrade estimate (%)	2.656**	3.113**	2.979***	3.185**
	(1.142)	(1.485)	(0.643)	(1.355)
Outcome mean (count/zip)	675.6	675.6	681.8	675.6
C. Single program				
Launch estimate (%)	−1.538**	−1.510	−0.902	−0.767
	(0.720)	(1.331)	(0.853)	(0.469)
Upgrade estimate (%)	−2.028**	0.006	−1.090	−2.942***
	(0.982)	(1.304)	(1.313)	(0.742)
Outcome mean (count/zip)	475.9	475.9	482.9	475.9
Number of zip codes	28	28	28	28
Number of counties	25	25	25	25

Notes: (see next page)

Table A.2: (cont'd.)

(b) Recipients in households with children

	(1)	(2)	(3)	(4)
	Main	Distance kernel	6-month bandwidth	Local linear
A. Any program				
Launch estimate (%)	1.320**	1.196	0.627	0.770**
	(0.600)	(1.153)	(0.694)	(0.304)
Upgrade estimate (%)	−1.529	−0.398	−0.815	−1.018*
	(0.945)	(1.136)	(1.027)	(0.560)
Outcome mean (count/zip)	789.2	789.2	796.8	789.2
B. Multiple programs				
Launch estimate (%)	4.067***	2.960*	0.988	1.110
	(0.867)	(1.656)	(1.179)	(1.625)
Upgrade estimate (%)	1.849	2.765*	2.662***	3.068**
	(1.166)	(1.551)	(0.724)	(1.284)
Outcome mean (count/zip)	395.1	395.1	399.4	395.1
C. Single program				
Launch estimate (%)	−1.894*	−1.897	0.280	−0.600
	(1.001)	(2.223)	(1.538)	(0.587)
Upgrade estimate (%)	−4.188***	−2.554	−4.088**	−4.599***
	(1.237)	(1.927)	(1.795)	(0.632)
Outcome mean (count/zip)	383.3	383.3	386.9	383.3
Number of zip codes	28	28	28	28
Number of counties	25	25	25	25

Notes: (see next page)

Table A.2: (cont'd.)

(c) Recipients in households with no children

	(1)	(2)	(3)	(4)
	Main	Distance kernel	6-month bandwidth	Local linear
A. Any program				
Launch estimate (%)	−0.834	−0.540	−1.744	−1.683*
	(1.012)	(0.694)	(1.227)	(0.911)
Upgrade estimate (%)	3.107***	4.063***	3.521***	0.786
	(0.977)	(1.195)	(1.120)	(1.351)
Outcome mean (count/zip)	373.2	373.2	378.5	373.2
B. Multiple programs				
Launch estimate (%)	0.628	1.692	0.599	−1.085
	(0.813)	(1.468)	(1.318)	(1.587)
Upgrade estimate (%)	4.437**	4.920*	3.463**	3.597
	(1.749)	(2.462)	(1.581)	(2.977)
Outcome mean (count/zip)	280.5	280.5	282.4	280.5
C. Single program				
Launch estimate (%)	−1.596	−1.660***	−2.989*	−1.542
	(1.519)	(0.582)	(1.724)	(1.314)
Upgrade estimate (%)	2.791**	4.326***	3.610**	0.233
	(1.241)	(1.142)	(1.641)	(1.215)
Outcome mean (count/zip)	92.6	92.6	95.9	92.6
Number of zip codes	28	28	28	28
Number of counties	25	25	25	25

Notes: This table reports estimates of β^{fb} and $\beta^{fb,15}$ from different variations of Equation (4). Column (1) reports estimates from the main specification. Column (2) additionally weighs zip code observations by a half-triangular kernel around zero distance. Column (3) restricts sample to zip codes within $\bar{d} = 0.5$ miles of their field office (instead of $\bar{d} = 0.5$). Column (4) uses a bandwidth of 6 months (instead of 12 months) around each cutoff date. Column (5) employs a local linear specification (instead of local quadratic). See notes of Table 3 for more details.

Table A.3: CommonHelp's targeting effects among households with no children

	(1)	(2)	(3)
	Effect	(S.E.)	Mean
A. Employment history characteristics, 2009–2011			
Any employment (> \$0, p.p.)	1.385***	(0.375)	0.419
Fraction of quarters employed	0.006***	(0.002)	0.216
Mean quarterly wage earnings	22.482*	(13.576)	756.9
S.D. quarterly wage earnings	31.155***	(8.471)	614.7
S.D. earnings among quarters with > \$0	42.307	(27.450)	1,433.6
Any crime committed (p.p.)	0.576***	(0.192)	0.158
B. Criminal history characteristics, 1998–2011			
Number of crimes committed	0.008***	(0.003)	0.229
Any violent crime committed (p.p.)	0.118	(0.079)	0.031
Any property crime committed (p.p.)	0.251**	(0.111)	0.070
Any drug crime committed (p.p.)	0.276**	(0.116)	0.065
C. Employment & criminal history interactions			
Not employed, no crime committed (p.p.)	−1.316***	(0.412)	0.497
Not employed, crime committed (p.p.)	−0.069	(0.113)	0.084
Employed, no crime committed (p.p.)	0.740**	(0.294)	0.345
Employed, crime committed (p.p.)	0.645***	(0.143)	0.074
D. Household characteristics			
Single-parent household (p.p.)	0.371	(0.458)	0.842
Two-parent household (p.p.)	−0.763**	(0.370)	0.108
Any children, ages 0–4 (p.p.)	−0.026*	(0.014)	0.011
Any children, ages 5–9 (p.p.)	−0.009	(0.007)	0.004
Any children, ages 10–17 (p.p.)	−0.023***	(0.008)	0.004

Notes: See notes of Table 5.

B Technical Appendix

B.1 Converting Regression Parameters to Percentage Point Effects

First, recall that β, β^{15} are expressed as *per mile growth rate* in program receipt at the *zip code* level. In other words, for a zip code z with distance $D_z = d_z$,

$$\begin{aligned} g_z &\equiv (\beta + \beta^{15}) \cdot d_z + \beta^{fb} \\ &= \mathbb{E}\left[\log(Y_{z,post}(1)) - \log(Y_{z,post}(0)) \,|\, D_z = d_z\right] \\ &\approx \mathbb{E}\left[\frac{Y_{z,post}(1) - Y_{z,post}(0)}{Y_{z,post}(0)} \,|\, D_z = d_z\right], \end{aligned}$$

where $Y_{z,post}(0), Y_{z,post}(1)$ are the number of recipients in the post-CommonHelp period under no CommonHelp and under CommonHelp, respectively. This means we can identify the counterfactual level of recipients in the post-CommonHelp period in each zip code by down-scaling the factual level by its growth rate:[1]

$$Y_{z,post}(0) = \frac{1}{1+g_z} \cdot Y_{z,post}.$$

Thus, the total change in the number of recipients of p in the population caused by CommonHelp is thus

$$\Delta Y \equiv \sum_{z \in \mathcal{Z}} \left[Y_{z,post}(1) - Y_{z,post}(0)\right] = \sum_{z \in \mathcal{Z}} \left(1 - \frac{1}{1+g_z}\right) \cdot Y_{z,post}.$$

B.2 Mapping Percentage-Point Effects to Latent Responder Shares

Under appropriate parallel trends and smoothness assumptions, our DD and RD designs help identify the causal percentage-point effect of CommonHelp on the share of recipients of program $p \in \mathcal{P}$ defined in Equation (7). Assuming expectations are taken over the population of recipients, this parameter corresponds to

$$\tilde{\beta}_p = \mathbb{E}[P_{p,it}(1) - P_{p,it}(0) \,|\, Post_t = 1].$$

1. This is because $Y_{z,post} = Y_{z,post}(1) = (1+g_z) \cdot Y_{z,post}(0)$.

From the law of total probability,

$$\begin{aligned}
\tilde{\beta}_p &= \Pr(P_{p,it}(1) = 1, P_{p,it}(0) = 0) - \Pr(P_{p,it}(1) = 0, P_{p,it}(0) = 1) \\
&= \Pr(P_{it}(0) \subset P_{it}(1) = p) - \Pr(P_{it}(0) = d \subset P_{it}(1)) \\
&= \sum_{\substack{p_0 \in \mathcal{P} \\ \text{s.t. } p_0 \subset p}} \omega_{(p_0,p)} - \sum_{\substack{p_1 \in \mathcal{P} \\ \text{s.t. } p \subset p_1}} \omega_{(p,p_1)}.
\end{aligned}$$

the penultimate equality follows from the monotonicity assumption and final equality follows from the definition of $\omega_{(p_0,p_1)}$.