8-23-2024

THREE ESSAYS ON ADMINISTRATIVE BURDEN AND THE SOCIAL SAFETY NET

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Abstract

In this dissertation, I investigate how administrative burdens in the social safety net affect payment accuracy, benefit levels, multiple program participation, and benefit redemption.

In the first chapter, I study simplified reporting, a policy that allowed states to reduce the amount of information Supplemental Nutrition Assistance Program (SNAP) recipients must report between certifications. I leverage the fact that simplified reporting affects only some households and in certain months to estimate its impact on the share of SNAP benefits issued in error and SNAP benefit amounts. Using administrative data from the federal SNAP Quality Control system, I find that simplified reporting meaningfully and robustly reduced SNAP payment error rates and increased SNAP benefits. The results imply that reducing administrative burdens can, not only stimulate program uptake, but also increase benefit levels.

In the second chapter, I examine if SNAP certification interviews generate spillover participation for other programs by facilitating referrals. Using administrative data on SNAP recipients from the state of Virginia and a novel primary policy data set, I leverage a unique COVID-era policy change to estimate how the elimination of SNAP certification interviews affected whether SNAP recipients participated in other government assistance programs. I estimate that waiving SNAP interviews actually increased the probability SNAP recipients received TANF. I cannot conclude that waivers increased receipt of childcare subsidies or Medicaid among SNAP participants, but estimates are sufficiently precise to rule out small negative effects. The results imply that making SNAP interview waivers permanent would likely reduce SNAP compliances costs without negatively influencing spillover participation from SNAP to other social programs.

In the third chapter, I investigate the effect of relaxing the federal requirement that certification appointments for the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) take place in person on food benefit redemption. Leveraging the uneven implementation of WIC physical presence requirement waivers across local WIC agencies during the COVID-19 pandemic and variation in household exposure to in-person appointments, I find evidence that physical presence waivers reduced monthly household WIC benefit redemption by about $15, or 17 percent of the sample mean. While eliminating WIC's in-person requirements for certification appointments can ease compliance burdens and improve program uptake, my findings imply that remote-only appointments may have had unintended consequences of reducing benefit redemption.

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B.S., University of Kentucky, 2018
M.P.P., University of Kentucky, 2020

Dissertation

Submitted in partial fulfillment of the requirements for the degree of

Doctor of Philosophy in Public Administration.

Syracuse University

August 2024

Acknowledgements

I am indebted to many people who made this dissertation possible.

I wish to thank Samuel Bondurant and Nichole Szembrot at the Census Bureau for helping me obtain restricted administrative data and navigate the subsequent disclosure process. To the Syracuse University Information Computing Technology staff, namely Edward Godwin and Candi Patterson, thank you for maintaining and equipping me with cutting-edge computing resources that trimmed my Stata runtimes from days to hours. I am certain some of my analyses would still be running if not for your efforts. I am also thankful for administrative support from Peg Austin, Katrina Fiacchi, Zia Jackson, Alyssa Kirk, Emily Minnoe, and the rest of the Center for Policy Research and Maxwell staff.

To the Maxwell faculty, thank you for teaching me the theoretical foundations and methodological tools necessary to complete this dissertation. I also wish to thank the Center for Policy Research faculty and graduate associates for their seminar participation. Not only did your comments and feedback improve the quality of my work, but I also learned a great deal from observing your presentations and engaging with your research.

I am grateful to my dissertation committee members, Len Lopoo, Sarah Hamersma, and Shannon Monnat for their guidance, feedback, and time. Thank you to Leslie Hodges and Laura Tiehen for helping me get the third chapter off the ground. Of course, I am also indebted to my dissertation advisor, Colleen Heflin. From my first day as a doctoral student, my research agenda has been propelled by your encouragement and shaped by your expertise and insight. Thank you for being my mentor.

Thank you to my fellow classmates and especially my cohort, Nick Oesterling, Jiho Kim, Graham Ambrose, Spencer Shanholtz, Anna Wiersma Strauss, Sarah Souders, and Lauryn Quick. Without you all, I would not have survived classes, comprehensive exams, and moving to a strange city at the height of the pandemic.

I first considered an academic career during my undergraduate and graduate studies at the University of Kentucky. During this time, I had the great fortune of learning from Rajeev Darolia, David Agrawal, Genia Toma, Merl Hackbart, Bethany Paris, Anthony Creane, Alejandro Dellachiesa, Carlos Lamarche, John Garen, Timothy Harris, and Alex Combs. You all nurtured my intellectual curiosity and inspired me to pursue a Ph.D. Thank you for your generous mentorship.

To my family and girlfriend, your love and support mean everything to me. I am grateful to have you all in my corner.

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Chapter I: Can Reducing Administrative Burdens Increase Benefit Amounts? Evidence from SNAP Simplified Reporting

1 Introduction

Administrative burden, or an individual's experience of policy implementation as onerous (Burden et al., 2012), is capturing the attention of policymakers and scholars in recent years. In response to mounting evidence that administrative burdens deter participation in public programs, foster negative experiences with government, generate psychological distress, and reinforce inequality (Herd & Moynihan, 2019), some lawmakers have sought to limit administrative burdens in public programs. For example, in 2021 the Biden administration ordered the Office of Management and Budget to author a report on the barriers underserved communities face when accessing public benefits and issue recommendations for federal agencies to improve program access and equity.[1] Some scholars have proposed the federal government institute formal audits assessing the merits of existing administrative burdens and scrubbing those that are excessive (Moynihan, 2021; Sunstein, 2021). In response to the COVID-19 pandemic, the US "waged a war" on administrative burden (Sunstein, 2021), as many public programs waived application requirements and other rules (Heflin et al., 2023) and began providing virtual options for households to engage with social services (Barnes & Riel, 2022).

These recent policy efforts have focused on relaxing administrative burdens to encourage program uptake. However, softening administrative burdens can influence other outcomes of interest to policymakers. For example, procedures associated with administrative burdens may facilitate eligibility determination and payment accuracy for social programs. Administrative procedures may also improve the targeting efficiency of public assistance and constrain public

[1] See https://www.whitehouse.gov/briefing-room/presidential-actions/2021/01/20/executive-order-advancing-racial-equity-and-support-for-underserved-communities-through-the-federal-government/

expenditures (Lipsky, 1980; Schuck & Zeckhauser, 2007). Administrative burden research, however, has focused on program take-up, seldomly considering how relaxing administrative burdens may impact other outcomes (Jenkins & Nguyen, 2022).

In this paper, I attempt to fill this gap by exploring the impact of simplified reporting, a state-level SNAP policy option, on SNAP benefit amounts. SNAP benefit levels are upstream from a host of important outcomes. A large body of literature shows that changes to SNAP benefit levels affect food insecurity (Katare & Kim, 2017; Restrepo, 2023), material well-being (Valizadeh & Smith, 2020), food spending (Valizadeh et al., 2021; Waehrer, 2023), time dedicated to food preparation (Kim et al., 2020), and food pantry use (Byrne & Just, 2021). SNAP benefit increases also reduce obesity and unmet healthcare needs among children (Hudak & Racine, 2021; Morrissey & Miller, 2020) and hospitalizations and healthcare costs for the elderly (Samuel et al., 2018). Consequently, understanding how SNAP policy options affect benefit levels, not just program uptake, is important for policymakers seeking to weigh the costs and benefits of such policies. Results from this paper contribute to this debate. While scholars have shown that simplified reporting encouraged enrollment (Bartfeld et al., 2015; Dickert-Conlin et al., 2021) and reduced administrative costs (Geller et al., 2019), I find that it also meaningfully increased SNAP benefits.

Simplified reporting is a state policy option that reduces the amount of information SNAP recipients must provide to SNAP agencies between certification periods. Specifically, simplified reporting only requires SNAP households to report changes occurring between certifications (hereafter, referred to as an “interim change”) that would make them gross income ineligible. Simplified reporting eliminates barriers to complying with SNAP requirements and thus encourages participation. Indeed, simplified reporting’s positive impact on SNAP enrollment is

well-documented (Bartfeld et al., 2015; Dickert-Conlin et al., 2021; Elkaramany, 2020; Ganong & Liebman, 2018), and some evidence suggests it reduced administrative costs (Geller et al., 2019) and staff workloads (Trippe et al., 2004). In addition to easing the burden of SNAP participation, simplified reporting also allows households to refrain from reporting interim changes that would decrease their benefits but continue reporting changes that would increase them. In this way, simplified reporting, not only encourages program participation through reducing administrative burden, but may also increase the amount in benefits households receive.

I analyze SNAP households in the SNAP Quality Control (QC) files to estimate the impact of simplified reporting on SNAP payment errors and benefit amounts. I leverage a novel source of household-level variation in simplified reporting exposure, stemming from reporting requirements in certification months, to implement difference-in-differences (DD) designs. To preview results, I find that error rates declined substantially under simplified reporting. I interpret this result as largely mechanical because simplified reporting revised the classification of payment errors, though, from a fiscal perspective, this finding is relevant for state finances because of federal incentives for SNAP payment accuracy. In the main analysis, I find that simplified reporting increased SNAP benefits by about 7 percent. I provide evidence that the results are largely driven by household reporting behavior.

To the extent of my knowledge, this study is the first to estimate the impact of simplified reporting on benefit amounts. As one of the largest safety net programs in the US, SNAP is important to consider. Rank and Hirschl (2009) estimate that nearly half of all children in the US will belong to a household that receives SNAP. Therefore, changes to SNAP administrative procedures have broad reach.

This study contributes to the small body of literature on the effects of SNAP policy options on benefit amounts. Previous studies find that state adoption of the standard medical deduction and the excess shelter deduction have been shown to increase benefit allotments (Ismail et al., 2024; Li et al., 2023). This paper builds on this research by showing that simplified reporting, another state-level SNAP policy option, also increased benefit amounts.

Simplified reporting increased net program costs but was arguably a desirable policy change from a social welfare perspective given the positive consequences, particularly for children, associated with increased SNAP participation and benefit amounts. The results of this paper may shed light on the effects of implementing similar reporting policies for other social programs that use household income or size, characteristics that may change during a certification period, to determine benefit levels, such as Temporary Assistance for Needy Families (TANF), Supplemental Security Income (SSI), the Housing Choice Voucher Program, and the child care subsidy program. Understanding the interactions between administrative burden and household instability is important given the high and growing prevalence of family, housing, and income instability, particularly for low-wage workers and other marginalized groups, in the US (Brown et al., 2016; Desmond & Perkins, 2016; Dynan et al., 2012; Hardy & Ziliak, 2014; Kalleberg, 2011; Manning et al., 2004; Morduch & Schneider, 2017).

As an additional contribution, this paper overcomes empirical challenges to existing research on simplified reporting in SNAP that principally rely on the staggered state-level rollout of the policy. Given emerging concerns associated with two-way fixed effects (TWFE) designs with differential treatment timing (Goodman-Bacon, 2021), I use a novel source of variation in exposure to simplified reporting at the household level. This household-level variation in exposure to simplified reporting permits estimating effects without relying on the staggered

state-level policy rollout, which may be biased from other SNAP policies that states adopted during the same period (Anders & Rafkin, 2022) or dynamic treatment effects (Goodman-Bacon, 2021).

The rest of the paper is organized as follows. In the next section, I frame the analysis of administrative burden, provide background on the SNAP program and simplified reporting, and review related literature. In Section 3, I describe the data and empirical approach. I present the main results in Sections 4 and 5. I explore mechanisms underlying the estimated effects in Section 6 and conduct a robustness check in Section 7. Finally, I offer concluding remarks in Section 8.

2 Framework and Background

A. Conceptual Framework

The conceptual foundation for modern administrative burden scholarship is rooted in the "ordeal mechanism" literature of economics, which describes "ordeals" as a targeting tool policymakers can deploy to screen out the less needy from receipt of public assistance (Nichols & Zeckhauser, 1982; Schuck & Zeckhauser, 2007). Ordeals are onerous administrative procedures constructed to ensure that only the most desperate households will endure the hassle of enrolling in social programs. Ordeals seek to maximize the targeting efficiency of social programs and shift administrative costs from the state to program participants (Heinrich et al., 2022).

Recent work in public administration has incorporated insights from behavioral and political science to build a broader conception of administrative burden that illuminates their unintended consequences, as well as how they are constructed and distributed (Moynihan et al., 2015). This conception of administrative burden uses a costs framework to describe how individuals experience policy implementation (Burden et al., 2012), with a particular emphasis on the

challenges of accessing public assistance and how burdens reinforce inequality (Herd & Moynihan, 2019).

Administrative burden may take the form of learning, compliance, or psychological costs (Moynihan et al., 2015). Learning costs are the time and effort one must expend to learn about the nature of a program, its eligibility parameters, the application process, and how to redeem benefits (Barnes, 2021; Moynihan et al., 2015). Compliance costs include the costs of submitting applications and documentation, as well as maintaining eligibility (Moynihan et al., 2015). Psychological costs are the stress, stigma, and frustration associated with accessing public assistance (Moynihan et al., 2015).

A wealth of research finds that administrative burdens deter participation in government programs (Bhargava & Manoli, 2015; Cherlin et al., 2002; Fox et al., 2019; Harris, 2021; Herd & Moynihan, 2019) and shape perceptions of government and bureaucracy (Bruch et al., 2010; Moynihan & Soss, 2014). While the ordeal mechanism framework predicts that administrative burdens deter those who are comparatively less needy from receiving public benefits, growing evidence finds that administrative burdens screen out the neediest households (Christensen et al., 2020; Deshpande & Li, 2019; Finkelstein & Notowidigdo, 2019; Gray et al., 2021; Nisar, 2018).

Existing social policy research has focused on the relationship between administrative burdens and program participation. In this paper, I provide new evidence of the impact of administrative burdens by documenting the effects of simplifying reporting requirements on SNAP benefit amounts.

B. SNAP Background

SNAP (formerly the Food Stamp Program) is a federal program designed to help low-income households afford nutritious foods. SNAP is one of the largest safety net programs in the US. In

fiscal year (FY) 2019 (prior to the COVID-19 pandemic which triggered enormous program growth), SNAP delivered over $55 billion in benefits to nearly 18 million households (U.S. Department of Agriculture Food and Nutrition Service, 2021b). Each month, SNAP households receive money on an Electronic Benefits Transfer (EBT) card to purchase food from authorized retailers for home consumption. The average SNAP household received a monthly benefit of $258 in FY 2019.

The federal government pays the full cost of SNAP benefits, while administrative costs are shared equally with states. Like many federal social programs, SNAP is state administered, but the federal government establishes general guidelines, such as eligibility and benefit levels.[2] With the exception of households with an elderly or disabled person, households meeting three criteria are eligible for SNAP: (1) household gross income does not exceed 130 percent of the federal poverty line (FPL); (2) household net income (gross income minus allowable deductions) does not exceed 100 percent of the FPL; and (3) the value of total household assets (excluding a home and retirement accounts) does not exceed $2,500. Despite federally established baseline eligibility parameters, the US Department of Agriculture (USDA) grants states some flexibility with these criteria. For example, most states use federal policy flexibilities to relax the gross income and asset tests. As of 2016, only four states required an asset test, and only 12 used the 130 percent gross income eligibility threshold, with 14 states expanding the gross income threshold to up 200 percent of the FPL (U.S. Department of Agriculture Economic Research Service, 2018).

To enroll in SNAP, applicants must (at minimum) submit documentation of residency, income, and expenses and participate in an interview (usually over the phone). SNAP enrollees

[2] Except for Alaska and Hawaii, SNAP benefit levels are the same for all states.

are eligible to receive monthly benefits typically for 6 or 12 months, although some elderly or disabled households may be eligible for extended certification periods. SNAP requires households to recertify at the end of their certification period if they wish to remain on the program. At recertification SNAP households resubmit current eligibility documentation to their SNAP agency. The agency then determines if households are still eligible for the program and recalculates benefits to reflect any changes to household circumstances. Most SNAP exits occur at recertification (Gray, 2019; Ribar et al., 2008).

Federal program rules require states to conduct assessments of a random sample of SNAP households each month under the SNAP QC system.[3] During these assessments, reviewers examine eligibility documentation in household case files and re-interview participants to establish whether state and local agencies correctly determined household benefit amounts and program eligibility. If reviewers find that a household's monthly benefit is more (less) than that to which it is legally entitled, USDA considers the agency to have issued an over (under) payment. The review information is entered into a raw datafile. USDA regional offices then re-review a subsample of households on the raw datafile to validate state findings and determine national error rates (Aussenberg, 2018). The data in the public QC datafiles are the financial and demographic information collected during review, with both the benefit amount and eligibility status determined by the caseworker as well as the error and eligibility status determined by the reviewer (U.S. Department of Agriculture, 2015).

While fraud is the source of some SNAP payment errors, most instances are the result of inadvertent mistakes on behalf of SNAP participants or agency staff, with about two-thirds emerging from participants as opposed to agency staff (Aussenberg, 2018). A state's error rate

[3] See U.S. Department of Agriculture, (2015) for a full description of the sampling procedure.

has important fiscal consequences, as the USDA imposes financial penalties for states with excessive errors.

C. Policy Context

SNAP certifies households to receive a monthly benefit amount that is a function of household net income and size. All else equal, a household with more members and less net income is entitled to a larger benefit. Importantly, SNAP benefit determinations are *prospective*. That is, agencies assume household net income and size used to determine a household's monthly benefit amount at an initial certification persist for the household's certification period. If a household's net income or size change during its certification period, federal rules require households to immediately[4] report these interim changes to their SNAP agency so the agency can adjust the household's benefit amount to reflect its new circumstances. If households fail to report interim changes to their SNAP agency (or the agency does not detect them through another source) and thus the agency does not update SNAP benefits accordingly, USDA considers the household benefits issued in error during QC reviews.

Traditional reporting exposed SNAP agencies to QC errors and represented a heavy burden on both SNAP recipients and agency staff (Trippe et al., 2004). This burden was exacerbated by an economic environment of increased earnings and household instability, particularly among those with low-incomes (Hill et al., 2017; Morris et al., 2015) who exclusively comprise the SNAP population, because of the growing number of changes households had to report between certifications. For state and local SNAP agencies, traditional reporting exacted administrative and regulatory costs, given the financial penalties the USDA can impose for excessive errors.

[4] Under traditional reporting, SNAP requires households to report changes within 10 days of the date that the changes occur or within 10 days before or after the end of the month in which the change occurs (U.S. Department of Agriculture Food and Nutrition Service, 2018).

Beginning in 2000, the USDA allowed states to adopt simplified reporting, a federal policy flexibility relaxing household reporting requirements, largely for nonelderly and nondisabled households (Trippe et al., 2004).[5] Under simplified reporting, SNAP households are only required to report interim changes rendering them gross income ineligible (> 130 percent of the FPL)[6], drastically reducing the scope of changes households are required to report to agencies during their certification period. I refer to interim changes households must report under simplified reporting as "eligibility changes." While simplified reporting requires households to only report eligibility changes, households may continue reporting other changes between certifications. Simplified reporting households may, for example, continue reporting interim changes that increase their benefits, such as a reduction in earnings or a new birth.

An important subtlety of simplified reporting is how SNAP agencies act on interim changes they detect through another source, such as data sharing with other government assistance programs. Simplified reporting requires SNAP agencies to act on detected changes for simplified reporting households that increase their benefits but prohibit agencies from acting on changes that would decrease their benefits, with exceptions (such as changes USDA considers "verified upon receipt").[7] Citing confusion over what changes should be considered an exception to this "positive change only" rule, some states received waivers from the USDA to act on all changes known to the state agency, not just those that would increase SNAP benefits (Trippe et al., 2004).

[5] While the federal government initially permitted states to place only households with earnings under simplified reporting, the 2002 Farm Bill allowed states to expand simplified reporting to households without earnings (Trippe et al., 2004). By November of 2007, 44 of the 48 states using simplified reporting had expanded it beyond households with earnings (U.S. Department of Agriculture Food and Nutrition Service, 2007).
[6] This includes states that have used Broad Based Categorical Eligibility to expand the gross income threshold up to 200 percent of the FPL.
[7] See https://www.fns.usda.gov/snap/recipient/reporting-state-agency-requirements for more information on federal guidelines for state agencies acting on known interim changes for simplified reporting households.

The USDA designed simplified reporting to soften the administrative burdens associated with participation in the program and ease staff workloads (Trippe et al., 2004), but simplified reporting also limits state and local agency exposure to QC errors. Prior to simplified reporting, the USDA considered any unreported or undetected interim changes to household circumstances an error for QC purposes. For simplified reporting households, however, USDA only requires households to report eligibility changes and thus does not issue errors for other unreported or undetected changes occurring between certifications.

The USDA first made simplified reporting available to states in 2000. By FY 2013, all states (including the District of Columbia) had adopted it. Figure 1.A1 in the Appendix shows that adoption occurred throughout the period between FY 2001 and FY 2013 but was particularly common between FY 2001 and FY 2004. Given the fiscal and administrative advantages of simplified reporting, states had a strong incentive to implement simplified reporting.

D. Related Literature

Existing literature has explored the consequences of administrative burden in SNAP. Highlighting the learning, psychological, and compliance costs of the program, surveys of eligible nonparticipants revealed stigma, misconceptions about eligibility criteria, and concerns about the paperwork and time costs associated with enrollment as primary drivers of nonparticipation (Bartlett et al., 2004). Similarly, requiring more frequent recertifications reduced program participation (Kabbani & Wilde, 2003). Another strand of research has found that policies designed to ease administrative burdens, such as removing asset tests, extending certification periods, and transitioning to web-based application and telephonic interviews, can boost SNAP take-up (Hanratty, 2006; Ratcliffe et al., 2008; Schwabish, 2012). While the evidence of administrative burden's impact on SNAP take-up is strong, few papers have

examined the impacts of administrative burden in the SNAP program on other outcomes. This paper attempts to fill this gap by exploring how relaxing reporting requirements for SNAP households impacted error rates and benefit amounts.

Like the broader SNAP administrative burden literature, enrollment effects are the central focus of much of the research on SNAP simplified reporting. Existing papers rely on the staggered adoption of simplified reporting across states and find that SNAP enrollment increased modestly in the years following the introduction of simplified reporting (Bartfeld et al., 2015). A few papers have used administrative data to address concerns about underreporting of SNAP receipt in household surveys (Meyer et al., 2015). These papers found larger impacts of simplified reporting on SNAP enrollment (Dickert-Conlin et al., 2021; Ganong & Liebman, 2018). For example, Dickert-Conlin and coauthors (2021) estimated that simplified reporting increased SNAP enrollment by about 5 percent. Using similar geographic and time variation in simplified reporting, Geller and coauthors (2019) found that SNAP administrative costs fell by about 7 percent for states that adopted simplified reporting. Consistent with this result, qualitative research found that SNAP agency staff report lighter workloads with simplified reporting in place (Trippe et al., 2004).

3 Empirical Strategy

A. Hypotheses

Simplified reporting will mechanically reduce payment errors because, for simplified reporting households, the USDA only penalizes states for failing to address interim eligibility changes (i.e., from eligible to ineligible), rather than changes that only alter benefit levels. However, SNAP

agency failure to comply with simplified reporting's "positive change only" rule[8] could expose agencies to more errors.

The impact of simplified reporting on SNAP benefit amounts is ambiguous. Simplified reporting incentivizes households to report interim changes that increase their benefits (though not legally required) but not report changes that decrease them (except for eligibility changes). This behavior would place upward pressure on SNAP benefits. Additionally, simplified reporting's "positive change only" rule requires state agencies, with some exceptions, to act on known interim changes that increase household benefits but prohibits acting on known changes that would decrease benefits. This feature of simplified reporting also places upward pressure on benefits.

Alternatively, simplified reporting households may no longer report *any* changes that are not legally required, even if reporting some changes would increase their SNAP benefit. This would either increase or decrease SNAP benefits, depending on the nature of unreported changes. Households might leave benefits on the table because of information frictions between agencies and recipients (Barnes & Riel, 2022) or psychological inertia (Madrian & Shea, 2001).

B. Data and Samples

To analyze the impact of simplified reporting on SNAP benefit errors and amounts, I draw on USDA SNAP QC data between FY 2005 and FY 2014. FY 2005 is the first year QC data contain simplified reporting indicators, and following FY 2014, the USDA updated its reporting methodology in response to concerns about the integrity of error rate measures (Aussenberg, 2018). Due to concerns about the comparability between the SNAP QC before and after this change, I only analyze fiscal years before 2015. Federal law requires states to conduct periodic

[8] SNAP agencies may act on interim changes that decrease SNAP benefits for simplified reporting households (which federal rules prohibit).

quality control reviews of a random sample of SNAP households to evaluate the accuracy of eligibility determinations and benefit amounts (Klerman & Danielson, 2011). The review entails examining eligibility documentation and re-interviewing participants to determine if households received the correct benefit amounts (U.S. Department of Agriculture, 2015). The publicly available SNAP QC files contain the data reviewers obtain during the examination.

The SNAP QC data record monthly information about demographic characteristics, income, and case information of SNAP households, including indicators for benefits received in error[9], benefit amounts, certification period length, months since last certification, and indicators for whether the household was on an initial or recertification period at the time of observation. The QC data's detailed SNAP case information makes it ideal for studying the effects of simplified reporting, and, importantly, its administrative nature avoids concerns about measurement error in program participation, benefit size, income, and household composition common to household surveys (Meyer et al., 2015).

The QC data are limited in three important ways. First, USDA recommends against using certain household demographic characteristics due to data quality concerns (U.S. Department of Agriculture, 2015). Consequently, I omit relevant household demographic characteristics, such as educational attainment and race/ethnicity, from the main analysis. Second, error rate measures in the QC data suffer from underreporting (Aussenberg, 2018). While the USDA updated the error rate measure in 2015, I restrict the analysis to the years before this date to ensure consistency across sample years. In addition to introducing measurement error, this also precludes analysis of more recent years, limiting generalizability. Lastly, to appear in the QC data, households must be

[9] I adjust error indicators to account for changes to the USDA's official error tolerance threshold (Aussenberg, 2018)).

enrolled in SNAP. Program attrition is thus a potential source of bias in the research design. I describe several steps taken to mitigate this threat in the Method subsection.

The raw QC data contain 462,217 SNAP households between FY 2005 and FY 2014 in states with simplified reporting. I drop households with missing data for any of the variables I use for the analysis (n=956) and households that, at the time of observation, were certified or recertified more than 6 months ago (n=138,098), since after 6 months into a certification period some households may be required to report changes to their local SNAP agency.[10] The main analysis sample contains 323,163 SNAP households in states with simplified reporting between FY 2005 and FY 2014.

I leverage variation in simplified reporting exposure stemming from the number of months since a household's last certification. SNAP rules require households to provide their local SNAP agency with current household eligibility information in certification months (either to enroll or re-enroll in the program at the end of a prior SNAP spell). Therefore, SNAP households in the QC data that were certified in the month of observation (i.e., whose "months since last certification"=0) provided their local SNAP agency with current household eligibility information in that month., regardless of simplified reporting status. The basic identification strategy uses simplified reporting households in these "reporting months" as a comparison group for simplified reporting households in "nonreporting months" (i.e., households that only had to report eligibility changes).

Table 1.1 presents summary statistics for SNAP households assigned to simplified reporting, stratified by reporting month. For all simplified reporters (column 1), about 6 percent of

[10] Federal rules require simplified reporting households to submit a periodic report after their sixth month, but the timing of these reporting requirements vary (U.S. Department of Agriculture Food and Nutrition Service, 2018). Since I cannot accurately identify months in which these households must report change information, I drop them. In Table 1.4, I demonstrate that the results are insensitive to this decision.

households received benefits in error, and the average monthly SNAP benefit was about $294. The average age of household heads was 38, and about 34 percent of households were headed by a single woman. About 15 and 8 percent of households had a disabled or elderly member, respectively. Nearly 59 percent of households were not working. The average household reported $355 in net income and contained 2.61 members. Certification lengths averaged about 9 months. About 40 percent of households were on an initial certification period at the time of observation.

Columns (2)-(3) display statistics separately for households observed in reporting and nonreporting months. Reporting month (column 2) refers to households certified or recertified in the month of observation ("months since last certification"=0). These households had already negotiated the initial certification or recertification process at the time of observation, so I consider them untreated because, despite being under simplified reporting, these households had to report current eligibility information to their SNAP agency for certification. Nonreporting month (column 3), on the other hand, refers to households not certified in the month of observation ("months since last certification">0). I consider these households exposed to simplified reporting because they only had to report eligibility changes in these months.

In nonreporting months, simplified reporting households were less likely to receive benefits in error, and their monthly benefits were about $34, or 13 percent, larger on average. These raw statistics are consistent with the hypotheses that simplified reporting reduced errors and increased SNAP benefits. However, households in nonreporting months were also slightly older, more likely to be headed by a single woman, and more likely to have a disabled or elderly member, though these differences were largely insubstantial in practical terms. Despite higher benefits, households in nonreporting months also had higher net incomes, which, all else equal, reduce benefits, but their larger household sizes work to offset this differential. In nonreporting

months, simplified reporting households also had longer certification lengths and were less likely to be on an initial certification period. Overall, simplified reporting households were different across observables in reporting and nonreporting months, but many differences were modest in practical terms, indicating potential comparability between the two groups.

C. Method

The summary statistics in Table 1.1 suggest simplified reporting households had lower error rates and received higher benefits in nonreporting months, consistent with the hypothesis that simplified reporting reduced errors and increased SNAP benefits. However, this simple comparison of means assumes difference in outcomes among simplified reporting households in reporting and nonreporting months would have been zero in the absence of simplified reporting.

There are two primary threats to this assumption. First, the composition of the SNAP population may change in reporting months due to attrition. Indeed, evidence suggests most exits from SNAP occur at recertification (Gray, 2019; Hastings & Shapiro, 2018; Ribar et al., 2008). Therefore, differences in outcomes among simplified reporters in reporting and nonreporting months may be driven by compositional shifts in the SNAP population following recertifications, not simplified reporting. Second, while simplified reporting permits households to only report changes that increase their benefits, SNAP households may engage in this asymmetric reporting even in the absence of simplified reporting, creating a positive correlation between nonreporting months and benefits. To mitigate these threats, I employ a regression framework and leverage households not assigned to simplified reporting as a separate comparison group to implement a difference-in-differences (DD) design. Specifically, I estimate the following equation:

$$Y_{ist} = \beta_0 + \beta_1 NoReportMth_{ist} + \beta_2 SimplifiedReporter_{ist} + \beta_3 (NoReportMth * SimplifiedReporter)_{ist} + \beta_4 Policy_{st} + \beta_5 UR_{st} + \beta_6 DemGov_{sy} + \beta_x X_{ist} + \gamma_s + \gamma_t + \varepsilon_{ist} \quad (1)$$

where Y_{ist} is an outcome (an indicator for SNAP benefits received in error or the monthly benefit amount) for household *i* in state *s* during month-year *t*. $NoReportMth_{ist}$ is an indicator for households not certified in the month of observation. $SimplifiedReporting_{ist}$ is an indicator for simplified reporting households. The coefficient of interest, β_3, indicates the differential impact of nonreporting months for simplified reporting households. $Policy_{st}$ is the Ganong-Liebman index (Ganong & Liebman, 2018) of other SNAP policies states adopted during the period. Aggregating SNAP policies with an omnibus measure reduces concerns about measurement error and precision (Ganong & Liebman, 2018). Results in Table 1.4 demonstrate that estimates are insensitive to using individual policy indicators in lieu of the Ganong-Liebman index. I control for the state unemployment rate (UR_{st}) and partisanship of the governor[11] ($DemGov_{st}$) to account for economic and political conditions. State unemployment rate data are monthly and come from the Bureau of Labor Statistics; gubernatorial partisanship data are annual from the University of Kentucky Center for Poverty Research (2022). I control for SNAP household characteristics, X_{ist}, including the age of the household head and separate indicators for households on an initial certification period (as opposed to a recertification period), households with any elderly members, and households with any disabled members. I also include fixed effects for certification period length; one of the strongest predictors of SNAP payment errors (Kabbani & Wilde, 2003); state, and month-year. For this and all subsequent analyses, I cluster standard errors at the household level.

[11] For District of Columbia, this measure captures the partisanship of the mayor.

By controlling for whether households were on an initial or recertification at the time of observation, the regression framework ensures that I do not compare outcomes for households that have not negotiated a recertification (where most SNAP exits occur) with households that have, mitigating bias from compositional changes following recertification months. Moreover, the DD approach relaxes that assumption that differences in outcomes between reporting and nonreporting months would have been zero in the absence of simplified reporting, which may be violated if some traditional reporters only report interim changes that increase benefits. By using the same differences for traditional reporters as a counterfactual, this approach assumes, conditional on observables, differences in outcomes between reporting and nonreporting months were common to all SNAP households.

This household-level DD design departs from existing research on simplified reporting that has relied on geographic and time variation in state adoption of simplified reporting for identification (Bartfeld et al., 2015; Dickert-Conlin et al., 2021; Geller et al., 2019). The household-level DD approach provides two advantages. First, estimates of the impact of simplified reporting that rely on staggered adoption at the state-level can be biased under standard TWFE and event-study frameworks, particularly in cases where all units eventually become treated (Baker et al., 2022; Callaway & Sant'Anna, 2021; Goodman-Bacon, 2021; Sun & Abraham, 2021). Second, scholars have raised concerns about research designs using state-level SNAP policy variation because of uncertainty about the timing of state implementation and potential bias emerging from coincident SNAP policy changes (Ganong & Liebman, 2018).

4 Results for Error Outcomes

Table 1.2 presents DD estimates from equation (1) for error outcomes. The first column of Table 1.2 suggests simplified reporting reduced the overall probability of households receiving

payments in error by 0.008 (about 12 percent of the sample mean). The coefficient is statistically significant at the 1 percent level.

I posit that simplified reporting reduced errors mechanically, because the federal government only penalizes failure to adjust benefits in response to changes that households are required to report, which are markedly reduced under the simplified reporting. However, simplified reporting should only reduce reporting-related errors, not errors emerging from benefit determination. In columns (2)-(4) of Table 1.2, I use information in the QC data on the timing, source of discovery, and nature of errors to investigate the effect of simplified reporting on errors that should be associated with reporting mistakes. I describe each of these three categories in turn.

Timing

First, simplified reporting should reduce interim errors. That is, errors occurring before or after a certification appointment, not during a certification appointment. Errors occurring at the time of a certification appointment are likely to be the result of eligibility workers incorrectly calculating benefits, whereas interim errors are likely to be the result of reporting mistakes. Column (2) of Table 1.2 presents results for "interim errors," which is an indicator that equals 1 if the error occurred before or after the certification appointment (0 if the error occurred during certification). The coefficient of interest implies simplified reporting reduced interim errors by about 0.3 percentage points, or 13 percent of the sample mean. The point estimate is significant at the 10 percent level.

Source of Discovery

Second, simplified reporting should reduce errors QC staff identify from interviews of recipients rather than the eligibility documentation in case records. Errors QC staff identify from case

records are likely to be the result of incorrect benefit and eligibility determinations, whereas interviews with recipients are likely to uncover unreported interim changes that would not appear in case records. Column (3) of Table 1.2 presents the estimated effect of simplified reporting on errors that were "discovered through interview," which equals 1 if the QC staff discovered the error through an interview (0 if otherwise[12]). In column (3), I estimate that simplified reporting reduced errors discovered through interviews by about 20 percent.

Nature

Finally, simplified reporting should reduce errors related to unreported interim changes, not eligibility determination or benefit calculation. In column (4) I present results for "change-related errors," which is an indicator that equals 1 if the nature of the error was interim change-related[13] (0 if otherwise[14]). The coefficient of interest suggests simplified reporting reduced change-related errors by a statistically significant 19.5 percent.

Overall, the results in Table 1.2 confirm that simplified reporting reduced total SNAP payment errors and that these declines were driven by the types of errors simplified reporting should affect. Given that simplified reporting reduces the number of unreported changes that USDA considers an error for QC purposes, these findings are unsurprising. Nonetheless, they have important fiscal implications for states that may face financial penalties for excessive errors and represent a useful "first stage" analysis, demonstrating that simplified reporting had "bite," (Cunningham, 2021).

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[12] Includes case records, employer, financial institution, landlord, government agency, or other.

[13] I classify the following types of errors as interim change-related: actual income varied from budgeted, employment status or earnings changed, residence or household size changed.

[14] The most common type of errors I do not classify as interim change-related include misapplying deductions, unreported sources of income, and not including all income. See https://snapqcdata.net/sites/default/files/2020-06/FY%202015%20Tech%20Doc.pdf (pp. 92-94) for a full description of all categories of changes.

5 Results for Benefit Amount Outcome

Table 1.3 presents results from the DD model for SNAP benefit amounts. The coefficient suggests monthly SNAP benefits were about $19 higher (or about 7 percent of the average monthly SNAP benefit) for simplified reporting households in nonreporting months. The coefficient is statistically significant at the 1 percent level.

A. Heterogenous Effects

In this subsection, I investigate whether simplified reporting differentially affected SNAP benefit levels for households with certain characteristics. Simplified reporting may disproportionately impact households with high levels of instability; thus, I focus on racially marginalized groups, households with low educational attainment, households with nonkin members, and households with young children, as household instability is more common for these groups (Desmond & Perkins, 2016; McLanahan, 2009; Raley et al., 2019). I assign households to the race/ethnicity category to which every member of the household belonged. If members of the same household belonged to different race/ethnicity categories, I classify the household as "mixed race/ethnicity." Education indicates the maximum level of education among all members of the household.

Figure 1.1 presents DD coefficients from a separate regression restricted to each group, but before describing the results, a caveat is in order. I urge caution in interpreting the results for race and education categories because they are measured with substantial error in the QC data (U.S. Department of Agriculture, 2015). Despite this qualification, point estimates are positive, indicating that simplified reporting was associated with higher SNAP benefits, for all groups. Estimates were larger for White households than Black households, but not statistically different across the other race/ethnicity groups. Simplified reporting may have had an outsized impact on more highly educated households (relative to less educated households), but these differences

were not statistically significant. Similarly, estimates were larger for households with at least one nonkin member than households with only kin members but too imprecise to conclude effects for the two groups were different. Overall, I find little evidence of treatment heterogeneity except for small differences between White and Black households.

B. Sensitivity Checks

Table 1.4 presents results for several sensitivity checks. Panels A and B display results for the error and SNAP benefit amount outcomes, respectively. Column (1) reproduces the main estimates. Models presented in column (2) restrict to only households with 6- or 12-month certification periods since households with longer or shorter certification periods may be subject to different eligibility requirements. Estimates in column (3) are from models that include households whose "months since last certification" was 6 or more at the time of observation, and models in column (4) use individual SNAP policy indicators in lieu of the Ganong-Liebman policy index. Collectively, these results provide consistent evidence that simplified reporting reduced errors and increased SNAP benefits.

6 Mechanisms

Section 5 presents evidence that simplified reporting increased SNAP benefit amounts. In this section, I explore the mechanisms underlying this effect. How might simplified reporting increase benefit amounts? Recall that under simplified reporting households have an incentive to report changes that would increase their benefits but not report changes that would decrease them. SNAP household reporting behavior is thus one potential mechanism underlying the positive estimate of the effect of simplified reporting on benefit amounts.

A second potential mechanism responsible for the positive relationship between simplified reporting and benefit amounts relates to SNAP agency behavior. Recall that simplified reporting

requires agencies to act on an interim change known to the agency only if the change increases benefits. Simplified reporting does not allow states to act on interim changes that would decrease benefits unless the household change information is considered "verified upon receipt" or meets other exceptions.[15] This asymmetry in how the federal government permits SNAP agencies to act on known interim changes under simplified reporting might also increase benefits (independent of SNAP household reporting behavior).

To tease apart these two channels, I leverage a policy waiver allowing states with simplified reporting to act on all known interim changes, including those that would decrease benefits. In August of 2005 (the starting year for the analysis), 27 of the 45 states with simplified reporting in place had relaxed the "positive change only" rule through this waiver. In these waiver states, the federal government allowed SNAP agencies to act on known interim changes that reduced benefits.

If simplified reporting increased SNAP benefit amounts primarily through the "positive change only" rule, then the estimated effect of simplified reporting on SNAP benefits should be markedly smaller in waiver states (i.e., states without the "positive change only" rule). Conversely, if SNAP household reporting behavior was the primary channel through which simplified reporting increased SNAP benefits, the estimated effect of simplified reporting on benefit amounts should be similar in waiver and nonwaiver states. To test which mechanism dominates, I re-estimate the household-level DD equation (described by equation 1) and stratify results by waiver status. For this analysis, I rely on "positive change only" waiver data in SNAP State Options Reports.[16]

[15] See https://www.fns.usda.gov/snap/recipient/reporting-state-agency-requirements for more information.
[16] SNAP State Options Reports are unavailable for the years 2008, 2010, 2013, and 2014. I set waiver status to 1 beginning in the first observable active month-year.

Table 1.5 presents the results from estimating equation (1) separately for states with and without a "positive change only" waiver. Column (1) presents estimates for states with waivers (i.e., permission to act on all interim changes), while column (2) presents estimates for states without waivers (i.e., required to act on interim changes that increase benefits). The results suggest simplified reporting had a slightly larger impact on SNAP benefits in states with the "positive change only" rule. Estimates imply that simplified reporting increased SNAP benefits by about 6 percent in states permitted to act on all changes and about 8 percent in states with the "positive change only" rule. Coefficients were statistically different at the 10 percent level.

These findings suggest simplified reporting increased SNAP benefits primarily through changes in household reporting behavior, not the "positive change only" rule. This result is perhaps unsurprising given that the "positive change only" rule is limited in scope, only applying to some interim changes. Even under the "positive change only" rule, SNAP agencies can act on interim changes that reduce benefits if, for example, the change information is considered "verified upon receipt[17]" or meets other criteria.[18] These results suggest eliminating the "positive change only" rule would not substantially diminish simplified reporting's positive impact on SNAP benefit amounts.

7 Robustness Check

The main empirical approach assumes differences in outcomes (conditional on observables) between reporting months and nonreporting months for traditional reporters are an appropriate counterfactual for simplified reporters. However, Table 1.A1 in the Appendix shows that

[17] USDA defines "verified upon receipt" as information (1) that is not questionable and (2) for which the provider is the primary source of the information, as indicated in the following examples: BENDEX or SDX from the Social Security Administration, SAVE from the Immigration and Naturalization Service, unemployment compensation from the state unemployment compensation agency, and worker's compensation from the state worker's compensation agency (Trippe et al., 2004).

[18] See https://www.fns.usda.gov/snap/recipient/reporting-state-agency-requirements for more information.

traditional reporters differed from simplified reporters across observables. For instance, traditional reporters were older, less likely to be working, had lower net incomes, and had fewer members than simplified reporters. These underlying differences between simplified and traditional reporting households may undermine the credibility of the identifying assumption of equation (1).

I assess the robustness of the main results by dropping likely traditional reporters and then using state-level variation in simplified reporting policy implementation (as opposed to variation in household assignment to reporting schemes in simplified reporting states) for identification. For this robustness check, I analyze SNAP QC data between FY 1997 and FY 2004. I choose FY 1997 as the starting year because it is the earliest year for which QC and SNAP policy data are available. I choose FY 2004 as the cutoff year for two reasons. First, the sample for the main results is between FY 2005 and FY 2014, so limiting the robustness check to before FY 2005 ensures I analyze an entirely different set of households. Second, most states had adopted simplified reporting by 2005, so state-level variation in simplified reporting after FY 2004 is limited (see Figure 1.A1).

I start with all SNAP households in the QC data between FY 1997 and FY 2004 (n=376,525). States were more likely to assign simplified reporting to nonelderly, nondisabled, and working households during this sample period (Trippe et al., 2004). Therefore, I drop households with zero earnings and elderly or disabled members (n=292,887). Like the main sample, I also drop households who, at the time of observation, were certified or recertified less than 6 months ago (n=8,598). The remaining sample (hereafter referred to as the “robustness sample”) contains 75,040 SNAP households. I estimate the following regression:

$$Y_{ist} = \beta_0 + \beta_1 NoReportMth_{ist} + \beta_2 SimplifiedReporting_{st} + \beta_3 (NoReportMth \quad (2)$$
$$* SimplifiedReporting)_{ist} + \beta_4 Policy_{st} + \beta_5 UR_{st} + \beta_6 DemGov_{sy}$$
$$+ \beta_x X_{ist} + \gamma_s + \gamma_t + \varepsilon_{ist}$$

where $SimplifiedReporting_{st}$ is indicator for whether state *s* had simplified reporting in place during month-year *t*; note that in equation (1), this term indicates whether the *household* was assigned to simplified reporting at the time of observation. The remaining terms are unchanged from equation (1). This design assumes, conditional on observables, differences in outcomes between nonreporting and reporting months for households in states without simplified reporting are a reasonable counterfactual for the same differences for households in states with simplified reporting.

Tables 1.A2, 1.A3, and 1.A4 in the Appendix reproduce Tables 1.2, 1.3 and 1.4 but for the robustness sample and from estimating equation (2). With respect to sign and statistical significance, point estimates for the robustness sample were remarkably consistent with the main analysis sample. The estimated magnitudes for error outcomes were larger, relative to sample averages, for the robustness sample: the estimated simplified reporting-induced decline in errors was about 12 percent of the sample mean for the main analysis sample, compared to about 24 percent for the robustness sample. This indicates that, if anything, the main analysis may understate simplified reporting's negative impact on errors. Simplified reporting might have had a larger impact during the earlier periods of the robustness sample because of inefficient or outmoded case management systems, which might have contributed to elevated error rates. The estimated magnitudes for the benefit amount outcome were slightly smaller in absolute terms for the robustness sample but, relative to sample means, similar to the main analysis sample. Both estimates imply about a 7 percent increase in SNAP benefits attributable to simplified reporting.

Together, the main results and robustness check provide strong evidence that simplified reporting reduced errors and increased benefits.

8 Conclusion

Given the rising focus in the policy community of mitigating administrative burdens, it is important to understand the full range of consequences of such interventions. Social policy research has shown that relaxing administrative burdens can improve participant interactions with the state and increase social program take-up (Herd & Moynihan, 2019). In this paper, I show that reducing administrative burdens through simplifying reporting requirements can also increase the amount in benefits households receive once enrolled.

Simplified reporting is a state SNAP policy option that reduces the amount of information participating households must report between certification periods. The USDA introduced simplified reporting to soften administrative burden and limit state and local SNAP agency exposure to errors (Trippe et al., 2004). Since its inception in 2000, it has been adopted by all 50 states (and the District of Columbia).

I leverage state- and household-level variation in exposure to simplified reporting to implement DD designs. I find evidence that simplified reporting reduced error rates. This finding is expected, given that simplified reporting reduces the number of unreported or undetected interim changes that USDA considers an error for QC purposes, but has important fiscal implications for states that may face financial penalties for excessive errors.

Additionally, I find that simplified reporting put upward pressure on SNAP benefits. The point estimate from the primary specification implies a $19 increase or about 80 percent of the current minimum SNAP benefit. Across all specifications, I consistently estimate increases of about 7 percent. For reference, the federal government temporarily increased SNAP benefits by

15 percent for only six months during the initial onset of the COVID-19 pandemic (U.S. Department of Agriculture Food and Nutrition Service, 2021a). In other words, simplified reporting was equivalent to a *permanent* increase in SNAP benefits (in nonreporting months) about half the size of the temporary, six-month increase the federal government imposed in response to a period of record economic distress.

Prior studies have estimated that simplified reporting reduced administrative costs by about 7 percent (Geller et al., 2019) and increased enrollment by about 5 percent (Dickert-Conlin et al., 2021). These results suggest simplified reporting also increased SNAP benefits by about 7 percent. Given that administrative costs comprise a small share of total SNAP spending, less than 6 percent in FY 2021, simplified reporting led to a net increase in aggregate program costs.

The federal government pays the full cost of SNAP benefits, whereas administrative costs are split equally with the states. From a fiscal federalism perspective, the costs and benefits of simplified reporting were unequally distributed between states and the federal government. States enjoyed meaningful administrative cost savings, lower SNAP payment error rates, and the economic stimulus associated with increased SNAP benefits and participation (Canning & Stacy, 2019). The federal government, on the other hand, netted an increase in program costs, as the simplified reporting-induced savings in administrative costs (which are shared with states) do not overcome the added costs of simplified reporting-induced enrollment and benefit growth (which are borne entirely by the federal government).

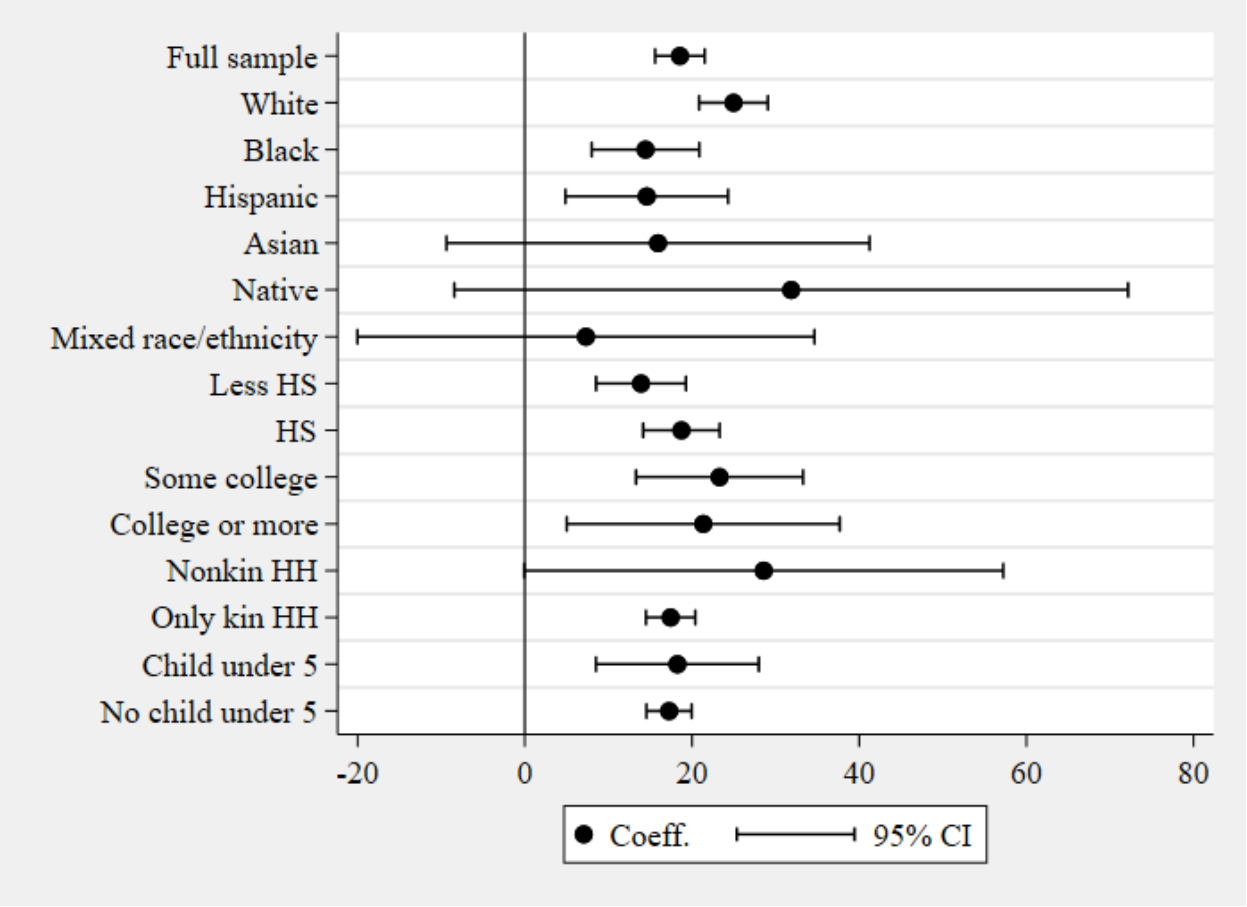
On one hand, some policymakers may view simplified reporting, a policy that encouraged SNAP participation, reduced administrative costs, and increased benefits, as desirable. On the other hand, some may view an indirect SNAP benefit increase as undermining program integrity and unduly inflating program costs, given that the USDA ostensibly introduced simplified

reporting to streamline administration, not increase benefit levels. Some policymakers may also worry about work disincentives associated with increased SNAP generosity, though existing evidence suggests negative labor supply responses to SNAP are modest or only materialize for certain subgroups (Moffitt, 2003, 2016).

Despite these concerns, simplified reporting is arguably a desirable policy because of the welfare-enhancing consequences of increased SNAP participation and benefit levels, including reductions in food insecurity (Schmidt et al., 2016), crime (Tuttle, 2019), psychological distress (Schmidt et al., 2023), Medicaid costs (Sonik, 2016), as well as improvements in life outcomes for children (Bailey et al., 2023).

The results of this paper emphasize the importance of considering how administrative burdens impacts other outcomes of interest to policymakers (beyond enrollment) and are informative for the potential consequences of extending simplified reporting to other social programs, such as TANF, SSI, the Housing Choice Voucher Program, and the child care subsidy program. Future research could continue this line of inquiry by exploring how administrative burdens interact (within or across programs). Simplified reporting, for example, relaxes compliance costs but may also impose learning costs for recipients who must adapt to new reporting rules. With respect to SNAP enrollment, existing evidence suggests that the simplified reporting-induced reduction in compliance costs outweighs any added learning costs, but other administrative burdens, such as applicant interviews, may provide information (thereby reducing learning costs) that, on balance, reduce overall burden. Understanding these potentially competing forces is crucial for a comprehensive evaluation of both the costs and benefits of burden-reducing policies.

Figure 1.1. Heterogenous Effects of Simplified Reporting on SNAP Benefit Amounts



Notes: The figure reports the point estimates and 95% confidence intervals for the impact of simplified reporting on SNAP benefit amounts from the differences-in-differences regression described by equation (1). Each row presents results from a separate regression corresponding to the characteristics listed. Standard errors are clustered at the household level.

Table 1.1. SNAP Household Characteristics, Simplified Reporters

	(1) Full sample	(2) Reporting month	(3) Nonreporting month
Any error	0.061	0.065	0.061**
SNAP benefit	293.87	266.21	300.20***
Age of HH head	37.73	37.32	37.82***
Single female headed household	0.34	0.33	0.34***
Any disabled members	0.15	0.14	0.15***
Any elderly members	0.08	0.07	0.08***
Nonworking HH	0.59	0.59	0.59
HH net income	355.24	339.83	358.77***
HH size	2.61	2.51	2.63***
Certification length	8.67	8.61	8.68**
Initial certification	0.40	0.49	0.38***
Observations	255,936	50,112	205,824

Notes: Sample contains simplified reporting SNAP households in the Quality Control files between FY 2005 and FY 2014. I restrict to households with non-missing data for all the variables presented in the table and households whose "months since last certification" was less than 6 at the time of observation. I weight observations using the household weights in the QC files. Income and benefit amounts are in nominal dollars. Column 2 contains observations that were certified in the month of observation (reporting month), whereas column 3 contains observations that were not certified in the month of observation (nonreporting month). P-values come from regressing each variable on an indicator for nonreporting month with household-level clustered standard errors *** $p < 0.01$.

Table 1.2. Estimated Effects of Simplified Reporting on Errors

	(1) Any error	(2) Interim error	(3) Discovered through interview	(4) Change-related error
Nonreporting Month X Simplified Reporter	-0.008***	-0.003*	-0.007***	-0.008***
	(0.003)	(0.002)	(0.002)	(0.002)
Observations	323,156	323,156	323,156	323,156
Outcome Mean	0.065	0.023	0.035	0.041
Implied % Change	-11.7%	-13.0%	-20.0%	-19.5%

Notes: All specifications include state and month-year fixed effects. Controls include the state unemployment rate and partisanship of the governor, the Ganong-Liebman policy index, age of the household head, indicators for recertified households, any elderly members, any disabled members, and fixed effects for certification period length. Estimates drop 7 Singleton observations. Outcome means are calculated for simplified reporting households in nonreporting months. Standard errors clustered at the household level and shown in parentheses * $p<0.1$ ** $p<0.05$ *** $p<0.01$.

Table 1.3. Estimated Effects of Simplified Reporting on SNAP Benefit Amounts

	(1) SNAP benefit
Nonreporting Month X Simplified Reporter	18.55***
	(1.515)
Observations	323,156
Outcome Mean	266.21
Implied % Change	7.0%

Notes: All specifications include state and month-year fixed effects. Controls include the state unemployment rate and partisanship of the governor, the Ganong-Liebman policy index, age of the household head, indicators for recertified households, any elderly members, any disabled members, and fixed effects for certification period length. Estimates drop 7 Singleton observations. Outcome mean is calculated for simplified reporting households in nonreporting months. Standard errors clustered at the household level and shown in parentheses * p<0.1 ** p<0.05 *** p <0.01.

Table 1.4. Estimated Effects of Simplified Reporting, Sensitivity Checks

	(1)	(2)	(3)	(4)
	Main estimate	6- or 12-month cert. only	Includes > 6 months since last cert.	SNAP policy indicators
Panel A. Outcome: Any error				
Nonreporting Month X Simplified Reporter	-0.008***	-0.008**	-0.006***	-0.007***
	(0.003)	(0.003)	(0.002)	(0.003)
Observations	323,156	282,142	461,250	323,156
Outcome Mean	0.065	0.065	0.067	0.065
Implied % Change	-12.3%	-12.3%	-9.0%	-10.8%
Panel B. Outcome: SNAP benefit				
Nonreporting Month X Simplified Reporter	18.55***	20.37***	17.69***	18.49***
	(1.515)	(1.859)	(1.347)	(1.516)
Observations	323,156	282,142	461,250	323,156
Outcome Mean	266.21	270.75	272.61	266.21
Implied % Change	7.0%	7.5%	6.5%	6.9%

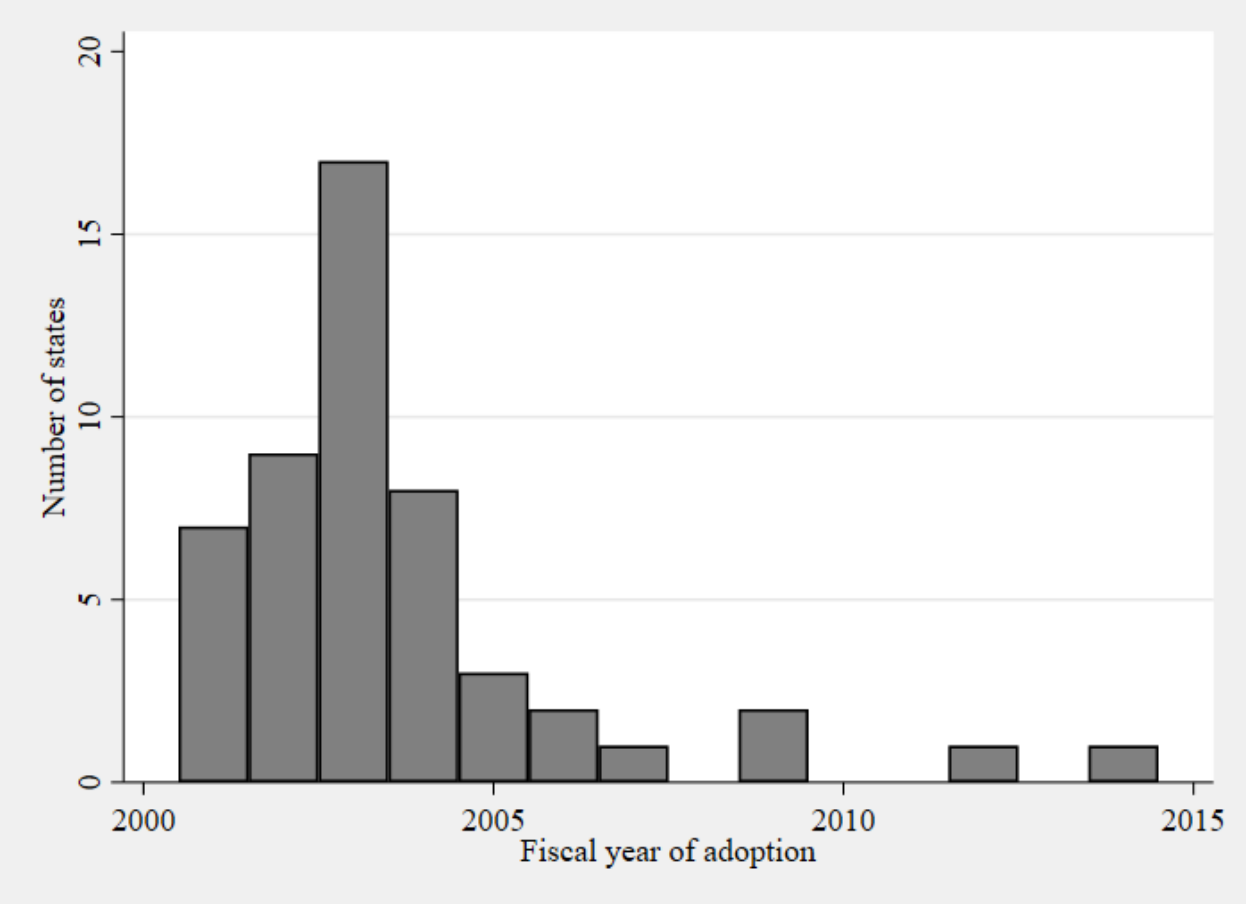
Notes: All specifications include state and month-year fixed effects. Controls include the state unemployment rate and partisanship of the governor, age of the household head, indicators for recertified households, any elderly members, any disabled members, and fixed effects for certification period length. Outcome means are calculated for simplified reporting households in nonreporting months. Standard errors clustered at the household level and shown in parentheses * p<0.1 ** p<0.05 *** p <0.01.

Table 1.5. Estimated Effects of Simplified Reporting on SNAP Benefit Amounts, by Waiver Status

	(1)	(2)
	SNAP benefit	
Nonreporting Month X Simplified Reporter	16.66***	22.11***
	(1.854)	(2.658)
Observations	213,240	109,908
Act on Interim Changes?	All Changes	Positive Changes Only
Outcome Mean	260	269
Implied % Change	6.4%	8.2%

Notes: All specifications include state and month-year fixed effects. Controls include the state unemployment rate and partisanship of the governor, the Ganong-Liebman policy index, age of the household head, indicators for recertified households, any elderly members, any disabled members, and fixed effects for certification period length. Estimates in columns (1) and (2) drop 4 and 11 Singleton observations, respectively. Outcome means are calculated for simplified reporting households in nonreporting months. Standard errors clustered at the household level and shown in parentheses * p<0.1 ** p<0.05 *** p <0.01.

Figure 1.A1. State-level Adoption of Simplified Reporting



Notes: I define fiscal year of adoption as the fiscal year in which the initial implementation month occurs.

Source: https://www.ers.usda.gov/data-products/snap-policy-data-sets/

Table 1.A1. SNAP Household Characteristics**,** All Household**s**

	(1) Full sample	(2) Simplified reporters	(3) Traditional reporters
Any error	0.061	0.061	0.058***
SNAP benefit	272.90	293.87	194.87***
Age of HH head	39.75	37.73	47.25***
Single female headed household	0.30	0.34	0.18***
Any disabled members	0.18	0.15	0.30***
Any elderly members	0.12	0.08	0.27***
Nonworking HH	0.65	0.59	0.90***
HH net income	333.71	355.24	253.58***
HH size	2.42	2.61	1.71***
Certification length	9.65	8.67	13.31***
Initial certification	0.40	0.40	0.40**
Observations	323,163	255,936	67,227

Notes: Sample contains simplified reporting SNAP households in the Quality Control files between FY 2005 and FY 2014. I restrict to households with non-missing data for all the variables presented in the table and households whose "months since last certification" was less than 6 at the time of observation. I weight observations using the household weights in the QC files. Income and benefit amounts are in nominal dollars. Column 2 contains simplified reporters, whereas column 3 contains nonsimplified reporters (traditional reporters). P-values come from regressing each variable on an indicator for nonreporting month with household-level clustered standard errors *** p <0.01.

Table 1.A2. Estimated Effects of Simplified Reporting on Errors, Robustness

	(1) Any error	(2) Interim error	(3) Discovered through interview	(4) Change-related error
Nonreporting Month X Simplified Reporting	-0.030***	-0.034***	-0.015**	-0.026***
	(0.007)	(0.007)	(0.006)	(0.008)
Observations	75,033	18,764	18,764	18,764
Outcome Mean	0.125	0.013	0.024	0.035
Implied % Change	-24.0%	-23.1%	-29.2%	-22.9%

Notes: All specifications include state and month-year fixed effects. Controls include the state unemployment rate and partisanship of the governor, the Ganong-Liebman policy index, age of the household head, indicators for recertified households and fixed effects for certification period length. Estimates in column (1) drop 7 Singleton observations. Estimates in columns (2)-(4) are restricted to FY 2003 to FY 2004 and drop 5 Singleton observations. Outcome means are calculated for households in simplified reporting states in nonreporting months. Standard errors clustered at the household level and shown in parentheses * $p<0.1$ ** $p<0.05$ *** $p<0.01$.

Table 1.A3. Estimated Effects of Simplified Reporting on SNAP Benefit Amounts, Robustness

	(1) SNAP benefit
Nonreporting Month X Simplified Reporting	14.96***
	(2.891)
Observations	75,033
Outcome Mean	215.26
Implied % Change	6.9%

Notes: All specifications include state and month-year fixed effects. Controls include the state unemployment rate and partisanship of the governor, the Ganong-Liebman policy index, age of the household head, indicators for recertified households and fixed effects for certification period length. Estimates drop 7 Singleton observations. Outcome mean is calculated for households in simplified reporting states in nonreporting months. Standard errors clustered at the household level and shown in parentheses * p<0.1 ** p<0.05 *** p <0.01.

Table 1.A4. Estimated Effects of Simplified Reporting, Robustness

	(1) Main estimate	(2) 6- or 12-month cert. only	(3) Includes > 6 months since last cert.	(4) SNAP policy indicators
Panel A. Outcome: Any error				
Nonreporting Month X Simplified Reporting	-0.030***	-0.038***	-0.028***	-0.030***
	(0.007)	(0.009)	(0.007)	(0.007)
Observations	75,033	41,286	83,616	75,033
Outcome Mean	0.125	0.123	0.125	0.125
Implied % Change	-24.0%	-30.9%	-22.4%	-24.0%
Panel B. Outcome: SNAP benefit				
Nonreporting Month X Simplified Reporting	14.96***	14.80***	15.83***	14.87***
	(2.891)	(3.418)	(2.844)	(2.892)
Observations	75,033	41,286	83,616	75,033
Outcome Mean	215.26	217.06	215.26	215.26
Implied % Change	6.9%	6.8%	7.4%	6.9%

Notes: All specifications include state and month-year fixed effects. Controls include the state unemployment rate and partisanship of the governor, age of the household head, indicators for recertified households, and fixed effects for certification period length. Outcome means are calculated for simplified reporting households in nonreporting months. Standard errors clustered at the household level and shown in parentheses * p<0.1 ** p<0.05 *** p <0.01.

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Chapter II: Do SNAP Interviews Cause Program Spillover? The Effects of SNAP Interview Waivers on Multiple Program Participation[1]

1 Introduction

Administrative burdens refer to the learning, compliance, and psychological costs individuals face when accessing government services (Moynihan et al., 2015). While a wide-ranging literature across disciplines finds that reducing administrative burdens in social programs can improve uptake (Aizer, 2003; Bhargava & Manoli, 2015; Gray, 2019; Herd et al., 2013; Herd & Moynihan, 2019; Jenkins & Nguyen, 2022; Kopczuk & Pop-Eleches, 2007), few studies have considered how some administrative burdens may ease the costs of others. Certification interviews, for example, represent a compliance cost that may preclude program uptake (Heflin et al., 2023). However, interviews can also provide social program applicants with information about program rules, which may ease learning costs associated with maintaining eligibility or using benefits, and thus boost program retention or benefit utilization. Certification interviews may also ease learning costs for other programs by facilitating referrals to additional services for which applicants may be eligible but not participating. The purpose of this paper is to examine whether eliminating certification interviews in the Supplemental Nutrition Assistance Program (SNAP) impacts spillover participation from SNAP to other safety net programs.

I leverage a COVID-era policy change that allowed SNAP agencies to temporarily waive interview requirements for new and recertifying SNAP households to estimate the effects of certification interview waivers on multiple program participation. Specifically, I focus on three safety net programs (in addition to SNAP): Temporary Assistance for Needy Families (TANF), childcare subsidies through the Child Care and Development Fund (CCDF), and Medicaid. The

[1] This study was funded by a US Department of Agriculture/Tufts University Research Innovation and Development Grants in Economics (RIDGE).

analysis primarily relies on monthly social service administrative records between 2018 and 2021 for the state of Virginia. By using administrative data, I avoid measurement error of program receipt in public surveys (Meyer et al., 2015; Meyer & Mittag, 2019), and the panel nature allows me to observe individual program receipt histories over time. The ability to separate SNAP participants who enrolled for the first time after waivers became available from those who had already been exposed to the program in the pre-waiver period allows me to isolate the effect of waivers from unobserved shocks coincident with waiver adoption. Specifically, I use SNAP participants with pre-waiver program exposure as a placebo, since this group had already completed a SNAP interview when waivers were made available.

I implement a difference-in-difference-in-differences (triple difference) design, comparing changes in outcomes for SNAP enrollees in counties that waived SNAP interviews with those that did not during the same period and using pre-waiver SNAP participants as a placebo group. I estimate that eliminating the SNAP interview requirement increased the probability that new SNAP enrollees subsequently received TANF. I fail to detect effects for CCDF subsidy or Medicaid participation, though estimates are sufficiently precise to rule out small negative effects. I conclude that SNAP interviews do not facilitate referrals to other programs.

This work makes four primary contributions. First, I refine the conceptual framework of administrative burden to consider how some burdens may reduce the costs associated with others. This is an important contribution because it illuminates potential tradeoffs policymakers seeking to soften administrative burdens may face. While concerns about fraud have historically formed the basis for opposition to efforts to soften administrative burden in the safety net (Herd & Moynihan, 2019), some administrative burdens may ease other, perhaps more formidable,

barriers. Existing administrative burden research has scarcely considered how these costs might interact; and in this case, offset each other.

Existing work in public administration has emphasized how street-level bureaucrats can provide referrals and social support to clients (Lavee, 2021; Tummers et al., 2015). Similarly, recent qualitive evidence highlights how certain features of social policy administration benefit, rather than burden, clients (Barnes et al., 2023). Building on the work in Linos et al. (2020), which documents that adding steps to an administrative process can reduce overall administrative burden, I investigate how eliminating compliance costs for one program may engender learning costs for others.

Second, this paper builds on the program spillover literature. Social policy scholars have noted how participating in one benefit program may facilitate enrollment in others (Cha & Escarce, 2022; Schanzenbach, 2023). On this front, Medicaid has received the most attention, with several studies demonstrating that participation in Medicaid leads to spillover participation in SNAP (Baicker et al., 2014; Burney et al., 2021; Yelowitz, 1996), TANF (Schmidt et al., 2019), and cash welfare (Decker & Selck, 2012).

Despite a robust body of research on the participation spillover effects of Medicaid, less attention has been paid to spillover from SNAP to other programs. Only recently have scholars begun to address this gap. Han (2020) is one of the first papers to examine program spillovers emanating from SNAP. Exploiting state-level variation in SNAP eligibility rules, the author finds that expanded SNAP eligibility increased participation in free school lunch and WIC but not Medicaid (Han, 2020). Other scholars have found evidence of spillover from SNAP to Medicaid, but document larger spillover from Medicaid to SNAP (Schmidt et al., 2024). This paper adds to this literature by providing some of the first evidence on whether SNAP facilitates spillover to

TANF and CCDF subsidy (previous research has only examined spillover from SNAP to Medicaid, WIC, and free school lunch).

What's more, my paper sheds light on a potential mechanism through which program spillovers operate. Scholars have posited two ways that receipt of one benefit program could increase participation in others. The first is changes to labor supply, whereby receipt of one benefit program causes households to reduce their earnings, making them newly eligible for others. Alternatively, enrolling in one program could reduce the transaction costs for others, either through bundled eligibility or improved program awareness. Existing literature suggests that transaction costs predominate (Han, 2020; Yelowitz, 1996), but scholars have been unable to isolate the role of communication with program staff from bundled eligibility. Exploiting variation in a policy that required new SNAP enrollees to receive a brochure on other programs during the application process, Han (2020) is the most convincing attempt to isolate program awareness from joint eligibility, but the author's analysis is limited by the inability to rule out the role of communication with SNAP workers during the certification interviews. Relying on a novel change to the SNAP certification process that temporarily eliminated interviews during the COVID-19 pandemic, I will provide the first evidence on whether communication with SNAP workers during the certification interview serves as a spillover mechanism for SNAP.

Third, this paper adds to the large body of research studying application requirements in safety net programs. Existing research leverages state policy variation in Medicaid and SNAP to investigate the impacts of allowing certification interviews to take place via the telephone (instead of face-to-face) on enrollment (Bartfeld et al., 2015; Wolfe & Scrivner, 2005). However, research on the effects of *eliminating* certification interviews (rather than modifying their form) has struggled with limited policy variation. COVID-induced SNAP interview waivers present a

unique opportunity to identify the impacts of relaxing interview requirements in safety net programs. Exploiting county-level variation in SNAP interview waiver use, Heflin et al. (2023) found that waiving SNAP interviews boosted aggregate caseloads by about 5 percent. Building on this nascent literature, this paper examines whether SNAP interviews promote multiple program participation. In recent years, states have sought ways to re-establish integrated enrollment mechanisms after welfare reform severed linkages across programs (Fox et al., 2023). The results of this paper shed light on whether eliminating certification interviews undermines these efforts.

Finally, this work contributes to a growing body of research that evaluates the impact of temporary, COVID-era administrative changes to SNAP and other safety net programs (Barnes & Petry, 2021; Fannin et al., 2024; Headrick et al., 2022; Heflin et al., 2023; Pukelis, 2023; Whaley & Anderson, 2021). Like many COVID-era changes to the safety net, SNAP interview waivers were temporary in scope, initially expiring only three months after the USDA first made them available to states. USDA eventually extended the availability of waivers through the end of the public health emergency, which ended in May 2023. Recently, some SNAP administrators and advocacy groups have urged the federal government to end SNAP interview requirements permanently (Headrick et al., 2022; Lewis, 2024). The results from this paper may be useful for policymakers considering such a change.

Certification interviews are pervasive in the US safety net. SNAP, WIC, Medicaid, and TANF require applicants to participate in some form of an interview (Holcomb et al., 2003). As an additional contribution, the results of this analysis may be also informative of the consequences of relaxing interview requirements for other safety net programs.

2 Background and Framework

A. Description of SNAP, TANF, Childcare Subsidies, and Medicaid

In this paper, I document how administrative procedures for SNAP impact enrollment for other programs Specifically, I investigate whether SNAP interviews influence the probability that new SNAP enrollees subsequently enroll in TANF, childcare subsidies, or Medicaid. SNAP, TANF, childcare subsidies, and Medicaid are federally funded but administered by the Virginia Department of Social Services (VDSS).

SNAP

SNAP helps low-income households purchase food for home consumption. Each month, SNAP recipients receive money on an Electronic Benefits Transfer (EBT) card, which is accepted at most grocery and convenience stores. SNAP is among the largest social safety net programs in the US (U.S. Department of Agriculture Food and Nutrition Service, 2021). In fiscal year 2019, SNAP provided an average monthly household benefit of about $130 per person, totaling over $55 billion in benefits to nearly 18 million households for the full year. In fiscal year 2020, SNAP reached 78 percent of all eligible people (Cunnyngham, 2023).

The federal government establishes SNAP benefit levels and baseline eligibility criteria but grants localities some program flexibilities. With some exceptions, federal guidelines restrict SNAP eligibility to households (1) whose gross income does not exceed 130 percent of the Federal Poverty Level (FPL), (2) whose net income (gross income minus allowable deductions) does not exceed 100 percent of the FPL, and (3) whose assets (excluding a home and retirement accounts) do not exceed $2,500. While eligibility criteria are established by the federal government, many states have modified them. For example, most states use federal policy flexibilities to relax the gross income and asset tests. As of 2016, only four states required an

asset test, and only 12 used the 130 percent gross income eligibility threshold, with 14 states expanding the gross income threshold to 200 percent of the FPL (U.S. Department of Agriculture Food and Nutrition Service, 2018). Effective July 1, 2021, Virgina no longer requires an asset test and expanded gross income eligibility to 200 percent of the FPL for SNAP (Virginia Department of Social Services, 2021).

TANF

TANF, which was created out of the Aid to Families with Dependent Children program following the passage of the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA), is a federal block grant that states can use in a variety of ways to support low-income families, including cash assistance, work support and training, and childcare assistance. Since the enactment of PRWORA in 1996, TANF enrollment has declined and in 2020 it reached only about 21 percent of eligible people, or 2.8 million people in an average month, a historic low (Crouse, 2022; Shrivastava & Thompson, 2022).

TANF eligibility in Virginia is restricted to households that (1) comprise a child under 18, (2) are deemed "needy," and (3) satisfy "good conduct" and work requirements (Jones, 2018). To be deemed "needy," household income cannot exceed the state's maximum monthly income limits. TANF monthly income limits vary by household size. To account for differences in the cost of living across the state, Virginia has income limits for two residence groups. As of July 2021, the TANF maximum monthly income limit in Virginia for a household of three was $920 or $1,119 (Virginia Department of Social Services, 2018), representing about 50 and 61 percent of the FPL, respectively. Note that in Virginia the SNAP gross income limit of 130 percent (and later 200 percent) of the FPL is considerably higher than TANF. "Good conduct" requirements primarily refer to school attendance and cooperation with child support assignment and

collection (Jones, 2018). Additionally, all non-elderly and non-disabled adult members of TANF households must participate in work-related activities through the Virginia Initiative for Education and Work Program.

Unlike SNAP, which sets benefit levels at the federal level, states independently determine benefit levels for TANF cash assistance. TANF cash payments in Virginia are tied to household size and location of residence, like the monthly income limits. As of July 2021, the maximum monthly TANF cash payment for a household of three was $459 or $559 (Virginia Department of Social Services, 2018).

Childcare Subsidies

The federal government subsidizes childcare expenses for low-income households through the Child Care and Development Fund (CCDF). In fiscal year 2019, combined state and federal CCDF dollars totaled over $6.7 billion, serving an average of about 1.4 million children per month or 16 to 23 percent of eligible children (ACF, 2021, 2022; Chien, 2022). CCDF subsidies are designed to support stable, high-quality childcare services and promote work by reducing the costs of childcare for low-income parents.

Under federal rules, CCDF subsidy eligibility is restricted to children that (1) are under age 13 (children with disabilities under age 19 are also eligible), (2) reside with a parent or parents who work or participate in work-related activities, and (3) belong to a household whose income and assets do not exceed 85 percent of the state median and $1 million, respectively (Lynch, 2022). As a block grant program, the federal government gives states considerable flexibility in designing the particulars of their childcare subsidy programs, such as defining what constitutes “work-related” activities, application and waiting list procedures, and family

copayments (Minton et al., 2019). Between 2019 and 2021, Virginia limited CCDF subsidy eligibility to low-income households in which all parents worked, actively looked for work, or participated in job-training or education activities through SNAP or TANF (Virginia Department of Social Services, 2019, 2023a). The maximum income eligibility thresholds for CCDF subsidies in Virginia during the same period were between 150 percent and 185 percent of the FPL, depending on location of residence (Virginia Department of Social Services, 2019, 2023a). Note that until July 1, 2021, when Virginia raised SNAP's gross income eligibility cutoff to 200 percent of the FPL, all Virginia SNAP recipients were income-eligible for CCDF subsidies, regardless of location of residence.

Medicaid

Medicaid provides health coverage to low-income households. With expenditures totaling $805.7 billion and enrollment over 90 million in 2022 (Centers for Medicare & Medicaid Services, 2022), Medicaid is the largest safety net program in the US. In 2019 the program reached between 78 and 80 percent of eligible parents and between 91 and 92 percent of eligible children (Haley et al., 2021).

Prior to 2019, eligibility for Medicaid[2] in Virginia was restricted to children and pregnant women with household incomes up to 200 percent of the FPL and parents with household incomes up to 49 percent of the FPL. Beginning in 2019, Virginia expanded Medicaid under the Affordable Care Act, extending eligibility to parents and childless adults with household incomes at or below 133 percent of the FPL, at which point all Virginia SNAP recipients became income-eligible for Medicaid (Centers for Medicare & Medicaid Services, 2020, 2024).

[2] I use Medicaid to refer to Medicaid and the Children's Health Insurance Program (CHIP).

B. SNAP Policy Changes

Virginia is one of ten states that administers SNAP at the county level (U.S. Department of Agriculture Food and Nutrition Service, 2018). The Virginia Department of Social Services (VDSS) requires households to file an application for SNAP benefits with their local department of social services. Upon receipt of an application, program rules require a SNAP eligibility worker to conduct a certification interview, typically by telephone. All households, including new and recertifying, must participate in a certification interview. VDSS permits the head of household, a spouse, an authorized representative, or any responsible member of the household[3] to sit for the interview.

During certification interviews, VDSS guidelines require eligibility workers to explain basic program procedures, household's rights and responsibilities, and inform applicants of the services the agency provides (Virginia Department of Social Services, 2018, 2022). Interviews are an opportunity for applicants to ask questions about the program and for eligibility workers to explore and resolve with the household any unclear or incomplete information contained in the application. Interviews are also an opportunity for eligibility workers to connect SNAP households to other programs for which they may be eligible but not participating, reifying the department's commitment to an integrated service approach that encourages referrals to other services within the agency, as formally stated in the VDSS Practice Model (Virginia Department of Social Services, 2022). This commitment to engaging SNAP applicants during the interview about needs beyond SNAP is mirrored by official USDA guidance to states, which explicitly encourages SNAP agencies to leverage certification interviews to offer other services such as

[3] Any household member 18 or older who has sufficient knowledge of the household's circumstances.

cash assistance, child care, or other benefit programs (U.S. Department of Agriculture Food and Nutrition Service, 2023).

But is there any evidence that eligibility workers facilitate referrals in this way? A longstanding literature in public administration documents how street level bureaucrats (SLBs) adopt a "social work narrative" (Dias & Maynard-Moody, 2007), similar to the notion of Public Service Motivation (Perry, 1996; Ritz et al., 2016), to engage in what Tummers and coauthors (2015) classify as "moving toward clients," or pragmatically adjusting their services to the particular needs of clients: these behaviors include offering informal assistance, like referrals and social support, to program applicants (Lavee, 2021; Tummers et al., 2015). Indeed, numerous social policy scholars have observed that, once an individual begins to engage with an eligibility worker for one program, the costs associated with learning about other programs decline substantially (Cha & Escarce, 2022; Schanzenbach, 2023; Schmidt et al., 2024; Yelowitz, 1996).

In the context of SNAP, Han (2020) describes how the application process may increase awareness of other programs through communication with SNAP workers during the interview. Moreover, states that have adopted broad-based categorical eligibility for SNAP are required to provide a brochure on other programs to SNAP applicants during the enrollment process (Han, 2020).

In response to the COVID-19 pandemic that triggered a tremendous surge in SNAP applications (The New York Times, 2021), the federal government allowed states to dispense with the certification interview requirement to simplify the enrollment process for both SNAP applicants and agencies (Center on Budget and Policy Priorities, 2020). USDA first made SNAP interview waivers available to all states in March 2020 through May 2020 (U.S. Department of Agriculture Economic Research Service, 2020). Between June 2020 and September 2020 USDA

required state agencies to apply for extensions each month. The Continuing Appropriations Act of 2021 (PL 116-159), however, converted interview waivers back to blanket state options in October 2020, and the USDA allowed states to extend their use through the month following the end of the national public health emergency (Center on Budget and Policy Priorities, 2020).

C. Conceptual Framework

I frame the analysis using the conceptual model of administrative burden in (Moynihan et al., 2015), which conceives of administrative burden as consisting of three distinct costs. Learning costs represent the effort applicants must expend to learn about a program, how to apply, and how to redeem benefits. Compliance costs include the time spent filling out forms and demonstrating eligibility. Psychological costs refer to the stress and stigma applicants might experience when engaging with social services.

SNAP's certification interview constitutes a compliance and potential psychological cost, which may preclude program participation. Unlike the SNAP application, which households can fill out at their convenience, certification interviews must occur during business hours and require coordination between applicant households and eligibility workers. Applicants may be forced to take time off work and feel stigmatized while answering intrusive questions about their household circumstances. If applicant households miss their scheduled interview, they must promptly reschedule or risk being denied benefits (Virginia Department of Social Services, 2022).

However, if certification interviews provide applicants with information about or referrals to other social services, they also reduce learning costs for other programs, creating an important tradeoff for policymakers to consider. Waiving the SNAP interview requirement may represent a reduction in compliance costs for SNAP (thereby increasing SNAP take-up) but an increase in

learning costs for other benefit programs (thereby precluding multiple program participation). Existing research has scarcely considered how these costs interact; and in this case, offset each other. This subtlety is important because it paints a more complicated picture for policymakers seeking to evaluate the effects of reducing administrative burdens. Policies that reduce compliance costs may be less attractive to policymakers if they also increase learning costs.

3 Empirical Strategy

A. Data and Sample

To study the impact of SNAP interview waivers on multiple program participation, I analyze monthly individual-level data on Virginia SNAP recipients between 2018 and 2021. The USDA first made SNAP interview waivers available in 2020. To have two full years of pre-treatment data, I begin the analysis in 2018. The final year of analysis is 2021, the most recent year for which data are available.

This analysis relies on two principal data sources: administrative data on SNAP recipients from the Virginia Longitudinal Data System (VLDS) and primary county-level SNAP interview waiver data. Administrative data from the VLDS are in panel form and contain information on all recipients of social services[4] residing in the state. Specifically, the VLDS data include demographic information (such as birthdate, race, ethnicity, gender), covered employment quarterly earnings[5] and number of jobs, and monthly SNAP, TANF, and Medicaid participation indicators. Using SNAP case numbers, I also identify households in which a child received CCDF subsidies to construct monthly household-level childcare subsidy indicators. I merge county information in the VLDS data with primary SNAP certification waiver data (Heflin et al.,

[4] SNAP, TANF, Medicaid, or CCDF subsidies.
[5] I adjust earnings data for inflation using the Consumer Price Index for all urban consumers (CPI-U).

2023) to determine whether an observation resided in a county that waived the certification interview.

While the administrative nature of the VLDS data overcomes important measurement problems common to household surveys (Meyer et al., 2015), the analysis is limited to Virginia. Therefore, the results will not necessarily generalize to other states, particularly those that do not administer SNAP at the county level. Nevertheless, Virginia is a populous state (12th in 2020) and compositionally similar to the broader U.S. in terms of race/ethnicity and economic development. Likewise, the SNAP population in Virginia resembles nationwide averages (Gray et al., 2023).

The VLDS data contain 34,790,532 person-months of SNAP participants between 2018 and 2021. I drop observations in counties with missing certification interview waiver data (n=1,268,984). Since information about non-SNAP programs received during certification interviews is particularly relevant to households without any previous exposure to other programs, I drop SNAP participants who were on another VDSS program (since at least 2016) before their initial SNAP enrollment (n=23,904,623). The remaining sample (n=9,616,925) contains only individuals whose first contact with the VDSS system was SNAP (and not another program); that is, SNAP participants whose multiple program participation decisions should be most affected by a referral during an interview. I refer to this group as "SNAP first" participants.

To get a sense of the size and composition of this "SNAP first" group, Table 2.1 presents the entry VDSS programs (i.e., the first program received) for all new VDSS participants between 2015 and 2022, stratified by race/ethnicity, gender, and age. SNAP was the entry VDSS program for about 26 percent of all enrollees over the period. The largest entry program was Medicaid (61 percent), and about 13 percent of VDSS participants first enrolled in two or more

programs. SNAP was more likely to be the entry VDSS program for Black enrollees (35 percent) compared to White enrollees (25 percent), whereas Medicaid was the entry program for 81 percent of VDSS enrollees belonging to the "other" race/ethnicity category, compared to 48 percent for Black enrollees and 63 percent for Hispanic and White enrollees. Black enrollees were slightly more likely to have had more than one entry program (16 percent) than other race/ethnicity groups. SNAP was less commonly an entry program for children (18 percent) than adults (30 percent).

While Medicaid was the most common entry VDSS program overall (61 percent), a little over a quarter of all VDSS participants were exposed to SNAP before any other program, which suggests that initial SNAP interviews may be a reasonable referral point. Moreover, SNAP was a particularly common VDSS entry point for Black enrollees. If SNAP interviews facilitate multiple program participation, then waiving them could raise racial equity concerns.

Table 2.2 presents summary statistics for "SNAP first" participants in the pre-waiver period (January 2018 and February 2020), stratified by waiver status. For the full sample of new SNAP enrollees (column 1), about 61 percent were on another program. Medicaid participation was far more common (60 percent) than TANF (3 percent) and CCDF subsidy (2 percent). About 55 percent of SNAP participants were female. White and Black SNAP participants comprised 49 and 44 percent of the sample, respectively, while the remaining 7 percent of the sample was split roughly equally between Hispanic and another racial identity. The average age of SNAP participants was 37, and 77 percent resided in an urban county. On average, SNAP participants belonged to households with 1.32 children and 1.33 adults, worked 0.34 jobs, and earned $1,049 quarterly. Note that number of jobs worked and quarterly earnings are small because the sample contains nonworking age individuals.

Columns 2-3 reveal that pre-waiver SNAP participant characteristics were remarkably similar between waiver and nonwaiver counties. Differences across virtually all observable characteristics were neither statistically nor economically significant. The similarity between waiver and nonwaiver counties across observables suggests county-level waiver adoption decisions were uncorrelated with the composition of the SNAP population and help motivate the empirical approach I describe in the subsection after next.

B. Likely Eligible Subsamples

To account for category-based differences in eligibility criteria between SNAP, TANF, and CCDF subsidies, I analyze only the subset of "SNAP first" participants who were likely categorically eligible for each non-SNAP VDSS program. Recall that TANF is restricted to households with at least one child under 18. When analyzing TANF participation, I restrict to SNAP participants belonging to a household with at least one child. To account for CCDF's child age and work requirements, I restrict the analysis of CCDF subsidy participation to SNAP participants belonging to a household with at least one child under 13 and in which all adults worked.

To account for income-based differences in eligibility criteria between SNAP, TANF, CCDF subsidies, and Medicaid, I further restrict the likely eligible subsamples based on household earnings.[6] I merge Virginia income limits for TANF, CCDF, and Medicaid from VDSS guidance manuals and Centers for Medicare and Medicaid Services using household size and county of residence (Centers for Medicare & Medicaid Services, 2020, 2024; Virginia Department of Social Services, 2018, 2019, 2023a). I use Virginia's TANF income limits as of July 2021

[6] Because Virginia's household-level income thresholds for TANF and CCDF are monthly, I assess income-eligibility for these programs using quarterly earnings of SNAP participants divided by three.

because I was unable to retrieve income limit data before 2021. Since Virginia's CCDF income limits before 2019 are not publicly available, I assign 2019 limits for 2018.

I further restrict to households that are *always* (rather than at the time of observation) likely eligible for each program so that the composition of the sample is static. In Panel A of Table 2.A1, I demonstrate that the estimates are largely insensitive to this decision.

C. Identification Strategy and Method

To estimate the effects of SNAP interview waivers on multiple program participation, I leverage variation in waiver adoption at the local SNAP agency level. Using a difference-in-differences framework, I compare changes in multiple program participation between SNAP participants in counties that waived certification interviews with those that did not.

Event-study Analysis

The difference-in-differences design assumes that, absent the waiver, multiple program participation rates would have evolved similarly for waiver and nonwaiver counties. I use the following event-study regression to assess the credibility of this assumption:

(1) $Y_{ict} = \beta_0 + \Sigma_{n=-N,n\neq-1}^{N} \beta_n Waiver_{ct}^{n} + \beta_x X_{ict} + \gamma_c + \gamma_t + \varepsilon_{ict}$

where the dependent variable, Y_{ict}, is an outcome (separate indicators for TANF, CCDF subsidy, and Medicaid participation) for individual *i* in county *c* in month-year *t*. $Waiver_{ct}^{n}$ are a set of dummy variables indicating each observations timing relative to initial waiver availability (I omit the month-year prior to availability). To account for changes in the composition of the SNAP population, I control for SNAP participant characteristics, X_{ist}, including gender, race/ethnicity,

age, quarterly earnings, indicators for urban residence[7], number of children and number of adults in the household, and number of jobs worked. I also include fixed effects for SNAP spell start month.

Additionally, I control for county characteristics that may correlate with waiver adoption and outcomes. For economic characteristics, I use annual county unemployment rate averages from the Bureau of Labor Statistics (U.S. Bureau of Labor Statistics, 2023). For COVID-19 characteristics, I use monthly new cases and deaths from The New York Times COVID database (The New York Times, 2022). I also control for WIC physical presence requirement waivers, which may also influence multiple program participation (Fannin et al., 2024). Finally, I include county and month-year fixed effects. I cluster standard errors at the county level.

Event-Study Results

Figure 2.1 presents estimates from the event study regressions for the full sample. The leftmost vertical line (at -1) represents the month before the federal government made waivers initially available. The rightmost vertical line (at+6) indicates the month before the federal government issued blanked approval for extensions of waivers until the end of the public health emergency.

Pre-waiver event-study coefficients are not statistically significant and largely exhibit no systematic pattern. TANF and CCDF subsidy participation increased slightly in waiver counties a year or more before waivers became available, but in the months of the year immediately preceding waiver availability trends for the waiver and nonwaiver counties were parallel. In the months following waiver availability, participation for all three programs declined in waiver

[7] I classify urban counties using the 2013 Economic Research Service Urban Rural Continuum Codes.

counties relative to nonwavier counties, suggesting that eliminating SNAP interviews resulted in a decline in multiple program participation.

Event-Study Placebo

While the lack of systematic pre-trends between waiver and nonwaiver counties during the lead-up to waiver adoption is comforting, parallel pre-trends are neither necessary nor sufficient to satisfy the identifying assumption of the difference-in-differences design (Cunningham, 2021; Kahn-Lang & Lang, 2020). Though multiple program participation rates in waiver and nonwaiver counties evolved similarly in the pre-period, perhaps they would have evolved differently in the post period even in the absence of waivers. For example, declining multiple program participation in waiver counties could be a function of unobserved policy or administrative changes to non-SNAP programs coinciding with waiver adoption, rather than the waiver itself.

To assess the merit of this potential threat, Figure 2.1 displays results from re-estimating equation (1) on a placebo subsample containing only SNAP participants who enrolled in SNAP *before* waivers became available, a group who experienced at least one certification interview in the pre-waiver period. If SNAP interviews provide referrals to other programs, SNAP participants who enrolled before waivers became available should have existing knowledge of other programs in the post-waiver period, and thus the absence of interviews in the post-waiver period should have no effect on their multiple program participation rates.

Since the placebo subsample only drops SNAP participants who first enrolled in the post-waiver period, pre-waiver event-study coefficients for the placebo sample in Figure 2.2 are identical to the full sample in Figure 2.1, as expected. However, post-waiver coefficients for the

placebo sample also exhibit a similar pattern to Figure 2.1, suggesting that TANF, CCDF subsidy, and Medicaid participation declined in waiver counties even among SNAP participants who had already experienced at least one certification interview in the pre-waiver period. The placebo tests indicate that declining multiple program participation in the post-wavier period observed in Figure 2.1 cannot be attributed solely to the waiver and may be partially a result of unobserved shocks correlated with waiver use.

Triple Difference Model

To account for unobserved shocks coincident with waiver adoption, I employ a triple difference approach, using participants in nonwaiver counties and pre-waiver enrollees in waiver counties as comparison groups. I estimate the following regression equation:

$$(2)\ Y_{ict} = \beta_0 + \beta_1 Treat_{ct} + \beta_2 PostEnrollee_{ict} + \beta_3 (Treat * PostEnrollee)_{ict} + \beta_x X_{ist} + \gamma_c + \gamma_t + \varepsilon_{ict}$$

where $Treat_{ct}$ is an indicator for whether the SNAP interview waiver was in place county *c* in month-year *t*. I omit the months of March 2020 through December 2020 from the analysis to reduce error in the waiver measure, which was collected between January 2021 and April 2021.[8] I test the sensitivity of the estimates to this decision in Panel B of Table 2.A1. $PostEnrollee_{ict}$ is an indicator for SNAP participants who enrolled after waivers became available, or the "treated" units (i.e., an indicator for *not* being in the placebo subsample described in the preceding subsection). The coefficient on $Treat_{ct}$ captures any change in outcomes among pre-

[8] A September 2020 interview waiver extension request from Virginia suggests take-up of the interview waiver was more widespread among localities between April 2020 and July 2020, the months immediately following initial waiver availability, than during the survey period for the waiver data used here (U.S. Department of Agriculture Food and Nutrition Service, 2020). Therefore, the waiver data used here is likely to misclassify waiver counties in these early months of waiver availability.

waiver enrollees (i.e., placebo units) correlated with waiver adoption. The coefficient on the interaction term $(Treat * PostEnrollee)_{ict}$ is the triple difference coefficient and represents the differential effect of waivers on post-waiver enrollees. The triple difference specification controls for unobserved, time-varying confounders that affect both pre- and post-waiver SNAP enrollees.

4 Main Results

Table 2.3 presents results from the regression described by equation (2). The sign of the triple difference coefficients is consistently positive for all three programs (columns 1-3). The positive signs on the coefficient of interest in the triple difference framework contrasts with the negative trends in multiple program participation I observe for waiver counties in the event study in Figure 2.1, affirming the importance of using pre-waiver SNAP enrollees as an additional control in a triple difference framework. In column (1) I find that certification interview waivers increased subsequent TANF participation among likely TANF-eligible new SNAP enrollees by a statistically significant 3.5 percentage points, or about 61 percent of the baseline pre-waiver TANF participation rate. While the coefficient of interest is statistically significant at conventional levels for only the TANF outcome, estimates are sufficiently precise to rule out small negative effects of waivers on CCDF subsidy and Medicaid participation for SNAP households likely eligible for each. Using the lower bounds of the 95% confidence intervals, I can rule out waiver-induced reductions in CCDF subsidy and Medicaid participation of 4 and 1 percent or more, respectively.

A. Heterogeneous Effects

To examine whether SNAP interview waivers had differential impacts on multiple program participation across SNAP households, I re-estimate equation (2) separately for a series of subgroups. Figures (2.3)-(2.5) present results for the three outcomes of interest. Each figure

displays triple differences coefficients from a separate regression for each subgroup. I reproduce estimates for the full sample as a benchmark.

Figure 2.3 displays results from the heterogeneity analysis for TANF participation. Barring households with an elderly member, estimates for all subgroups are positive and largely statistically significant, like the estimate for the full sample. Estimated effects were larger for households without elderly members than for those with them, but confidence intervals for the other subgroups overlap, making it difficult to detect differences.

Estimates for CCDF subsidy participation in Figure 2.4 are near zero with narrow confidence intervals. However, I estimate a statistically significant negative effect for individuals in households with an elderly member, like TANF (Figure 2.3). Subsample estimates for Medicaid (Figure 2.5) also generally mirror the estimates for the full sample and are not statistically different across groups.

B. Sensitivity of Estimates

Likely Eligible Subsamples

The main triple difference analysis restricts each sample to the subset of SNAP participants who were likely eligible for each respective program for the entire sample period (i.e., always eligible). In Panel A of Table 2.A1, I analyze SNAP participants who were likely eligible for each respective program at the time of observation (i.e., contemporaneously eligible). To illustrate the subtlety of the two approaches, let us consider an eligibility criterion for CCDF subsidies. CCDF subsidies are largely limited to families with a child under 13 (among other criteria). In the main triple difference analysis (Table 2.3), I restrict the CCDF sample to only SNAP participants with a child under 13 for the entire sample period, whereas in Panel A of

Table 2.A1 I analyze SNAP participants with a child under 13 at the time of observation. The former is a more exclusionary approach but mitigates concerns about bias from compositional shifts in the eligible population.

Results from Panel A of Table 2.A1 demonstrate that estimates for TANF and Medicaid participation are similar to the main results in Table 2.3. However, I am less confident that waivers did not negatively influence CCDF subsidy participation among the "contemporaneously eligible" population. I can rule out waiver-induced reductions in CCDF subsidy participation of 4 percent or more for the "always eligible" sample (Column 2, Table 2.3) but only 20 percent or more for the "contemporaneously eligible" sample (Column 2, Panel A, Table 2.A1).

Sample Period

In the main triple difference analysis presented in Table 2.3, I also omit the months between March 2020 and December to minimize attenuation bias from measurement error in the treatment variable, which is from survey of local SNAP agencies that occurred between January 2021 and April 2021. If waiver implementation was dynamic in the months leading up to the survey (i.e., before January 2021), then the SNAP interview waiver data will misclassify waiver and nonwaiver counties in between March 2020 and December 2020, biasing estimates towards zero. Panel B of Table 2.A1 presents estimates using the full sample period, between January 2018 and December 2021 (hereafter referred to as the "broadened sample"). The estimates for the broadened sample period are modestly attenuated, as anticipated, but largely mirror the main results in Table 2.3.

"SNAP First"

Panels C and D of Table 2.A1 present results using alternative definitions of the "SNAP first" sample. In the main analysis, I define "SNAP first" as individuals whose initial SNAP enrollment occurred before TANF, CCDF subsidy, or Medicaid. However, SNAP certification interviews may influence multiple program participation in the month of SNAP enrollment, if, for example, SNAP eligibility workers encourage SNAP applicants to apply for other programs at the SNAP certification interview. In Panel C of Table 2.A1, I analyze individuals whose initial SNAP enrollment occurred before or in the same month of TANF, CCDF subsidy, or Medicaid, to account for this possibility. The estimates are similar to the main results in Table 2.3.

Given Medicaid's reach (recall from Table 2.1 that Medicaid was the entry program about 61 percent of all VDSS enrollees between 2015 and 2022), some might worry that the "SNAP first" sample, which ignores "Medicaid first" individuals, is unusual or unrepresentative. To assess this concern, Panel D restricts to individuals whose initial SNAP enrollment occurred only before TANF or CCDF subsidy (but not Medicaid). The estimates in Panel D for the TANF and CCDF subsidy outcomes are qualitatively similar to the main results in Table 2.3, though the lower bound is larger in magnitude for the CCDF subsidy outcome. The coefficient of interest for the Medicaid outcome in Panel D becomes negative but remains statistically insignificant and near zero in practical terms.

5 Conclusion

During the COVID-19 pandemic, policymakers enacted sudden and drastic administrative changes to social programs. One such change, SNAP interview waivers, gave state and local SNAP agencies the option to temporarily waive certification interviews for new and recertifying SNAP participants. While eliminating SNAP interviews improved SNAP uptake (Heflin et al., 2023), SNAP interviews can provide information to SNAP households about other programs for

which they may be eligible but not participating. Consequently, eliminating SNAP interviews may improve SNAP take-up but impose new learning costs for other programs, inhibiting multiple program participation. Ultimately, I do not find evidence to support this view.

Using administrative records for SNAP participants in Virginia, I construct a triple difference design, exploiting waiver variation stemming from two comparison groups of SNAP participants: (1) those who enrolled before waivers became available (and thus experienced a certification interview in the pre-waiver period) and (2) those in nonwavier counties. I estimate that SNAP interview waivers increased—not decreased—subsequent TANF participation by about 61 percent among new SNAP enrollees. Though speculative, a potential explanation for this positive finding is that SNAP interview waivers may have simultaneously eliminated the TANF interview requirement because of how VDSS processes TANF applications. Virginia regards all applications for TANF, except those on which the household opts out, as joint applications for both TANF and SNAP (Virginia Department of Social Services, 2018). Furthermore, Virginia policy forbids joint TANF-SNAP applicants from being subjected to two certification interviews, requiring VDSS workers to conduct only a single certification interview for joint TANF-SNAP applicants (Virginia Department of Social Services, 2018).

The language of the SNAP interview waiver is silent on whether it may be used to eliminate interviews for joint TANF-SNAP applications or just SNAP-only applications. Nonetheless, several states elected to align SNAP interview waivers with their TANF and cash assistance program requirements, using SNAP interview waivers to effectively eliminate both the TANF and SNAP interview requirements (District of Columbia Department of Human Services, 2024; Hawaii Department of Human Services, 2021; Maryland Department of Human Services, 2023; Texas Health and Human Services, 2022). According to VDSS agency documents, Virginia also

appeared to have bundled SNAP interview waivers with TANF in this way (Arlington County Department of Human Services, 2022; Virginia Department of Social Services, 2023b). Consequently, the positive estimated effect of SNAP interview waivers on TANF participation may be the result of the coincident elimination of TANF's interview requirement. This finding reflects how administrative burdens may function at the agency level (rather than the program level), given that a single government agency typically administers multiple programs (Wu & Meyer, 2023).

While I do not find that waivers yielded similarly large and statistically significant increases in subsequent CCDF subsidy and Medicaid participation among new SNAP enrollees, estimates are sufficiently precise to rule out small negative effects, implying that SNAP interviews did not meaningfully reduce CCDF subsidy and Medicaid participation.

The analysis has two primary limitations. First, the findings may not generalize outside of Virginia. While characteristics of Virginia SNAP participants resemble the broader US (Gray et al., 2023), state heterogeneity in the implementation of SNAP interviews remains a threat to external validity. For example, due to differences in local SNAP agency policy, practices, or culture, some states may be more likely to emphasize program referrals during SNAP interviews than Virginia, in which case eliminating SNAP interviews might have a negative impact on multiple program participation.

A second concern is about internal validity. Unobserved non-SNAP policy changes correlated with SNAP interview waiver use could also affect multiple program participation. For example, if policy changes affecting program accessibility for TANF, CCDF subsidies, or Medicaid were coincident with SNAP interview waiver adoption, then the estimated effect of waivers on multiple program participation would be confounded. I control for county-level changes to WIC

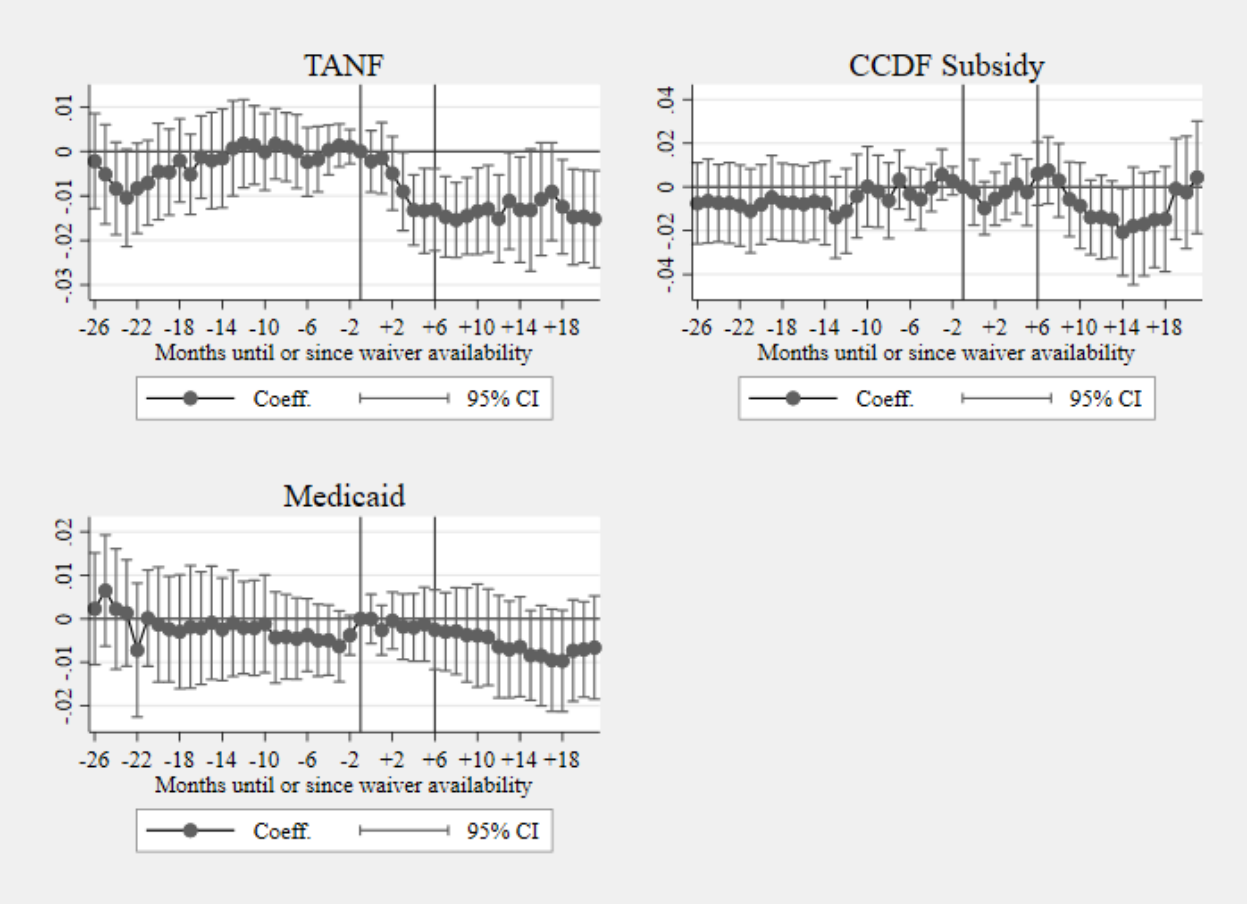
physical presence requirements to mitigate the threat from WIC. I am not unaware of any changes to TANF, CCDF subsidy, or Medicaid that varied across counties in Virginia during the sample period, but I cannot rule this threat out definitively considering the results from the placebo event study in Figure 2.2, which revealed that multiple program participation declined following SNAP interview waiver adoption even for SNAP participants who had already experienced a certification interview. Nonetheless, the triple difference set up controls for unobserved changes to waiver counties affecting all SNAP participants. Consequently, a remaining confound would have to be an unobserved change correlated with waiver adoption and multiple program participation but only affecting SNAP enrollees who enrolled in the post-waiver period.

These findings have several implications for policymakers and future research. Paired with evidence that waivers increased SNAP caseloads in Heflin et al., (2023), the results from this paper imply that making SNAP interview waivers permanent will reduce SNAP compliance costs and is unlikely to have negative impacts on multiple program participation of SNAP participants. Moreover, the estimates are potentially informative about the consequences of eliminating certification interviews for other programs that require them, such as WIC, Medicaid, and TANF (Holcomb et al., 2003). These findings also add to the program spillover literature. In particular, the results complement Han (2020), which found spillover from SNAP to other programs but was unable to disentangle communication with SNAP workers from other mechanisms. Leveraging a temporary waiver of SNAP interviews, I find no evidence of communication-induced spillover from SNAP to CCDF subsidy or Medicaid.

While I do not find evidence to support the notion that SNAP interviews reduce learning costs for other programs, future research is needed to evaluate whether they reduce learning costs

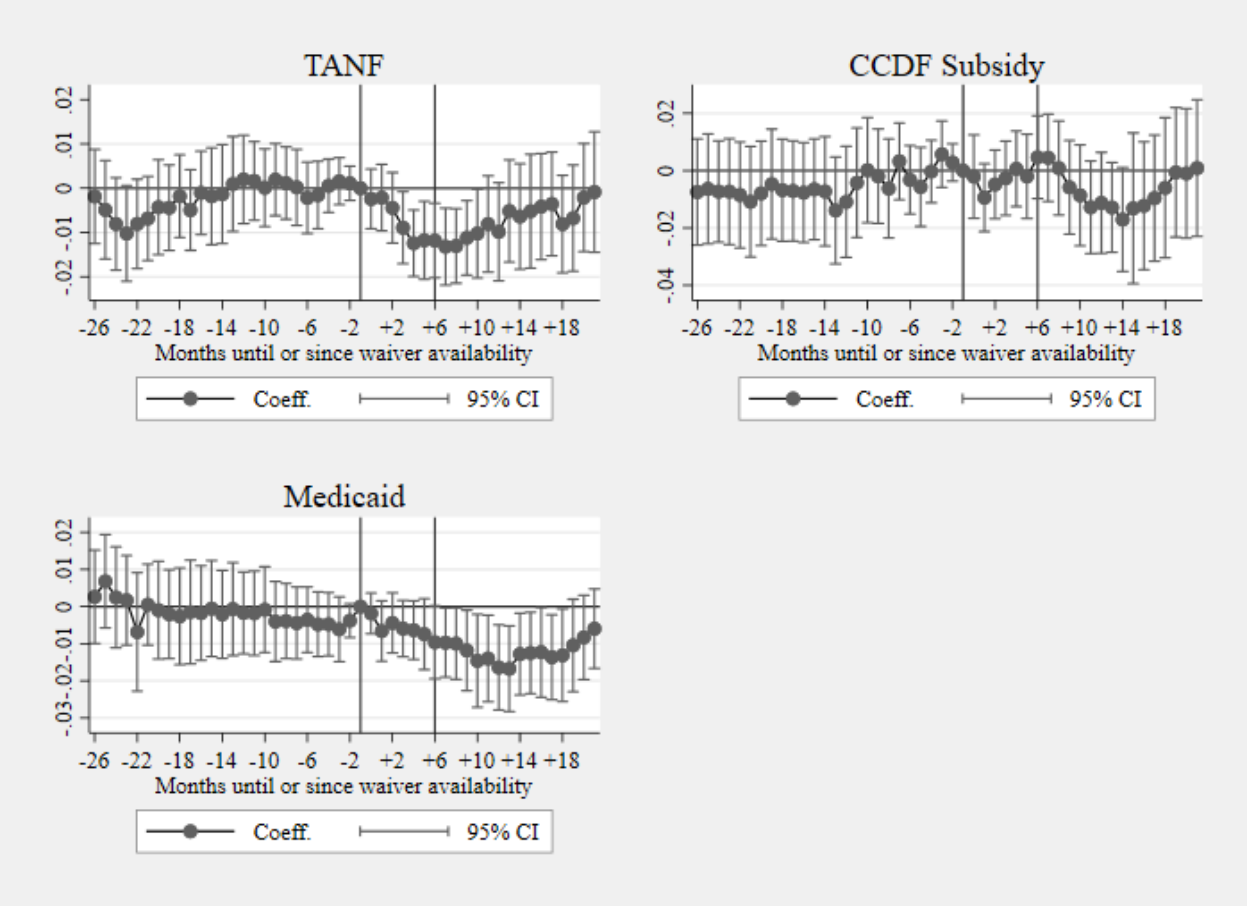
for SNAP itself, considering that certification interviews are an opportunity for SNAP applicants to learn about SNAP rules and ask questions about the program. Future research might consider whether SNAP interview waivers influenced the accuracy of eligibility determination or whether SNAP households received the full benefit amounts available to them.

Figure 2.1. Event Study of Changes in Multiple Program Participation, Full Sample



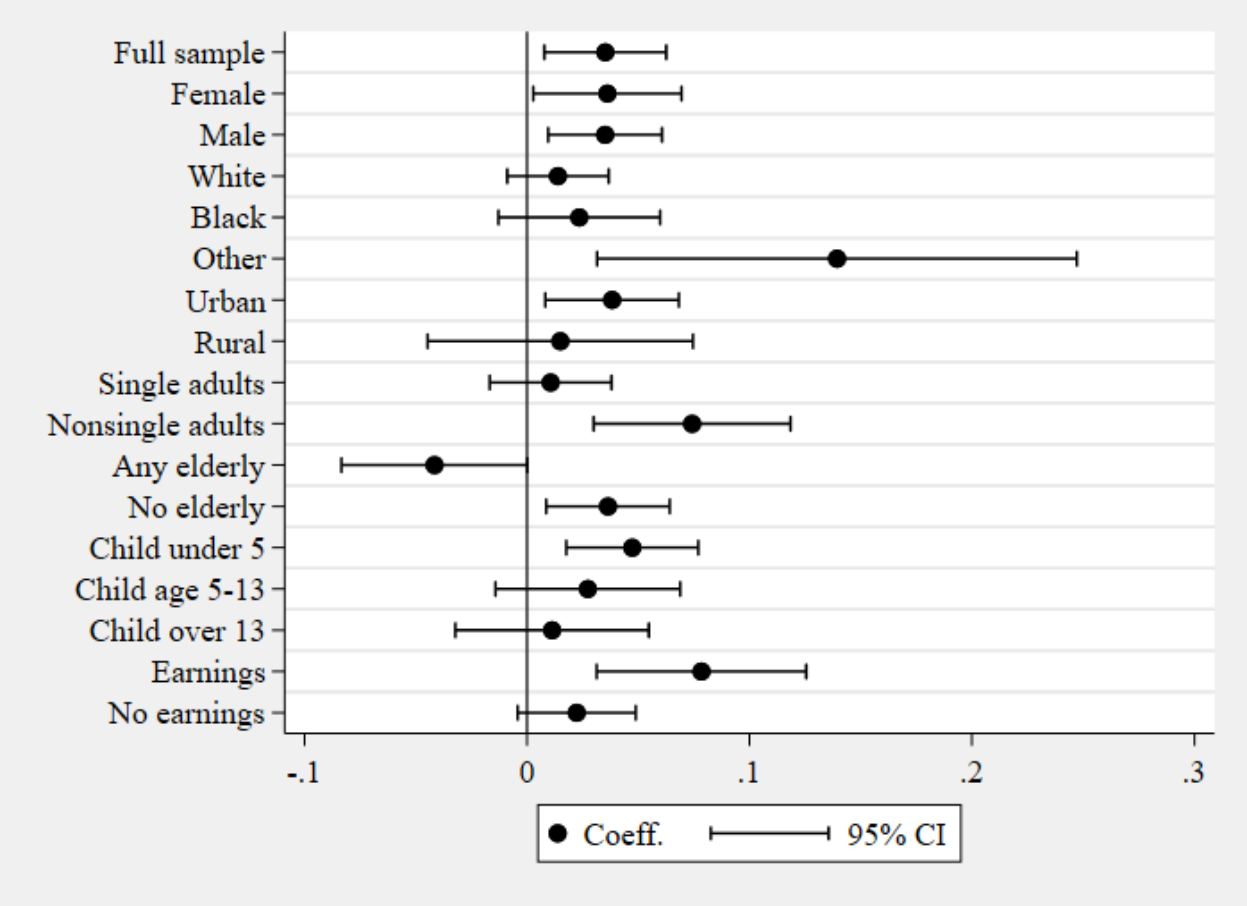
Notes: This figure presents the event-study estimates from equation (1) for the full sample. First vertical line (at -1) and second vertical line (at +6) indicate the month before the USDA made waivers available initially and issued a blanked extension of the waivers, respectively. Model includes county and month-year fixed effects and a full set of controls. I cluster standard errors at the county level.

Figure 2.2. Event Study of Changes in Multiple Program Participation, Placebo



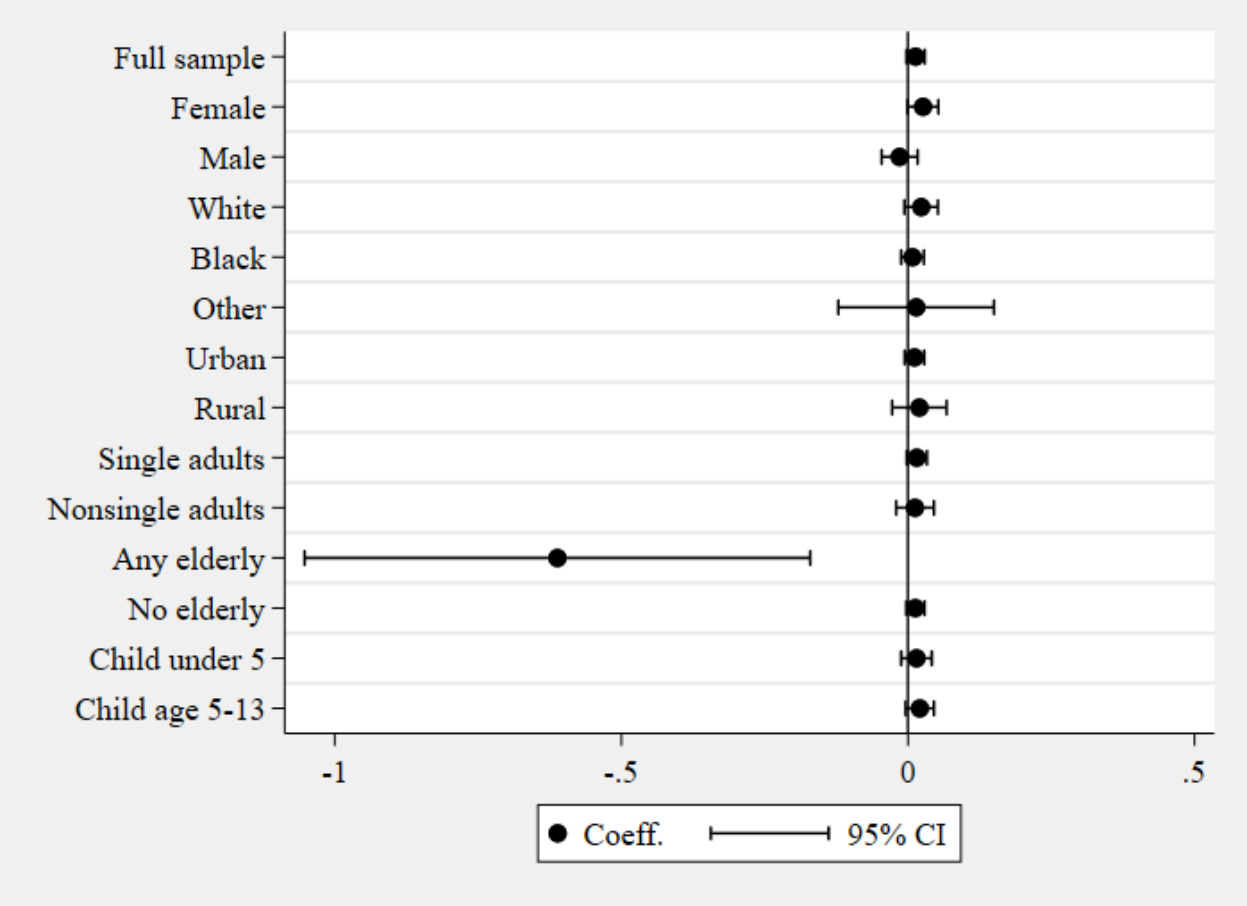
Notes: This figure presents the event-study estimates from equation (1) for the full sample. First vertical line (at -1) and second vertical line (at +6) indicate the month before the USDA made waivers available initially and issued a blanked extension of the waivers, respectively. Model includes county and month-year fixed effects and a full set of controls. I cluster standard errors at the county level.

Figure 2.3. Heterogeneous Effects of SNAP Interview Waivers on TANF Participation



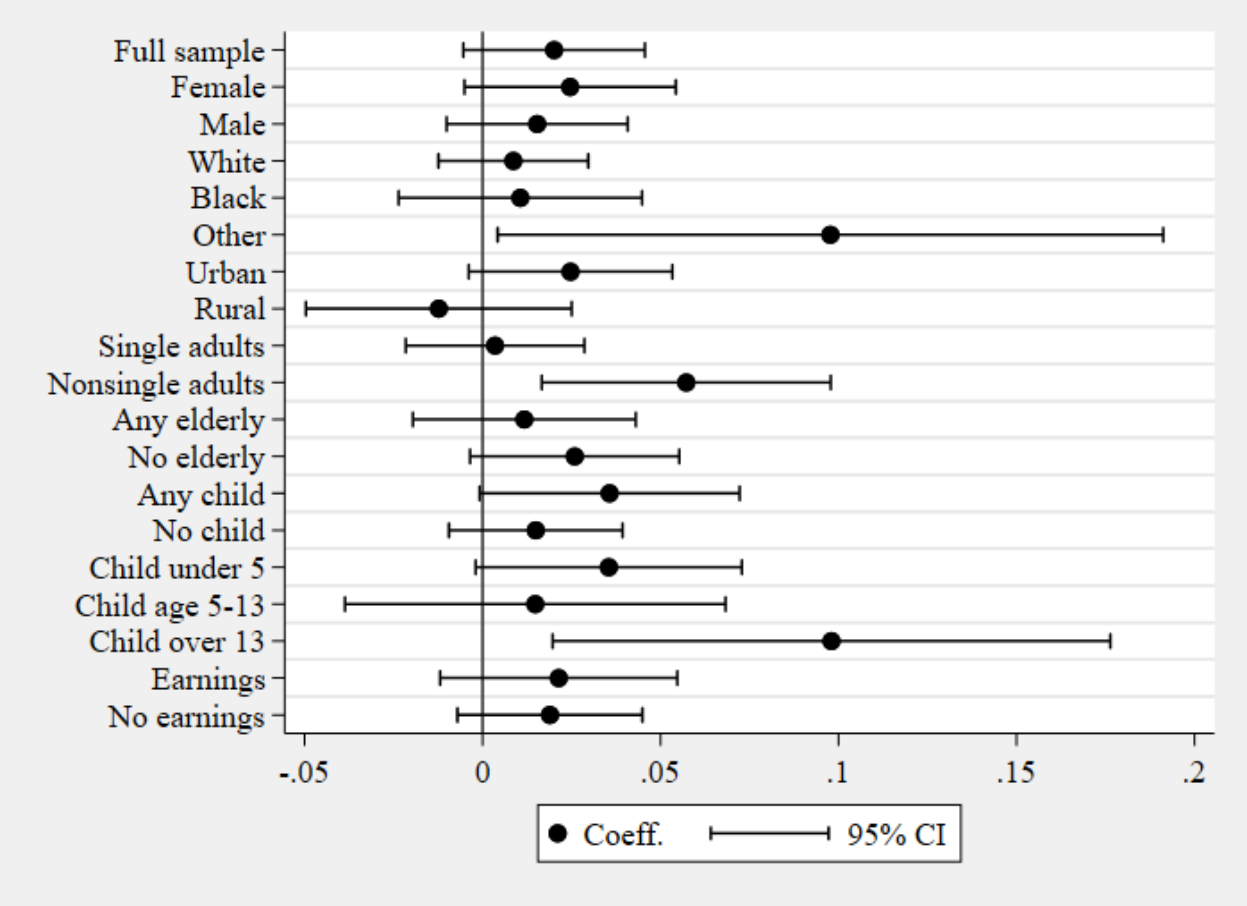
Notes: The figure reports the point estimates and 95% confidence intervals for the impact of SNAP interview waivers from the triple difference regression described by equation (2). Each row presents results from a separate regression corresponding to the characteristic listed. Standard errors are clustered at the county level.

Figure 2.4. Heterogeneous Effects of SNAP Interview Waivers CCDF Subsidy Participation



Notes: The figure reports the point estimates and 95% confidence intervals for the impact of SNAP interview waivers from the triple difference regression described by equation (2). Each row presents results from a separate regression corresponding to the characteristic listed. Standard errors are clustered at the county level.

Figure 2.5. Heterogeneous Effects of SNAP Interview Waivers on Medicaid Participation



Notes: The figure reports the point estimates and 95% confidence intervals for the impact of SNAP interview waivers from the triple difference regression described by equation (2). Each row presents results from a separate regression corresponding to the characteristic listed. Standard errors are clustered at the county level.

Table 2.1. Entry Program for New VDSS Enrollees, 2015-2022

	(1) Full Sample	(2) White	(3) Black	(4) Hispanic	(5) Other	(6) Female	(7) Child	(8) Adult
SNAP	0.26	0.25	0.35	0.22	0.11	0.24	0.18	0.30
TANF	0.003	0.002	0.005	0.003	0.002	0.003	0.006	0.002
CCDF subsidy	0.002	0.002	0.004	0.002	0.001	0.002	0.006	0.000
Medicaid	0.61	0.63	0.48	0.63	0.81	0.63	0.63	0.60
Multiple	0.13	0.12	0.16	0.14	0.08	0.13	0.18	0.10
Observations	1,558,156	851,865	471,443	30,634	204,214	827,861	558,623	999,533

Notes: Sample contains individuals who enrolled in a DSS program at any point between 2015 and 2022 in Virginia. Sample excludes left-censored individuals. Data are at the person-level.

Table 2.2. Characteristics Waiver and Nonwaiver Virginia SNAP Participants, Pre-Policy

	(1) Full Sample	(2) Waiver	(3) Nonwaiver
Multiple programs	0.61	0.62	0.60
SNAP	1.00	1.00	1.00
TANF	0.03	0.03	0.03
CCDF subsidy	0.02	0.02	0.02
Medicaid	0.60	0.61	0.60
Female	0.55	0.56	0.55*
White	0.49	0.46	0.50
Black	0.44	0.44	0.44
Hispanic	0.03	0.04	0.03
Other	0.04	0.06	0.03
Age	36.64	36.60	36.67
Urban	0.77	0.79	0.76
Number of children in household	1.32	1.34	1.31
Number of adults in household	1.33	1.32	1.33
Quarterly number of jobs worked	0.34	0.34	0.34
Quarterly earnings ($2021)	1,048.66	1,068.56	1,036.36
Observations	5,109,716	1,951,883	3,157,833
N counties	127	47	80

Notes: Sample includes SNAP participants in Virginia between January 2018 and February 2020 who were on SNAP before any other VDSS program ("SNAP first"). Data are at the person-month level. Waiver classification refers to whether the county ever used the interview waiver. *P*-values come from regressing each variable on waiver status with county-level clustered standard errors * p<0.1 ** p<0.05 *** p<0.01.

Table 2.3. Estimated Effects of SNAP Interview Waivers, Triple Difference

	(1)	(2)	(3)
	TANF	CCDF Subsidy	Medicaid
Treat X PostEnrollee	0.0352**	0.0133	0.0201
	(0.014)	(0.008)	(0.013)
Observations	984,607	349,665	3,004,955
Outcome Mean	0.058	0.074	0.711
Implied % Change	61%	18%	3%
Implied % Change (Upper Bound, 95% CI)	94%	26%	7%
Implied % Change (Lower Bound, 95% CI)	13%	-4%	-1%

Notes: Sample includes Virginia "SNAP first" participants between 2018 and 2021, excluding March 2020 through December 2020. Each column restricts to SNAP participants who were likely eligible for the respective outcome program for the entire sample period. Outcome means are from the pre-waiver period for the respective analysis samples. I cluster standard errors at the county level and shown in parentheses * p<0.1 ** p<0.05 *** p<0.01.

Table 2.A1. Sensitivity of Estimated Effects of SNAP Interview Waivers, Triple Difference

	(1)	(2)	(3)
	TANF	CCDF Subsidy	Medicaid
A. Contemporaneously Eligible			
Treat X PostEnrollee	0.0324***	0.00269	0.0196
	(0.011)	(0.008)	(0.014)
Observations	2,278,909	1,292,620	5,426,914
Outcome Mean	0.07	0.069	0.704
Implied % Change	49%	4%	3%
Implied % Change (Upper Bound, 95% CI)	83%	27%	7%
Implied % Change (Lower Bound, 95% CI)	15%	-20%	-1%
B. Broadened Sample Period			
Treat X PostEnrollee	0.0276**	0.00812	0.00274
	(0.011)	(0.009)	(0.011)
Observations	1,185,999	415,904	3,795,487
Outcome Mean	0.058	0.074	0.711
Implied % Change	48%	11%	0.4%
Implied % Change (Upper Bound, 95% CI)	85%	35%	4%
Implied % Change (Lower Bound, 95% CI)	11%	-13%	-3%
C. Include Coincident SNAP Enrollment			
Treat X PostEnrollee	0.0387*	0.0157	0.00471
	(0.021)	(0.011)	(0.017)
Observations	3,718,306	1,128,252	10,877,150
Outcome Mean	0.09	0.12	0.87

Table 2.A1. (Continued)

	(1)	(2)	(3)
	TANF	CCDF Subsidy	Medicaid
Implied % Change	43%	13%	1%
Implied % Change (Upper Bound, 95% CI)	89%	32%	4%
Implied % Change (Lower Bound, 95% CI)	-3%	-6%	-3%
D. Exclude Medicaid from "SNAP First" Definition			
Treat X PostEnrollee	0.0214*	0.00497	-0.00391
	(0.011)	(0.009)	(0.009)
Observations	4,798,297	1,491,282	14,143,777
Outcome Mean	0.08	0.11	0.88
Implied % Change	27%	5%	-0.4%
Implied % Change (Upper Bound, 95% CI)	55%	21%	2%
Implied % Change (Lower Bound, 95% CI)	-1%	-12%	-2%

Notes: Panel A includes Virginia "SNAP first" participants between 2018 and 2021, excluding March 2020 through December 2020, with each column restricting to SNAP participants who were likely eligible for the respective outcome program at the time of observation. Panel B includes Virginia "SNAP first" participants between 2018 and 2021, with each column restricting to SNAP participants who were likely eligible for the respective outcome program for the entire sample period. Panel C includes Virginia SNAP participants between 2018 and 2021, excluding March 2020 through December 2020, whose first enrollment in SNAP occurred before or in the same month of TANF, CCDF subsidy, or Medicaid. Panel D includes Virginia SNAP participants between 2018 and 2021, excluding March 2020 through December 2020, whose first enrollment in SNAP occurred before TANF or CCDF subsidy (but not Medicaid). Outcome means are from the pre-waiver period for the respective analysis samples. I cluster standard errors at the county level and shown in parentheses * $p<0.1$ ** $p<0.05$ *** $p<0.01$.

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Chapter III: The Effects of WIC Physical Presence Requirements on Benefit Redemption: Evidence from Remote Services during the COVID-19 Pandemic[1]

1 Introduction

Decisions about how to administer social safety programs are fraught with challenges for policymakers seeking to balance competing objectives. For example, policymakers may seek to make safety net programs accessible and limit fraud, but efforts to improve accessibility, such as reducing administrative burdens, may increase the risk of fraud. Recent social policy scholarship has emphasized how reducing administrative burdens can improve program uptake, but less attention has been devoted to how such efforts may sacrifice other policy goals.

I study these tradeoffs in the context of the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC). WIC provides nutritional counseling and food benefits (via quantity vouchers) to low-income pregnant women and young children. Despite an extensive body of research documenting WIC's beneficial health effects (Chorniy et al., 2020; Hoynes et al., 2011; Kreider et al., 2020; Rossin-Slater, 2013), the program struggles to reach all eligible families. For instance, only 51 percent of eligible individuals received benefits in 2021 (Kessler et al., 2023). In addition to low program uptake, WIC also faces challenges to achieving full benefit utilization because of the program's complicated benefit redemption process that involves identifying WIC-eligible foods and quantities in stores. Indeed, many WIC participants only redeem a fraction of their benefits once enrolled, constraining the full nutritional impact of the program. A recent study, for example, found that only 17 percent of recipients fully redeemed their food package in a given month (Li et al., 2021).

[1] Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau has reviewed this data product to ensure appropriate access, use, and disclosure avoidance protection of the confidential source data used to produce this product. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 3059. (CBDRB-FY24-P3059-R11458)

Many studies attribute WIC's low take-up to the program's policy of mandatory in-person appointments (Liu & Liu, 2016; Panzera et al., 2017; Pelto et al., 2020), prompting some advocacy groups, such as the National WIC Association, to urge the federal government to relax the program's physical presence requirements (National WIC Association, 2021). However, eliminating WIC's longstanding practice of face-to-face appointments may exacerbate barriers to benefit redemption. Some proponents of in-person appointments contend that they support benefit utilization by promoting personalized food packages and mitigating the learning costs associated with WIC benefit redemption (U.S. Department of Agriculture Food and Nutrition Service, 2024d). This presents something of a dilemma for policymakers attempting to balance goals of program accessibility and benefit utilization: transitioning from in-person to remote appointments may soften barriers to program enrollment but harden barriers to benefit redemption.

Until recently, research on the effects of physical presence requirements had been infeasible because, as a federal program requirement, the policy did not vary across geography or time. That changed in the wake of the COVID-19 pandemic when the federal government allowed WIC agencies to temporarily waive the requirement that WIC appointments be conducted in person. Several studies have exploited waiver-induced variation in physical presence requirements across counties and over time to provide some of the first quantitative evidence of the positive effects of eliminating WIC's physical presence requirements on program caseloads (Fannin et al., 2024; Whaley & Anderson, 2021).

In this paper, I build on this literature by investigating whether physical presence waivers stifled WIC benefit redemption. I principally rely on two unique sources of data: longitudinal WIC administrative records from the US Census Bureau covering the universe of WIC

participants in four US states and information on local agency waiver use from a comprehensive survey of all local WIC agencies from the US Department of Agriculture (USDA) (Wroblewska et al., 2023). The WIC administrative records avoid underreporting of program receipt common to public survey data (Meyer et al., 2015; Meyer & Mittag, 2019) and contain monthly benefit redemption amounts for each WIC household, which are absent in publicly-available data on WIC participants. Both the administrative WIC records and USDA waiver data contain local geographic information, enabling me to identify the waiver status of local WIC agencies for the counties in which WIC participants resided at the time of observation.

I leverage variation in physical presence waiver use at the local WIC agency level to estimate the effect of remote-only appointments on WIC redemption amounts. A unique feature of the longitudinal WIC administrative records is that I can distinguish post-waiver WIC enrollees (i.e., participants who were first exposed to the program after waivers became available) from pre-waiver WIC enrollees (i.e., participants who were first exposed to the program before waivers became available). I implement a triple difference research design that uses variation in waiver exposure at the county- and household-level. My source of household-level variation stems from the fact that pre-waiver WIC enrollees, even in waiver (i.e., treated) counties, had already been exposed to in-person appointments at the time of waiver adoption. Using data on the universe of WIC participants across four US states between 2018 and 2021, I find that physical presence waivers reduced monthly household WIC redemption by about $15, or about 17 percent of the pre-waiver sample average. I conclude that, while physical presence waivers boosted program caseloads (Fannin et al., 2023; Whaley & Anderson, 2021), they also reduced the amount in food benefits households redeemed.

This paper makes several contributions to existing literature. First, it adds to a surge of recent studies investigating the consequences of novel, temporary administrative changes to safety net programs during the COVID-19 pandemic (Ali & Wehby, 2022; Barnes, 2023; Barnes & Petry, 2021; Barnes & Riel, 2022; Collinson et al., 2024; Dague et al., 2022; Dague & Ukert, 2023; Fannin et al., 2024; Headrick et al., 2022; Heflin et al., 2023; Leifheit et al., 2021; Pukelis, 2023; Ruffini & Wozniak, 2021). Within this literature, a growing number of studies indicate that COVID-era WIC policy waivers increased caseloads and were well-received by WIC recipients and staff (Barnes, 2023; Barnes & Petry, 2021; Fannin et al., 2023; Vasan et al., 2021; Ventura et al., 2022; Whaley & Anderson, 2021), but this work also uncovers some unintended drawbacks to these changes. For example, WIC staff indicated concerns with the "quality of conversation" during remote (as opposed to in-person) appointments (Asada et al., 2024), and some WIC participants expressed a preference for in-person appointments, viewing them as more personalized than remote appointments (Barnes & Petry, 2021; Halverson & Karpyn, 2023; Ventura et al., 2022). This study complements this research by providing new evidence on the negative effects of COVID-era waivers for WIC beneficiaries: I find that remote-only WIC operations led to meaningful reductions in WIC benefit redemption. This finding is also relevant to a small body of literature studying the drawbacks to modernizing social services (Eubanks, 2018; Wu & Meyer, 2023).

More broadly, this study adds to the large literature on the effects of administrative burdens in US benefit programs (Aizer, 2003; Bhargava & Manoli, 2015; Bitler et al., 2003; Finkelstein & Notowidigdo, 2019; Fox et al., 2023; Hanratty, 2006; Herd & Moynihan, 2019; Jenkins & Nguyen, 2022). In recent years, scholars have emphasized how administrative burdens contribute to incomplete take-up of social programs, limiting the number of households to which

the well-documented benefits of program participation, such as improved health and life outcomes (Almond et al., 2011; Bailey et al., 2023; Hoynes et al., 2011; Tuttle, 2019), accrue. However, the salubrious effects of benefit receipt are largely predicated on successful benefit redemption. Much of the existing administrative burden literature focuses on barriers to claiming benefits. Less is known about barriers to using or redeeming benefits, which is an important omission, as incomplete benefit redemption, a particular concern in WIC and other programs that rely on vouchers which tightly restrict beneficiaries to certain goods and vendors, can undermine program efficacy (Barnes, 2021). Indeed, low redemption is linked to increased risk of premature exit from WIC (Anderson et al., 2023). Relatedly, this study enriches the WIC redemption literature by documenting how an administrative feature of the WIC program (in-person appointments) affects benefit utilization, in contrast to previous studies which focus on the influence of stigma, household characteristics and the retail environment, and food package composition (Chauvenet et al., 2019; Leone et al., 2022; Li et al., 2021; Payne et al., 2018; Powell et al., 2015; Vercammen et al., 2023; Zhang et al., 2022).

This research is also a response to recent calls by some scholars to reframe the administrative burden literature to consider not just the costs but also the benefits of citizen-state interactions (Barnes et al., 2023). To that end, this study adds to our nascent understanding of how administrative burdens interact. Linos and coauthors (2020), for example, argue that adding certain compliance costs to an administrative process can sometimes reduce overall burden. In the context of this study, I find evidence that in-person WIC appointments (a compliance cost) ease redemption costs. These results are consistent with recent qualitative research finding that WIC beneficiaries view engagement with WIC staff positively, as a source of support rather than a burden (Barnes et al., 2023). The implications of these findings are complicated for

policymakers considering making WIC physical presence waivers permanent. If eliminating in-person requirements in the WIC program reduces compliance burdens and improves program uptake (Fannin et al., 2024; Whaley & Anderson, 2021) but diminishes benefit redemption, the overall welfare consequences of such a change are ambiguous.

2 Background

A. The WIC Program

Program Overview

WIC seeks to promote the health and nutritional well-being of low-income pregnant/postpartum women and young children. To that end, WIC participants receive monthly food vouchers they can redeem for a specific set of foods at participating WIC stores and personalized nutrition education. As established by federal regulations, WIC nutrition education has two goals: (1) emphasize the relationship between nutrition, physical activity, and health and (2) support clients in achieving a positive change in dietary and physical activity habits, resulting in improved health through optimal nutrition practices and use of WIC food benefits. In addition to monthly food packages and nutrition education, WIC participants also receive breastfeeding support and referrals to healthcare and other social services.

WIC eligibility is categorically restricted to children under age five and pregnant or postpartum women. Participants must have household income below 185 percent of the federal poverty level and be classified as “at nutritional risk” by WIC staff. As part of the certification process, WIC staff screen for nutritional risks (such as high maternal weight gain, inadequate diet, and anemia) by collecting anthropometric and biochemical data, including height, weight, and bloodwork, and administering health questionnaires. The nutrition assessment is a key component of the WIC certification process, as the results from it inform how WIC staff tailor

food packages and nutrition education provided to clients. WIC households are typically certified for a year and must attend quarterly appointments, where they receive nutrition education and food package benefits for the subsequent three months (Kline et al., 2022). Though a handful of states offered telehealth options for nutrition education pre-pandemic, most still conducted nutrition education in person (Barnes & Petry, 2021).

While similar in spirit to SNAP, WIC is unique in a few ways. WIC encourages rich interpersonal engagement between program staff and clients through participant-centered nutritional counseling, health screenings, and referrals to other resources. WIC is the only federal nutrition assistance program that requires beneficiaries to participate in nutrition education (Koleilat et al., 2017), and WIC mandates that certification appointments take place in-person. Moreover, WIC food benefits are restrictive, specifying the types and quantities of foods participants may purchase. SNAP, on the other hand, is typically administered remotely and provides a cash-like benefit that SNAP participants can use to purchase any food for home consumption.

In an average month in 2021, WIC reached about 6.2 million people, serving 39 percent of all infants in the US (Kessler et al., 2023). Children between ages one and four comprised about 55 percent of all WIC participants in 2022, while the remaining 45 percent of the WIC population was split almost equally between women and infants (Zvavitch et al., 2024). WIC's take-up rate (percentage of the eligible population that participates in the program) is relatively low: 50 percent in 2020 (U.S. Department of Agriculture Food and Nutrition Service, 2023), compared to 78 percent for SNAP in the same year (Cunnyngham, 2023).

WIC households are assigned to a general food package corresponding to their participant category (pregnant woman, postpartum mother, infant, or child). For example, the maximum

monthly food package for a child between ages two and four consists of the following: 64 ounces of juice, 3.5 gallons of milk, 36 ounces of breakfast cereal, one dozen eggs, 24 ounces of whole wheat bread, six ounces of canned fish, one pound of dry legumes (64 ounces if canned) or 18 ounces of peanut butter, and a $26 cash value benefit for fruits and vegetables (U.S. Department of Agriculture Food and Nutrition Service, 2024a). Food packages for women and infants are further particularized depending on breastfeeding status (no breastfeeding, partially breastfeeding, or only breastfeeding). As of 2022, all state WIC agencies had transitioned from paper vouchers to electronic benefit transfer (EBT) cards (U.S. Department of Agriculture Food and Nutrition Service, 2022a)

Benefit Tailoring

In addition to matching food packages to participant category and breastfeeding status, WIC staff also tailor food packages to suit the particulars of each participant. WIC beneficiaries may receive a tailored food package based on documented medical or nutritional conditions (e.g., swapping whole milk with reduced fat if obesity is a concern). WIC staff may also adjust food packages based on participant preferences (e.g., peanut butter instead of beans), household conditions (e.g., shelf-stable foods for participants without access to refrigeration), or food intolerances or restrictions (e.g., substituting dairy with soy- or tofu-based foods for lactose intolerant participants). Food package tailoring is widespread among the WIC population, particularly for milk. A recent survey of WIC participants found that over 90 percent of women and children received an adjusted milk allotment, with many households receiving less than the standard milk allotment, often in exchange for cheese and/or yogurt (U.S. Department of Agriculture Food and Nutrition Service, 2024c). Excluding milk, rates of food package tailoring were lower overall and varied by participant category. Children had the highest rates of non-milk

tailoring (22 percent) and postpartum women had the lowest (9 percent) (U.S. Department of Agriculture Food and Nutrition Service, 2024c).

WIC staff also tailor nutrition education, often based on information obtained during nutrition assessments. In a recent survey of local WIC agencies, over 98 percent reported using results from nutrition assessments to adapt nutrition education to the particular needs of clients (U.S. Department of Agriculture Food and Nutrition Service, 2024c). The most common way WIC staff tailor nutrition education is by focusing on topics that are relevant to each participant's specific interests, concerns, and identified nutritional risks (U.S. Department of Agriculture Food and Nutrition Service, 2024d). During nutrition education sessions, WIC staff may also prioritize topics that they (or WIC clients) view as the most severe nutrition or health risks (U.S. Department of Agriculture Food and Nutrition Service, 2024d).

Benefit Redemption

WIC food packages restrict beneficiaries to certain products based on several factors including, packaging size, nutritional content, brand, and cost. For example, only dry cereal that contains a minimum of 28 milligrams of iron and no more than 21.2 grams of added sugar (per 100 grams) is WIC-eligible (U.S. Department of Agriculture Food and Nutrition Service, 2024b). While WIC's regulations on food benefits are meant to limit program costs and promote nutritional health, they create a burdensome redemption process, as clients must identify WIC vendors, and once in the store, select the appropriate WIC-eligible products, brands, and quantities corresponding to their food package, in contrast to SNAP, which provides a cash-like benefit that can be used on virtually any food item at SNAP retailers. In several qualitative studies, WIC participants reported experiencing a "learning curve" to redeeming their food benefits,

particularly with identifying WIC-approved foods and appropriate packaging sizes (Barnes, 2021; Bertmann et al., 2014; Chauvenet et al., 2019; Leone et al., 2022).

Several studies tie WIC's onerous benefit redemption process to premature program exit (Anderson et al., 2023; Gago et al., 2022) and incomplete food package use (Barnes, 2021; Weber et al., 2018), resulting in participants not getting the full nutritional benefits of the program. Indeed, partial redemption of WIC food benefits is common. A recent study of WIC redemption patterns found that, in an average month, only 17 percent of beneficiaries used all of their food package allotment, and 11 percent did not redeem any of their benefits (Li et al., 2021). Some studies link WIC benefit utilization to household characteristics. In a study of the cash value benefit for fruits and vegetables, Zhang and coauthors (2022) found higher redemption rates for White participants than Black participants, while Hispanic participants had the highest. The authors also observed higher redemption rates for households with a child participant (compared only women and infants) and estimated a positive correlation between household size and redemption.

B. WIC Policy Changes

WIC's mandatory in-person appointments created challenges for WIC agencies seeking to comply with social distancing guidelines during the COVID-19 pandemic. In response, Congress passed the Families First Coronavirus Response Act in March 2020, allowing states to temporarily[2] conduct WIC appointments remotely. Specifically, the USDA allowed states to waive WIC's physical presence requirement for certification and benefit issuance (hereafter, I refer to both waivers as the "physical presence waiver"). In practice, these waivers allowed states

[2] Beginning in September 2020, the USDA extended the availability of COVID waivers to all WIC agencies until the end of the national public health emergency under WIC Policy Memorandum #2020-6.

to collect eligibility information over the phone or digitally, issue benefits remotely, and defer collecting anthropometric and biochemical information from applicants. For applicants unable to submit eligibility documentation digitally, states could accept a self-attestation of eligibility and offer them a temporary certification. In addition to the administrative flexibilities, the federal government also issued waivers allowing for greater latitude in food package adjustments, enabling WIC households to purchase a broader range of products with their food benefits. For example, the food package substitution waivers permitted WIC participants to purchase 18-count cartons of eggs when 12-count cartons were unavailable.

Note that the physical presence waivers did not eliminate the mandatory nutritional risk assessment, but rather the requirement to collect certain anthropometric and biochemical data. In both remote and in-person contexts during the pandemic, the nutritional risk assessment process was similar, involving the same set of questions and administrative steps, but heigh and weight measurements and blood tests were only reliably available for WIC staff conducting in-person appointments (U.S. Department of Agriculture Food and Nutrition Service, 2024d). For example, WIC staff directly obtained height and weight measurements at WIC offices for 87 percent of in-person appointments, whereas this information was self-reported or unavailable for 84 percent of remote appointments (U.S. Department of Agriculture Food and Nutrition Service, 2024d).

Most WIC participants expressed high satisfaction with the shift from in-person to remote services, viewing it as an improvement to the program (Barnes, 2023). Specifically, WIC participants appreciated the safety, flexibility, and convenience of remote services, noting shorter wait times and overall appointment durations, relief from an onerous trip to a WIC office, which is viewed as particularly burdensome for WIC participants with multiple young kids, and the reduced risk of COVID-19 transmission (Asada et al., 2024; Barnes & Petry, 2021; Halverson &

Karpyn, 2023). The notion that physical presence waivers reduced administrative burdens and improved program accessibility is borne out in quantitative research, with several recent studies finding a positive effect of remote services on WIC caseloads (Fannin et al., 2023; Vasan et al., 2021; Whaley & Anderson, 2021).

While most WIC participants expressed positive experiences with remote services, qualitative studies document several drawbacks to remote services and reservations among stakeholders about making them a permanent feature of the program. One study found that only 25 percent of WIC participants preferred WIC services remain remote, 75 percent expressed a desire for some in-person contact (Ventura et al., 2022). A common theme emerging from this literature is that WIC participants view in-person appointments as fostering deeper conversations and more personal connections between beneficiaries and staff (Barnes & Petry, 2021), enabling more individualized advice (Halverson & Karpyn, 2023). Some participants appreciate in-person appointments because of the chance to review printed diagrams and pamphlets, finding them easier to comprehend in person (U.S. Department of Agriculture Food and Nutrition Service, 2024d). Some also indicated that remote visits were impersonal and did not allow them feel heard or were interrupted by technological issues (U.S. Department of Agriculture Food and Nutrition Service, 2024d).

In a recent USDA study, WIC staff echoed the concerns of participants, noting that remote appointments made it more difficult to establish a rapport with clients, engage in free-flowing discussion, and that participants were more distracted (U.S. Department of Agriculture Food and Nutrition Service, 2024d). They also reported missing the ability to see and visually assess their clients' well-being (U.S. Department of Agriculture Food and Nutrition Service, 2024d). As a result, WIC staff believed remote appointments restricted the type of guidance they

could offer to participants (U.S. Department of Agriculture Food and Nutrition Service, 2024d). Consistent with claims that remote appointments were rushed and involved less free-flowing discussion, the authors found that remote appointments were substantially shorter on average (25 minutes for remote compared to 40 minutes for in-person) and involved fewer activities (U.S. Department of Agriculture Food and Nutrition Service, 2024d).

C. Potential Channels

I posit that remote WIC services negatively impacted WIC benefit redemption. This supposition is supported by a USDA survey which found that WIC participants whose local agency switched to remote services during the COVID-19 pandemic were significantly less likely to report expecting to eat all the food that WIC gave them (U.S. Department of Agriculture Food and Nutrition Service, 2024d). In this section, I describe two channels through which remote services may have influenced WIC benefit utilization: food package redemption instruction and benefit tailoring.

Food Package Redemption Instruction

As discussed earlier, WIC food benefits involve a steep learning curve. To mitigate these learning costs, WIC staff are responsible for explaining how to redeem benefits during certification appointments, which often involve going over handouts detailing WIC-eligible foods and WIC vendors. WIC staff devoted less time to food package redemption instruction during the COVID-19 pandemic. For example, the share of WIC participants who received education on WIC procedures declined by over 50 percent following the introduction of physical presence waivers (U.S. Department of Agriculture Food and Nutrition Service, 2024c). If this decline was concentrated primarily among remote WIC offices, then it could be a mechanism through which remote services inhibited benefit redemption.

There is some evidence that remote services diminished, not just time devoted to WIC procedures, but the quality of food package redemption instruction, with some WIC staff expressing a preference for in-person appointments because they viewed them as a superior environment to explain the benefit redemption process. For example, some WIC staff felt like the handouts were less effective when delivered in a remote environment:

> I would say [the remote appointment is] a little bit, of course, less personal because we're not able to ... show [participants] the growth charts. We have several handouts here that we're able to give them in person. Now we can upload them through email and email them out that way; it's just a little bit more of a challenge to go that route than, of course, it is in person. So it's a little bit harder to kind of get the point across, maybe, as well as you would like to. And [the participants/caregivers] just have a lot of distractions going on, the TV, other people in the house, kids, whatever; [the cellphone] connections, that just doesn't get clearly put across compared to in-person (U.S. Department of Agriculture Food and Nutrition Service, 2024d, p.p. 3-19 to 3-20).

In an open-ended response to another survey of local WIC agencies, one WIC staff member stated that they "like to have brand new people in-person for their appointment to explain how to use the [EBT] card. That is hard over the phone," (Fannin et al., 2023). This sentiment is echoed by Barnes and Riel (2022), who found that some participants expressed a preference for in-person appointments, particularly when discussing issues with benefit redemption, because they felt "it would be a struggle talking over the phone about it." Indeed, in-person staff were considerably more likely to vary the mode of nutrition education, including using pamphlets/written materials, than in-person staff (70 percent for in-person compared to 39 percent for remote) (U.S. Department of Agriculture Food and Nutrition Service, 2024d). In

these ways, remote services may have negatively impacted the quality of food package redemption instruction participants received during WIC appointments, resulting in participants being confused about how to use their benefits or lacking a sufficient understanding about WIC-eligible items and packaging sizes, ultimately depressing WIC benefit redemption.

Benefit Tailoring

Another way remote services could stifle benefit redemption is through benefit tailoring. If remote WIC services diminish the quality or prevalence of food package and nutrition education tailoring, WIC benefits may be less personalized to participants' preferences or nutritional needs, making households less likely to fully redeem their food packages.

There is evidence that in-person WIC offices were more likely to tailor nutrition education than remote offices. For instance, in-person staff were more likely than remote staff to vary topics discussed, counseling methods used, mode of delivery, and make other modifications to nutrition education (U.S. Department of Agriculture Food and Nutrition Service, 2024d). While the same survey found that differences in rates of food package tailoring between remote and in-person WIC agencies were not statistically significant (U.S. Department of Agriculture Food and Nutrition Service, 2024d), it may be that the *quality* or *nature* of food package tailoring matters more than the presence of *any* tailoring.

During WIC appointments, WIC staff commonly ask participants open-ended questions about their family's dietary preferences and food preparation practices to facilitate food package tailoring:

> Normally I just ask [the caregiver], 'What types of foods does the child like to eat?' And usually when I just have a kind of a broad open question like that, they'll go in and tell

> me that they eat certain Indian foods ...or they make their own yogurt or sometimes they're vegan. And they'll kind of describe the foods to me. So then I can use that information to tailor the food package (U.S. Department of Agriculture Food and Nutrition Service, 2024d, p. 3-8).

Remote appointments were considerably shorter, and staff may have devoted less time to asking about clients' preferences, dietary restrictions, or culture. Indeed, in-person appointments were significantly less likely to explore participants' cultural behaviors and beliefs (U.S. Department of Agriculture Food and Nutrition Service, 2024d), and participants reported feeling "rushed" or "talked over" during remote appointments and like they were not given time to voice their questions or explain their situation (Barnes, 2023). Consequently, food packages may have been less suited to the specific dietary restrictions or preferences of WIC participants, making them less likely to redeem their food benefits.

As discussed earlier, the nutritional risk assessment is a crucial aspect of the WIC enrollment process that supports participant-centered benefit tailoring. Remote agencies were far less likely to obtain measurements for nutritional risk assessments, and in-person staff were significantly more likely to agree that risk assessments were effective (U.S. Department of Agriculture Food and Nutrition Service, 2024d). The inability of WIC staff to conduct effective nutritional risk assessments and collect anthropometric data under remote services may have resulted in food packages that were less individualized to the health needs of participants, and staff may have been less equipped to emphasize the importance of using WIC food benefits to address participants' specific health risks and goals. Some remote staff reported that their inability to obtain reliable and accurate anthropometric data from participants limited their ability to provide nutritional counseling, as they could not be sure if participants were underweight,

overweight, or on an appropriate growth trajectory (U.S. Department of Agriculture Food and Nutrition Service, 2024d). Remote services may have resulted in food packages that were less relevant to the health risks of participants, thereby discouraging benefit redemption.

3 Empirical Strategy

A. Data Sources

To estimate the effects of WIC physical presence waivers on benefit redemption, I primarily rely on USDA WIC waiver data linked to restricted WIC administrative records from the US Census Bureau. I also incorporate county-level data from several public sources.

WIC Waiver Data

Implementation of WIC physical presence waivers varied at the local agency level (Fannin et al., 2024). To capture this local variation, I rely on data from a USDA survey of the universe of local WIC agencies fielded between March 2021 and April 2021 (U.S. Department of Agriculture Food and Nutrition Service, 2021; Wroblewska et al., 2023). The data are at the ZIP code level and contain information on physical presence requirement waiver use for 1,833 local WIC agencies (97 percent response rate). Nearly all local WIC agencies reported offering some remote services at the time of the survey, but 22 percent of local agencies reported continuing to conduct in-person appointments, despite the availability of the physical presence requirement waiver (U.S. Department of Agriculture Food and Nutrition Service, 2021).

WIC Enrollee Data

I merge the locational information in the WIC waiver data to restricted-use administrative WIC data for the universe of WIC enrollees in four US states (Arizona, Illinois, Kansas, and South Dakota), which I obtained through an agreement with the US Census Bureau. The administrative WIC records are monthly (between January 2018 and December 2021) and contain information

on benefit redemption, ZIP code of residence, and basic demographic information (e.g., gender, race, age).

The administrative WIC data have a few key advantages in this context. First, the administrative nature of the data alleviates concerns about mismeasurement of program participation that plague publicly available household survey data (Meyer et al., 2015; Meyer & Mittag, 2019). Second, the WIC data are longitudinal, allowing me to observe participants' benefit receipt history over time and thereby isolate newly enrolled households, which is necessary for my research design and would not be possible using publicly available cross-sectional data on WIC participants. Third, the administrative WIC data contain crucial program information, such as the nominal dollar value of monthly benefits redeemed, which is unavailable in many other public datasets. Finally, the narrow geographic information available in the administrative records allows me to link WIC records to the WIC waiver data. Public-use WIC files, on the other hand, only contain local geography for individuals in particularly populous counties, if at all. Despite these advantages, the WIC administrative redemption data are available for only four US states, limiting the external validity of the findings.

Comparison of WIC Participants in Sample and Non-Sample States

To assess the representativeness of WIC participants in my sample states to the rest of the WIC population, I present summary statistics in Table 3.1 using data from the Current Population Survey's Annual Social and Economic Supplement (CPS ASEC), extracted from the Integrated Public Use Microdata Series (Flood et al., 2023). The CPS is a monthly survey of about 54,000 US households and is the primary source of labor force statistics. The ASEC is primarily conducted in March and asks detailed questions about work, earnings, and noncash benefits, including receipt of WIC. The universe for the CPS ASEC's question about WIC receipt is all

females in the survey. I limit the sample to females in the CPS ASEC who reported receiving benefits from WIC at any point in the previous calendar year. I analyze a wide range of years (2011 to 2021) to bolster sample sizes. I adjust income values for inflation using the Consumer Price Index for All Urban Consumers (CPI-U).

Table 3.1 presents summary statistics, shown separately for observations in my four sample states and the remaining non-sample states. Compared to non-sample states, WIC participants in sample states were less likely to be White, Black, Asian, or multiracial but more likely to belong to another race/ethnicity or be Hispanic. WIC participants in sample and non-sample states were of similar age and educational attainment. Sample states had slightly lower rates of marriage but similar rates of citizenship. Household incomes and rates of SNAP receipt were modestly larger for sample states, but both averaged about two children per household. It is important to note that all differences between sample and non-sample states were not statistically significant at conventional levels (though the sample size for sample states is small) and few differences were practically meaningful. Overall, the WIC population in my four sample states appear to resemble the broader WIC population according to CPS ASEC data.

Other Data

I also integrate public-use county-level data covering economic and COVID conditions, including monthly new COVID-19 infections and deaths from The New York Times (The New York Times, 2022), annual labor force, employment, and unemployment counts from the Bureau of Labor Statistics (U.S. Bureau of Labor Statistics, 2023), annual median income and child poverty rates from the Census (U.S. Census, 2019), and annual food prices (measured by average cost per meal) from Map the Meal Gap (Feeding America, 2024).

ZIP-County Crosswalk

To facilitate merging the ZIP-level WIC enrollee and waiver data with county-level data, I allocate county FIPS codes to ZIP codes using the HUD-USPS ZIP Code Crosswalk files from the US Department of Housing and Urban Development (U.S. Department of Housing and Urban Development, 2024). For each ZIP code, the crosswalk files contain associated county FIPS codes and the share of ZIP residents that live in each (referred to as the "residential ratio") based on quarterly domestic address data from the US Postal Service (Wilson & Din, 2018). Residential ratios enable researchers to allocate counties to ZIP codes with a high degree of confidence. For duplicate ZIP codes, I allocate the county with the highest residential ratio (i.e., the county in which most residents lived in the quarter of observation). Between 2018 and 2021, 89 percent of ZIP codes for my sample states either (1) did not overlap multiple counties or (2) had a county with a residential ratio of 0.9 or more, meaning that 90 percent or more of residents in the ZIP code lived in a single county. Among the 11 percent of ZIP codes that did not have an overlapping county with a residential ratio of 0.9 or more, the median max residential ratio was still high, 0.78.

B. Sample and Summary Statistics

My analysis draws on restricted-use administrative WIC records, containing the universe of WIC participants for Arizona, Illinois, Kansas, and South Dakota. I drop a trivial number of observations (<1 percent) with missing ZIP codes and the 6 percent of observations with missing WIC waiver data. I also exclude the 3 percent of observations with missing fields for any of the variables I use for the analysis. Further, I drop the 33 percent of the sample residing in counties with impartial waiver adoption or that reported using remote services before the pandemic and the 11 percent of observations with implausibly large WIC benefit redemption values for a single

month ($400 or more). The final analysis sample contains approximately[3] 7,973,000 person-months between January 2018 and December 2021 across the four sample states.

Comparison with National WIC Data

As discussed previously, a limitation of this study is that my estimates may not generalize outside of the four sample states. While Table 3.1 uses CPS ASEC data to demonstrate that characteristics of WIC participants in sample states largely resembled non-sample states between 2011 and 2021, this approach is limited by measurement error of WIC receipt in the CPS (Meyer et al., 2015) and small sample sizes, which make detecting differences in means difficult. As an additional check, I compare the compositions of sample and national WIC populations on observables. For characteristics of the national WIC population, I rely on data from *WIC Participant and Program Characteristics 2020* (Kline et al., 2022). The USDA report presents national WIC characteristics using administrative records comprising the universe of WIC participants in each state for 2020.

Table 3.2 presents summary statistics for WIC participants in the pre-waiver period (between January 2018 and February 2020) for sample states and for the national WIC population in 2020. In the pre-waiver period, the most common race/ethnicity of WIC participants in sample states was Hispanic (43 percent), followed by White (31 percent), Black (20 percent), Asian (3 percent), multiracial (2 percent), and another race/ethnicity (2 percent). National data for 2020 exhibit a similar pattern: Hispanic (41 percent) was the most common race/ethnicity, followed by White (29 percent), Black (21 percent), Asian (4 percent), multiracial (4 percent), and another race/ethnicity (1 percent). The age profiles for sample and national

[3] Federal disclosure rules require approximated values.

populations were also similar. About 25 percent of participants in sample states were infants (compared to 24 percent nationally) and 22 percent were adults (compared to 23 percent nationally). However, a smaller share of child participants in sample states was age one (17 percent in sample states compared to 20 percent nationally) and a larger share was age four (10 percent compared to 7 percent nationally). Despite these modest differences, Table 3.2 suggests that the WIC population in sample states resembled the national WIC population on observable demographics, as differences were not practically meaningful.

Characteristics of Waiver and Non-waiver Counties

Table 3.3 displays average WIC redemption amounts and key individual, household, and county characteristics measured in the months before waivers initially became available (before March 2020). I convert nominal dollar values to 2021 values using the Chained Consumer Price Index for All Urban Consumers (C-CPI-U). I present statistics separately for waiver (Column 2) and non-waiver counties (Column 3). "Waiver" refers to counties in which all local WIC agencies reported conducting only remote appointments, whereas "non-waiver" refers to counties in which all local WIC agencies reported conducting in-person appointments (though "non-waiver" counties may have used remote services in some capacity).

In the pre-waiver period, the average WIC participant in waiver counties (Column 2) redeemed about $89 worth of WIC benefits per month, whereas non-waiver counties (Column 3) averaged about $79 per month, though the difference was not statistically significant at conventional levels. Waiver counties were more racially diverse. Specifically, WIC participants in waiver counties were less likely to be White ($p < 0.05$) and more likely to be Black ($p < 0.01$). Waiver counties also had a larger share of infant WIC participants, though this difference was not statistically significant. Waiver and non-waiver counties had similar child age profiles, as

differences in the share of WIC participants who were at different child ages between one and 17 were not practically meaningful, though waiver counties had a slightly larger share of adult WIC participants ($p < 0.01$).

Turning next to household characteristics, differences in average monthly household income and household size were small and not statistically significant at conventional levels. WIC participants in waiver counties were more likely to belong to a household with an infant ($p < 0.1$) or an adult ($p < 0.01$) and less likely to belong to a household with a child ($p < 0.1$). These differences imply that waiver counties had a higher concentration of pregnant/postpartum and infant WIC cases than non-waiver counties. Finally, differences across all observable county-level characteristics were not statically significant. In summary, the WIC population for waiver counties were less White, more Black, and more likely to be adult or infant WIC cases. However, household incomes and family sizes were comparable for waiver and non-waiver participants, and waiver and non-waiver counties had similar unemployment rates, median household incomes, child poverty rates, and per meal food costs.

C. Identification Strategy and Method

As discussed previously, I posit that the elimination of in-person appointments led to a reduction in WIC redemption, either through diminished quality of benefit redemption instruction or benefit tailoring. To examine the effect of physical presence waivers on benefit redemption, I leverage variation in physical presence waivers use across counties and the fact that remote services would only impact benefit redemption patterns for new WIC enrollees. When physical presence waivers initially became available, existing WIC participants had already completed a nutritional risk assessment and had received in-person benefit redemption instruction or benefit

tailoring. Therefore, effects should only materialize for WIC participants who first enrolled after waivers became available (hereafter, I refer to this group as "post-waiver enrollees").

To estimate the effect of physical presence waivers on WIC redemption, one might consider using the following two-way fixed effects approach:

$$(1) \qquad Y_{ict} = \beta_0 + \beta_1 Treat_{ct} + \beta_x X_{ict} + \gamma_c + \gamma_t + \varepsilon_{ict}$$

for each WIC participant *i* residing in county *c* in month-year *t*. The dependent variable, Y_{ict}, is the dollar amount of household WIC benefits redeemed, and $Treat_{ct}$ is a binary indicator for whether county *c* is a waiver county in month-year *t*. I define waiver county as counties in which every WIC agency eliminated in-person appointments (I exclude counties with impartial waiver adoption).[4] The vector X_{ict} contains individual, household, and county demographic and economic variables. The parameters γ_c and γ_t are county and year fixed effects, respectively.

The primary concerns with this estimation strategy are twofold. First, it does not account for the fact that remote services should only impact benefit redemption for post-waiver enrollees. As specified, the model estimates the average effect of physical presence waivers on benefit redemption for the entire WIC population. Second, this approach assumes that WIC redemption for waiver and non-waiver states would have evolved similarly in the absence of treatment. Unobservable factors correlated with both waiver implementation and WIC redemption could violate this assumption. For example, physical presence waiver adoption at the county-level may have been coincident with changes to the local food market (increased shelf prices), transitions from paper vouchers to WIC EBT cards, or the implementation food package substitution waivers, both of which could affect WIC redemption patterns, biasing the coefficient on $Treat_{ct}$.

[4] In results not shown, I find that this decision has little impact on the point estimates.

The transition to WIC EBT was complete in the pre-waiver period for all sample states except Illinois, which implemented WIC EBT in 2020 (U.S. Department of Agriculture Food and Nutrition Service, 2022b). While all sample states reported using the food package substitution waiver, state reports may mask local agency variation.

I modify the regression described by equation (1) to implement triple difference design that leverages my ability to distinguish pre- and post-waiver WIC enrollees in the administrative data. Though subject to many of the same local economic conditions (shelf prices) and policies (WIC EBT, food package substitution waivers) that could bias the coefficient on $Treat_{ct}$ in equation (1), the redemption behavior of pre-waiver WIC enrollees should be unaffected by the adoption of physical presence waivers, since this group had already completed an in-person appointment when waivers went into effect. Leveraging this placebo group as another control, I introduce an additional variable, $PostEnrollee_{ict}$, to the equation, yielding the following specification:

$$(2) \quad Y_{ict} = \beta_0 + \beta_1 Treat_{ct} + \beta_2 PostEnrollee_{ict} + \beta_3 (Treat * PostEnrollee)_{ict} + \beta_x X_{ict} + \gamma_c + \gamma_t + \varepsilon_{ict}$$

where $PostEnrollee_{ict}$ is an indicator for participants who first[5] enrolled in WIC after physical presence waivers became available to local agencies. The coefficient on the interaction term $(Treat * PostEnrollee)_{ict}$ represents the difference in WIC redemption for post-waiver WIC enrollees in waiver counties. The variable $Treat_{ct}$ now controls for unobserved events correlated with waiver adoption and WIC redemption, such as the implementation of food package substitution waivers or changes to local food markets, affecting both pre- and post-waiver WIC enrollees. Thus, β_1 represents any change in WIC redemption (among pre-waiver WIC

[5] I can only observe WIC receipt history going back to 2018, so I define post-waiver enrollees as WIC participants who enrolled in March 2020 or later and did not receive WIC at any point since January 2018 (at least).

participants) correlated with waiver adoption. The remaining terms are unchanged from equation (1). I cluster standard errors at the county level.

Controls

I include several individual, household, and county covariates that could affect redemption. The contents of WIC food packages vary by participant category (woman, infant, or child), and existing evidence suggests participant and family characteristics influence WIC redemption patterns (Zhang et al., 2022). Therefore, compositional shifts among post-waiver enrollees are an important threat to identification. While existing evidence suggests that observable WIC participant characteristics remained stable during the pandemic (U.S. Department of Agriculture Food and Nutrition Service, 2024c), I control for several individual and household characteristics to mitigate this threat: a participant gender dummy, participant race/ethnicity group dummies (White, Black, Asian, other race, multiracial, Hispanic), participant age, household income, household size group dummies, and dummies for an infant, child or adult on the WIC case. The contents of WIC food packages also vary by breastfeeding status, but this detail is unavailable in the administrative WIC records and thus not accounted for by the regression. I further control for the following county characteristics: unemployment rate, median household income, child poverty rate, and food prices. Because WIC food packages restrict beneficiaries to fixed quantities, differences in redemption amounts across households may reflect variation in shelf prices. Controlling for food costs mitigates this threat. I also include county-level controls for monthly new COVID-19 infections and deaths.

4 Results

Main Results

Table 3.4 presents results from estimating the regression described by equation (2). All specifications include time and county fixed effects. The first column displays results without covariates. The coefficient of interest on the interaction term for this model implies that physical presence waivers reduced WIC benefit redemption by about $14 ($p < 0.01$), or 15 percent of the sample mean, for post-waiver WIC enrollees. The second column of Table 3.4 displays results with a full set of individual, household, and county controls. The coefficient of interest is remarkably stable with the inclusion of controls. The triple difference estimate in this model implies that WIC physical presence waivers reduced WIC benefit redemption by about $15 ($p < 0.01$), or 17 percent of the sample mean, for post-waiver WIC enrollees. The stability of the point estimate with the inclusion of an extensive set of controls is reassuring, given concerns about bias from compositional shifts in the WIC population and economic factors correlated with waiver implementation.

The coefficient on $Treat_{ct}$ represents the change in WIC redemption among pre-waiver WIC enrollees, or the placebo group. The estimate is positive and statistically significant for both models (but larger in magnitude for the model with covariates), suggesting that waiver adoption was associated with an increase in WIC redemption among WIC participants with pre-waiver program exposure. I am unable to shed light on the forces driving the positive association between waiver adoption and WIC redemption among the placebo group, but the pattern is consistent with several previously mentioned confounds: transitions from paper vouchers to WIC EBT, food package substitution waivers, or increases in shelf prices (not captured by the food cost control variable).

5 Conclusion

Policymakers seeking to balance competing objectives face challenges when designing social programs, as efforts to achieve one goal (program accessibility) may come at the expense of another (program integrity). I study these issues in the context of the availability of WIC physical presence waivers during the COVID-19 pandemic, which allowed WIC agencies to temporarily eliminate the requirement that WIC appointments take place in person.

WIC is dealing with dual issues. On one hand, WIC's take-up rate is low: only 51 percent of eligible individuals enrolled in the program in 2021 (Kessler et al., 2023). On the other hand, even individuals who successfully enroll struggle to make full use of their benefits: a recent study determined that only 17 percent of WIC participants fully redeemed their food benefits in an average month (Li et al., 2021). Physical presence requirement waivers increased program enrollment (Fannin et al., 2023; Whaley & Anderson, 2021) and were viewed as a convenient change by participants (Barnes, 2023; Barnes & Petry, 2021; Ventura et al., 2022). Therefore, making physical presence waivers permanent could improve long-term program access. However, my estimates suggest that a lack of face-to-face engagement between WIC clients and staff exacerbated existing barriers to WIC benefit redemption. Using administrative WIC data covering the universe of WIC participants across four US states between 2018 and 2021, I find that physical presence waivers reduced WIC benefit redemption by a statistically significant $15, or 17 percent of the pre-waiver average. This result is consistent with recent studies that document some negative consequences of remote WIC services, such as diminished benefit tailoring and degraded quality of appointments (Barnes, 2023; U.S. Department of Agriculture Food and Nutrition Service, 2024d).

My findings contribute to a voluminous literature examining the effects of administrative burdens in safety net programs (Herd & Moynihan, 2019). Much of this literature documents how administrative burdens preclude program uptake (Aizer, 2003; Bhargava & Manoli, 2015; Elkaramany & Edwards, 2023; Fox et al., 2023; Gray, 2019; Hanratty, 2006; Heflin et al., 2023). Complementing recent scholarship in Barnes et al. (2023) and Linos et al. (2020), my findings document how some administrative procedures (in-person appointments) support, rather than burden, program beneficiaries by promoting benefit redemption and offsetting other burdens, such as redemption costs (Barnes, 2021). This study also contributes to the WIC redemption literature by exploring how an administrative feature of WIC (rather than household or retail characteristics) impact WIC redemption patterns and adds to a small body of literature on the disadvantages of modernizing social services (Eubanks, 2018; Wu & Meyer, 2023).

This study has three principal limitations. First, the results may not generalize outside of the sample states. Though Tables 3.1-3.2 indicate that WIC participants in sample states exhibited similar observable characteristics to the broader WIC population, physical presence waivers may have been implemented differently across states. For example, remote WIC agencies in non-sample states may have maintained robust benefit tailoring and high-quality benefit redemption instruction, lessening the negative impact of physical presence waivers on WIC redemption. Although WIC agencies across the country reported similar challenges with remote services (U.S. Department of Agriculture Food and Nutrition Service, 2024d), the existence of such waiver implementation heterogeneity would limit the external validity of these findings. Second, I am unable to disentangle the benefit redemption instruction channel from the benefit tailoring channel. The inability to illuminate causal mechanisms makes the policy implications of my findings less clear. If we knew, for example, that the negative impact of waivers on WIC

redemption was driven primarily by benefit tailoring, WIC administrators could pursue innovative ways to maintain benefit tailoring in remote environments, mitigating the negative effects of remote services on benefit redemption. Unfortunately, my analysis is unable to specify whether the negative effects are driven by degraded quality of benefit redemption instruction or benefit tailoring. Finally, I define treatment as WIC agencies that offered *only* remote appointments (or "fully remote" agencies), compared to partially remote agencies, agencies that offered some remote services mixed with in-person. Therefore, my analysis is silent on the effects of transitioning from in-person-only to partially remote WIC services.

My findings paint a complicated picture for policymakers considering eliminating WIC physical presence requirements. On one hand, WIC staff and participants report high satisfaction with waivers, and quantitative evidence indicates that waivers increased WIC enrollment. On the other hand, my results suggest waivers had a negative effect on benefit redemption, which may limit the program's salubrious effects on health outcomes. Policymakers seeking to maximize WIC enrollment and benefit redemption appear to face a tradeoff when it comes to the question of whether to relax the program's in-person requirements.

Most remote WIC appointments were conducted over the telephone (Wroblewska et al., 2023), limiting the staff's ability to synchronously go over handouts with clients. Future research could consider whether the negative impacts of waivers on benefit redemption hold for agencies that employed video-conferencing platforms, such as Zoom, that facilitate the use of visual aids. A second avenue for future scholarship is to assess if negative effects were concentrated among WIC participants lacking nutritional risk assessment data. One might, for example, estimate if the effects of remote services were different for remote participants whose anthropometric and biochemical data were submitted by a healthcare worker (as opposed to those who self-reported

this information or did not report it at all). Documenting conditions under which the negative effects of remote services on benefit redemption are lessened could help policymakers devise ways to implement long-term remote appointment options without exacerbating benefit redemption costs.

Table 3.1. Characteristics of WIC Recipients in Sample and Non-Sample States in the CPS ASEC, 2011-2021

	(1) Sample	(2) Non-Sample
Female	1.00	1.00
White	0.37	0.39
Black	0.17	0.21
Asian	0.03	0.04
Other	0.03	0.01
Multiracial	0.01	0.02
Hispanic	0.39	0.33
Age	28.97	29.38
High school or less	0.57	0.58
More than high school	0.43	0.42
Married	0.46	0.49
Citizen	0.81	0.81
HH income ($2021)	51,119	49,895
Number of children in HH	1.99	1.99
SNAP	0.57	0.54
Observations	1,718	23,099

Notes: Table presents summary statistics for WIC recipients in the Annual Social and Economic Supplement (ASEC) of the Current Population Survey (CPS) between 2011 and 2021. Column 1 restricts to observations in sample states (Arizona, Illinois, Kansas, and South Dakota), whereas Column 2 excludes observations in sample states. Statistics are weighted using CPS individual weights. *P*-values come from regressing each variable on an indicator for residing in a sample state, with state-level clustered standard errors.

Table 3.2. Characteristics of WIC Participants, Comparison of Sample States with National WIC Data

	(1) Sample States (January 2018-February 2020)	(2) National (2020)
White	0.307	0.291
Black	0.195	0.211
Asian	0.027	0.041
Other	0.02	0.012
Multiracial	0.024	0.040
Hispanic	0.426	0.405
Age < 1	0.25	0.240
Age 1	0.171	0.202
Age 2	0.13	0.136
Age 3	0.114	0.120
Age 4	0.101	0.074
Age 5-17	0.01	N/A
Age > 18	0.223	0.228
Observations	4,647,000	7,034,919

Notes: This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 3059 (CBDRB-FY24-P3059-R11458). N in Column 1 is a rounded value. Column 1 contains person-month observations of WIC participants in the pre-waiver period (before March 2020) for sample states (Arizona, Illinois, Kansas, and South Dakota) using restricted administrative WIC records. Column 2 contains author's calculations from public USDA data on person-level observations of WIC participants in 2020 (Kline et al., 2022).

Table 3.3. County and WIC Participant Characteristics, Pre-Waiver Period

	(1) Full Sample	(2) Waiver	(3) Non-Waiver
Household WIC Redemption Amount ($2021)	88.37	88.98	78.79
Individual			
Female	0.607	0.607	0.601*
White	0.307	0.293	0.523**
Black	0.195	0.205	0.046***
Asian	0.027	0.028	0.012
Other	0.02	0.017	0.065
Multiracial	0.024	0.023	0.04
Hispanic	0.426	0.434	0.313
Age < 1	0.25	0.251	0.239
Age 1	0.171	0.171	0.179**
Age 2	0.13	0.13	0.137*
Age 3	0.114	0.114	0.119
Age 4	0.101	0.101	0.104
Age 5-17	0.01	0.01	0.013*
Age > 18	0.223	0.224	0.208***
Household			
Monthly HH Income ($2021)	1,838	1,830	1,977
Household Size	1.86	1.86	1.866
Infant in HH	0.481	0.482	0.456*
Child 1-4 in HH	0.592	0.59	0.62*
Adult in HH	0.459	0.461	0.431***
County			
Unemployment Rate	0.04136	0.04231	0.03798
Median HH Income ($2021)	60,420	60,830	59,000
Child Poverty Rate	0.1774	0.1784	0.1741
Cost Per Meal (Nominal $)	3.088	3.095	3.066
Observations	4,647,000	4,366,000	281,000
N Counties	250	200	50

Notes: This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 3059 (CBDRB-FY24-P3059-R11458). Ns are rounded values. Sample contains person-month observations of WIC participants in the pre-waiver period (before March 2020) using restricted administrative WIC records for sample states. *P*-values come from regressing each variable on waiver status, with county-level clustered standard errors * p<0.1 ** p<0.05 *** p<0.01.

Table 3.4. Estimated Effects of Physical Presence Waivers on WIC Redemption Amounts

	(1)	(2)
Treat	3.564*	9.881***
	(2.050)	(2.488)
PostEnrollee	18.38***	12.76***
	(2.330)	(2.431)
Treat X PostEnrollee	-13.64***	-15.16***
	(3.689)	(3.936)
Outcome mean	88.98	88.98
As a % of mean	-15.33%	-17.04%
Covariates	N	Y
Observations	7,973,000	7,973,000

Notes: This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 3059 (CBDRB-FY24-P3059-R11458). Observations are rounded values. All regressions include county and month-year fixed effects. Standard errors are clustered at the county level and in parentheses *** $p<0.01$.

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Employment

2020—	Graduate Associate, Center for Policy Research, *Syracuse University*
2019 – 2020	Policy Analyst, *The Council of State Governments*
2018 – 2019	Research Assistant, Martin School of Public Policy and Administration, *University of Kentucky*
2017 – 2018	Intern, Department of Finance, *Lexington-Fayette Urban County Government*

Education

2024	Ph.D., Public Administration, Syracuse University Fields: Social Policy, Public Finance Committee: Colleen Heflin, Sarah Hamersma, and Leonard M. Lopoo Dissertation: *Three Essays on Administrative Burden and the Social Safety Net*
2020	M.P.P., University of Kentucky
2018	B.S., Economics, *summa cum laude*, University of Kentucky Departmental Honors Lewis Honors College

Publications

Peer-Reviewed Journal Articles

Fannin, W. Clay, Colleen M. Heflin, and Leonard Lopoo. "Examining the Effects of WIC Physical Presence Requirement Waivers on Program Caseloads." (Forthcoming at *Social Service Review*)

Fannin, W. Clay, Colleen M. Heflin, and Taryn W. Morrissey (2024). "Earnings and Employment Patterns following Child Care Subsidy Receipt." *Social Service Review*, 98(2), 329-365.

Heflin, Colleen, **W. Clay Fannin**, and Leonard Lopoo (2023). "Local Control, Discretion, and Administrative Burden: SNAP Interview Waivers and Caseloads During the COVID-19 Pandemic." *The American Review of Public Administration*, 53(7-8), 334-346.

Morrissey, Taryn W., Colleen M. Heflin, and **W. Clay Fannin** (2023). "Room to grow: examining participation and stability in child care subsidies using state administrative data." *Early Childhood Research Quarterly*, 62, 150-162.

In Progress

Fannin, W. Clay. "Can Reducing Administrative Burdens Increase Benefits Amounts? Evidence from SNAP Simplified Reporting" (R&R at *Applied Economic Perspectives and Policy*)

Fannin, W. Clay. "Do SNAP Interviews Cause Program Spillover? The Effects of SNAP Interview Waivers on Multiple Program Participation"

Fannin, W. Clay. "The Effects of WIC Physical Presence Requirements on Benefit Redemption: Evidence from Remote Services during the COVID-19 Pandemic"

Policy Reports, Research Briefs, and Blogs

Heflin, Colleen, **W. Clay Fannin**, Leonard M. Lopoo, and Siobhan O'Keefe. "Waiving SNAP Interviews during the COVID-19 Pandemic Increased SNAP Caseloads" Center for Policy Research. Policy Brief #9. 2024.

W. Clay Fannin. "Billions in COVID-19 Rental Assistance Fails to Reach Tenants" Data Slice #48. Lerner Center Population Health Research Brief Series. 2021.

Morrissey, Taryn, Colleen Heflin, and **W. Clay Fannin**. "The U.S. Child Care Subsidy Program Is Underused but Well-Positioned to Promote Racial Equity" Research Brief #54. Lerner Center Population Health Research Brief Series. 2021.

Morrissey, Taryn, Colleen Heflin, and **W. Clay Fannin**. "The U.S. Must Invest More in the Child Care Subsidy Program" Data Slice #45. Lerner Center Population Health Research Brief Series. 2021.

W. Clay Fannin. "Occupational Licensure Interstate Compacts in Action." The Council of State Governments, National Center for Interstate Compacts. 2019.

W. Clay Fannin. "Wisconsin Reduces Licensing Fees for Nearly 75% of Licensed Occupations." The Council of State Governments (Blog). August 2019.

W. Clay Fannin. "New Kentucky Law Streamlines Occupational Licensing for Military-Affiliated Professionals." The Council of State Governments (Blog). March 2019.

W. Clay Fannin. "Florida Gov. Ron DeSantis Holds Occupational Licensure 'Deregathon' in Orlando." The Council of State Governments (Blog). March 2019.

Presentations

The Effects of SNAP Certification Interview Waivers on Multiple Program Participation
Population Association of America (PAA) Annual Meeting – 2024

Examining the Effects of WIC Physical Presence Waivers on Program Caseloads
Association for Public Policy Analysis & Management (APPAM) Fall Conference – 2023
COVID19 and Policy: Looking Backward and Looking Forward, Syracuse University – 2023
Center for Policy Research Seminar Series, Syracuse University – 2023
Population Association of America (PAA) Annual Meeting – 2023 (accepted as poster)

Education and Social Policy Workshop, Syracuse University – 2023

The Effects of SNAP Simplified Reporting on Errors and Benefit Amounts
Association for Public Policy Analysis & Management (APPAM) Fall Conference – 2022

COVID-19 and the Effects of the Decision to Implement Federal Waivers for SNAP
Population Association of America (PAA) Annual Meeting – 2022
Education and Social Policy Workshop, Syracuse University – 2022

Variation in Access to WIC and SNAP during the COVID Crisis
Association for Public Policy Analysis & Management (APPAM) Fall Conference – 2021

Teaching

Public Administration & Democracy (Syracuse University, Teaching Assistant, Summer 2023)

Public Policy & Program Evaluation (Syracuse University, Teaching Assistant, Spring 2022)

Interdisciplinary Perspectives on the COVID-19 Crisis (Syracuse University, Teaching Assistant, Fall 2020)

Grants and Funded Projects

2023-2024	CO-PI, USDA Economic Research Service Cooperative Agreement (#58-4000-3-0072): "The Effects of WIC Physical Presence Requirements on Program Participation Outcomes." $30,000
2023-2024	PI, USDA/Tufts University Research Innovation and Development Grants in Economics (RIDGE): "Exploring the Effects of SNAP Interview Waivers on Program Participation Outcomes." ~$25,000.
2023	Graduate Student Organization Professional, Academic, and Creative Work Grant. $500.
2022	Graduate Student Organization Professional, Academic, and Creative Work Grant. $500.
2022	Spencer D. Parratt Summer Research Award. $1,500.
2021	Spencer D. Parratt Summer Research Award. $1,500.

Honors and Awards

2022	Field examinations passed (Public Finance and Social Policy), Syracuse University
2022	Larry D. Schroeder Award for Excellence in PhD Research, Syracuse University
2021	Comprehensive examinations passed (high passed Methods and Policy Foundations), Syracuse University

2020	Outstanding MPP Student, University of Kentucky
2020	Pi Alpha Alpha, Global Honor Society for Public Affairs and Administration
2018	Department of Economics Commendation for Academic Excellence, University of Kentucky
2017	University Scholar, University of Kentucky

Professional Activities and Service

Reviewer for *Journal of Policy Analysis and Management*

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