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

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Abstract

Multiple program participation among low-income households 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 "geographic" and "fixed" 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 significant share of multiple program participation likely occurred along 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.

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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 well-being. Nevertheless, reducing administrative burdens could serve as a screening device by causing 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 a limited understanding of how many individuals receive multiple programs and how they differ from those receiving a single program. Moreover, there is scarce empirical evidence on the role of administrative burdens in hindering participation *jointly* across programs and how that shapes the targeting efficiency of the safety net. 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 novel 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 intercon-

nected 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 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 2024, 31 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 highlighting the promise of "leverag[ing]...information management technology" to "improve access and streamline delivery to beneficiaries" (2022, p. 2). Thus, our focus on ameliorating burdens for *multiple* programs is particularly policy relevant and has promise to inform state policies around the country.

We introduce a novel empirical framework for decomposing the effects of reducing administrative burdens into two distinct types: "geographic" and "fixed" burdens. Geographic 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 geographic 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 variant of a regression discontinuity design around the time of treatment among zip codes in close proximity to their field office that differences out seasonality effects around fiscal year and quarterly cycles. Collectively, these strategies enable us to identify the total effects of CommonHelp. Importantly, our framework can be more widely adopted to analyze the impacts of any policy change that is rolled out simultaneously to an 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 geographic burdens, participation in any program increased by 2.6% for every 10 miles the zip code was located farther from a field office. Effects occur immediately after CommonHelp's launch and jump again after a subsequent upgrade of the

online platform. A considerable share of these gains came from a 2.5% 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 discontinuity estimates show that CommonHelp caused a 1.9% jump in multiple program receipt among zip codes within close proximity to local offices. This effect is again largely attributable to households with children. Conversely, enrollment in single programs *fell* by 1.2% among households with children living in close zip codes, which is consistent with transitions from a single program to multiple programs. Bringing together estimates from the combination of reducing geographic and fixed burdens, we estimate that CommonHelp brought in nearly 90,000 recipients onto any program and nearly 62,000 recipients in multiple programs.

Given the 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, linking in data from administrative earnings and crime records to proxy for labor market skill and socioeconomic disadvantage. We find that CommonHelp tended to bring in individuals with a greater degree of self-sufficiency, characterized by increased work histories and fewer criminal offenses. Next, we examine "complier" characteristics based on unobserved counterfactual outcomes—a novel dimension in the empirical literature on program targeting. 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 between 17–55% of the increase in program receipt among households with children occurred along the intensive margin, implying that CommonHelp successfully reached those who would have otherwise been only partially attached to the social safety net and leaving benefits on the table. In ongoing work, we are combining these estimates to examine the welfare impacts of the policy change. Specifically, we will evaluate the Marginal Value of Public Funds (MVPF) allowing for changes in only one program versus multiple programs, which will highlight the value of analyzing effects jointly across programs.

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, allowing us 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.

The remainder of our paper proceeds as follows. Section 2 summarizes the safety net programs and policy changes we study. Section 3 describes the data sources used in our analyses. Section 4 presents descriptive facts on single and multiple program participation, which help motivate our subsequent analyses. Section 5 develops our conceptual framework for analyzing geographic and fixed burdens and describes our research designs for identifying the causal impacts of CommonHelp. Section 6 reports CommonHelp's effects on program enrollment. Section 7 describes effects on targeting efficiency based on observed pre-treatment characteristics, while Section 8 examines targeting based on unobserved counterfactual outcomes. 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 (Centers for Medicare & Medicaid Services 2024; USDA Food and Nutrition Service 2024). 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.[1]

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 (beyond our analysis period).

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

1. Work requirements were temporarily waived during the Great Recession and reinstated in 2013.

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

	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	None§	None	30 hours per week¶
Time limits	None	None	6 years

Notes: Based on 2012 program rules. *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. ¶Exemptions granted to minors, 18-year olds in secondary school, the disabled, elderly, and primary caretakers of a child under 12 months or a disabled family member.

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.

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 appli-

cation form where they elected to apply to SNAP, TANF, and Medicaid, as well as separate recertification forms for each program to continue receiving benefits.[2] 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 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

2. 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.

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.[3] 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.[4] 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

3. 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.

4. 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.

as "zip codes" for simplicity.[5]

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 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.[6]

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.

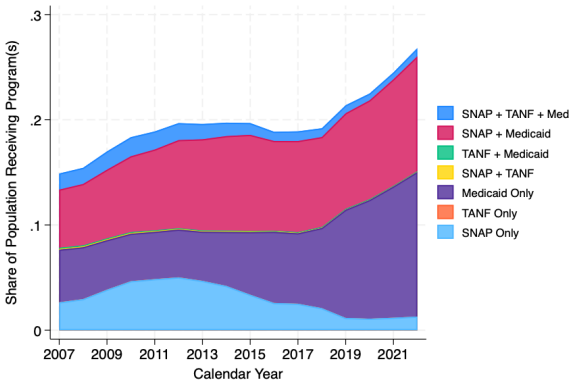
Figure 1, Panel (a), begins by illustrating trends in the share of the population in Virginia

5. 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.

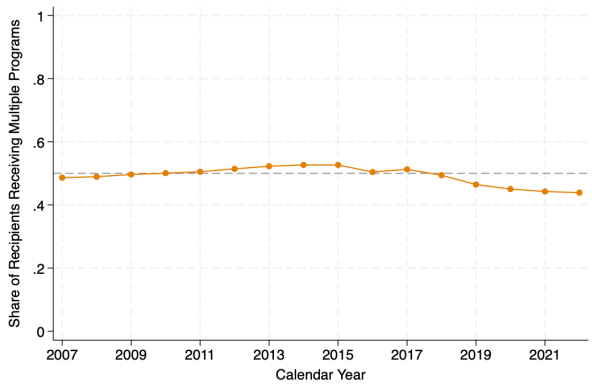
6. We also calculate driving and transit distances using Google API, which we utilize in robustness checks.

Figure 1: Trends in Single and Multiple Program Participation

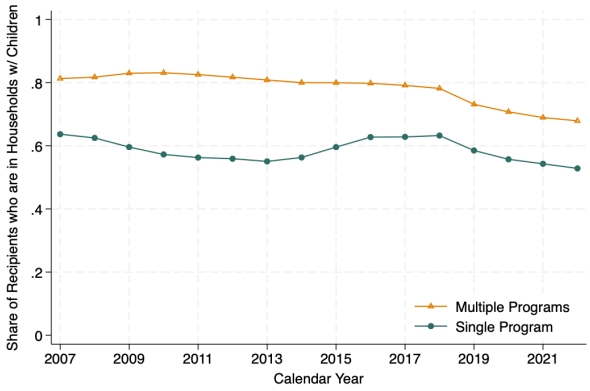
(a) Participation in Bundles of Programs



(b) Share of Multiple Program Recipients



(c) Share 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.

participating in individual programs and combinations of programs.[7] 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, 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

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

(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 often 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.

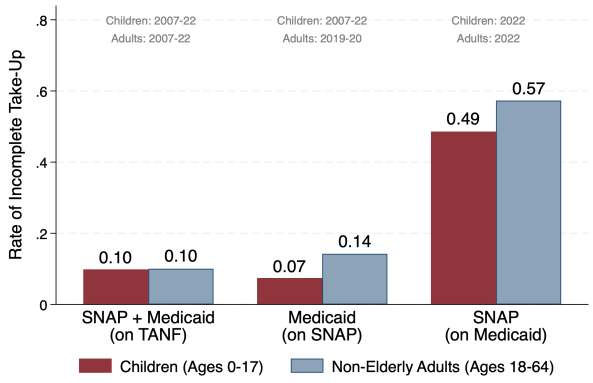
We find persistent evidence of incomplete take-up over time.[8] Examining first TANF recipients (who are categorically eligible for all three programs), Figure **??**, Panel (a) shows that roughly 10% of child and adult recipients do not participate in other programs. Among children on SNAP, we find 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 (b) 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 mul-

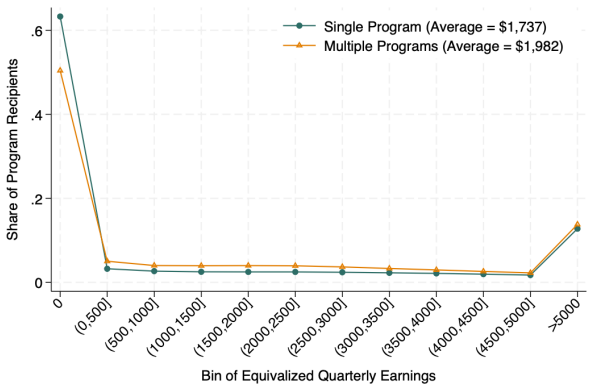
8. Note that this analysis examines annualized program participation, which provides a lower bound on the degree of incomplete take-up. We use annual data rather than monthly data to avoid conflating incomplete take-up with delays in transitioning into steady-state program bundles.

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

(a) Conditional Take-Up Rates



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



Notes: Panel (a) plots conditional participation rates of different programs among children and adults who are inferred to be eligible based on their participation in other programs. The gray text notes the time periods used for the data, which differ due to changes in eligibility criteria. Individuals on TANF (both left bars) are categorically eligible for both SNAP and Medicaid and children on SNAP (red middle bar) are eligible for Medicaid throughout the time period. On January 1st, 2019, Medicaid expansion increased the income eligibility threshold for adults above SNAP (gray middle bar); on July 1st, 2021, SNAP expanded its income eligibility threshold beyond Medicaid (right bars). Panel (b) 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.

tiple 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 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.

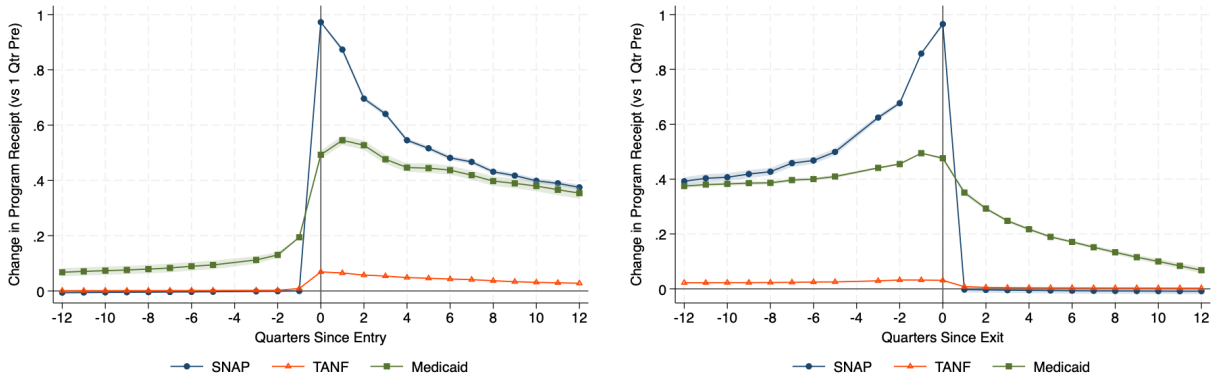
Fact 3: Dynamic interactions in program take-up are prevalent, with individuals often entering and exiting multiple programs simultaneously.

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

Figure 3: Multiple Program Interactions Around Program Entry and Exit

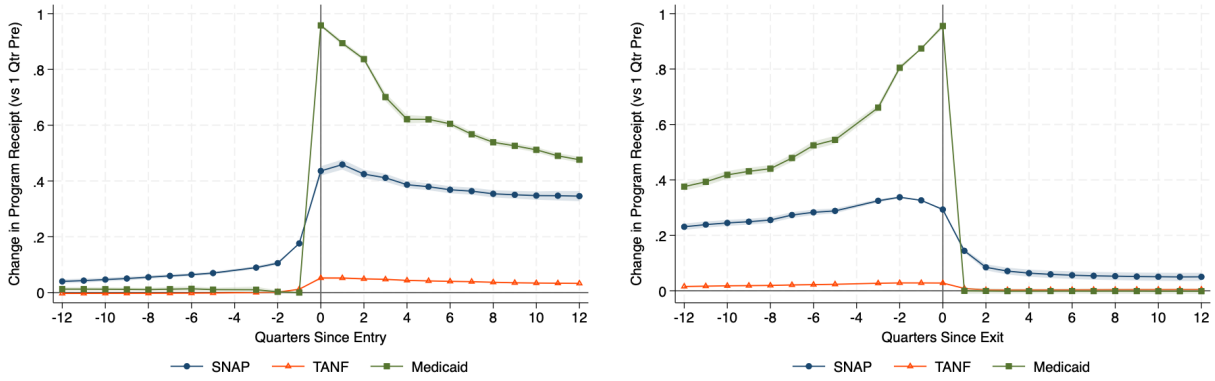
(a) SNAP entry

(b) SNAP exit



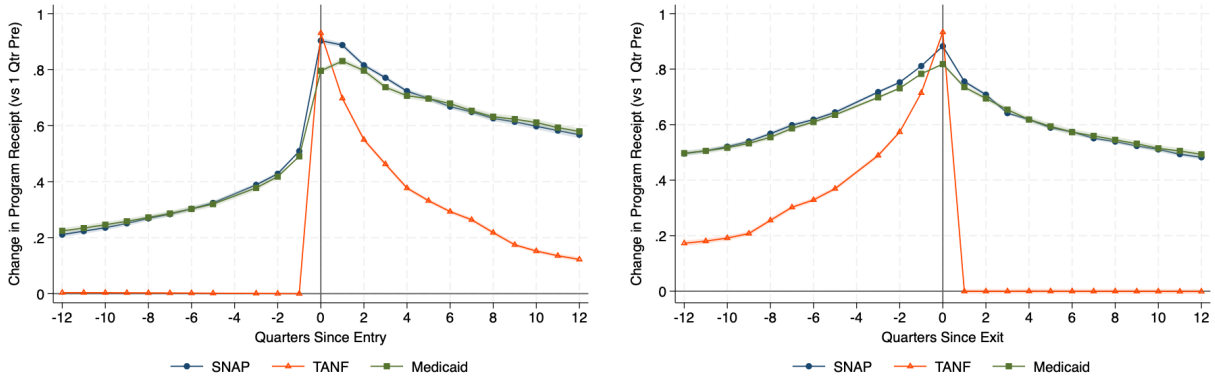
(c) Medicaid entry

(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.

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.

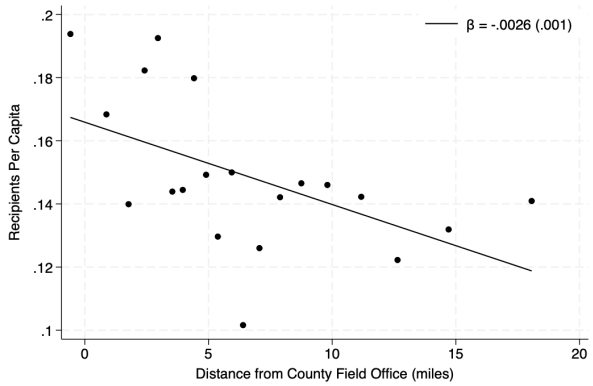
On the exit margin, we continue to see inflection points in the receipt of other programs around exit out of a given program. Panel (f) shows that those exiting TANF experience a steady, though not sharp, decline in the receipt of SNAP and Medicaid. Indeed, half of TANF exiters continue to receive these 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 multiple programs, suggesting that administrative burdens are spatially distributed.

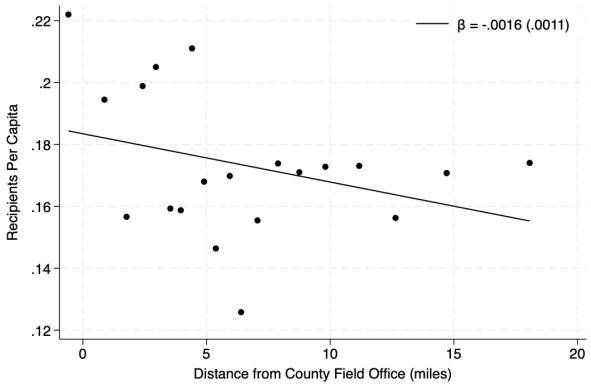
As alluded to in Section 2.2, prospective applicants residing in zip codes farther from their assigned LDSS office may face greater administrative burdens than more proximal residents.

Figure 4: 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.

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 Conceptual Framework and Empirical Strategy

5.1 Conceptual Framework: Distinguishing Burden Types

We develop a simple framework that distinguishes between two broad types of administrative burdens that can impede access to safety net programs: "geographic burdens" that increase with geographic distance from field offices and "fixed burdens" that all applicants face regardless of distance. This distinction helps illuminate the mechanisms through which burdens operate and how reforms like CommonHelp can ameliorate them. This framework in turn motivates different research designs to identify impacts along each burden type, collectively allowing us to assess CommonHelp's *total* effect on the population. Ignoring one burden type may severely understate the policy's total effect. More generally, our novel 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) so long as there are heterogeneous effects among different subpopulations (e.g., geographic areas).

Geographic Burdens. We define "geographic burdens" as costs to program enrollment that broadly depend on geographic distance to field office. Their existence is suggested by the negative association between geographic distance and program receipt highlighted in Fact 4. Residents of more distant zip codes likely face higher geographic burdens from the hassle and financial costs of traveling to their field office for in-person assistance and document submission. These residents also likely have reduced access to information and guidance, with fewer opportunities for face-to-face interactions with caseworkers who can help navigate complex eligibility rules and application procedures. Geographic burdens may also capture costs or barriers associated with the composition of populations living in more distant areas, such as differences in the salience of social stigma.

Since CommonHelp reduced the need for in-person field office visits and lowered information frictions, we expect that it delivered a larger "treatment dosage" to more distant zip codes with higher geographic 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. Consider first the scenario where CommonHelp only reduced geographic 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^g after its launch and further by β^{g15} after the 2015 upgrade. Assuming the difference in counterfactual log recipient counts between the distant versus close zip code is stable over time, then a time-averaged difference-in-differences (DD) strategy would identify the total effects of CommonHelp's launch and upgrade—illustrated by the full blue shaded region in Panel (a). If instead learning costs were present or CommonHelp spurred snowballing peer effects in program participation (Dahl et al. 2014), then Panel (b) shows that effects in the distant zip code would grow over time. Here, an event-study DD specification would appropriately capture these treatment effect dynamics. Section 5.2 discusses these research designs in greater depth.

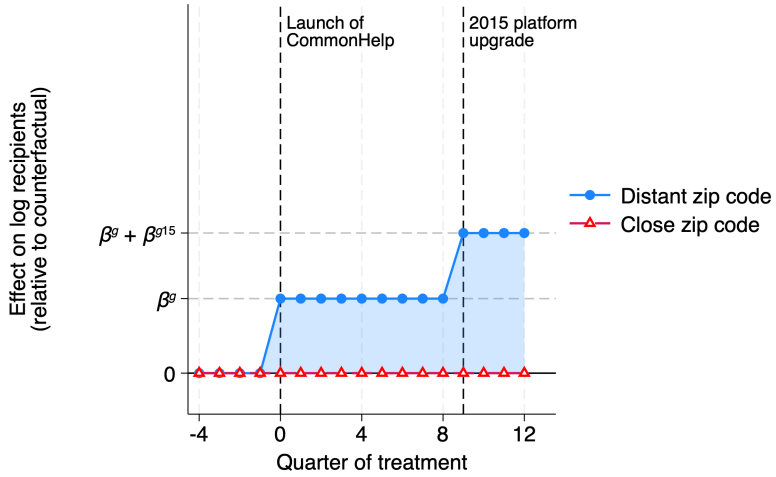
Fixed Burdens. We define "fixed burdens" as costs to program enrollment that are independent of geographic distance. These may include the social stigma associated with public, in-person office visits and caseworker interactions. They may also include the cognitive costs associated with completing complicated paperwork and time costs associated with office congestion and waiting periods. We hypothesize that CommonHelp also reduced fixed burdens by permitting virtual submission of applications (mitigating stigma) and substituting away from paper to online applications (reducing office congestion). Due to spillover effects from less crowded offices, CommonHelp may have increased 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^f and β^{f15} around CommonHelp's launch and upgrade, respectively. Critically, the DD strategy described earlier would *fail to identify* these effects, since it captures the *relative* effects on program receipt in distant zip codes and thereby purges the effects experienced by close zip codes. This would lead to an underestimate of the policy's total effect on recipient growth.

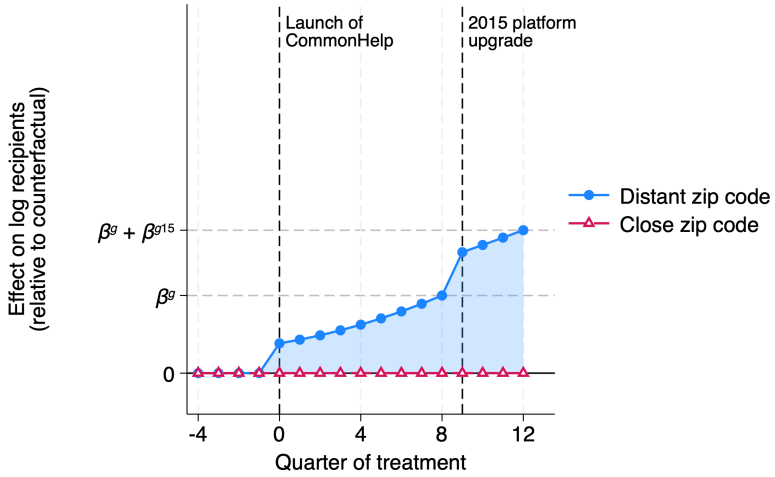
To identify the effects generated by reduced fixed burdens, we pursue an alternative identification strategy that is a variant of a regression discontinuity (RD) design among zip codes proximal to their field office. If CommonHelp's reductions in fixed burdens generated static effects in program receipt, then an RD design around CommonHelp's launch and 2015

Figure 5: Expected Impacts of CommonHelp on Program Receipt under Different Assumptions

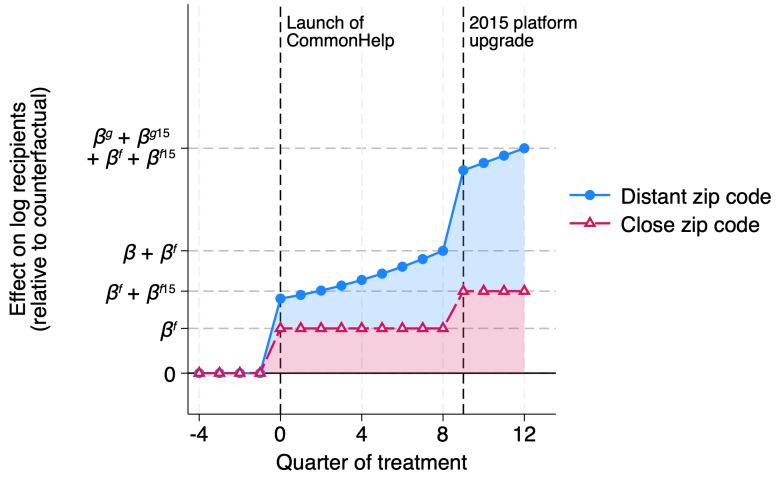
(a) Reduction in Geographic Burdens Only: Static Effects



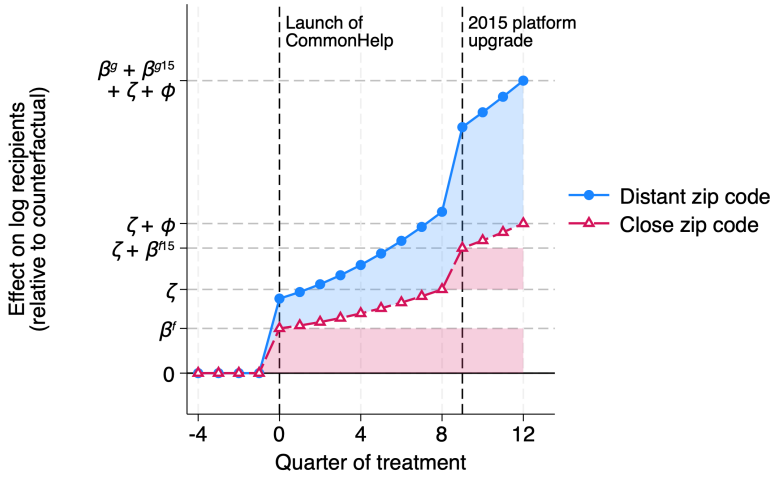
(b) Reduction in Geographic Burdens Only: Dynamic Effects



(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.

upgrade would—under appropriate smoothness conditions[9]—identify impacts from reducing fixed burdens, denoted β^f and β^{f15}. 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 across both close and distant zip codes.

There are several reasons why we should expect to observe enrollment impacts immediately after the implementation of CommonHelp. First, processing times for programs like SNAP, TANF, and Medicaid often have rapid turnarounds. In Virginia, SNAP mandates that applications be processed within 30 days, with expedited cases addressed within seven days. Similarly, TANF applications are required to be processed within 30 days, and Medicaid applications generally have a processing timeframe of up to 45 days (Virginia Department of Social Services 2017). In addition, benefits in Virginia are generally provided retroactively to the month of application for eligible households (Virginia Department of Social Services 2024). Therefore, even if applications were submitted and processed over several weeks following the launch of CommonHelp, the effective receipt of benefits would be backdated to the application date.

Nevertheless, it is plausible that CommonHelp's reduction in fixed burdens may have evolved over time. For example, if there is increasing substitution from paper to online applications over time, then the number of field office visitors may also decline over time. Panel (d) illustrates this scenario by allowing the red line to grow after CommonHelp's launch from β^f to ζ, and further increase from β^{f15} to ϕ after the 2015 upgrade. Since the RD designs zero in on the contemporaneous impacts of the platform's launch and upgrade (β^f and β^{f15}), we hypothesize they identify a *lower-bound* on the CommonHelp's effects on extensive-margin or multiple program receipt via fixed burdens ($\beta^f < \zeta$; $\beta^{f15} < \phi$).[10] This is depicted by the partially-full shaded red region below the red line. Section 5.3 describes our variant of the RD design in more detail.

Total Effects. In sum, our conceptual framework demonstrates that the identified parameters from the DD and RD-type designs ($\beta^g + \beta^{g15}$ and $\beta^f + \beta^{f15}$, respectively) provide the relevant ingredients for quantifying CommonHelp's total effect on recipient growth. Specifically, if β^g and β^{g15} are expressed as linear per-mile growth parameters and D_z denotes zip code z's distance from field office,[11] then the total growth experienced by z after Common-

9. As we discuss later, seasonality effects violate the smoothness conditions required for standard RD designs. We therefore pursue a "difference-in-RD" design.

10. Under a monotonicity assumption, CommonHelp can induce only inflows into extensive-margin (or "any") program receipt or multiple program receipt. In contrast, single program receipt can have both inflows and outflows, and thus cannot be bounded. We revisit this point in Section 6.3.

11. 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^g, β^{g15} are rescaled by $1/D_{\text{far}}$.

Help's launch and upgrade can be written as:

$$g_z \equiv (\beta^g + \beta^{g15}) \cdot D_z + (\beta^f + \beta^{f15}). \tag{1}$$

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 over the entire Virginia 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 Geographic Burdens

Our difference-in-differences (DD) design compares changes in program participation between zip codes located farther from field offices (where geographic burdens are higher) 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-study specification:[12]

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

Here, Y_{zt} is the number of recipients of a given combination of programs 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.[13] 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 those in households with children.

12. While we prefer employing a continuous "treatment" variable to exploit all variation in distance across zip codes, we employ a binarized DD design where "treated" and "control" zip codes fall above or below a certain distance threshold in our robustness checks.

13. 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.

Our main coefficients of interest in Equation (2) are $\{\beta_k^g \,|\, k \geq 0\}$, which measure CommonHelp's marginal effect (per mile distance) on program receipt growth 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^g represents a positively-weighted average of reducing geographic burdens incurred from an additional mile of distance over all zip codes (Callaway et al. 2024). Critically, this parameter captures effects *relative to* the most proximal zip codes, which may have also been impacted by CommonHelp. 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). Since CommonHelp was implemented as a single policy change rather than a staggered rollout, we do not face biases from differential treatment timing in staggered difference-in-differences designs (Goodman-Bacon 2021).

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 geographic burdens.[14] 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 may have affected the growth trajectories of program recipients in more or less distant zip codes. We probe these threats to identification in our robustness checks below. Moreover, the parameters $\{\beta_k^g \,|\, k < 0\}$ constitute our pre-trends test to help validate our parallel trends assumption. In particular, if there are no differential trends in program enrollment between more or less distant zip codes prior to CommonHelp, we expect $\beta_k^g = 0$ for $k < 0$.

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

$$\log(Y_{zt}) = \beta^g \cdot (D_z \times Post_t) + \beta^{g15} \cdot (D_z \times Post15_t) + \delta_z + \lambda_t + \boldsymbol{X}_{zt}\boldsymbol{\gamma} + \varepsilon_{zt}, \tag{3}$$

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^g and β^{g15} 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 geographic burdens.[15] 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 by race/ethnicity and gender. Nevertheless, all results hold without controls.

14. 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.

15. We also estimate a single pre-post variant of Equation (2) that identifies the time-averaged impact following CommonHelp's launch.

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.[16] We also restrict our sample to zip codes with at least five recipients on average prior to CommonHelp and weigh zip codes by these average enrollment counts.[17] 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

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, we pursue a difference-in-regression discontinuity (DRD) strategy. This research design compares the discontinuity at CommonHelp's launch and upgrade with those occurring in prior and subsequent years in order to isolate the causal effect of CommonHelp's implementation from broader seasonal and reporting-driven fluctuations in program participation. For example, seasonality in employment could drive seasonality in program receipt and states may align application processing, eligibility redeterminations, and policy adjustments with the federal fiscal calendar.[18] Such seasonality violates smoothness conditions around the cutoffs that are required in standard RD designs.

Operationally, the DRD identification involves (1) estimating an RD in program receipt around the launch or upgrade date (the "treatment windows"), (2) estimating the seasonality effect by conducting an RD around the corresponding cutoff months in other calendar years (the "control windows"), and (3) taking differences between both RD estimates ("treatment" minus "control"). Assuming seasonality patterns are additively separable and constant over time, the DRD purges these confounding seasonality patterns to isolate the CommonHelp's impact on program receipt from reducing fixed burdens.

Formally, let $Treat_t$ and $Treat15_t$ indicate the time windows around CommonHelp's launch and 2015 upgrade, respectively. Similarly, let $Control_t$ and $Control15_t$ indicate time windows in different years around the same cutoff month as the launch (October) and up-

16. By employing a log-linear specification, we require that zip codes grow at the same proportional rate. Thus, our 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.

17. 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.

18. In particular, Medicaid enrollment records are reported at the quarterly level in the Medicaid Statistical Information System (MSIS) files submitted to the federal government, and state administrative systems may similarly process eligibility updates or backlog clearances in quarterly cycles. Appendix Figure **[forthcoming]** shows Medicaid enrollment exhibits some degree of bunching within each quarter.

grade (January). Let our running variable t be normalized to zero at the cutoff within each treatment or control window. If we restrict our sample to zip codes with distance below some threshold $D_z < \bar{d}$, the DRD model is

$$\begin{aligned}\log(Y_{zt}) = &(\beta_1 \cdot \mathbb{1}\{t \geq 0\} + f_{1,pre}(t) \cdot \mathbb{1}\{t < 0\} + f_{1,post}(t) \cdot \mathbb{1}\{t \geq 0\}) \times Treat_t \\ &(\beta_0 \cdot \mathbb{1}\{t \geq 0\} + f_{0,pre}(t) \cdot \mathbb{1}\{t < 0\} + f_{0,post}(t) \cdot \mathbb{1}\{t \geq 0\}) \times Control_t + \epsilon_{zt}\end{aligned} \tag{4}$$

where $\{f_{k,pre}(t), f_{k,post}(t)\}_{k=0,1}$ are smooth functions of log program recipients on either side of the date cutoff. Here, $\beta^f \equiv \beta_1 - \beta_0$ is the impact of CommonHelp's launch on program enrollment growth from reducing fixed burdens; we denote β^{f15} as the analogous parameter around the upgrade, which is identified by modifying Equation (4) in the obvious way. These map precisely to the β^f, β^{f15} parameters labeled in the bottom panels of Figure 5. As discussed earlier, β^f and β^{f15} capture CommonHelp's *immediate* impact from reducing fixed burdens.

In our main analysis, we specify $f_0(\cdot)$ and $f_1(\cdot)$ as linear functions of t with varying slopes on either side of the cutoff, and limit our attention to a 6-month window around CommonHelp's launch and upgrade. To estimate seasonality effects, we pool all six possible control windows from our main analysis period (2009–2015) to maximize precision. For example, to isolate the seasonality component of CommonHelp's launch, we use a 6-month window around October of 2009–11 and 2013–15.[19] As usual, we weigh zip codes by their mean enrollment levels prior to CommonHelp and cluster standard errors at the county level. 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.

The ideal data for identifying this parameter would be daily address-level receipt. Daily data would provide an (approximately) continuous running variable, permitting us to estimate discontinuities precisely at the launch and upgrade cutoff dates. Address-level data would allow us to focus on locations that are immediately neighboring field offices so that $\bar{d} \approx 0$, ensuring we identify impacts from reducing *only* fixed burdens. Since we only possess monthly zip-level data, our DRD strategy may rely on observations too far from the cutoff and may pick up impacts driven by variable burden reductions. We take a number of measures to minimize these potential biases. First, we employ a nonparametric local linear regression model by applying a triangular kernel that gives greater weight to months closer to the cutoff dates. Second, we restrict our attention to only zip codes within $\bar{d} = 1$ mile of their field office. This distance threshold is roughly 10% of the mean and median zip code

19. There is a data quality break around January 2016 that prevents us from employing 2016 as a control window for the upgrade. RD estimates around this cutoff are negative—just like the 2009–2014 control windows—but of substantially larger magnitude, which would inflate our DRD estimates.

distance to field office and limits our analysis sample to just 51 zip codes—4.7% of all zip codes in our sample—that are distributed over 42 counties. We revisit whether our results are sensitive to alternative specification choices in our robustness checks, such as alternative choices of $\bar{d}$.

6 Impacts on Safety Net Program Participation

6.1 Effect Estimates from Reduced Geographic Burdens

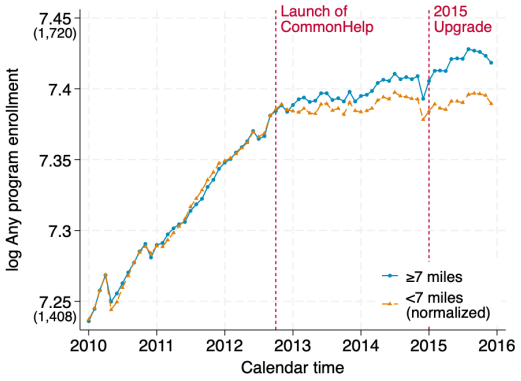
Visual Evidence. We begin by presenting visual evidence that CommonHelp impacted participation in social safety net programs in zip codes that were relatively farther from their assigned LDSS office. The left panels of Figure 6 illustrate raw trends in the log number of program recipients between zip codes that were more versus less than 7 miles away from their office.[20] 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 6 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. As we will show in Section 6.3, CommonHelp's reductions in geographic burdens at aggregate brought in tens of thousands of new recipients. 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.

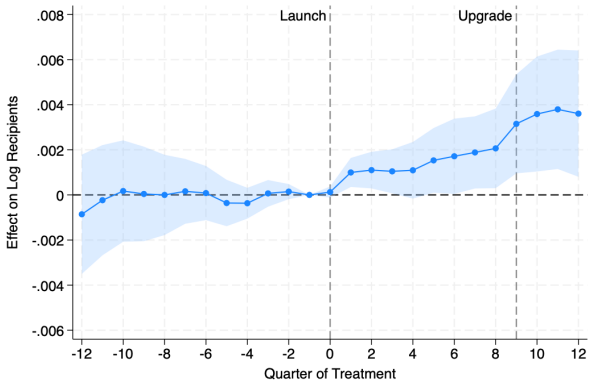
20. Appendix Figure A.5 illustrate raw trends using 5 and 9 miles as distance thresholds.

Figure 6: Raw Trends and Event-Study Estimates Before and After CommonHelp's Launch

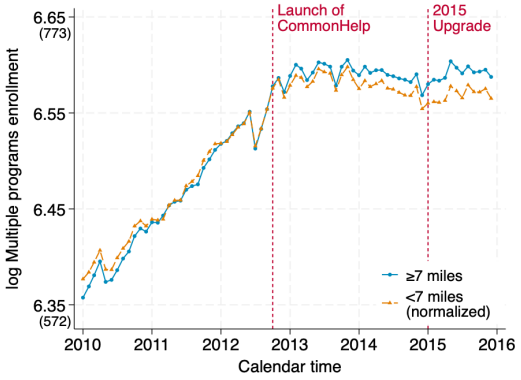
(a) Any program: Raw trends



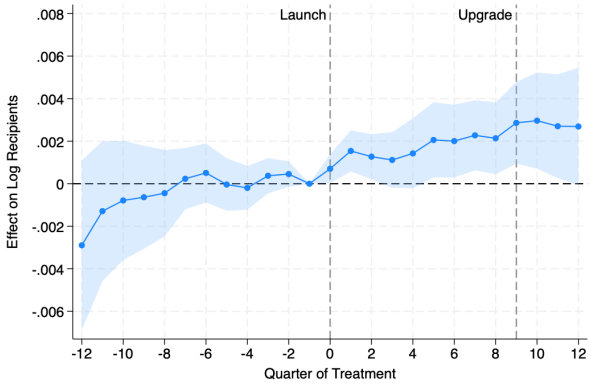
(b) Any program: Event study



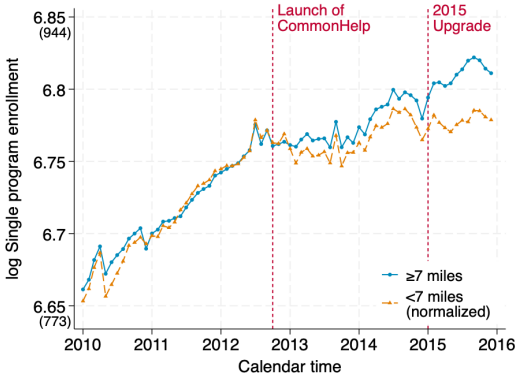
(c) Multiple programs: Raw trends



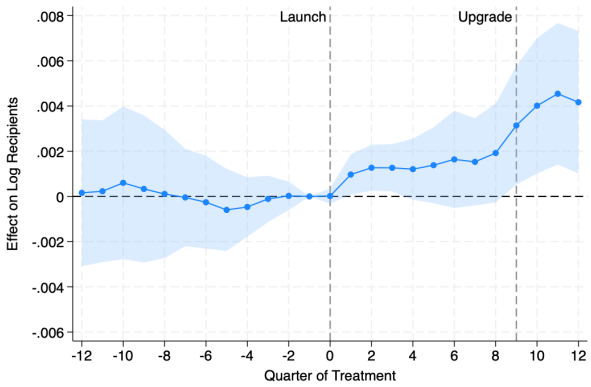
(d) Multiple programs: Event study



(e) Single program: Raw trends



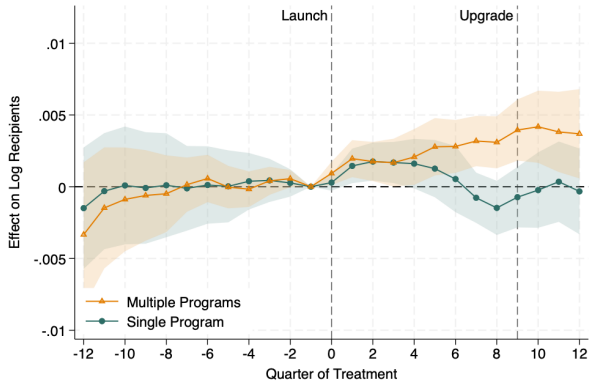
(f) Single program: Event study



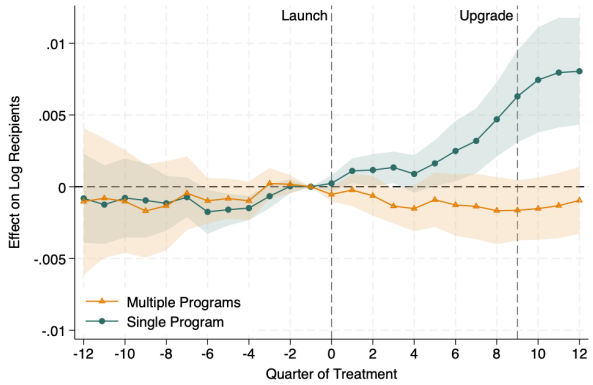
Notes: Left panels plot raw monthly trends in log program enrollment for the average zip code based on their distance to their assign county field office. The level of zip codes less than 7 miles away are normalized to the level of zip codes above 7 miles away in September 2012. Right panels illustrate estimates of $\{\beta_k^g\}$ 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.

Figure 7: 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.

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 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 7 reveals striking heterogeneity in CommonHelp's impact when comparing households with and without 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, 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^g and β^{g15} 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

Table 2: CommonHelp's Effects of Reducing Geographic Burdens on Program Receipt

	(1)	(2)	(3)
	All	With children	No children
A. Any program			
Overall effect (%/10 miles)	2.553**	2.103**	3.085***
	(1.011)	(1.028)	(1.093)
Launch effect (%/10 miles)	1.392	1.199	1.745*
	(0.853)	(0.930)	(0.931)
Upgrade effect (%/10 miles)	3.779***	2.941**	4.361***
	(1.188)	(1.182)	(0.983)
Outcome mean (count/zip)	801.3	521.7	227.4
B. Multiple programs			
Overall effect (%/10 miles)	2.482**	3.304**	−0.192
	(1.253)	(1.435)	(1.093)
Launch effect (%/10 miles)	1.688	2.324*	−0.321
	(1.139)	(1.327)	(0.979)
Upgrade effect (%/10 miles)	2.583**	3.190***	0.418
	(1.012)	(1.089)	(0.960)
Outcome mean (count/zip)	368.1	273.9	67.1
C. Single program			
Overall effect (%/10 miles)	2.616***	0.525	4.509***
	(0.972)	(1.002)	(1.190)
Launch effect (%/10 miles)	1.355*	0.261	2.651***
	(0.758)	(0.830)	(1.010)
Upgrade effect (%/10 miles)	4.103***	0.859	6.045***
	(1.270)	(1.391)	(1.202)
Number of zip codes	1,555	1,555	1,555
Number of counties	134	134	134

Notes: This table reports estimates of different version of Equation (3), 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

nearly identical effect estimates; see Appendix Figure A.8.[21] 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

21. 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.

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 and adjusting measurement of distance of zip codes containing field offices, indicating that the closest or furthest zip codes do not drive our effect estimates. Our results are also robust to alternate measures of distance, including travel distances from driving or public transit. All results continue to hold when using binarized treatment variables based on binarized distance thresholds where "treatment" is redefined as distance of at least 5, 7, and 9 miles from their field office; 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.

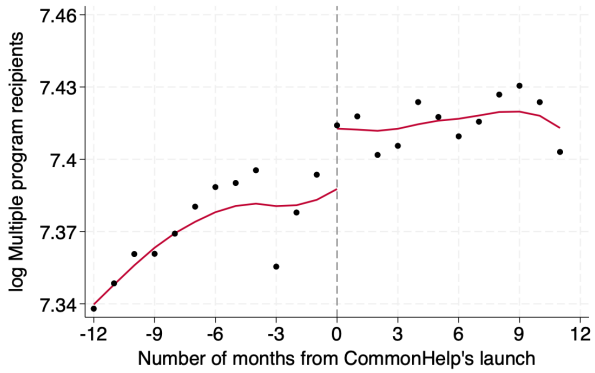
6.2 Impacts from Reduced Fixed Burdens

Visual Evidence. The top panels of Figure 8 illustrate discontinuities in multiple program receipt around CommonHelp's launch and 2015 upgrade. As mentioned, this figure and the ensuing analysis limits our sample to the 51 zip codes within $\bar{d} = 1$ mile of their field office. The black dots are raw monthly log multiple program recipient counts, pooled over these zip codes, that visualize the underlying noise in the data. The red lines are nonparametric fits separately estimated on each side of the cutoff dates. These illustrate the trends in the data and magnitude of discontinuities that would be identified by traditional RD.

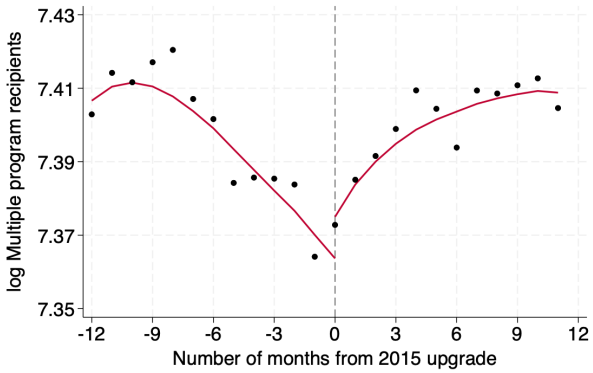
Enrollment in multiple programs appears to jump to the right of the cutoffs by roughly 1–3%. This suggests that CommonHelp's launch and upgrade immediately impacted multiple program receipt in close zip codes, but it is possible these are an artifact of seasonality patterns. To check this, the middle panels replicate the same analysis one year *prior* to the launch and upgrade dates. Some discontinuities are indeed present, indicating that applying RD in the top panels would confound CommonHelp's impacts from reducing fixed burdens with seasonality effects. Specifically, seasonality appears to inflate the discontinuity around CommonHelp's launch; in contrast, it generates downward bias around CommonHelp's upgrade. The bottom panels plot the RD estimates (based on our preferred specification) that correspond to the top and middle panels, along with all other possible control windows. The RD estimates in the control windows (hollow blue triangles) systematically differ from the main treatment window RD estimate (red circles) and are relatively similar in magnitude, indicating that seasonality is a persistent feature in the data. The DRD design differences the pooled estimate from control window RDs from the treatment window RD estimate

Figure 8: Visualization of Difference-in-Discontinuity:
Impacts on Multiple Program Receipt from Reducing Fixed Burdens

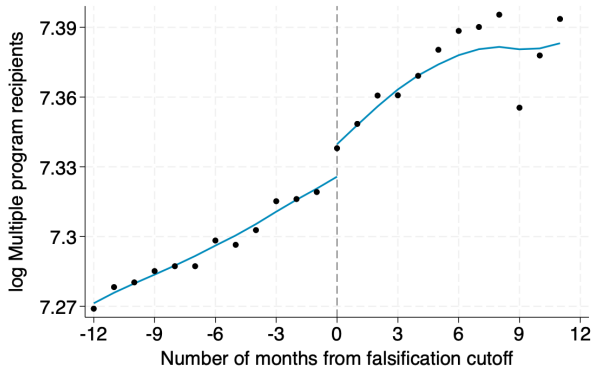
(a) Launch treatment window: October 2012



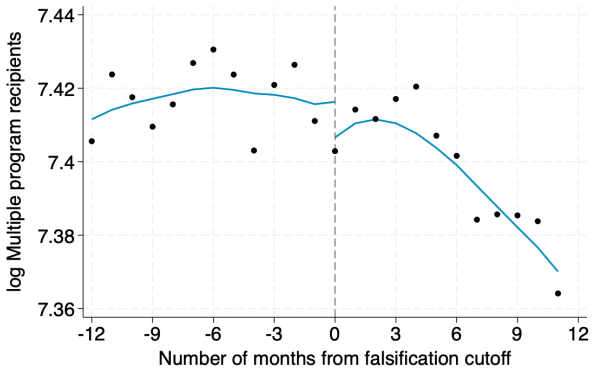
(b) Upgrade treatment window: January 2015



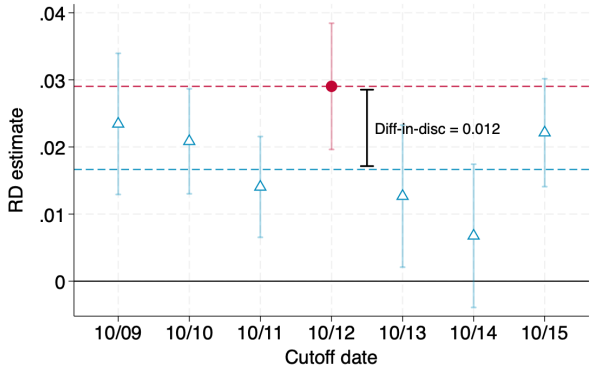
(c) Launch control window: October 2011



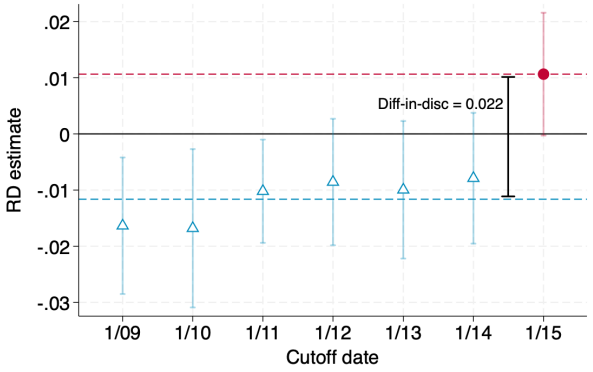
(d) Upgrade control window: January 2014



(e) Launch RD and DRD estimates



(f) Upgrade RD and DRD estimates



Notes: The top and middle panels plot raw monthly enrollment (black dots) and local linear fits (red and blue lines) around different windows. Bottom panels plot RD estimates in each estimation window between 2009–2015 and notes the corresponding DRD estimate. These estimates are based on local linear regressions with a 6-month bandwidth and triangular kernel around the cutoffs. Red circular markers and red dashed lines correspond to estimates from the treatment windows; the blue dashed line corresponds to the average estimates from the control windows. Left (right) panels correspond to windows around the launch (upgrade) cutoff. Sample is restricted to zip codes within $\bar{d} = 1$ mile of their field office. All estimates are weighted by zip code pre-recipient counts and (inverse) distance from field office.

to purge these confounding effects, indicating the multiple program receipt impacts from reducing fixed burdens range between 1.2-2.2%.

Appendix Figures [TBD] and [TBD] provide analogous graphical results for any program receipt and single program receipt. For the sake of brevity, we discuss these outcomes in our formal regression results.

Discontinuity Estimates. Table 3 reports DRD estimates on the effects from reducing fixed burdens on any program, multiple programs, and single program caseload growth. To summarize impacts, the first rows of each panel report the overall effect that averages the effects from the launch combined with the upgrade between October 2012–December 2015. Our results indicate that CommonHelp's reductions in fixed burdens promoted program receipt, but particularly in multiple programs. Focusing on Column (1), CommonHelp increased overall program receipt in the population by 0.72%; this impact is driven by a large and statistically-significant 1.92% gain in multiple program receipt. As evident from Figure 8, increases in receipt was spurred by the launch and further amplified by the upgrade. In contrast, Panel C reveals that CommonHelp's reductions in fixed burdens caused a modest though statistically insignificant *decline* in *single* program receipt.

Column (2) reports that CommonHelp's impacts from reducing fixed burdens were concentrated among households with children. Taken together, CommonHelp's launch and upgrade led to a nearly 2.3% increase in multiple program receipt, consistent with the fact that these households are typically eligible for multiple programs. Panel C shows that the modest decline in single program receipt was driven by a large and significant 1.2% decline among this group. This provides suggestive evidence that CommonHelp induced *intensive-margin* responses in safety net participation: some households with children who would have received a single program, absent CommonHelp, instead opted to receive *multiple* programs simultaneously. These findings suggest that fixed burdens, driven from office congestion, social stigma, and cognitive burdens from the paper-form application may pose barriers as significant as distance-related burdens to low-income households with children.

Finally, Column (3) turns to impacts among households without children. Overall, we find CommonHelp led to small, statistically insignificant program growth over our analysis period. Interestingly, we find the 2015 upgrade led to substantial growth in program receipt—including multiple program receipt. This contrasts with our DD results. To better understand this result, Appendix Table **[insert here]** further examines heterogeneity by age and finds most of the increase in multiple program receipt is driven by those over age 50. This group likely captures individuals with disabilities and the elderly, who would particularly benefit from a fully remote application and supplemental document submission

Table 3: CommonHelp's Effects on Program Receipt from Reducing Fixed Burdens

	(1)	(2)	(3)
	All	With children	No children
A. Any program			
Overall effect (%)	0.716***	0.824***	0.287
	(0.212)	(0.278)	(0.476)
Launch effect (%)	0.501***	0.777***	−0.330
	(0.163)	(0.242)	(0.403)
Upgrade effect (%)	0.697**	0.152	2.006***
	(0.280)	(0.295)	(0.617)
Outcome mean (count/zip)	1,525.6	1,061.8	463.8
B. Multiple programs			
Overall effect (%)	1.924***	2.273***	0.186
	(0.551)	(0.673)	(0.675)
Launch effect (%)	1.239**	1.644**	−0.572
	(0.512)	(0.622)	(0.497)
Upgrade effect (%)	2.227***	2.045***	2.465**
	(0.463)	(0.409)	(1.122)
Outcome mean (count/zip)	676.0	545.2	130.7
C. Single program			
Overall effect (%)	−0.307	−1.195**	0.330
	(0.373)	(0.490)	(0.701)
Launch effect (%)	−0.202	−0.630	−0.282
	(0.313)	(0.467)	(0.609)
Upgrade effect (%)	−0.342	−1.836***	1.989**
	(0.423)	(0.499)	(0.795)
Outcome mean (count/zip)	834.2	501.3	332.9
Number of zip codes	51	51	51
Number of counties	42	42	42

Notes: This table reports DRD estimates of β^f and β^{f15} from Equation (4) along with their pooled estimate. All regressions employ local linear fits with a 6-month bandwidth, triangular kernels around the cutoff dates, use all treatment and control windows between 2009–2015, and weight zip-level observations by their outcome means measured prior to CommonHelp. Sample is restricted to zip codes within $\bar{d} = 1$ mile of field office. 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

processes. Note, however, that the baseline mean number of childless households on multiple programs is significantly smaller than that of households with children.

Robustness Checks. To increase confidence in our DRD design, the supplementary appendix reports a variety of falsification and specification checks, focusing on multiple pro-

gram receipt as a primary outcome. We begin by estimating DRD estimates at placebo cutoff months, where we construct 6-month treatment windows around the placebo month of the calendar year of the launch or upgrades and control windows around the same placebo month in the other six calendar years. This exercise checks whether the DRD might be driven by other factors such as month-to-month volatility in measured participation rates or confounding policy or economic changes in 2012 or 2015. Reassuringly, Appendix Figure A.12 finds that all of the placebo DRD estimates are statistically insignificant or of opposite sign, suggesting that such confounders are not driving our main results.

A central challenge of our DRD design is that identification of the launch and upgrade effects relies on valid identification of seasonality effects via RD. Reassuringly, the bottom panels of Figure 8 show that all control window RD estimates are statistically indistinguishable from their average, suggesting that the pooled estimate reliably captures seasonality effects. The magnitudes of the upgrade control window RD estimates are quite stable, while the launch estimates are somewhat more volatile, with a few statistically indistinguishable from the treatment window RD estimate.[22] This raises the concern that the main launch RD estimate is simply an anomalous seasonality effect.

To probe this issue, we estimate all possible placebo RDs between January 2009 and December 2015 to check these RD estimates' magnitude relative to the distribution of *all possible* placebo estimates. First, some of the largest placebo RD estimates are outside of October, suggesting some of the larger control RD estimates are not atypical in magnitude.[23] Moreover, Appendix Figure A.13 shows that the true launch estimate is larger than *all but one* placebo estimate (roughly 99%).[24] Thus, it is unlikely that the estimated RD's magnitude around the launch is due to random chance.

We also conduct a battery of specification checks to probe the robustness of our main results. Appendix Figure A.14(a) illustrates our effect estimates on multiple program receipt are robust to bandwidth selection. Panel (b) shows these estimates are highly robust to alternative distance thresholds $\bar{d}$, which defines our sample inclusion criteria. Unsurprisingly, estimates are slightly more unstable and more imprecise for $\bar{d} \in [0.4, 0.6]$, but the point estimates are remarkably similar to our preferred estimates at $\bar{d} = 1$. Next, Appendix Table A.1 reports additional checks, such as controlling for zip-level characteristics and specifying $\{f_{k,pre}(\cdot), f_{k,post}(\cdot)\}_{k=0,1}$ as parametric fits or higher-order local polynomials. To further guard

22. Note, however, that the control windows closest to the launch date (around October 2011 and 2013) are statistically significantly different from the main RD estimate.

23. More than half of the placebo cutoffs with larger RD estimates than the pooled control RD occur in February, March, August, and September.

24. The largest RD estimate occurs at September 2012—just one month before the launch date—so this result is likely influenced by the true launch effect.

against the possibility that our results are affected by CommonHelp's reduction in variable burdens, one of our checks additionally applies a kernel that weighs zip codes that are *closer* to their field office. Across all specifications, our results remain qualitatively unchanged.

Finally, it is worth emphasizing that while these DRD results focus on a highly selected sample—zip codes within 1 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.15 plots DRD estimates for zip codes by various distance bins. This provides a nonparametric way of estimating the *total* immediate impacts of CommonHelp's launch and upgrade on multiple program receipt, capturing effects from reducing both fixed and variable burdens borne from distance. The RD estimates tend to linearly increase in distance bins. In fact, the gradient of the launch DRD estimates in distance bins is close to the $t = 0$ event-study estimate from our DD analysis.[25] This further validates our collective set of identification strategies.

6.3 Summarizing CommonHelp's Total Effects

Having separately estimated CommonHelp's enrollment effects via geographic and fixed burdens, we now turn to assessing CommonHelp's total effects on the population. We draw on the DD and DRD estimates from Tables 2 and 3 to quantify a lower bound of CommonHelp's causal effect on the total zip-level growth rate in program receipt g_z, à la Equation (1). For the average zip code (mean distance $D_z = 9.95$ miles), we estimate that, between October 2012 and December 2015, CommonHelp increased any program participation by at least 3.26% on average.[26] Cumulating effects from the launch and upgrade by the end of 2015 amplifies the average zip-level growth rate to 6.35%.

CommonHelp had particularly prominent positive effects on multiple program participation along both geographic and fixed burdens. For the average zip code during the analysis period, we estimate that multiple program receipt grew on average by 4.37%. In contrast, CommonHelp appeared to *promote* greater single program receipt via geographic burdens but *discourage* single program receipt via fixed burdens. This suggests that CommonHelp promoted intensive-margin responses (single to multiple program receipt) among those in close proximity to their field office but extensive-margin responses (none to single program

25. Similarly, the upgrade DRD estimates fall within the 90% confidence interval of the *difference* between the event-study estimates corresponding to 2014Q4 and 2015Q1, which roughly captures discontinuities around January 2015. We do not need to take differences of event-study estimates when comparing with the launch DRD estimates gradient in distance since our event-study normalizes $t = -1$ to zero.

26. $\mathbb{E}[\hat{\beta}^g \cdot D_z + \hat{\beta}^f] = 0.255\%/\text{mile} \times 9.95 \text{ miles} + 0.716\% = 3.26\%$.

receipt) among those who faced greater geographic barriers to program entry. These competing forces aggregate to a modest 2.30% average growth in single program caseloads. The heterogeneity in the total effects on multiple versus single program receipt aligns with the nature of the CommonHelp. Streamlining multiple applications and re-certification processes onto an centralized online platform successfully encouraged multiple program receipt, often among eligible individuals previously enrolling in a single program.

How many individuals did CommonHelp bring onto program rolls? Appendix B.1 shows that we can convert estimates of the total growth effect g_z, which represents *percent* changes at the *zip*-level, into the total 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 reports estimates of ΔY for different choices of g_z in the odd-numbered columns. Overall, when averaging impacts on both geographic *and* fixed burdens from its launch and upgrade between October 2012–December 2015 (Panel A), CommonHelp brought nearly 90,000 new program recipients on average during this period. Summing both launch and upgrade effects together (Panel C) implies about 161,000 more individuals received programs by the end of 2015, indicating reduced geographic and fixed burdens induced by the platform's upgrade drove a significant share of caseload growth. Much of this growth appears to be driven by multiple program recipients. 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 90,000 recipients corresponds to roughly an overall 2.31% growth in recipient caseloads, likely driven by a 1.58 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 geographic or fixed burdens. Row 2 of each panel, Column (1), find that excluding impacts via fixed burdens would underestimate the overall number of recipients by roughly 30–36%, 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. By extension, Row 3 reports that excluding impacts via geographic burdens results in opposite biased lower bound estimates; in fact, it results in a sign flip for single program recipients. 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.

Table 4: Total Effects of CommonHelp on Program Enrollment

	(1)	(2)	(3)	(4)	(5)	(6)
	Any		Multiple		Single	
	ΔY	p.p.	ΔY	p.p.	ΔY	p.p.
A. Overall effects						
1. Total: $g_z = \bar{\beta}^g \cdot D_z + \bar{\beta}^f$	**89,813**	2.305	**61,690**	1.583	**28,239**	0.725
2. Geographic only: $g_z = \bar{\beta}^g \cdot D_z$	63,005	1.617	28,152	0.723	34,270	0.880
3. Fixed only: $g_z = \bar{\beta}^f$	27,690	0.711	34,579	0.888	−6,240	−0.160
B. Launch effects only						
1. Total: $g_z = \beta^g \cdot D_z + \beta^f$	**53,837**	1.382	**41,244**	1.059	**13,945**	0.358
2. Geographic only: $g_z = \beta^g \cdot D_z$	34,743	0.892	19,294	0.495	17,972	0.461
3. Fixed only: $g_z = \beta^f$	19,438	0.499	22,415	0.575	−4,099	−0.105
C. Launch and upgrade effects						
1. Total: $g_z = (\beta^g + \beta^{g15}) \cdot D_z + (\beta^f + \beta^{f15})$	**161,001**	3.518	**90,823**	1.985	**56,923**	1.244
2. Geographic only: $g_z = (\beta^g + \beta^{g15}) \cdot D_z$	109,346	2.389	29,936	0.654	70,757	1.546
3. Fixed only: $g_z = \beta^f + \beta^{f15}$	54,184	1.184	62,836	1.373	−14,594	−0.319
Outcome mean (count/zip)	801.3		368.1		423.6	
Number of zip codes	1,555		1,555		1,555	
Number of counties	134		134		134	
Share of recipients (%)	100.0		47.0		52.0	

Notes: Odd-numbered columns report estimates of the effects of CommonHelp on the number of program recipients ΔY (defined in Equation (5) under different choices of the total growth parameter g_z (defined in Equation (1)). Bold-faced estimates highlight the total effect estimates from CommonHelp's launch and upgrade via geographic 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. **(Note: standard errors for Panel B's estimates are in progress.)**

Appendix Tables A.2 and A.3 replicate this analysis for households with and without children. Multiple program receipt grows *more* for recipients in households with children than *any* program receipt while single program receipt overall *declines*. This indicates that CommonHelp generated clear intensive-margin movements among this subpopulation. We revisit this with a more formal analysis in Section 8. In contrast, among households with no children, over 90% of overall program receipt appears to be driven by single program receipt.

As mentioned in our conceptual framework, these total effect estimates for any and multiple program receipt likely represent lower bounds if CommonHelp's reduction in fixed burdens generate dynamic effects over time. It is difficult, however, to interpret the total effects on single program receipt as bounds given that CommonHelp generates non-monotone responses with respect to this choice margin. In particular, evolving fixed burdens can result in greater intensive-margin responses, causing *lower* single program receipt over time than what is implied by our DRD analysis. Thus, our results on single program receipt are

valid under the assumption that CommonHelp's effects via fixed burdens is static over time (as illustrated in Figure 5((c)). Appendix **[TBD]** critically examines the sensitivity of our conclusions to alternative assumptions about the dynamic effects of fixed burden reductions.

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 more precisely proxy different dimensions of well-being or potential self-sufficiency or ability

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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

(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.[27] 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 program receipt. Quarterly wages among recipients measured in the three years leading up

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

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.

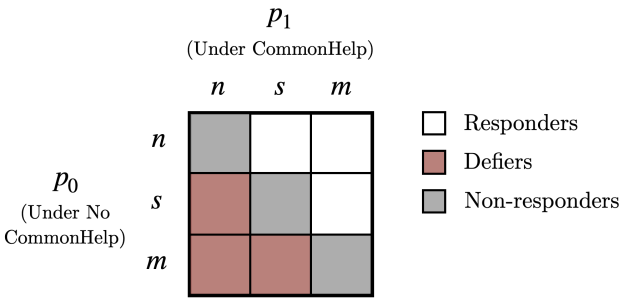
Panel B reveals that CommonHelp reduced the share of multiple program recipients with criminal histories, particularly for violent crimes (decreasing by 0.114 percentage points per mile on a base mean of 0.011). This suggests that the reform was particularly effective at reaching families without criminal 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, as measured by the number of parents or 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 counterfactual* 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

Figure 9: Permissible responder types under the monotonicity assumption



Notes: See main text for details.

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, monotonicity restricts behavioral responses to CommonHelp to be one of the following groups:[28]

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

28. 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.

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 $p \in \mathcal{P}$ 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.[29] 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.

To complement the traditional targeting analysis in Section 7, we focus on the subpopulation of households with children. Table 6 utilizes the percentage-point effect estimates on single and multiple recipient shares from Appendix Table A.2—which documents total effect estimates among households with children—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 (see Column (3)). In other words, we can definitively conclude that CommonHelp induced individuals to participate in *additional* safety net programs than they would have otherwise received. Based on the overall effects in the analysis period, the share of these responders ranges between 0.27–1.54 percentage points; the 2015 upgrade augmented these responses to 0.67–1.61 percentage points. This implies that intensive-margin multiple program recipients represent between 17–55% (on average) and 42–63% (after 2015)

29. 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 among Households with Children

	(1)	(2)	(3)
	$\omega_{(n,s)}$	$\omega_{(n,m)}$	$\omega_{(s,m)}$
Overall effects (p.p.)	[0, 1.272]	[0, 1.272]	[0.267, 1.539]
Assuming $\omega_{(n,m)} = 0$	1.272	0	1.539
Assuming $\omega_{(n,s)} = 0$	0	1.272	0.267
Launch effects only (p.p.)	[0, 0.970]	[0, 0.970]	[0.143, 1.113]
Assuming $\omega_{(n,m)} = 0$	0.970	0	1.113
Assuming $\omega_{(n,s)} = 0$	0	0.970	0.143
Launch and upgrade effects (p.p.)	[0, 0.937]	[0, 0.937]	[0.671, 1.608]
Assuming $\omega_{(n,m)} = 0$	0.937	0	1.608
Assuming $\omega_{(n,s)} = 0$	0	0.937	0.671

Notes: This table reports the bounds defined in Equation (8) based on the estimates reported in the even-numbered columns of Appendix Table A.2, Row 1. Each row also reports values of each share under different assumptions.

of all recipients who responded to CommonHelp.[30] This reveals that technological platforms like CommonHelp can help close the incomplete take-up rates among existing safety net recipients illustrated in Figure 2(a).

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 such 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 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 geographic

30. 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 of $\omega_{(s,m)}$ based on the overall effects is $0.267 \div (0.267 + 1.272) \times 100\% \approx 17\%$.

burdens 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 2.6% and multiple program receipt by 2.5%. Among zip codes very close to their field office, CommonHelp's reduction in fixed burdens led to a 0.7% increase in overall program receipt and a 1.9% increase in multiple program receipt. Crucially, the gains in multiple program participation were almost entirely concentrated among households with children. Coupling these estimated impacts of reducing geographic and fixed burdens, we find that CommonHelp generated nearly 90,000 new recipients and 62,000 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 responders 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.

Looking ahead, our current research setting provides a unique opportunity to explore a range of downstream effects from increased access to multiple programs. Future work will examine how CommonHelp's reduction in administrative burdens translates into changes in measures of self-sufficiency, including employment, earnings, and duration of reliance on safety net programs. We also plan to investigate interactions with the criminal justice system, analyzing effects on offense rates, types of offenses, and incarceration sentences. Additionally, we intend to assess longer-run outcomes for children in affected households. These analyses will enhance our understanding of how access to multiple programs can shape broader socioeconomic trajectories and intergenerational outcomes, providing valuable insights for evaluating the modern social safety net.

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

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January 2025

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A Supplemental Figures and Tables

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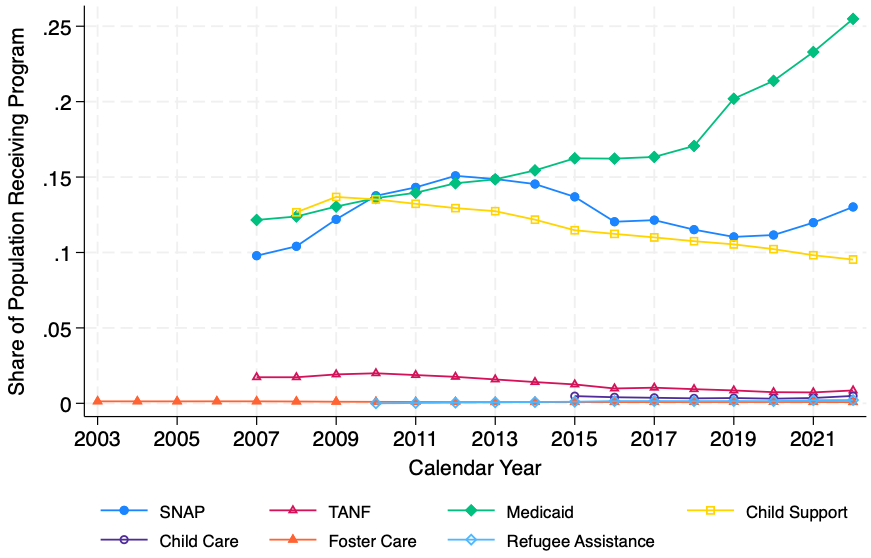
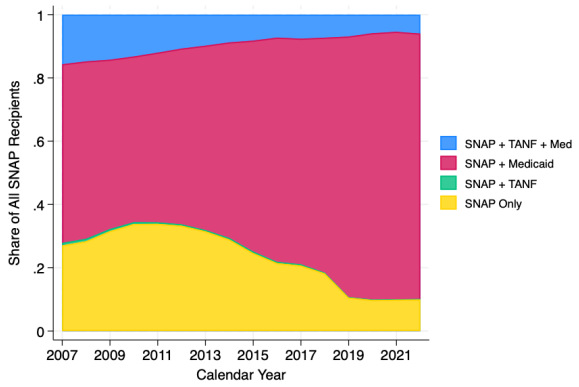
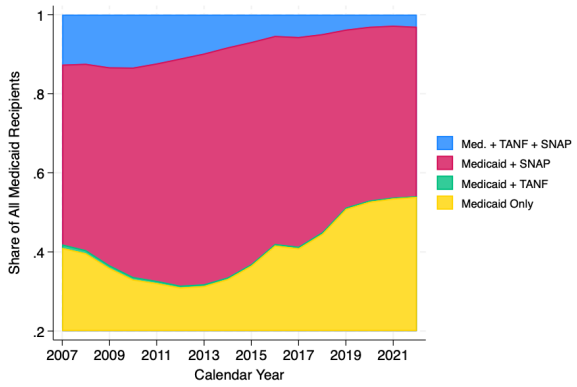


Figure A.2: Participation Conditional on Receipt of Given Program

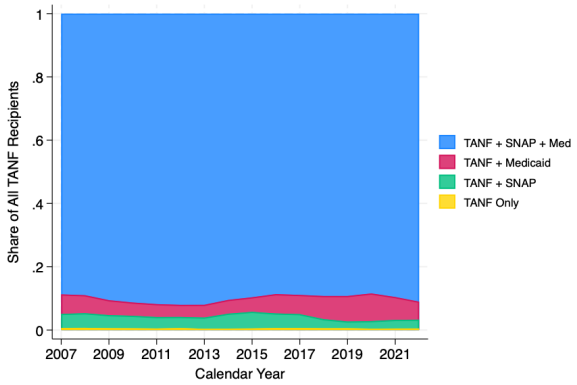
(a) Conditional on SNAP Receipt



(b) Conditional on Medicaid Receipt



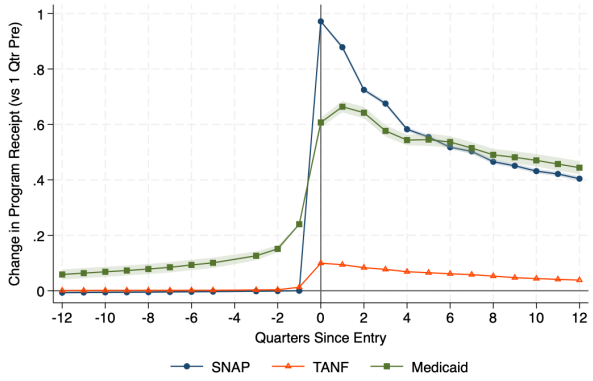
(c) Conditional on TANF Receipt



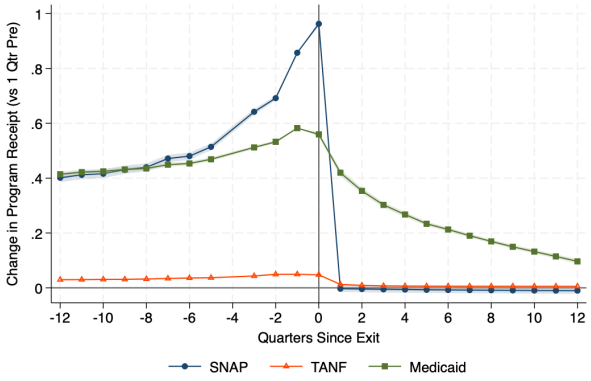
Notes: This figure reports shares of the population receiving each program bundle conditional on receiving a given program.

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

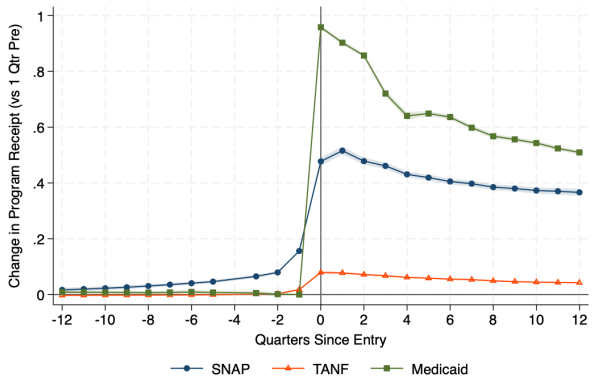
(a) SNAP entry



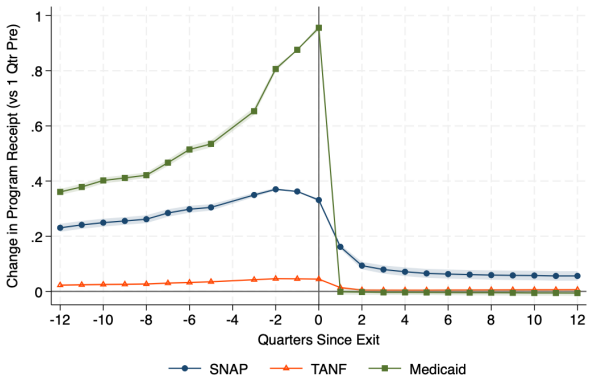
(b) SNAP exit



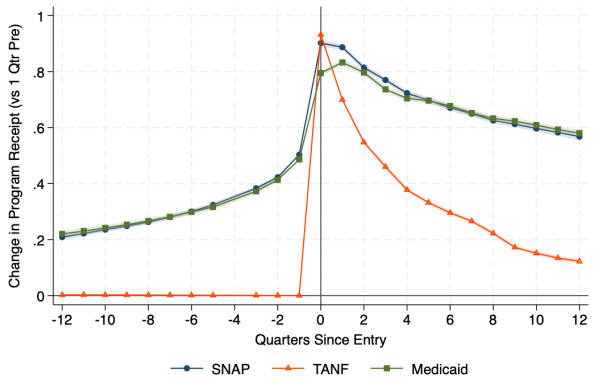
(c) Medicaid entry



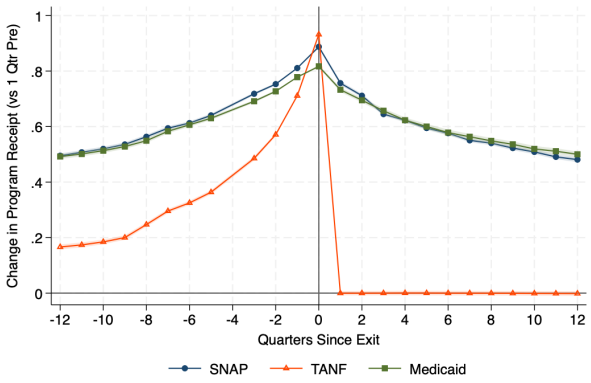
(d) Medicaid exit



(e) TANF entry



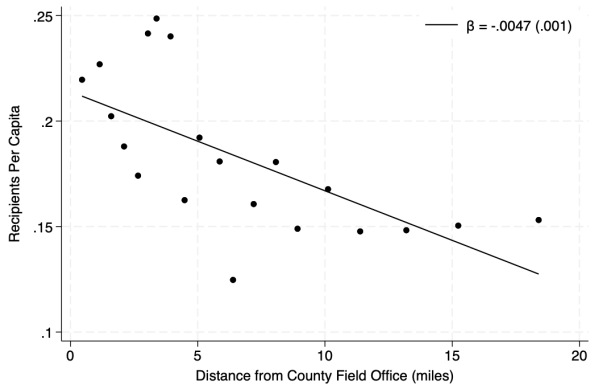
(f) TANF exit



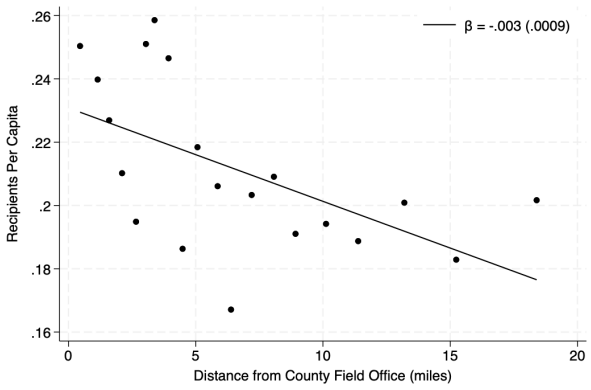
Notes: These figures plot event-study estimates of each program receipt on program entrants and exiters, focusing on individuals in households with children. Left panels condition on individuals who enter a given program and did not receive the program in the prior 12 quarters. Right panels condition 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



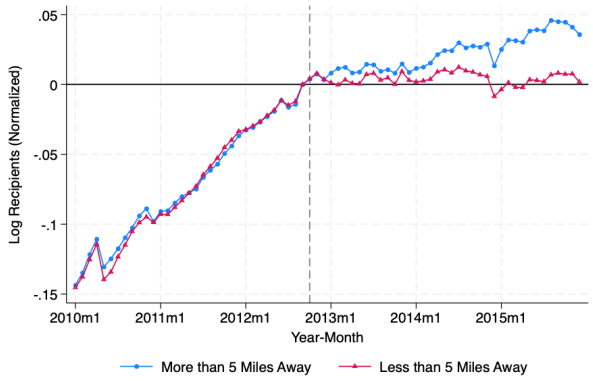
(b) Single Program Receipt



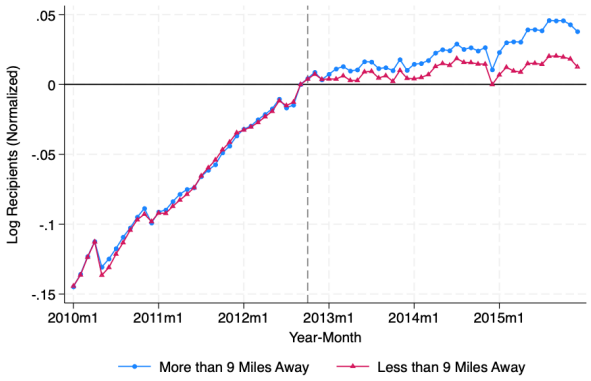
Notes: This figure illustrates the association between zip-level single and multiple program participation rates and zip code distance to their assigned county field office using monthly data between January 2007 and December 2022. Participation rates are expressed in per capita terms using population 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

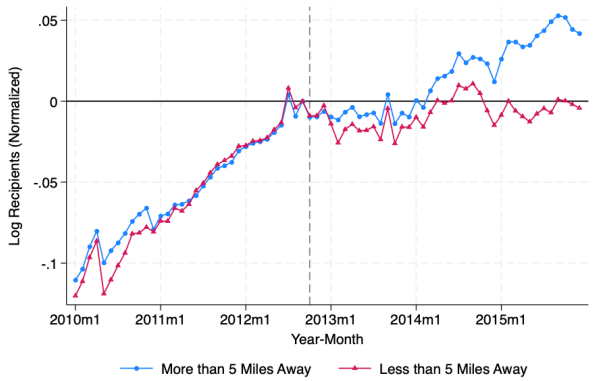
(a) Any program, 5 miles



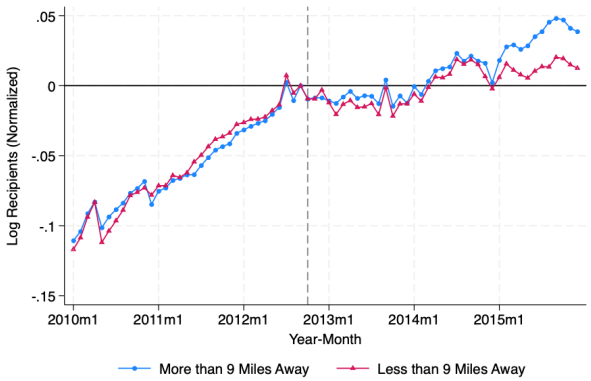
(b) Any program, 9 miles



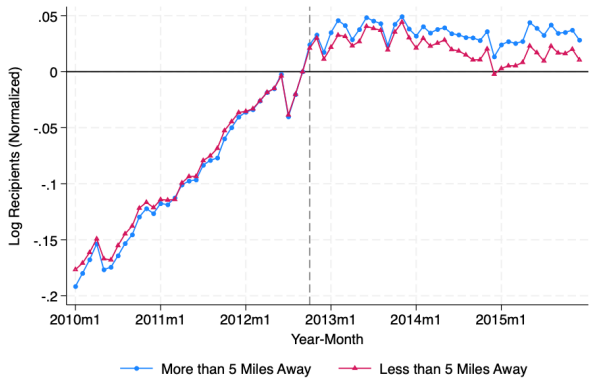
(c) One program, 5 miles



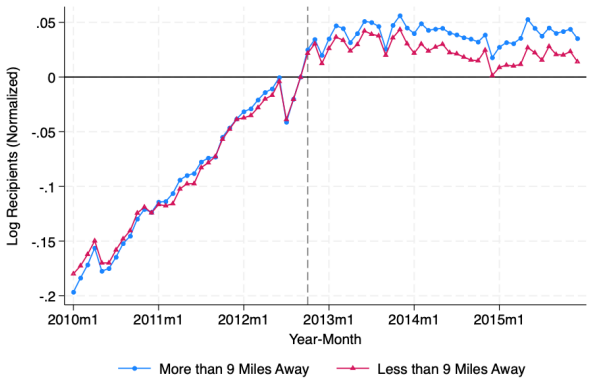
(d) One program, 9 miles



(e) Multiple programs, 5 miles



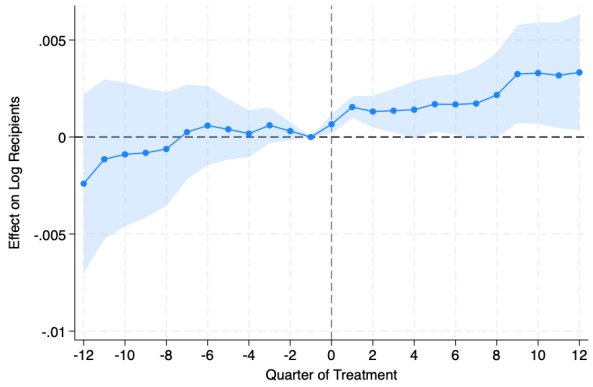
(f) Multiple programs, 9 miles



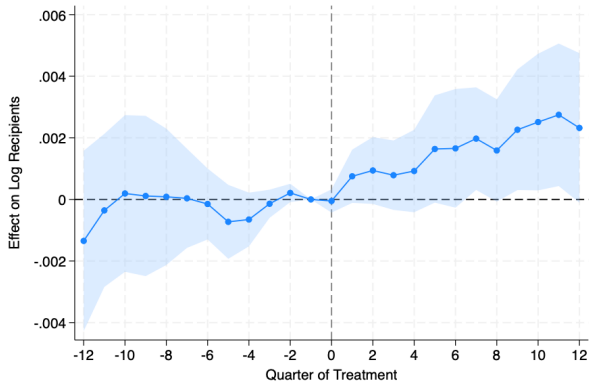
Notes: This figure plots raw monthly trends in log program enrollment for the average zip code based on their distance to their assign county field office. Levels are normalized to zero in September 2012. Zip codes are weighted by their average program recipient count between October 2010–September 2012.

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

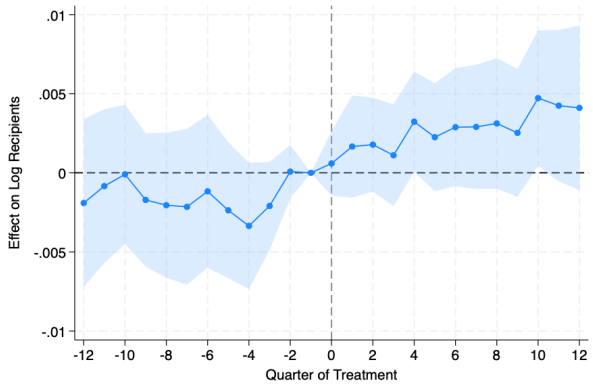
(a) SNAP



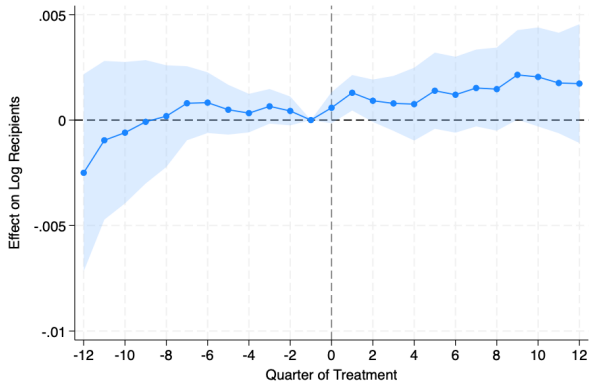
(b) Medicaid



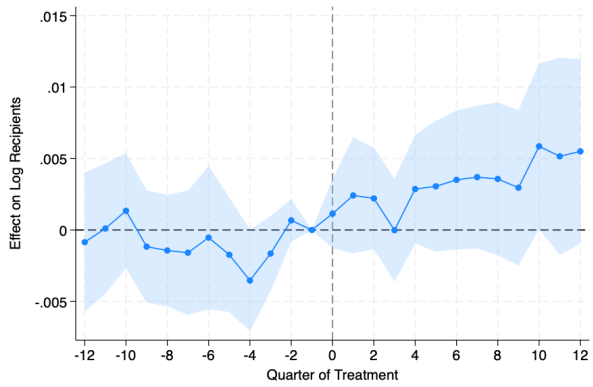
(c) TANF



(d) SNAP and Medicaid



(e) SNAP, Medicaid, and TANF



Notes: These plots illustrate estimates of $\{\beta_k^g\}$ 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

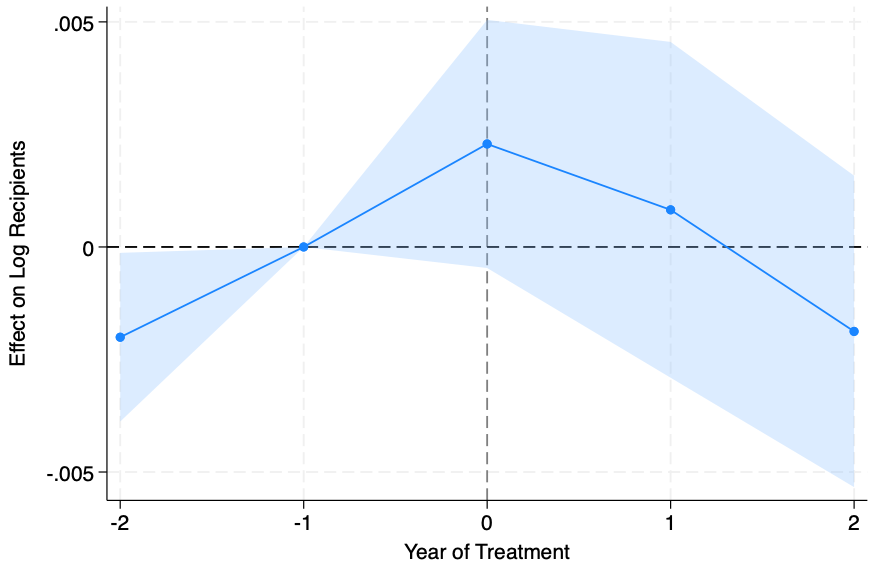


Figure A.8: Robustness checks: Sensitivity of DD Estimates to Controls

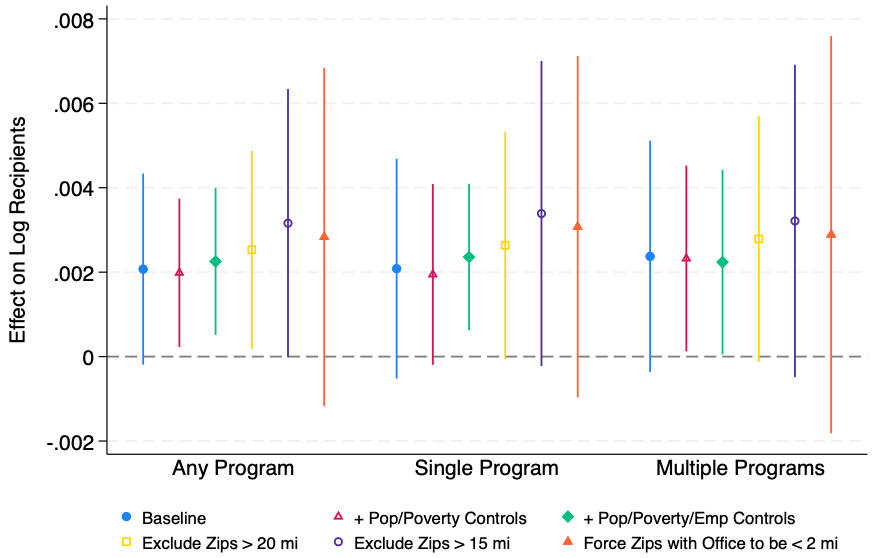
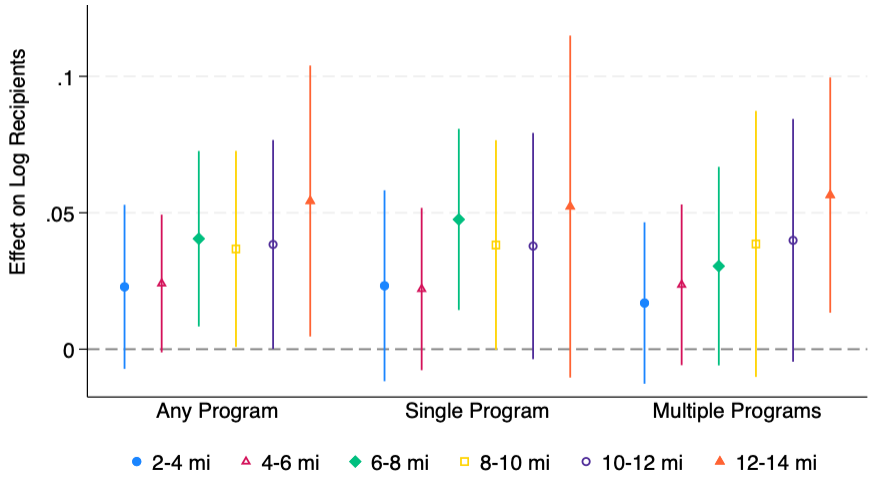


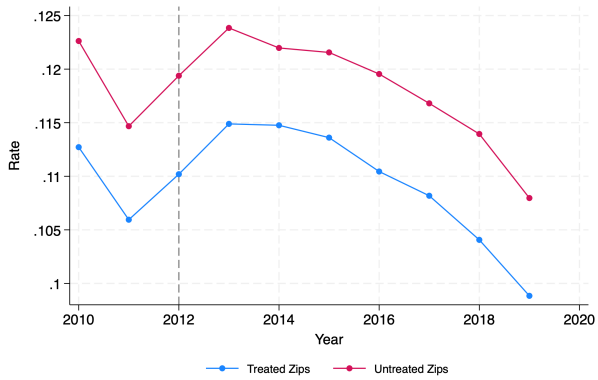
Figure A.9: CommonHelp's impact on program receipt increases in distance to field office



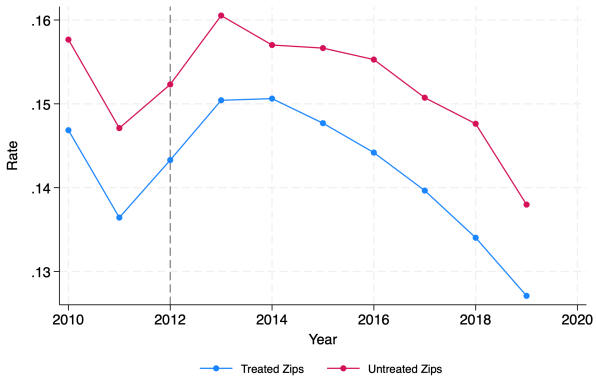
Notes: This figure reports estimates of β^g based on a pre-post version of Equation (2) in which D_z is replaced with $\mathbb{1}\{D_z \in [d_0, d_1)\}$ that is estimated using zip codes with $D_z < 2$ or $D_z \in [d_0, d_1)$ for different choices of d_0, d_1. Vertical lines are 95% confidence intervals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

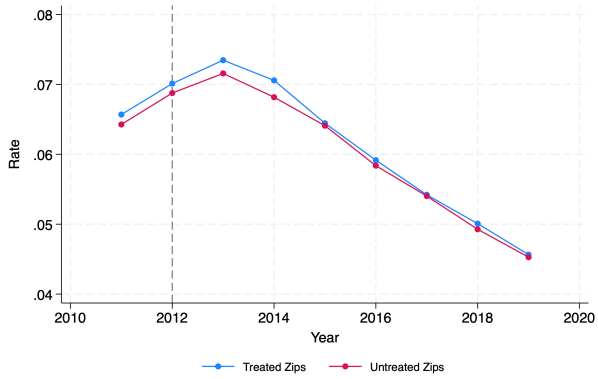
(a) Poverty rates



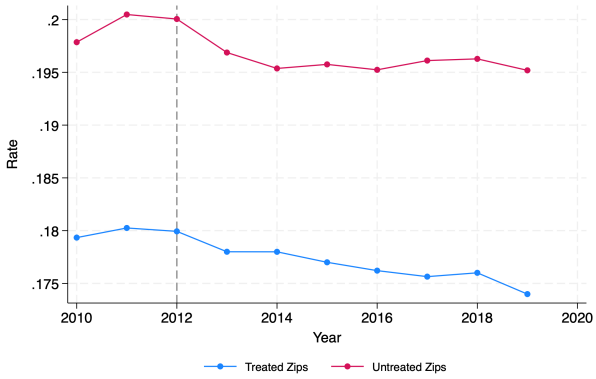
(b) Child poverty rates



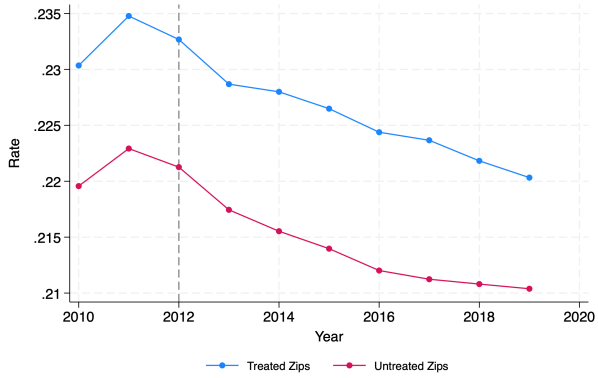
(c) Unemployment rate



(d) Share of Black non-Hispanic population



(e) Share of population age 0–17



(f) Share of population age 65+

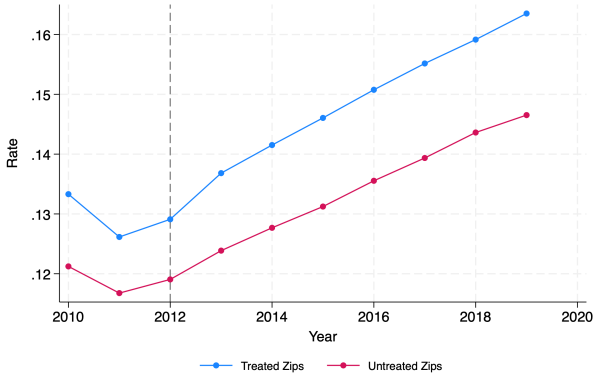


Figure A.11: Event-study estimates with extended pre-trends: Any program receipt

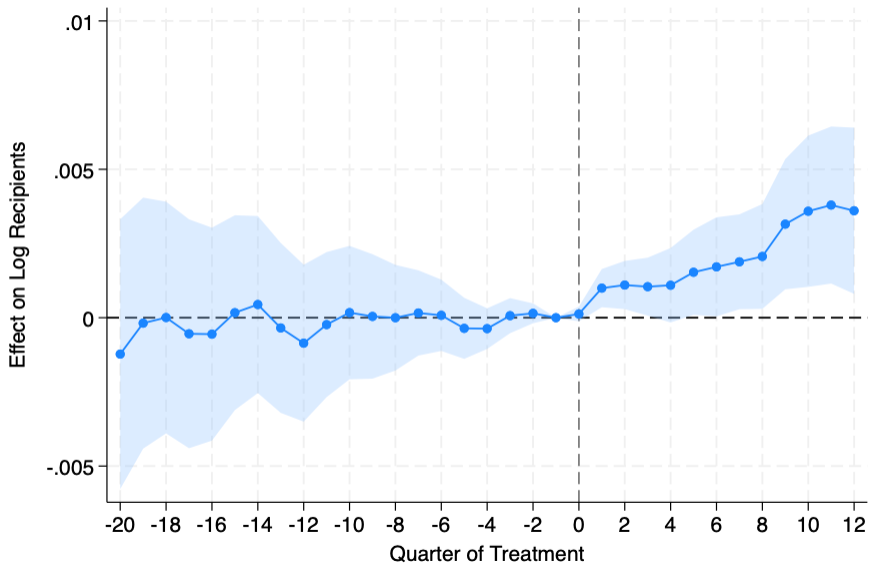
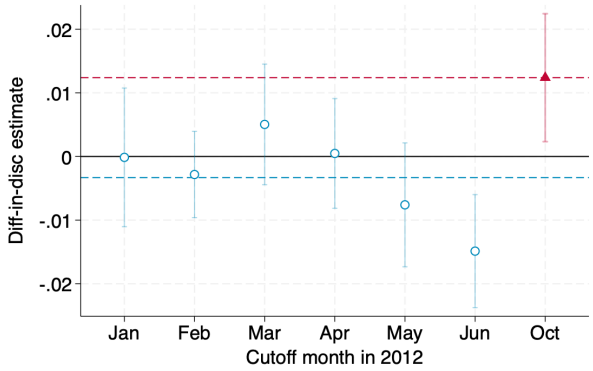
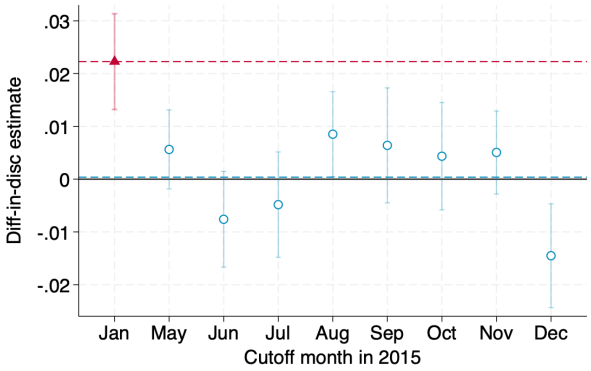


Figure A.12: Placebo DRD effect estimates on multiple program receipt

(a) Launch estimates



(b) Upgrade estimates



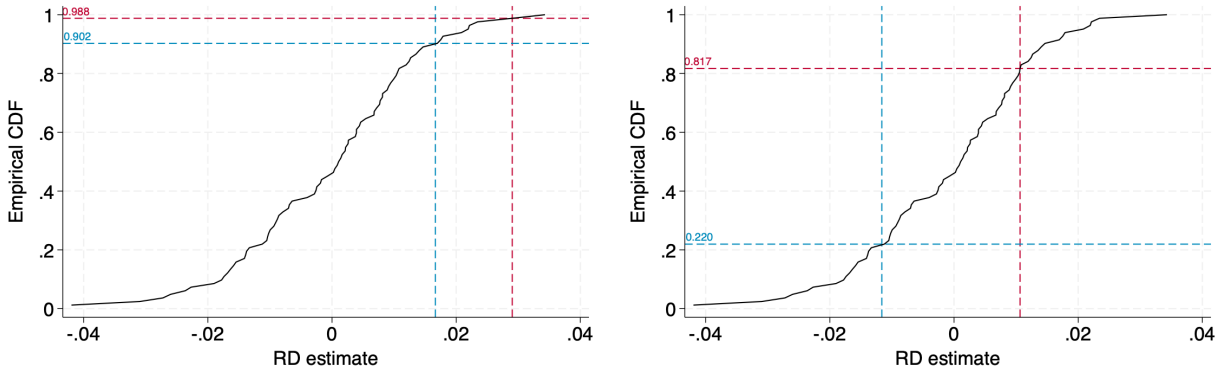
Notes: This figure plots DRD placebo estimates, where placebo cutoff months are at least 3 months away from the treatment month (to avoid having the effect at the true cutoff to contaminate the placebo effect in its estimation window) and within the same calendar year as the treatment. Besides changing the cutoff month, the DRD regression specifications are identical as before. Vertical capped bars are 95% confidence intervals based on standard errors clustered at the county level. The left (right) panel excludes the RD estimate around the upgrade (launch) date. Dashed red lines indicate the DRD estimate; dashed blue lines indicate the mean DRD estimates from the placebo estimates.

Figure A.13: Placebo RD effect estimates on multiple program receipt

(a) Empirical CDFs of placebo RD estimates

(i) Launch estimates

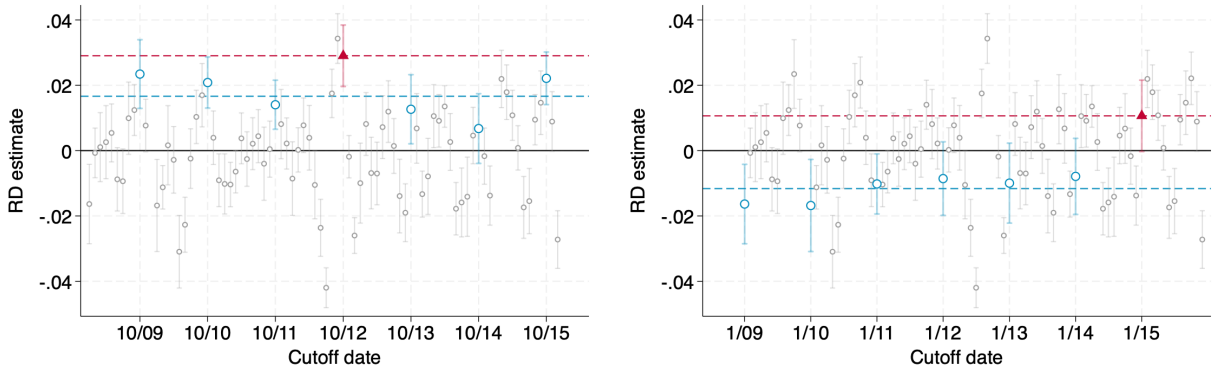
(ii) Upgrade estimates



(b) All placebo RD estimates

(i) Launch estimates

(ii) Upgrade estimates



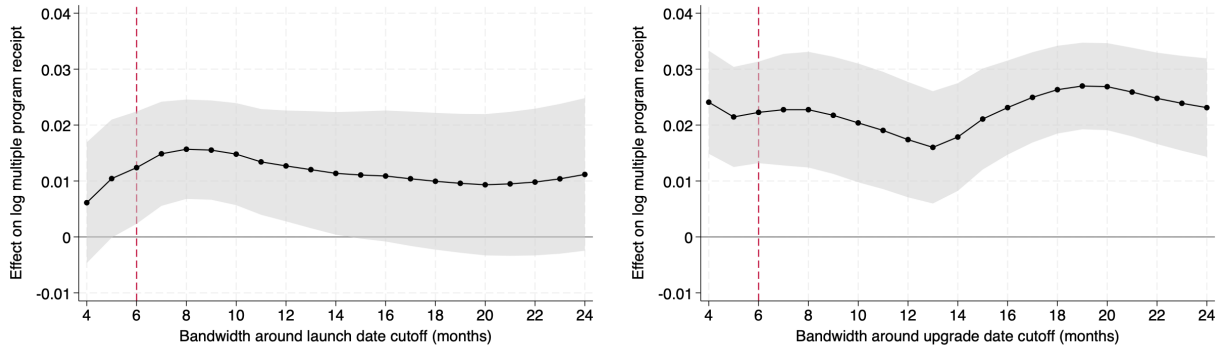
Notes: The top panels plot empirical cumulative distribution functions of RD placebo estimates, where placebo cutoff dates are all months between January 2009–December 2015. The bottom panels plot RD placebo estimates, where placebo cutoff dates are all months between January 2009–December 2015. Vertical capped bars are 95% confidence intervals based on standard errors clustered at the county level. The left (right) panels exclude the RD estimate around the upgrade (launch) date. Dashed red lines indicate the RD estimate corresponding to the main cutoff date; dashed blue lines indicate the mean RD estimate from the control windows.

Figure A.14: DRD effect estimates on multiple program receipt are robust to alternative bandwidths and distance thresholds

(a) Varying bandwidth

(i) Launch estimates

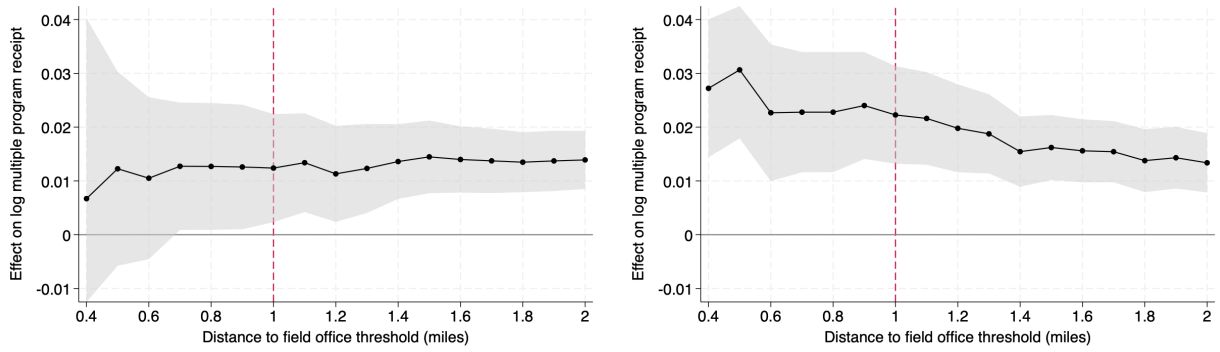
(ii) Upgrade estimates



(b) Varying distance threshold

(i) Launch estimates

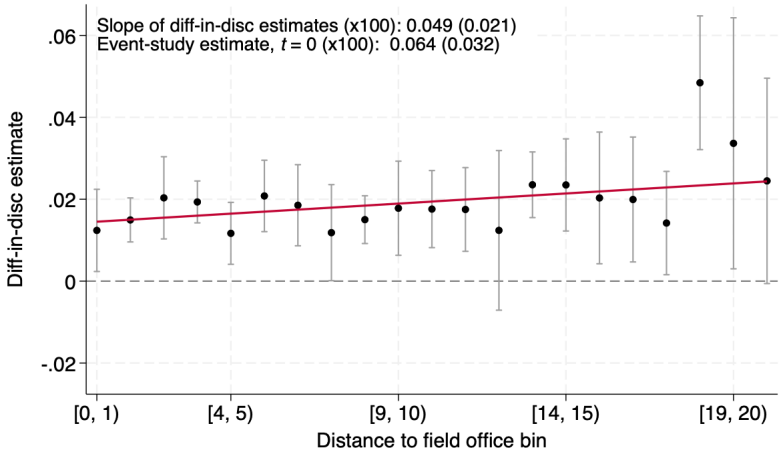
(ii) Upgrade estimates



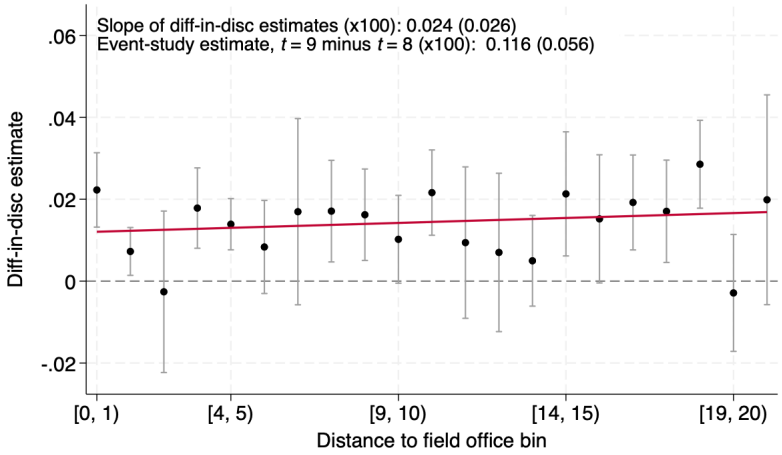
Notes: The top panels plot DRD estimates for different choices of bandwidths from the date cutoff. The bottom panels plot DRD estimates for different distance thresholds $\bar{d}$ that selects the analytical sample. Dashed red lines indicate the selected bandwidth and distance threshold using our preferred specification. Shaded areas are 95% confidence intervals based on standard errors clustered at the county level.

Figure A.15: DRD estimates on multiple program receipt by distance bin (total effects via variable and fixed burdens)

(a) Launch



(b) Upgrade



Notes: This figure plots DRD estimates 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 (left panel) and $\beta_9 - \beta_8$ (right panel) 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: DRD effect estimates are robust to alternative specifications

	(1)	(2)	(3)	(4)	(5)	(6)
	Main	Covariates	Distance kernel	Parametric	Quadratic	Cubic
A. Any program						
Overall effect (%)	0.716***	0.692***	0.709***	0.716***	1.104***	0.659
	(0.212)	(0.202)	(0.241)	(0.212)	(0.283)	(0.409)
Launch effect (%)	0.501***	0.526***	0.420**	0.485**	0.772***	0.286
	(0.163)	(0.156)	(0.168)	(0.185)	(0.243)	(0.329)
Upgrade effect (%)	0.697**	0.539**	0.940**	0.597**	1.079***	1.214***
	(0.280)	(0.243)	(0.403)	(0.262)	(0.277)	(0.417)
Outcome mean (count/zip)	1,525.6	1,525.6	1,525.6	1,525.6	1,531.7	1,531.7
B. Multiple programs						
Overall effect (%)	1.924***	2.182***	1.774**	1.924***	2.643***	0.566
	(0.551)	(0.483)	(0.784)	(0.551)	(0.564)	(0.752)
Launch effect (%)	1.239**	1.585***	1.031	1.430***	1.733***	−0.088
	(0.512)	(0.415)	(0.787)	(0.502)	(0.506)	(0.738)
Upgrade effect (%)	2.227***	1.941***	2.415***	2.344***	2.956***	2.128***
	(0.463)	(0.402)	(0.492)	(0.528)	(0.596)	(0.526)
Outcome mean (count/zip)	676.0	676.0	676.0	676.0	683.3	683.3
C. Single program						
Overall effect (%)	−0.307	−0.355	−0.391	−0.307	−0.160	0.792
	(0.373)	(0.362)	(0.415)	(0.373)	(0.470)	(0.798)
Launch effect (%)	−0.202	−0.232	−0.352	−0.387	−0.060	0.482
	(0.313)	(0.308)	(0.351)	(0.289)	(0.411)	(0.716)
Upgrade effect (%)	−0.342	−0.399	−0.127	−0.744*	−0.326	1.010
	(0.423)	(0.415)	(0.526)	(0.407)	(0.496)	(0.714)
Outcome mean (count/zip)	834.2	834.2	834.2	834.2	833.5	833.5
Number of zip codes	51	44	51	51	51	51
Number of counties	42	37	42	42	42	42

Notes: This table examines DRD estimates from alternative specifications. Column (1) reports our main results. Column (2) includes time-averaged zip-level covariates from our DD specification (log total population; population shares of women, blacks, and elderly adults; and poverty, unemployment, and labor force participation rates). Column (3) additionally weighs zip codes by their proximity to their field office through a triangular kernel. Column (4) applies parametric linear fits instead of local linear fits. Columns (5) and (6) apply local quadratic and cubic fits, respectively, using a 12-month bandwidth.

Table A.2: Total Effects on Program Enrollment among Households with Children

	(1)	(2)	(3)	(4)	(5)	(6)
	Any		Multiple		Single	
	ΔY	p.p.	ΔY	p.p.	ΔY	p.p.
A. Overall effects						
1. Total: $g_z = \bar{\beta}^g \cdot D_z + \bar{\beta}^f$	58,059	1.490	59,954	1.539	−10,393	−0.267
2. Geographic only: $g_z = \bar{\beta}^g \cdot D_z$	36,529	0.938	29,207	0.750	4,372	0.112
3. Fixed only: $g_z = \bar{\beta}^f$	22,119	0.568	32,012	0.822	−14,871	−0.382
B. Launch effects only						
1. Total: $g_z = \beta^g \cdot D_z + \beta^f$	41,568	1.067	43,367	1.113	−5,589	−0.143
2. Geographic only: $g_z = \beta^g \cdot D_z$	21,017	0.539	20,734	0.532	2,177	0.056
3. Fixed only: $g_z = \beta^f$	20,872	0.536	23,291	0.598	−7,794	−0.200
C. Launch and upgrade effects only						
1. Total: $g_z = (\beta^g + \beta^{g15}) \cdot D_z + (\beta^f + \beta^{f15})$	83,217	1.818	73,600	1.608	−30,708	−0.671
2. Geographic only: $g_z = (\beta^g + \beta^{g15}) \cdot D_z$	56,653	1.238	26,838	0.586	9,352	0.204
3. Fixed only: $g_z = \beta^f + \beta^{f15}$	27,587	0.603	48,610	1.062	−40,536	−0.886
Outcome mean (count/zip)	548.2		290.8		247.9	
Number of zip codes	1,555		1,555		1,555	
Number of counties	134		134		134	
Share of recipients (%)	69.5		37.0		31.6	

Notes: See notes of Table 4 for details.

Table A.3: Total Effects on Program Enrollment among Households with No Children

	(1)	(2)	(3)	(4)	(5)	(6)
	Any		Multiple		Single	
	ΔY	p.p.	ΔY	p.p.	ΔY	p.p.
A. Overall effects						
1. Total: $g_z = \bar{\beta}^g \cdot D_z + \bar{\beta}^f$	25,935	0.666	249	0.006	24,378	0.626
2. Geographic only: $g_z = \bar{\beta}^g \cdot D_z$	22,663	0.582	−481	−0.012	21,899	0.562
3. Fixed only: $g_z = \bar{\beta}^f$	3,400	0.087	728	0.019	2,619	0.067
B. Launch effects only						
1. Total: $g_z = \beta^g \cdot D_z + \beta^f$	9,130	0.234	−3,065	−0.079	10,923	0.280
2. Geographic only: $g_z = \beta^g \cdot D_z$	12,985	0.333	−803	−0.021	13,105	0.336
3. Fixed only: $g_z = \beta^f$	−3,940	−0.101	−2,252	−0.058	−2,255	−0.058
C. Launch and upgrade effects only						
1. Total: $g_z = (\beta^g + \beta^{g15}) \cdot D_z + (\beta^f + \beta^{f15})$	66,332	1.449	10,754	0.235	55,337	1.209
2. Geographic only: $g_z = (\beta^g + \beta^{g15}) \cdot D_z$	42,033	0.918	1,336	0.029	39,044	0.853
3. Fixed only: $g_z = \beta^f + \beta^{f15}$	25,645	0.560	9,468	0.207	17,557	0.384
Outcome mean (count/zip)	253.1		77.3		175.7	
Number of zip codes	1,555		1,555		1,555	
Number of counties	134		134		134	
Share of recipients (%)	30.5		10.0		20.5	

Notes: See notes of Table 4 for details.

Table A.4: 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^g, β^{g15} 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 $D_z = d_z$,

$$\begin{aligned} g_z &\equiv (\beta^g + \beta^{g15}) \cdot d_z + \beta^f \\ &= \mathbb{E}\left[\log(Y_{z,post}(1)) - \log(Y_{z,post}(0)) \,|\, D_z = d_z\right] \\ &\approx \mathbb{E}\left[\frac{Y_{z,post}(1) - Y_{z,post}(0)}{Y_{z,post}(0)} \,|\, D_z = d_z\right], \end{aligned}$$

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 $\omega_{(p_0,p_1)}$.