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IZA DP No. 16294

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ABSTRACT

Certification and Recertification in Welfare Programs: What Happens When Automation Goes Wrong?*

How do administrative burdens influence enrollment in different welfare programs? Who is screened out at a given stage? This paper studies the impacts of increased administrative burdens associated with the automation of welfare caseworker assistance, leveraging a unique natural experiment in Indiana in which the IBM Corporation remotely processed applications for two-thirds of all counties. Using linked administrative records covering nearly 3 million program recipients, the results show that SNAP, TANF, and Medicaid enrollments fell by 15%, 24%, and 4% one year after automation, with these heterogeneous declines largely attributable to cross-program differences in recertification costs. Earlier-treated and higher-poverty counties experienced larger declines in welfare receipt. More needy individuals were screened out at exit while less needy individuals were screened out at entry, a novel distinction that would be missed by typical measures of targeting which focus on average changes overall. The decline in Medicaid enrollment exhibited considerable permanence after IBM's automated system was disbanded, suggesting potential long-term consequences of increased administrative burdens.

JEL Classification: H53, I38

Keywords: welfare programs, automation, take-up, targeting, administrative burdens

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1 Introduction

Safety net programs play a key role in insuring individuals against economic risk. Medicaid and the Supplemental Nutrition Assistance Program (SNAP) are among the largest means-tested transfers in the United States, reaching 23% and 10% of the population at a collective federal cost of $465 billion in 2019. However, program enrollment often requires overcoming numerous administrative burdens that can lead to incomplete take-up among eligible individuals (Currie 2006, Herd and Moynihan 2018).[1] These burdens could serve a useful targeting purpose if they screen out less needy individuals with a higher opportunity cost of time (Nichols et al. 1971, Nichols and Zeckhauser 1982, Besley and Coate 1992), but they may also screen out more needy individuals if application costs are negatively correlated with cognitive ability (Deshpande and Li 2019) or if needier individuals are prone to present bias (Bertrand et al. 2004, Mani et al. 2013, Mullainathan and Shafir 2013).

Given these different theoretical predictions, the effects of administrative burdens can hinge critically on the context in which they appear. A given set of burdens, for example, may have different effects across programs based upon differences in their enrollment procedures or populations served. Within a program, burdens can also appear at multiple points throughout the application and recertification process, with initial effects on entry potentially differing from dynamic effects on spell durations. Understanding the differential effects of enrollment barriers across programs and application stages is crucial for deciding how to allocate benefits to those needing them the most. Yet, little is known about how the enrollment and targeting effects of administrative burdens differ along these dimensions.

This paper investigates how barriers to enrollment affect the take-up of three important safety net programs - SNAP, Medicaid, and Temporary Assistance for Needy Families (TANF)[2] - and the types of individuals that are screened out at a given application stage. By

[1]For example, information about program eligibility may be difficult to acquire (Daponte et al. 1999, Manoli and Turner 2014, Armour 2018, Barr and Turner 2018). Application forms may be onerous to complete and require the submission of numerous documents to prove eligibility (Currie and Grogger 2001, Bettinger et al. 2012, Bhargava and Manoli 2015), and applicants may also have to physically travel to a local welfare office to conduct an interview (Rossin-Slater 2013). Individuals may also face psychological costs such as stigma (Moffitt 1983).

[2]TANF is a cash welfare program aimed at families with children, reaching less than 1% of the population with federal expenditures of $14 billion in 2019.

examining effects for multiple programs and decomposing changes along the entry and exit margins, this study contrasts with prior studies that commonly focus on a single program and a single stage (often initial application). This paper analyzes administrative burdens associated with the automation of welfare caseworker assistance, which states have increasingly adopted to allow individuals to apply and recertify remotely.[3] These changes are often thought to provide greater convenience for program applicants and lower administrative costs for program operators (Needels et al. 2000, Rowe et al. 2010), but they may also lead to greater inflexibility and an inability to tailor services to individual circumstances.

The research setting is the state of Indiana, which in 2007 outsourced the management of its welfare services to the IBM Corporation. IBM used online and phone platforms to replace face-to-face interactions with local caseworkers, resulting in a lack of personalized assistance, lower tolerance for errors, and long wait times at overwhelmed call centers. Not only did these burdens affect those enrolling in multiple programs, but they also materialized at multiple enrollment stages for a given program. IBM's automated system rolled out to only two-thirds of Indiana's counties (covering 48% of the state's caseload) before suddenly halting in 2009 due to performance problems.[4] By facilitating a comparison of outcomes over time between those counties receiving the rollout (treated areas) and all other counties (untreated areas), this natural experiment enables the identification of a causal effect. The key assumption, which is validated through numerous checks, is that outcomes in treated and untreated counties evolved in parallel prior to treatment and would have likely continued to evolve in parallel in the absence of treatment.

The analyses rely primarily on a unique source of data: administrative longitudinal welfare records covering nearly 3 million program recipients in Indiana linked to Internal Revenue Service (IRS) microdata.[5] By using administrative data sources to measure welfare receipt, one can circumvent issues of measurement error that pervade survey reports (Meyer

[3] By 2016, every state had either set up online applications, established call centers, or enabled telephone interviews for SNAP applicants - up from ten states in 2002 (Appendix Figure A1).

[4] State of Indiana's Amended Complaint for Damages and Declaratory Relief at 67, IN v. International Business Machines Corp., No. 49D10-1005-PL-021451, 2010 WL 5677110 (Ind. Super. Nov. 4, 2010).

[5] These restricted-use administrative records are part of the Comprehensive Income Dataset (CID) Project at the U.S. Census Bureau (Medalia et al. 2018). For a set of analyses that can be replicated using public-use county-level data, it is worth noting that the results are nearly identical regardless of whether they are based on public- or restricted-use data.

and Mittag 2019). The longitudinal nature of the administrative panel data also enables the disentangling of enrollment and targeting effects along the entry and exit margins, which would not be possible using cross-sectional data. Linking to IRS tax records further mobilizes a battery of well-being measures to rigorously measure program targeting.

Leveraging a generalized difference-in-differences approach, the results show that SNAP, TANF, and Medicaid enrollments fell by 15%, 24%, and 4% one year after the rollout of IBM's automated system. All three programs experienced statistically significant declines in entry rates that were similar in magnitude, but differential increases in exit rates that were largest for TANF and smallest for Medicaid. Cross-program differences in recertification costs, rather than recipient composition, were an important contributor to heterogeneous effects across programs in the short run. For Medicaid, the overall enrollment reduction also exhibited considerable permanence; four years after the automated system was disbanded (and six years after the initial rollout), enrollment in treated counties remained 2% lower than in untreated counties. Enrollment reductions were most pronounced in earlier-treated and higher-poverty counties that were less able to anticipate or handle the increased burdens, and in lower-unemployment counties whose residents may have had more earnings to verify.

Our analyses then consider who is screened out by IBM's automated system at a given stage. On one hand, more needy individuals - including those with lower pre-treatment incomes, less education, higher per-person benefits, and higher disability levels - are screened out at exit, suggesting that IBM's automated system is an ineffective screen at recertification. On the other hand, less needy individuals are screened out at entry, suggesting that IBM's automated system could be an effective screen at initial application. This novel distinction would be missed by typical measures of targeting which focus on average changes overall or at a single margin. These empirical results can be rationalized by a model in which administrative burdens screen out both the least and most needy individuals, with the least needy more likely to appear at initial application and the most needy more likely to appear at recertification due to differential selection of individuals into each stage.

This paper makes several distinct contributions. First, it adds to our nascent understanding of the *distribution* of take-up and targeting effects by showing how they vary across entry and exit.[6] This decomposition reveals targeting implications that would be masked by an emphasis on overall mean effects. Prior studies typically examine changes only along a single margin (initial take-up or retention) and few explicitly distinguish between the effects on both margins. We offer a new explanation for why existing empirical studies of targeting may find seemingly inconsistent results (see, e.g., Alatas et al. 2019, Deshpande and Li 2019, Gray 2019, Shepard and Wagner 2021, Unrath 2021). Finkelstein and Notowidigdo (2019), for example, find that SNAP application costs lead to improved targeting for initial applicants, while Homonoff and Somerville (2021) find the opposite result when studying SNAP recertifiers. This study suggests that the theoretical tension between neoclassical and behavioral models of targeting could be better understood by focusing on the applicability of a given theory to the types of individuals appearing at a given stage.

Second, the research setting examines how a common set of administrative burdens affects access to *multiple* programs, contrasting with prior studies that typically evaluate effects for a single program. It is often the case that a single government agency administers multiple programs, and administrative burdens may very well exist at the agency level.[7] Whereas cross-program comparisons in the literature often embed large differences in research settings (Hendren and Sprung-Keyser 2020), this study compares programs in a controlled setting. It focuses specifically on differences across programs in transaction costs, which have been shown to be important determinants of take-up for well-established programs that require periodic recertification (Currie 2006). A given set of burdens can thus have markedly different effects across programs given baseline differences in their enrollment procedures.

Third, this is one of the first papers to empirically examine the drawbacks to automating social services, in contrast to prior studies analyzing the benefits of automatic enrollment (Madrian and Shea 2001, Shepard and Wagner 2021) or technological changes to enrollment procedures (Kopczuk and Pop-Eleches 2007, Ebenstein and Stange 2010, Schwabish 2012). As these changes become widely adopted and imperfectly administered, they are likely to also

[6] To formalize this, the appendix presents a simple theoretical model of program participation that allows for application costs to vary across programs, applicant types, and application stages.

[7] For example, a state welfare agency may administer programs ranging from SNAP, TANF, and Medicaid to the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) and the Low Income Home Energy Assistance Program (LIHEAP). The Social Security Administration (SSA) also administers both Social Security and Supplemental Security Income, and SSA offices are used for determining initial eligibility for Medicare.

induce complexities that are less well understood. Even a short-lived automation spell - like the one studied in this paper - can lead to potentially long-term adverse effects when poorly implemented. This relates more broadly to the intended and unintended consequences of governments engaging in policy experimentation (Callander and Harstad 2015). This study also contributes to a growing literature analyzing the effects of privatizing services typically offered by the public sector. Prior studies have examined contexts ranging from health care (Duggan et al. 2017) to prisons (Mukherjee 2021).

While this paper focuses on a single state during the Great Recession, its policy significance holds broad applicability. Automation can take different forms, but many states - and even countries - have implemented systems automating the role of caseworkers.[8] During the COVID-19 pandemic, states out of necessity have also accelerated the move towards having computers or call center workers make remote eligibility determinations. These changes have come on the heels of caseworkers seeing their roles diminish over time in response to perceived inefficiencies and complaints about fraud. This lack of human contact, however, may have different screening effects at different stages. Because the types of people affected can differ across stages, policymakers may be well-advised to adapt procedures differentially across stages to whom they intend to target. More generally, this paper highlights how the design of safety net programs can play a vital role in their ability to target benefits to those needing them the most.

The rest of this paper is structured as follows. Section 2 provides background information about the welfare programs and IBM's automated system, drawing from sources that include court documents. Section 3 discusses data sources and compares the baseline characteristics of treated and untreated counties. Section 4 formalizes the empirical strategy. Sections 5 and 6 show regression results on enrollment and targeting, and Section 7 presents robustness checks. Section 8 concludes.

[8]For example, Florida in 2004 replaced caseworkers with online questionnaires that allowed it to reduce its welfare staff. In return, thousands of community partners with little funding or training were asked to provide computer access to clients and help with casework. Texas in 2005 also contracted with Accenture to build a system to computerize eligibility decisions for the state's Children's Health Insurance Program. After thousands of cases were incorrectly terminated following a two-county rollout, Texas cancelled the contract in 2007. The performance problems associated with IBM's automated system also bear a striking resemblance to those associated with the United Kingdom's rollout of its new social security program (Universal Credit).

2 Background on Programs and Natural Experiment

2.1 Description of SNAP, TANF, and Medicaid Programs

This paper focuses on three important safety net programs for low-income individuals: the Supplemental Nutrition Assistance Program (SNAP), Temporary Assistance for Needy Families (TANF), and Medicaid. A key feature of these federally-funded programs is that they are administered by the individual states. SNAP is the largest food assistance program, whose benefits are widely available to all low-income households and consist of in-kind vouchers - typically in the form of Electronic Benefit Transfer (EBT) cards - that can be used to purchase items from grocery stores and related outlets (Currie 2003). Households are usually eligible for SNAP if their gross and net monthly incomes fall below 130% and 100%, respectively, of the federal poverty line and their countable assets are below $2,250 ($3,500 if elderly or disabled).[9] In 2019, the maximum monthly benefit for a family of four was $642, amounting to 30% of the total income for a two-adult/two-child family at the poverty line.[10]

TANF, formerly known as Aid to Families with Dependent Children (AFDC), is a cash transfer targeted to low-income families with children. Families receiving TANF typically have a single parent or no parents at all.[11] After welfare reform in the late 1990s, a number of restrictions were placed on TANF recipients with the goal of helping them achieve economic self-sufficiency. Adults cannot receive TANF payments for more than sixty months over their lifetimes (although states can exempt this requirement for some families), and 50% of all TANF families must work for at least 30 hours per week (Ziliak 2016).[12] The current maximum monthly benefit for a family of four in Indiana is $346.[13] Households receiving TANF appear less well-off than SNAP or Medicaid recipients on many dimensions, including cognitive ability based on the Armed Forces Qualification Test (Appendix Table A1).

[9] Under the Broad-Based Categorical Eligibility (BBCE) policy, many states have also increased the income limit to 200% of the poverty line and eliminated the asset test. Indiana is not one of these states.

[10] See https://www.fns.usda.gov/snap/allotment/COLA.

[11] Children who are part of TANF cases with no parents (i.e., child-only cases) generally live with another relative or guardian (such as a grandparent) or with a parent who does not qualify for TANF for certain non-financial reasons (Golden and Hawkins 2011). In October 2007, 46% of TANF cases nationwide had no parents while 25% of TANF cases in Indiana had no parents.

[12] In Indiana, adults cannot receive TANF for more than 24 months and children cannot receive TANF for more than 60 months.

[13] See https://www.in.gov/fssa/dfr/files/TANF-Brochure-English.pdf.

Medicaid is the largest means-tested transfer in the U.S. and pays for medical costs accrued by low-income individuals. Prior to its expansion under the Affordable Care Act (ACA) in 2014, which extended eligibility to single adults without dependents, eligible groups mainly included low-income families with children, elderly individuals, and disabled individuals. Indiana extended eligibility to low-income single adults starting in 2008, being one of a few states to do so prior to ACA.[14] Compared to SNAP and TANF recipients, typical Medicaid recipients have higher levels of income, education, and homeownership, lower levels of unemployment and disability, and fewer material hardships (Appendix Table A1).

2.2 Application and Recertification Processes

The enrollment processes for SNAP, TANF, and Medicaid each involve undergoing an initial application process and recertifying eligibility at periodic intervals. The initial application process requires applicants to provide demographic information on every individual in the assistance unit and an exhaustive list of income sources and expenses. Applicants are also required to submit numerous supporting documents, ranging from proof of identity to proof of all income sources and deductions. As an example, Appendix Figure A2 shows a list of the documents that Medicaid applicants in Indiana may be required to submit, depending on the types of income sources, assets, and expenses that are listed on one's application. In many states (including Indiana), individuals can apply for SNAP and TANF using a single application, while applying for Medicaid requires filling out a separate application.[15]

After submitting their applications, applicants are usually contacted by their local welfare office to conduct an interview to verify eligibility. Successful applicants typically start receiving benefits within 60 days of first submitting their application, although particularly needy households may qualify for expedited benefits within a week of application. Each program then requires that its recipients periodically recertify their eligibility, which involves completing a renewal form listing changes in one's income and household structure, providing

[14]Most beneficiaries in Indiana also received Medicaid benefits on a fee-for-service basis prior to 2005, before Indiana shifted to offering the majority of Medicaid benefits through managed care plans.

[15]For Indiana, the Medicaid application is similar to the SNAP/TANF application in many ways, although it asks for some additional information (e.g., health coverage from current employer if applicable).

supporting documentation, and conducting another interview if needed. Medicaid recipients typically recertify every 12 months, SNAP recipients recertify their eligibility every 6 to 12 months,[16] and TANF recipients have the shortest recertification intervals (6 months). TANF recipients interact with their welfare office even more frequently, as they have to report all income changes (which occur often given work and job search requirements). As a result, only 30% of TANF recipients in Indiana are still enrolled 12 months after initial enrollment, compared to 52% of SNAP recipients and 76% of Medicaid recipients (Appendix Figure A3). Exits from TANF are particularly high during the first six months of the program spell, while exits from SNAP and Medicaid are concentrated around recertification periods.[17]

2.3 Automating Caseworker Assistance in Indiana

In 2006, Indiana Governor Mitch Daniels announced an effort to modernize the state's welfare system, which he described as "plagued by high error rates, fraud, wasted dollars, poor conditions for its employees, and very poor service to its clients."[18] This initiative followed prior endeavors by Governor Daniels to privatize several of the state's public services, including toll roads and meal services in prisons.[19] State officials believed that transitioning from a face-to-face casework system to a virtual platform would streamline the processing of applications and recertifications. There were also concerns that the existing system, which relied on caseworkers developing personal relationships with recipients, invited fraud.[20]

[16]Indiana requires that its SNAP recipients provide updates on unit structure and incomes/expenses every 6 months, while renewal interviews may happen every 6 or 12 months. Prior to 2003, Indiana required that SNAP recipients report any income changes immediately to their welfare office so that benefit amounts could be adjusted accordingly. After 2003, however, Indiana enacted "simplified reporting" rules requiring SNAP recipients only to report income changes outside of the six-month recertification interval when their new incomes exceed the gross income limit and therefore terminate their eligibility (USDA 2019).

[17]Recent studies indicate that many who exit SNAP do so because of missed deadlines rather than ineligibility (Mills et al. 2014, Gray 2019), suggesting that administrative burdens may play a prevalent role in the re-enrollment process.

[18]Indiana v. Int'l Bus. Machs. Corp., 4 N.E.3d 696, 703 (Ind. Ct. App. 2014).

[19]See https://www.latimes.com/archives/la-xpm-2011-jun-24-la-na-indiana-privatize-20110624-story.html and https://finance.yahoo.com/news/ind-court-sets-hearing-ibm-172824630.html.

[20]State officials dealt with several high-profile fraud cases in the mid-2000s, including one in which a caseworker colluded with members of an Indianapolis church congregation to defraud the state out of $62,000 in food stamps and cash assistance (see https://www.wthr.com/article/news/church-leaders-charged-with-food-stamp-fraud/531-4351d507-4cf0-42d0-91ca-32ea302abb17). Yet, Indiana's SNAP error rates during fiscal year 2007 - while slightly exceeding national averages in both under- and over-payment - were still below

In December 2006, after seeking various vendors for the project, Indiana awarded a 10-year, $1.3 billion contract to the IBM Corporation.[21] The primary goals of the new system were to combat fraud, lower administrative costs, improve the welfare-to-work pipeline, and improve access through a more flexible platform.[22] While the state retained operational and policymaking control over SNAP, TANF, and Medicaid (including making final eligibility determinations), IBM would assist in processing initial applications and recertifications.[23] The new system in Indiana would allow citizens to apply for benefits online or through a call center, without the need for a face-to-face meeting with a local caseworker (Aman 2013). Approximately 1,500 of the 2,200 employees of Indiana's Family and Social Services Administration (FSSA) were transferred to IBM's private call centers, which would process applications on a centralized, statewide basis rather than in each county's welfare office.[24]

The rollout of IBM's automated system proceeded in several stages. In March 2007, Indiana informed program recipients about the upcoming changes and began transitioning state employees to IBM.[25] Starting in October 2007, Indiana rolled out IBM's automated system to counties in several waves (Figure 1). The rollout sought to initially avoid the most populous counties, as they were likely to have a harder time transitioning to the new system.[26] In the first wave (October 2007), IBM was rolled out to 12 relatively rural counties in north-central Indiana.[27] The second rollout wave (March 2008) reached 27 more counties in southern and central Indiana, including the cities of Bloomington and Terre Haute.[28] The third rollout wave (May 2008) reached 20 more counties in southwest and northeast Indiana, covering cities such as Fort Wayne and Evansville (the second- and third-largest cities in the state).[29] IBM's system was originally intended to roll out to all 92 counties, but the expansion was halted due to performance issues.[30]

those of neighboring states Ohio and Michigan (Appendix Figure A3).

[21] Intl' Bus. Machs. Corp. v. Indiana, 112 N.E.3d 1088, 1093 (Ind. Ct. App. 2018).

[22] Indiana v. Int'l Bus. Machs. Corp., 51 N.E.3d 150, 154 (Ind. 2016).

[23] Id. at 154.

[24] See https://www.wthr.com/article/news/daniels-signs-1-billion-welfare-outsourcing-deal/531-e74baf2c-5662-40bd-8c89-1c5215beb77b.

[25] Am. Compl. at 54.

[26] Id. at 67.

[27] Id. at 58.

[28] Id. at 62.

[29] Id. at 63.

[30] Id. at 68.

Call centers were quickly overwhelmed after the initial rollout, leading to tens of thousands of unanswered calls.[31] Moreover, call center operators - many of whom were not adequately trained on how to handle the new system - often served as the sole point of contact for clients in the automated counties (Eubanks 2018, p. 50). Under the previous system, local caseworkers were assigned to a docket of families with whom they interacted face-to-face and guided through the full application process. Under the new automated system, caseworkers were assigned to tasks dropped into a computerized queue, and applicants spoke with a different operator every time they called.[32] The lack of consistent guidance from a dedicated caseworker led to more mistakes on application and recertification forms.

Exacerbating these issues was a reduced tolerance for certification and recertification errors, arising partly out of the mechanized processing of applications. Mistakes were often assumed to be the fault of the client and interpreted as a refusal to cooperate in establishing eligibility (Eubanks 2018, p. 42-43). This situation often led to a notification of a denied application or the imminent expiration of benefits. As these notifications never specified the nature of the underlying mistakes, clients were often unaware of the reasons for denial. IBM also required that all records be electronically scanned as part of the move to a "paperless" system, placing the burden on clients to resubmit all documents that were previously available only as hard copies.[33] This led to a number of incomplete recertifications for recipients who could not locate documents in a timely manner.

Technical glitches on the IBM platform and a backlog of unanswered phone calls further led to numerous individuals being improperly denied benefits.[34] Moreover, the verification documents that IBM required clients to submit often went unprocessed, with a single missing document being enough to warrant the denial of an application. Approximately 11,000 documents remained unprocessed in December 2007, with this number surging to nearly 283,000

[31] Indiana, 4 N.E.3d at 708.

[32] See https://www.thenation.com/article/archive/want-cut-welfare-theres-app/.

[33] Indiana, 51 N.E.3d at 153. See also p. 50 of Eubanks (2018).

[34] Indiana, 51 N.E.3d at 156. In a subsequent court case in 2010, a number of individuals provided anecdotes about how they were adversely impacted by the automated system. One individual was denied Medicaid and subsequently SNAP benefits because she missed her scheduled recertification interview while in the hospital suffering from terminal cancer. Even though she called IBM's call center to inform them of the circumstances, her benefits were cut off anyway (Am. Compl. at 114). In another case, a deaf individual was unable to comply with IBM's requirement that she have a telephone interview, causing considerable delay in granting her benefits (Am. Compl. at 115).

lost documents by early 2009 (Eubanks 2018, p. 50). Finally, call center workers would be so far behind in processing applications that they sometimes recommended premature denials simply to make their performance appear more timely (Eubanks 2018, p. 51).[35]

As a result of these performance problems, the rollout of IBM's automated system was halted in January 2009.[36] Ultimately, 59 out of Indiana's 92 counties (covering 48% of the state's caseload) were transitioned to the automated system, although some of the most populous areas in Indiana - including Indianapolis, northwest Indiana, and South Bend - were never automated.[37] This created a natural experiment, where counties receiving the rollout can be thought of as treated areas and all other counties as untreated areas whose outcomes can be compared over time. After the rollout was halted, Indiana asked IBM in March 2009 to formulate a "Corrective Action Plan" to show improvement in the 59 automated counties.[38] In July 2009, Indiana and IBM agreed on the terms of a Corrective Action Plan.[39] Indiana eventually decided to terminate its contract with IBM in December 2009 in favor of a hybrid modernization approach, which moved eligibility determinations back to local welfare offices with greater opportunities for face-to-face interactions with caseworkers.[40]

3 Data Sources and Summary Statistics

3.1 Data Sources

This paper relies primarily on restricted-use administrative microdata covering the universe of program participants linked to microdata on various outcomes. These data are part of the Comprehensive Income Dataset (CID) Project at the U.S. Census Bureau. Administrative SNAP, TANF, and Medicaid records for Indiana contain information on monthly benefits paid out to the universe of SNAP and TANF recipients (starting in July 2004) and

[35]Applicants were then recommended to either reapply or appeal the decision, with either avenue potentially taking months if pursued.

[36]Am. Compl. at 71.

[37]Id. at 67.

[38]Indiana, 4 N.E.3d at 709.

[39]Indiana, 4 N.E.3d at 710.

[40]Int'l Bus. Machs. Corp., 124 N.E.3d at 1094.

monthly enrollment for the universe of Medicaid recipients (starting in January 2005). These program data also contain information on incomes (as observed by Indiana's FSSA), county of residence, eligibility characteristics, and some limited demographic information (e.g., gender, race, education, etc.). A key advantage of these longitudinal welfare records is that they measure a case's receipt history over time and thereby enable analyses along the entry and exit margins that would not be possible using cross-sectional data. Dollar amounts are adjusted for inflation using the Personal Consumption Expenditure (PCE) price index.[41]

These welfare records are linked to Internal Revenue Service (IRS) microdata on various income measures that can be used to examine targeting. These include formal sector wages from IRS Form W-2s and retirement income from IRS Form 1099-Rs, which are available regardless of whether or not one files a tax return. They also include adjusted gross income from IRS Form 1040s, which are available only for those filing a tax return. The data are linked using individual identifiers called Protected Identification Keys (PIKs), which can be approximately thought of as anonymized Social Security Numbers. Nearly 100% of individuals in all administrative data sources link to a PIK.

Accompanying the restricted-use data sources are public-use data on county-level program enrollment and various county-level characteristics. Monthly county-level enrollment data for SNAP, TANF, and Medicaid (starting in October 2002) are obtained from Indiana's FSSA.[42] These county-level data are used both to validate the quality of the restricted-use microdata and to conduct additional robustness checks. Also available are county-level data sources covering other demographic and economic characteristics, including monthly unemployment rates from the Bureau of Labor Statistics (BLS) and quarterly data on jobs, establishments, and wages by industry from the BLS Quarterly Census of Employment and Wages.[43] Annual data on county-level population counts (by age, race/ethnicity, and gender

[41] The PCE deflator is better than the Consumer Price Index (CPI) in accounting for the ability of consumers to substitute between broad product categories when prices change (Bullard 2013, Winship 2016).

[42] The SNAP and TANF data cover the number of enrolled cases (assistance units), individuals, and benefit dollars, while the Medicaid data cover the number of enrolled individuals. Monthly county-level Medicaid enrollment records are available online starting in September 2013, and earlier records were obtained through a Freedom of Information Act request.

[43] Note that county-level unemployment rates are not directly measured by the BLS but are instead imputed based on state-level employment rates and county-level Unemployment Insurance records. Consequently, these county-level rates are likely subject to some measurement error (Ganong and Liebman 2018).

categories), poverty rates, and median incomes are available from the U.S. Census Bureau.

This paper also brings in annual county-level enrollment data for a number of other programs not administered by IBM's automated system (for placebo tests). These include the numbers of individuals enrolled in both Social Security and Supplemental Security Income (SSI), measured by the Social Security Administration in December of every year. County-level enrollment counts for Medicare come from the Centers for Medicare and Medicaid Services (for 2007 onward). In addition, the Department of Education's Common Core of Data contains annual school-level counts of students receiving free and reduced meals, which can be aggregated to the county level.

3.2 Characteristics of Treated and Untreated Counties in Indiana

Table 1 compares untreated and treated counties in Indiana across a range of characteristics measured in September 2007 (the month prior to treatment).[44] The summary statistics for the 33 untreated counties are calculated for all such counties (Column 1) and also for a "less populous" subset that omits Marion and Lake counties (Column 2), the two most populous counties in Indiana. The summary statistics for the 59 treated counties are also calculated across all such counties (Column 3) and subdividing by treatment wave (Columns 4-6). Compared to the "less populous" set of untreated counties, treated counties have higher rates of SNAP and Medicaid receipt and similar rates of TANF receipt. However, the counties treated in Wave 1 tend to have the highest rates of welfare receipt for each program, while the counties treated in Wave 3 tend to have the lowest rates of welfare receipt for each program (except for TANF). Treated counties are also more rural and more white than their untreated counterparts (even after omitting Marion and Lake counties from the untreated group), in addition to having fewer children and more elderly adults.

Turning next to economic characteristics, treated counties have a higher share of individuals employed in the manufacturing sector and a lower share of workers in the construc-

[44] Annual numbers for population counts and poverty/income measures are for calendar year 2007, quarterly numbers for employment characteristics are for the third quarter of 2007, and monthly numbers for welfare receipt and unemployment rates are for September 2007. Compared to those in the U.S., program recipients in Indiana are more likely to be single parents, disabled, and unemployed and are less likely to be elderly, homeowners, and high-school educated (Appendix Table A1 and Appendix Table A2).

tion, wholesale/retail trade, and professional/administrative services sectors. Additionally, treated counties have slightly higher unemployment rates than untreated counties. Finally, treated counties have median household incomes below those of untreated counties. Poverty rates are therefore considerably higher in treated counties, which is consistent with these counties having higher rates of program receipt. In summary, the counties exposed to IBM's automated system are on average smaller, poorer, more rural, less racially and ethnically diverse, and more welfare-reliant than the counties that were never automated. For the most part, these differences are larger for counties treated in earlier waves.

Despite differences between treated and untreated counties in the baseline *levels* of these characteristics prior to treatment, it remains to be seen whether or not they follow different *trajectories* in treated and untreated counties prior to treatment. Figure 2a shows trends in SNAP enrollment for treated counties and untreated counties (omitting Marion and Lake counties) over an 18-year period preceding treatment. These trends evolve in a strikingly parallel fashion over a period that featured multiple recessions, economic booms, and program expansions. Figure 2b further shows that trends in unemployment rates - which are key determinants of program participation - also follow consistent trajectories over time for treated and untreated counties. These patterns help to motivate the differences-in-differences empirical strategy that we describe in the next section.

4 Empirical Strategy

To assess the causal effects of IBM's automated system on SNAP, TANF, and Medicaid enrollment, this paper relies on a generalized difference-in-differences design. This empirical strategy exploits the fact that counties were naturally assigned to treated and untreated groups whose outcomes can be tracked over time. The identifying assumption is that treated and untreated counties, despite differing on baseline characteristics, would have had similar trends in outcomes in the absence of treatment.

4.1 Regression Specification

The following dynamic specification compares how enrollment in treated counties (i.e., those automated by IBM) evolves relative to enrollment in untreated counties:

$$y_{ct} = \mu_c + \lambda_t + \sum_k \gamma_k D_{ct}^k + \beta X_{ct} + \varepsilon_{ct}, \tag{1}$$

where y_{ct} is an aggregate outcome (e.g., log number of SNAP cases) for county c and year-month t. D_{ct}^k is a dummy variable equaling one if county c receives treatment and month t is k quarters after (or before, if k is negative) IBM's automated system is implemented. The coefficients of interest are the γ_k's, which measure the difference in outcomes between treated and untreated counties k quarters after automation.[45] The main estimates rely on a window of 12 quarters (3 years) before treatment and 24 quarters (6 years) after treatment. County- and month-fixed effects are denoted respectively by μ_c and λ_t, and X_{ct} is a vector of county- and time-varying covariates.[46]

By measuring effects on county-level enrollment (aggregated from individual-level enrollment data), one can capture changes at both the entry and exit margins. County-month observations are weighted by enrollment volume in September 2007 (the month prior to IBM's initial rollout). Marion and Lake counties (which are part of the metropolitan areas of Indianapolis and Chicago, respectively) are excluded from the untreated group, as they are by far the two most populous counties in Indiana and unlikely to be good counterfactuals for any of the treated counties. Appendix Figure A5 shows that Marion and Lake counties are outliers on a number of dimensions relevant to program enrollment, including urbanicity and the population shares of black and Hispanic individuals.[47]

While equation (1) allows for the decomposition of treatment effects across event-time,

[45]The dummy variable corresponding to $k = -1$ is omitted, meaning the estimates of γ_k should be interpreted as being relative to event-time $k = -1$ (the quarter immediately prior to treatment).

[46]These covariates include total population, the numbers of white, black, and Hispanic individuals, the number of females, the number of children, and the number of non-elderly adults (aged 18-64). All of these variables are logged when the outcome is also logged, and they vary by county and year (rather than month).

[47]Despite representing only 2% of all counties in Indiana, Marion and Lake counties constitute 22% of the overall state population and 32% of the state SNAP caseload. They are also the two most urban counties and have the highest population share of black individuals in Indiana.

the following specification can be used to summarize average effects:

$$\log(y_{ct}) = \mu_c + \lambda_t + \gamma D_{ct} + \beta X_{ct} + \varepsilon_{ct}, \tag{2}$$

where D_{ct} is a dummy variable equaling one if county c receives treatment and if month t is after the rollout of the automated system. Estimates of γ (the time-averaged difference-in-differences coefficient) use observations within a window of 12 quarters before treatment and 12 or 24 quarters after treatment. As a result, the estimated γ in equation (2) can be roughly interpreted as a weighted average of the estimated γ_k's in equation (1). While a variety of recent studies have identified biases associated with traditional two-way fixed effects or event study specifications with variation in treatment timing (see, e.g., Callaway and Sant'Anna 2021, Goodman-Bacon 2021, Sun and Abraham 2021), Section 7 provides evidence that these biases are small in the current context. This result is due to the staggered IBM rollout occurring over a short time frame (7 months) relative to the overall time frame for the analyses (72 months or 108 months).

Finally, the main estimates of the standard errors are clustered at the county level, as doing so accounts for serial correlation within counties over time. However, there may also exist spatial correlation across counties within a treatment region, since IBM's automated system was rolled out to a group of counties at a time. As a robustness check, standard errors are also clustered at the region level, where regions are defined by the presence and/or timing of treatment. A complication under this approach is that the number of clusters (i.e., regions) is only six, while accurate estimates of clustered standard errors rely on the number of clusters being large. The robustness check therefore uses a wild cluster bootstrap procedure to account for the small number of clusters (Cameron et al. 2008).

4.2 Raw Trends in Welfare Receipt

To provide visual evidence for the difference-in-differences strategy, Figure 3 shows monthly trends in SNAP, TANF, and Medicaid receipt rates for the counties automated by IBM (categorized by rollout wave) and the never-automated counties. Focusing first on

SNAP receipt, Figure 3a shows that the enrollment patterns for treated counties track the patterns for untreated counties until precisely each set of treated counties is automated. Six months after the counties were automated, the rate of SNAP receipt had fallen by 10% for the counties treated in Wave 1 and by 3% or less for the counties treated in Waves 2 and 3. A major reason for these raw differences in receipt rates is that treatment in Waves 2 and 3 tended to coincide with increases in SNAP applications resulting from the Great Recession, as indicated by the steadily increasing enrollment rates in the untreated counties.

Similar to the patterns for SNAP, TANF enrollment following treatment fell dramatically in the counties treated in Wave 1 while there was a more muted decrease in the counties treated in Waves 2 and 3 (Figure 3b). Unlike the patterns for SNAP, the trend in TANF enrollment remained relatively flat for the untreated counties. For Medicaid, there was no sharp drop in enrollment for any of the treated sets of counties, although they experienced flatter growth rates than the untreated counties (Figure 3c).

5 Effects of IBM Automation on Enrollment

This section describes the overall effects of IBM's automated system on SNAP, TANF, and Medicaid enrollment. The first subsection discusses dynamic effects on total enrollment - following their evolution for six years after treatment - and static treatment effects. The next subsection disaggregates changes in total enrollment along the entry and exit margins.

5.1 Overall Effects on SNAP, TANF, and Medicaid Enrollment

Focusing first on SNAP enrollment, Figure 4 shows no pre-trends in the treated counties relative to the untreated counties before a sharp drop in SNAP enrollment immediately after the automated system is rolled out. Four quarters after the implementation of IBM's automated system, treated counties have 15% fewer SNAP individuals than untreated counties. The decline in SNAP enrollment reverses in the ensuing quarters, with the turnaround corresponding to IBM's contract revision. The rebound in SNAP enrollment among the treated counties slows down after the termination of IBM's contract. Four years after IBM's automated system was disbanded, SNAP enrollment in treated counties remained 3% lower than in untreated counties. These longer-run estimates, although statistically insignificant, suggest some level of permanence in the reduction of SNAP receipt in the treated counties.

Figure 4 shows an ever steeper initial drop in TANF receipt as a result of the automated system. Treated counties had 24% fewer TANF individuals than untreated counties after the first four quarters of automation, and this difference expanded to 27% after four more quarters. This decline was more pronounced among recipients in cases with an adult and less substantial among recipients in child-only cases - likely a result of adults facing work requirements and thus having more incomes to verify (Appendix Figure A6). The reduction in TANF enrollment in the treated counties hit a trough ten quarters after initial treatment, but - unlike the effects for SNAP - the differences in TANF enrollment between the treated and untreated counties disappeared six years after the automated system was first rolled out. The long-term rebound in TANF enrollment can likely be explained by the continuous turnover among TANF recipients, as lifetime enrollment caps implied that most TANF recipients at the time of IBM's rollout were no longer recipients several years later.

Medicaid receipt also noticeably dropped after treatment, although the initial reduction for Medicaid was smaller than that for SNAP or TANF. Four quarters after automation, treated counties had 4% fewer Medicaid individuals than untreated counties. Although there was a modest reversal in the decline two years after the automated system was disbanded, the estimates show a level of permanence in the reduction of Medicaid receipt. Four years after the automated system was terminated, Medicaid enrollment in the treated counties remained 2% lower than in the untreated counties.

Table 2 summarizes the effects of IBM's automated system on log total cases, individuals, and benefit dollars. These estimates correspond to γ in equation (2). For SNAP, IBM's automated system leads to statistically significant decreases in individuals, cases, and benefit dollars of 10.1%, 10.3%, and 8.8% over the three years following treatment. Using a longer post-treatment window of six years, the treatment effects attenuate slightly to 7.4%, 8.0%, and 6.5% for individuals, cases, and benefit dollars - although they remain statistically significant at the 1% level. For TANF, the automated system leads to reductions in individuals, cases, and benefit dollars of 19.0%, 17.5%, and 15.0% (statistically significant at the 1% level) over the three years after treatment and 13.2%, 10.5%, and 8.5% (statistically significant at the 5% or 10% level) over the six years after treatment. The short-run enrollment declines for TANF are more substantial for cases with an adult than for child-only cases, with the long-run declines driven almost entirely by reductions among cases with an adult. Finally, Medicaid individuals and cases decline by 2.8% and 3.2% in the three years following automation, with these effects remaining relatively uniform at 2.7% and 3.3% when calculated using a post-treatment window of six years.[48] The treatment effects are statistically significant at the 1% level for Medicaid cases but only at the 10% level for individuals.

In summary, the negative effects of IBM's automated system on enrollment are initially largest for TANF and smallest for Medicaid, while the long-run effects are most persistent for Medicaid and least persistent for TANF (a reversal of the patterns for initial enrollment).

5.2 Disaggregating Enrollment Effects by Entry and Exit

Given that IBM's automated system imposed administrative burdens at multiple stages of the application process, it is useful to disaggregate the extent to which the reductions in total enrollment are driven by decreased entry versus increased exit. Let $Entrants_{ct}$ and $Exiters_{ct}$ denote the number of individual recipients in county c entering and exiting a program, respectively, between year-months t and $t+1$.[49] Monthly entry and exit rates can then be defined as follows:

$$Entry\ Rate_{ct} = \frac{Entrants_{ct}}{Population_{ct} - Recipients_{ct}}$$

[48] After clustering standard errors at the region level using the wild cluster bootstrap, the short-run estimates for SNAP and TANF enrollment remain statistically significant at the 10% level while the short-run estimates for Medicaid enrollment become statistically insignificant (Appendix Table A3). In the long-run, only the SNAP enrollment estimates remain significant at the 10% level after clustering at the region level.

[49] Using the longitudinal welfare records, entry and exit indicators are calculated based on PIK identifiers rather than individual IDs assigned by the welfare agency. Whereas PIKs are objective identifiers assigned by the Census Bureau (based on a recipient's Social Security Number), the individual IDs assigned by the welfare agency could have changed endogenously if IBM assigned different IDs to individuals who churned back onto a program. In practice, the results are insensitive to whether entry and exit indicators are calculated using PIKs or IDs assigned by the welfare agency.

$$Exit\ Rate_{ct} = \frac{Exiters_{ct}}{Recipients_{ct}},$$

where $Population_{ct}$ denotes total population in county c in month t and $Recipients_{ct}$ denotes total recipients in county c and month t. Scaling these rates by the relevant exposed populations allows one to calculate the probabilities of entering a program conditional on not currently being enrolled and exiting a program conditional on being enrolled, which are the transition probabilities in a two-state Markov model.

Table 3 shows the effects of IBM's automated system on entry and exit rates, estimated using the regression framework in equation (2) and a post-treatment window of 3 years.[50] First, IBM automation causes statistically significant declines in entry rates ranging from 0.03 percentage points to 0.05 percentage points - relatively similar effects - for all three programs. Because average entry rates are lowest for TANF (0.002%) and highest for SNAP (0.009%), the percentage declines in entry are larger for TANF and smaller for SNAP when scaled by the relevant baseline means. Furthermore, even though the percentage point effects are similar across programs, there are differences in the dynamic patterns across programs. The entire decline in entry for SNAP is concentrated in the first two quarters of treatment, while the decline in entry for TANF and Medicaid is initially smaller and more protracted (Figure 5a). For SNAP, the rapid bounceback in entry (and slight increase in subsequent entry rates) can likely be explained by churn among those recently exiting SNAP.

Next, the results show large differences across programs in the effect of IBM's automated system on exit rates. IBM automation causes an increase of 1.05 percentage points (statistically significant at the 10% level) in the exit rate for TANF, 0.74 percentage points (statistically significant at the 1% level) for SNAP, and a statistically insignificant 0.04 percentage points for Medicaid. When limiting TANF recipients to TANF cases with an adult, the results show an even more substantial increase in the exit rate of 1.75 percentage points (statistically significant at the 5% level).[51] These cross-program differences in the effects on

[50] When the entry rate is the regression outcome, the covariates include the mean shares of race/ethnicity, gender, and age group populations among non-recipients in X_{ct} and county-months are weighted by the number of non-recipient individuals measured in September 2007. When the exit rate is the regression outcome, the covariates include the mean shares of race/ethnicity, gender, and age group populations among recipients in X_{ct} and county-months are weighted by the number of recipient individuals measured in September 2007.

[51] It makes sense that the increase in the TANF exit rate is driven entirely by increased exit among cases with an adult, as child-only cases are likely to face very little risk of not being recertified (given that they

exit rates may partly be a function of differences across programs in their baseline exit rates.

5.3 Differences in Program Costs vs. Recipient Composition

This subsection explores various hypotheses for why enrollment effects, particularly along the exit margin, are initially largest for TANF and smallest for Medicaid. One reason for these cross-program differences may be that recertification costs are highest for TANF and lowest for Medicaid. Recertification intervals are shortest for TANF and longest for Medicaid. TANF recipients may also receive less assistance than SNAP or Medicaid recipients, as TANF is designed to wean its recipients off of welfare. Conversely, those eligible for Medicaid may have more avenues for application assistance, including from healthcare providers who may have an incentive to enroll uninsured patients onto Medicaid. A second reason may be that TANF recipients tend to be more needy than Medicaid recipients (Appendix Table A1). If those who are more needy are less able to cope with administrative burdens, then the differences in enrollment effects could also be driven by cross-program differences in recipient composition. The appendix provides a theoretical description of the impacts of these channels on program-specific enrollment changes, showing how a common set of administrative burdens can yield different impacts across programs that vary in their enrollment costs or recipient composition.

To empirically test these hypotheses, program-specific enrollment changes are calculated for existing recipients of multiple programs (as of September 2007, the month prior to IBM rollout) and compared to the enrollment changes for existing recipients of each individual program. Similar enrollment effects across programs after holding fixed recipient composition would suggest that the original cross-program differences in enrollment effects are mainly a result of differences in recipient composition. Differences in enrollment effects among multiple-program recipients would point to differing enrollment costs playing a role in explaining the original cross-program differences in enrollment effects. Moreover, conditioning on those with receipt prior to treatment implicitly focuses on the exit margin.

have little to no income information and are not subject to work requirements). Appendix Figure A7 shows the dynamic effects on exit rates among TANF recipients in child-only cases and cases with an adult.

Figure 6 illustrates the effects of IBM automation on SNAP, TANF, and Medicaid enrollment for existing recipients of each individual program and of all three programs. Note that the vast majority (88%) of TANF recipients receive all three programs, compared to 13% of SNAP recipients and 9% of Medicaid recipients in treated counties. The patterns in enrollment effects remain remarkably similar regardless of whether one conditions or does not condition on multiple program receipt. Calculated over a post-treatment window of 3 years, TANF enrollment falls by 10.1% and 11.5%, SNAP enrollment falls by 4.5% and 4.2%, and Medicaid enrollment falls by a statistically insignificant 0.5% and 0.02% for all existing recipients and multiple-program recipients, respectively (Table 4). Focusing on existing recipients of both SNAP and Medicaid (who constitute 77% of all existing SNAP recipients and 55% of all existing Medicaid recipients), the results again show declines in SNAP and Medicaid enrollment of 4.2% and 0.4% that are very similar to the declines of 4.5% and 0.5% among all existing recipients of SNAP and Medicaid. These results suggest that differences across programs in recertification costs - rather than recipient composition - explain the bulk of the differences in enrollment effects across programs.

6 Who is Screened Out by IBM Automation?

To shed light on the welfare implications of the policy change, this section investigates what types of recipients are screened out by IBM's automated system. This first subsection analyzes how targeting efficiency (measured using proxies of individual need or well-being) changes as a result of IBM automation and distinguishes between the effects on those screened out at entry versus exit. The second subsection examines what types of counties experience the largest enrollment declines.

6.1 Effects on Targeting Efficiency

To study the individual-level targeting effects of IBM's automated system, the following specification estimates how average well-being among individual recipients changes in treated counties relative to untreated counties before and after treatment:

$$\bar{y}_{ct} = \mu_c + \lambda_t + \gamma D_{ct} + \beta X_{ct} + \varepsilon_{ct}, \tag{3}$$

where $\bar{y}_{ct}$ is the average value of a characteristic for recipients in county c and year-month t. Not only is this regression estimated separately for each program (SNAP, TANF, and Medicaid), but - for a given program - it is also estimated for all remaining recipients and separately for entrants and exiters.[52] Doing so allows for a novel examination of the degree to which targeting effects vary across application stages.

This section measures targeting using various indicators of ability or earnings capacity. The first set of outcomes encompasses various income measures linked from IRS tax records, corresponding to calendar year 2007 or earlier (thus uncontaminated by treatment). These measures include three continuous income measures: tax income in 2007, formal sector wages in 2007, and tax income averaged over 2005-2007. Tax income is defined as adjusted gross income from Form 1040 for filers and the sum of wages and retirement income from Forms W-2 and 1099-R for non-filers. Formal sector wages are defined as wages, tips, and other compensation (Box 1) from Form W-2.[53] We also examine a binary indicator for having earnings in 2007 (i.e., having wages reported on a 1040 or W-2 or self-employment income reported on a 1040 or 1099-MISC) and a binary indicator for having asset income in 2007 (i.e., receiving either a Form 1099-INT or Form 1099-DIV).

A variety of outcomes also come directly from the administrative program records. For SNAP and TANF, average benefit dollars per person can be thought of as a measure of need from the welfare agency's perspective (as higher-benefit recipients are typically considered to be needier as they have lower incomes and/or greater expenses). The length of one's

[52] When estimated over all recipients of a given program, the covariates include the mean shares of race/ethnicity, gender, and age groups among the entire population and county-months are weighted by the number of recipients in September 2007. When estimated over entrants, the covariates include the mean shares of race/ethnicity, gender, and age groups among non-recipients and county-months are weighted by the number of non-recipients in September 2007. When estimated over exiters, the covariates include the mean shares of race/ethnicity, gender, and age groups among recipients and county-months are weighted by the number of recipients in September 2007.

[53] To account for differences among assistance units in size and composition, an equivalence scale of the form $(A + PK)^F$ is applied, where A and K designate the number of adults and children in the assistance unit, respectively (Citro and Michael 1995). Following Meyer and Sullivan (2012), P and F are set to equal 0.7. Incomes are then re-scaled to be representative of an assistance unit with two adults and two children.

spell (namely the upcoming spell for entrants and the terminating spell for exiters) can also implicitly serve as a proxy for disadvantage. Program records also contain various demographic indicators for whether an assistance unit has an elderly member, a disabled member, a single parent, multiple parents, and - in the case of TANF - no adults. The administrative records for SNAP and TANF also contain information on a recipient's highest education level, which is converted into years of education.[54]

Another targeting outcome is a composite "deprivation index", constructed using predicted values from the SIPP based upon demographic and income variables in the administrative program records.[55] Higher values of the deprivation index signify greater need (and vice-versa). Whether or not a recipient is non-white may also shed light on racial inequities within the welfare application and recertification process (even if such an indicator is not necessarily an objective proxy for "disadvantage"). Finally, for Medicaid, an indicator for whether or not a recipient receives "home care" can provide a gauge of being in poor health.

Focusing on SNAP, Table 5a presents targeting estimates for all remaining recipients, entrants, and exiters. The coefficients in Columns 1, 5, and 9 correspond to γ in equation (3), calculated over a post-treatment window of 3 years. Over the cross section of recipients, IBM automation leads to slightly better targeting for 12 out of 13 outcomes, with estimates statistically significant at the 5% level for 10 outcomes. For example, those remaining on SNAP have 2.8% lower 3-year incomes, 0.8% fewer years of education, 1.5% higher benefit dollars per recipient, and 2.5% higher disability rates in the treated counties after treatment.

Yet, an interesting pattern appears when disaggregating targeting effects across entry and exit. IBM automation appears to be an effective screen at the entry stage for all 14 outcomes, as those entering SNAP in the treated counties tend to be less well-off than their untreated counterparts (with estimates statistically significant at the 5% level for 13 outcomes).[56] For example, those entering SNAP have 8.9% lower 3-year incomes, 2.0% fewer

[54] When examining targeting using income or education measures, the covariates in equation (4) additionally include the average age and age-squared of the assistance unit head (at the county- and month-level).

[55] This deprivation index is constructed based upon 32 measures of material hardships, home quality problems, food security issues, health problems, and lack of appliances taken from the topical modules in Wave 5 of the 2004 SIPP Panel and Wave 6 of the 2008 SIPP Panel. An advantage of looking at these measures is that they are relatively objective measures of material need (see, e.g., Meyer et al. 2021, Meyer, Wu, and Curran 2021). A disadvantage of using this deprivation index is that variation in the index tends to be small, as the R-squared from the prediction equation is about 0.1.

[56] Spell length is an additional outcome examined for entrants and exiters, even though it is not examined

years of education, 8.1% higher benefit dollars per recipient, 6.7% higher disability rates, and 12.4% longer spells in the treated counties after treatment. Conversely, IBM automation appears to be an ineffective screen at the exit stage for all 14 outcomes, as those exiting SNAP in the treated counties after treatment tend to be less well-off than their untreated counterparts (with estimates statistically significant at the 5% level for 12 outcomes). Those exiting SNAP have 7.9% lower 3-year incomes, 0.8% fewer years of education, 3.6% higher benefit dollars per recipient, 5.1% higher disability rates, and 9.3% longer spells in the treated counties. Exiters in the treated counties are also 3.5% more likely to be non-white.

Table 5b and Table 5c show similar patterns in targeting efficiency for TANF and Medicaid, although the estimates are noisier than those for SNAP (likely because there is less variation across characteristics for TANF recipients and smaller enrollment effects for Medicaid recipients). Moreover, the set of targeting outcomes examined differs slightly across programs, due either to a lack of data availability or relevance. For example, the Medicaid enrollment records do not contain information on recipients' education levels. For TANF, it also makes less sense to analyze differences in multiple parent or elderly status, given that the program is targeted mainly to non-elderly families with single or no parents.

For TANF, IBM automation again leads to better targeting overall for 10 out of 11 outcomes, with estimates statistically significant at the 5% level for 6 outcomes. Targeting efficiency improves at entry for 10 out of 12 outcomes with 2 outcomes being statistically significant, and worsens at exit for 9 out of 12 outcomes with 3 outcomes being statistically significant. Turning to Medicaid, IBM automation improves targeting among the stock of recipients for 8 out of 12 outcomes, with estimates being statistically significant at the 5% level for only 2 outcomes. Once again, targeting efficiency improves at entry for 8 out of 13 outcomes with 3 outcomes being statistically significant, and worsens at exit for 11 out of 13 outcomes with 7 outcomes being statistically significant.

Appendix Table A4 shows targeting estimates for a variety of supplemental outcomes, including current-year incomes, migration status since birth, and urban/rural status. The patterns in targeting efficiency, particularly comparing entrants versus exiters, remain largely consistent with those in Table 5a-Table 5c. Taken together, these results - suggesting that

for all remaining recipients.

the effects on targeting efficiency may differ across entry and exit - offer a potential explanation for conflicting results in prior empirical studies (particularly those focusing on SNAP). Finkelstein and Notowidigdo (2019), for example, find that SNAP application costs lead to improved targeting when studying individuals applying for the first time. Homonoff and Somerville (2021), on the other hand, find the opposite result when studying the effects of administrative burdens at SNAP recertification. Note that there is overlap in the targeting outcomes examined by the aforementioned papers and this paper, with Finkelstein and Notowidigdo (2019) analyzing benefit amounts and race and Homonoff and Somerville (2021) analyzing benefit amounts, spell length, and the presence of children.

These empirical results can be rationalized by a model in which administrative burdens screen out *both* the least and most needy individuals (see appendix for a theoretical outline). This assumption is consistent with taking the *union* of the neoclassical and behavioral models of targeting, in contrast to prior studies that often choose one theory over the other to explain their results. But due to differential selection of individuals into each stage, the least needy (who are less attached to welfare programs) are more likely to appear at initial application while the most needy (who are more attached to welfare programs) are more likely to appear at recertification. Analyzing the entry margin thus reveals an improvement in targeting as the least needy are disproportionately screened out, while analyzing the exit margin reveals a worsening in targeting as the most needy are disproportionately screened out. Ultimately, these non-monotonic effects and their nuanced implications would be masked by a focus on overall mean effects.

Finally, one may argue that the extent to which IBM's automated system is an effective screen at initial application depends on the relationship between screening out those who are "less needy" and reducing Type II errors (false positives) in the benefit award process. Consistent with the majority of prior studies and the perspective of a social planner, this paper analyzes targeting along a continuous spectrum of ability. Yet, from the perspective of a welfare agency, one can also view targeting in more binary terms based on whether or not an individual is truly eligible or ineligible for a program.

To shed some light on the latter perspective, Appendix Figure A8 uses administrative SNAP Quality Control microdata, which contain a random sample of active SNAP cases selected to be audited, to examine how SNAP error rates evolve over time.[57] Specifically, overpayment rates - defined as the share of SNAP cases deemed to receive erroneously high benefits (and of which Type II errors are a subset) - fall sharply in Indiana after 2008 compared to the U.S. This finding suggests that some of the less needy individuals who are screened out by IBM's automated system are likely to be truly ineligible, in line with an early goal of the system to reduce welfare fraud. What does this imply about the optimality of the administrative burdens associated with IBM's automated system? Kleven and Kopczuk (2011) posit that an optimally designed program may include a burdensome application process if such a process allocates sufficiently fewer benefits to ineligible individuals (fewer Type II errors), even if slightly more eligible individuals are rejected (more Type I errors). However, this paper finds that the large increase in Type I errors is concentrated among the more needy, suggesting some suboptimality associated with IBM's automated system.

6.2 Heterogeneous Effects by County-Level Characteristics

This subsection analyzes what types of counties experience the largest enrollment declines, using the following specification that extends the framework in equation (2):

$$\log(y_{ct}) = \mu_c + \lambda_t + \gamma D_{ct} + \sum_{j \in \mathcal{J}} \gamma^j D_{ct} A_c^j + \beta X_{ct} + \varepsilon_{ct}, \tag{4}$$

where y_{ct} denotes the total number of program recipients in county c and year-month t. D_{ct} is a dummy variable equaling one if county c receives treatment and month t is after treatment. A_c^j is a time-invariant dummy variable indicating whether or not county c has characteristic j (e.g., high-poverty, low-population), given a set of county-level characteristics $\mathcal{J}$.

The baseline effect of IBM's automated system on log enrollment is given by γ, and γ^j is the additional effect on enrollment for counties with a given characteristic j (holding constant the interactive effects of treatment with all other characteristics ℓ). $\mathcal{J}$ includes indicators for six characteristics: whether or not a county has an above-median unemployment rate, has

[57] Because the public-use SNAP Quality Control files contain only state identifiers, we can only compare trends between Indiana and the U.S. rather than trends between treated and untreated counties within Indiana.

an above-median poverty rate, has a below-median population size, has an above-median share of non-white individuals, is treated in Wave 2, and is treated in Wave 3. The first four characteristics are measured in September 2007 (prior to treatment). There is no need to include the A_c^j's as separate regressors, as they are subsumed by the county-fixed effects μ_c. X_{ct} is again a vector of county- and time-varying covariates.

Table 6 presents static regression estimates of the γ^j's in equation (4) for log SNAP, TANF, and Medicaid individuals, estimated using a post-treatment window of 3 years (Columns 1-3) and a post-treatment window of 6 years (Columns 4-6). Counties with higher poverty rates have additional declines in SNAP, TANF, and Medicaid enrollment of 8.3, 14.0, and 1.8 percentage points beyond the baseline effects, respectively, during the three years after treatment.[58] These marginal effects amount to 51%, 45%, and 31% of the baseline treatment effect, although the difference for Medicaid is not statistically significant. As a share of the baseline treatment effect, these interactive effects are even larger (86% for SNAP and TANF and 49% for Medicaid) using a post-treatment window of six years. Counties with smaller populations and more non-white individuals also tend to experience larger declines in program enrollment, although none of these differences are statistically significant.

In contrast, counties with higher unemployment rates experience smaller declines in SNAP, TANF, and Medicaid enrollment of 8.3, 2.5, and 4.8 percentage points relative to the baseline effects over the three years after treatment. These marginal effects correspond to 51%, 8%, and 82% of the baseline treatment effect, although the difference for TANF is not statistically significant. Over the six years after treatment, higher-unemployment counties experience enrollment effects that are 75%, 29%, and 117% smaller than baseline for SNAP, TANF, and Medicaid. While this result appears to qualitatively contrast with the patterns for the other characteristics (which show that more "disadvantaged" counties tend to see larger reductions in enrollment), it is plausibly a result of counties with higher unemployment having recipients with more income sources (who are more at risk of being affected by IBM automation since they have more information to verify).

Finally, even after controlling for interactions with other characteristics, counties treated in later waves have smaller reductions in program enrollment than those treated in Wave 1.

[58]Note that the proportional effects are higher because the dependent variable (enrollment) is in logs.

These differences are most pronounced for TANF and also statistically significant for SNAP. One reason for these differences is that counties treated earlier in calendar time experienced the termination of IBM's contract later in event time. The counties treated in Wave 1 were thus exposed to the burdens for longer than the counties treated in Waves 2 or 3. Another possible reason is that earlier-treated counties were less able to foresee (and therefore more overwhelmed by) the difficulties associated with IBM's automated system, while the counties treated later were better able to anticipate and prepare for IBM automation.

7 Robustness Checks

This section presents a series of robustness checks that validate the difference-in-differences design and therefore a causal interpretation of the effects of IBM's automated system on program enrollment. Specifically, the analyses suggest that treated counties would have had similar trends in outcomes as untreated counties in the absence of treatment. They also address other potential threats to identification and inference.

Validity of Parallel Trends Assumption. Appendix Figure A9 shows that the lack of pre-trends in SNAP, TANF, and Medicaid receipt persists even after extending the pre-treatment period from three years to five years. Yet, the existence of parallel trends between treated and untreated counties in the pre-treatment period does not guarantee that these trends would have persisted in the absence of treatment (Kahn-Lang and Lang 2018). For example, time-varying shocks that influence enrollment in welfare programs may affect treated and untreated counties differentially during the post-treatment period.

One example of such a shock is the Great Recession. Enrollment in welfare programs typically rises during recessions, and research has found evidence of spatial variation in Great Recession severity across local areas in the U.S. (see, e.g., Yagan 2019). Since treated counties in Indiana tend on average to be smaller, poorer, and more rural than untreated counties, one may be concerned that these counties experienced different changes in economic conditions than other counties after treatment. Yet, Appendix Figure A10 shows that treated and untreated counties have similar trends in overall unemployment rates and employment-to-population ratios, as well as employment shares in the two largest economic sectors in Indiana (manufacturing and wholesale/retail trade). A caveat with analyzing unemployment rates and employment shares is that these measures may follow different trends in treated and untreated counties as a direct result of treatment. However, behavioral responses on the part of treated individuals should not be large enough to noticeably change overall employment patterns, since those affected constitute a small minority of all working-age adults.

Appendix Table A5 shows estimates using a number of additional specifications to validate the robustness of the main difference-in-differences results. Columns 1-2 start by showing that the treatment effects on SNAP, TANF, and Medicaid enrollment are nearly identical when calculated using either the public- or restricted-use data sources. Relying on the public-use data, Columns 3-5 then show that the treatment effects on SNAP, TANF, and Medicaid enrollment are highly stable with respect to the inclusion of county- and time-varying covariates as well as the choice of covariates used.[59] Column 6 shows estimates that exclude the five most populous counties in Indiana outside of Marion and Lake counties (two of which are untreated) and find little to no differences compared to the main results on program enrollment. Column 7 shows estimates that exclude the 26 counties in Indiana severely impacted by floods in September 2008 (which could have led to increased disaster payments to affected areas) and again find small differences compared to the main results.

Appendix Table A6 shows placebo tests comparing enrollment changes between treated and untreated counties for a number of other government programs. The analyses focus on Social Security, SSI, Medicare, and free and reduced-price school meals (FARM), since these programs were not administered under IBM's automated system. The placebo estimates rely on annual county-level enrollment data for these programs spanning 2005-2011, with 2008 set as the calendar year corresponding to the first year of treatment.[60] The results show statistically insignificant effects of IBM's automated system on (logged) participation in each

[59] Estimates that control for no covariates beyond county- and month-fixed effects (Column 3) and estimates that control for log county population (Column 4) are similar to the preferred estimates that control for population and demographic covariates. Estimates remain largely stable after controlling for unemployment rates (Column 5), which importantly reflect local economic conditions but may not be pre-determined.

[60] Because county-level Medicare data are available only starting from 2007, there is only a single year covering the pre-treatment period for Medicare.

of the non-automated programs, with the exception of SSI. The magnitudes of the percentage declines are close to zero for Social Security and Medicare, while they are slightly larger (between 2-3%) for SSI and FARM. Note that the eligible populations for SSI and FARM overlap to some degree with those eligible for SNAP, TANF, and/or Medicaid. As a result, IBM's automated system may have also constrained applicants from receiving information about other programs that they may otherwise obtain from in-person caseworkers.

Robustness to Alternative Difference-in-Difference Estimators. A flurry of recent studies have identified issues associated with TWFE or event study specifications with variation in treatment timing (see, e.g., Callaway and Sant'Anna 2021, de Chaisemartin and D'Haultfoeuille 2020, Sun and Abraham 2021, Goodman-Bacon 2021, Borusyak et al. 2021). These issues include the potential for negative weights to arise on certain treatment effect parameters and for some earlier-treated units to be improperly used as control units for later-treated units. In light of these concerns, this subsection discusses comparisons of the main two-way fixed effects (TWFE) estimates to difference-in-differences estimates calculated using alternative methods proposed in recent studies.

Appendix Table A7 compares the main static TWFE estimates against two alternative estimators: a stacked difference-in-differences (DiD) estimator (see, e.g, Cengiz et al. 2019, Deshpande and Li 2019) and the estimator from Callaway and Sant'anna (2021) (CS). Columns 1 and 2 first show that the TWFE point estimates are virtually indistinguishable regardless of whether the panel is balanced on event time or calendar time. The stacked DiD estimates (in Column 3) are obtained by comparing each group of treated counties to the untreated counties and "stacking" these comparisons on top of one another. Because each unit is always treated or untreated in a given "stack", this approach circumvents issues with negative weights. The stacked DiD and TWFE estimates are very similar to each other (generally within 10% of each other), although the stacked DiD estimates are always larger in magnitude. Alternatively, the CS estimator - which relies on assumptions of parallel trends and limited treatment anticipation - robustly identifies the average treatment effect at time t among the units first treated at time g. Column 4 shows the overall CS treatment effects aggregated over t and g. The patterns continue to be very similar to the TWFE estimates in both sign and statistical significance.

To provide some context for these patterns, Appendix Table A8 uses the methodology from Goodman-Bacon (2021) to decompose the main TWFE estimate. Depending on the length of the post-treatment period, 82-87% of the TWFE estimate comes from comparisons of treated counties to never-treated counties and only 8-11% of the TWFE estimate comes from comparisons of treated units with different treatment times.[61] The small weight on the timing group comparisons makes sense given that the staggered IBM rollout occurs over a short time frame (7 months) relative to the overall time frame for our analyses (72 or 108 months). Moreover, the treatment effect from the treated vs. never-treated comparisons is always larger in magnitude than the main TWFE estimate. This can explain why the stacked DiD and CS estimates - which rely heavily on "clean" comparisons between treated and never-treated units - are usually larger in magnitude than the TWFE estimates.

Comparison to Other States. This final subsection discusses alternative estimates of treatment effects involving comparisons to other states. These estimates focus on SNAP enrollment, given that public-use data on county- and state-level program receipt for the entire U.S. are readily accessible for SNAP but not for TANF or Medicaid. The first alternative specification calculates the effects of IBM's automated system on SNAP enrollment in Indiana's treated counties, compared to SNAP enrollment both in Indiana's untreated counties and in border counties in neighboring states (Illinois, Kentucky, Ohio, and Michigan). Appendix Figure A11 shows that these estimates (comparing to untreated counties in Indiana and counties in neighboring states) are very similar to baseline estimates that only compare to untreated counties in Indiana. These similarities suggest that SNAP enrollment in Indiana's untreated counties follow similar trends as SNAP enrollment in neighboring states during the post-treatment period.

The second specification uses a synthetic control method to compare changes in SNAP enrollment in Indiana (the single treated state) to those of a "synthetic control" state constructed using a weighted combination of untreated states (see, e.g., Abadie et al. 2010, Cunningham and Shah 2018, Jones and Marinescu 2022).[62] The weights are calculated by

[61] The remaining 4-8% of the TWFE estimate comes from within-group variation as a result of covariates.
[62] This state-level analysis is carried out only for SNAP, as monthly state-level administrative TANF

matching all other states to Indiana on observable pre-treatment covariates.[63] The identification strategy assumes that the synthetic control group for Indiana yields outcome patterns that would accurately represent Indiana's outcomes in the absence of treatment.

Appendix Figure A12 shows the change in log SNAP cases over time for Indiana and for the "synthetic control" group. Four states collectively constitute 99% of the weight given to "synthetic Indiana": Ohio (29%), Utah (26%), Wisconsin (23%), and Kentucky (21%).[64] Notably, three of these states are in the same geographic region as Indiana, even though geographic proximity did not explicitly factor into the matching process. Panel (a) compares the trends in log SNAP enrollment between Indiana and synthetic Indiana, showing that the levels and trends in enrollment are very similar prior to October 2007. However, the trends diverge after Indiana received IBM's automated system, with enrollment growing much faster in synthetic Indiana as a result of the Great Recession. Panel (b) plots the difference in log enrollment between Indiana and synthetic Indiana, showing a gap of approximately 10% at the height of the enrollment decline that only incompletely bounces back in subsequent years. One should expect the synthetic control estimates to be somewhat smaller in magnitude than the county-level difference-in-differences estimates, since the former estimates are based on state-level changes in SNAP enrollment and only a fraction of Indiana's population is treated. These cross-state estimates consequently provide a useful benchmark that further validates the cross-county difference-in-differences estimates.

records are of questionable quality and monthly state-level administrative Medicaid records are not easily obtainable for earlier years from a single national source.

[63] Specifically, the weights are constructed by matching on the following state- and time-varying covariates: the number of SNAP recipients for each of the 12 quarters preceding treatment, the numbers of males and females (logged), the numbers of white, black, and other race individuals (logged), the numbers of individuals aged 0-4, 5-17, 18-24, 25-44, 45-64, and 65 plus (logged), median income (logged), unemployment rate, and a policy index taking values between 0 and 3 for the number of other selected policy changes (simplified reporting, broad-based categorical eligibility, and the exclusion of vehicles from the asset test) adopted by a state during a particular month.

[64] Alabama, Mississippi, Louisiana, and Florida are omitted from the donor pool of states for the synthetic control group, as they experienced abnormally large increases in SNAP enrollment in 2005 as a result of Hurricane Katrina. Given that at least one of these states received non-trivial weights via the synthetic control method, including them in the donor pool led to non-parallel trends in enrollment prior to treatment.

8 Conclusions

In 2006, Indiana awarded a 10-year, \$1.3 billion contract to the IBM Corporation to automate caseworker assistance for the state's welfare services. Expected to lower administrative costs and increase convenience for clients and operators alike, IBM's automated system began rolling out to counties in late 2007. However, due to performance problems, the system was terminated two years later after reaching only 59 out of Indiana's 92 counties. Leveraging the natural experiment in this setting that distinguishes treated counties receiving the rollout to untreated counties not receiving the rollout, this paper develops a framework to compare the effects of barriers to enrollment on take-up and targeting among initial applicants and recertifiers. This paper concludes that IBM's automated system created a number of burdens associated with application and recertification, leading to sharp declines in SNAP, TANF, and Medicaid enrollment in the treated counties.

Using administrative welfare records covering nearly 3 million recipients, this paper finds statistically significant declines in TANF (24%), SNAP (15%), and Medicaid (4%) enrollments one year after the rollout of IBM's automated system. These effects can largely be attributed to insufficient personalized assistance from caseworkers, lower tolerance for application and recertifications errors, and staggering delays and technical glitches at overwhelmed call centers. Disaggregating the overall enrollment effects across application stages, this paper finds decreases in entry rates for all three programs that are similar in magnitude and increases in exit rates that are largest for TANF and smallest (and statistically insignificant) for Medicaid. The larger effects on exit rates, and thus overall enrollment, for TANF can be attributed to higher transaction costs stemming from shorter TANF recertification intervals and fewer avenues for assistance (vice-versa for Medicaid).

Linking these program participation records to IRS microdata enables a full analysis of targeting effects. Overall, IBM's automated system screens out individuals who appear less needy, as those remaining on the program rolls typically have lower pre-treatment incomes, fewer years of education, higher per-person benefits, and higher disability levels. Yet, these overall effects conceal striking and novel differences across application stages, with more needy individuals screened out at exit and less needy individuals screened out at entry. These differences are likely due to both the least and most needy individuals, who appear at different application stages, being disproportionately screened out. At the county level, proportional enrollment reductions are also largest in earlier-treated and higher-poverty counties, as well as in lower-unemployment counties whose residents may have more earnings to verify.

The natural experiment setting and groundbreaking data sources in this paper can be further used to examine the downstream outcomes and well-being of those individuals cut off from welfare programs. This includes examining whether or not welfare cuts induce individuals to increase their short-run labor supply and potentially induce greater self-sufficiency in the long run. One can also examine the extent to which individuals turn to other forms of insurance (e.g., from other government programs) as a result of sharp negative income shocks. Changes in earnings and other transfer income can serve as mediators in helping to understand the broader effects of welfare cuts on financial solvency, health, and education. These analyses can provide more direct evidence on the mechanisms underlying the long-term effects on SNAP and Medicaid enrollment - identified in this paper - that persist beyond the life of IBM's automated system.

States and localities are increasingly automating the role of caseworkers and shifting towards remote determinations of eligibility, a trend that has accelerated during the COVID-19 pandemic. While these changes are often thought to make the enrollment and recertification processes more convenient, they may also induce complexities that are less well understood. As more governments engage in policy experimentation, it is important to understand the combination of intended and unintended consequences that may result from such efforts (e.g., Callander and Harstad 2015). This paper shows that the lack of human contact with caseworkers can have different effects at different stages depending on the types of clients appearing at a given stage. As a result, "one size fits all policies" may not be optimal when applied across different stages of the program certification process. Policymakers would be well-advised to adapt procedures differentially across stages to whom they intend to target.

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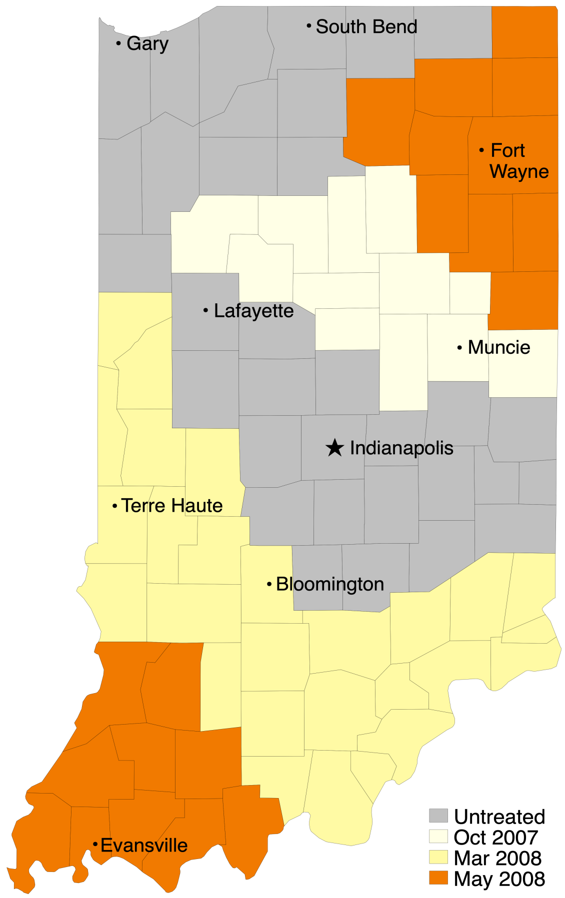
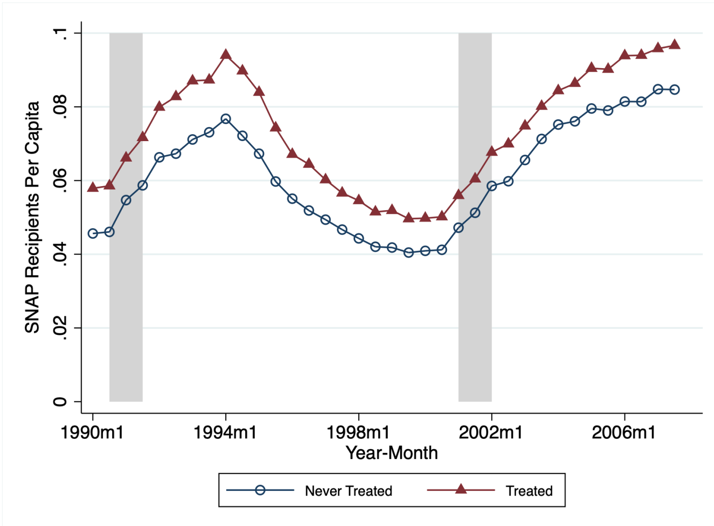
Figures and Tables

Figure 1. Rollout of IBM's Automated System Across Counties

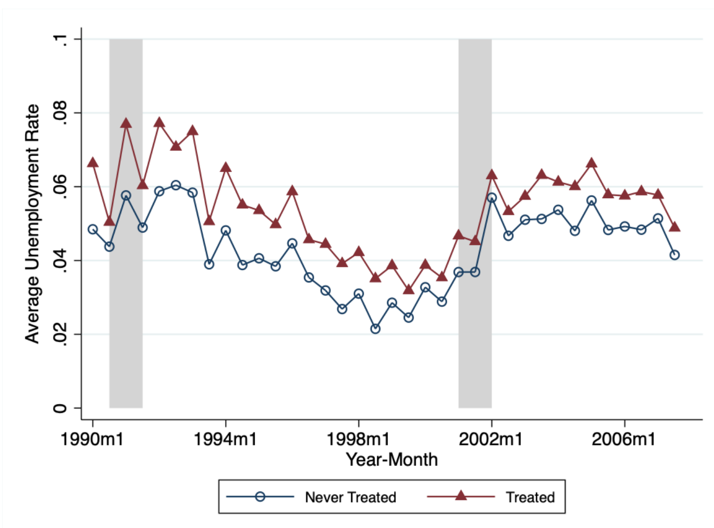


Notes: This map illustrates the rollout of IBM's automated system across Indiana's counties. Counties in light yellow received the first wave of automation treatment (in October 2007), counties in dark yellow received the second wave of automation treatment (in March 2008), and counties in dark orange received the third wave of automation treatment (in May 2008). Counties in gray never received the automation treatment.

Figure 2. Trends in Pre-Treatment SNAP Enrollment and Unemployment Rates



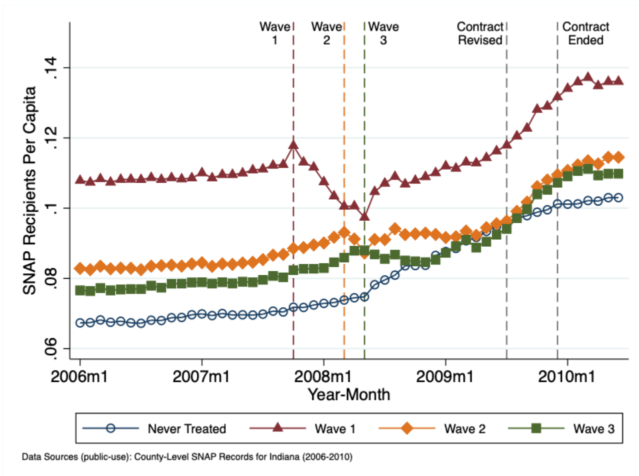
(a) SNAP Enrollment Rates



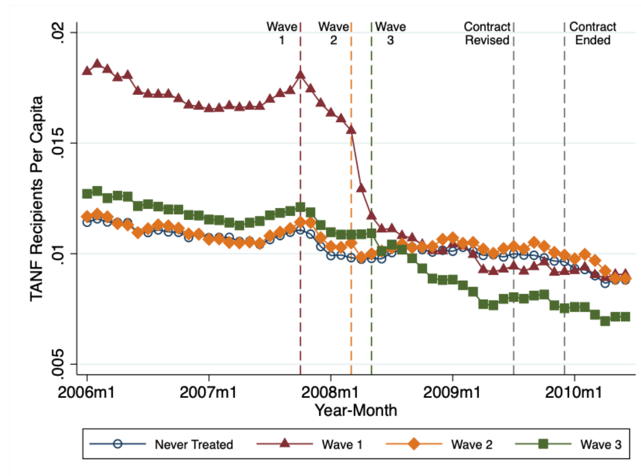
(b) Unemployment Rates

Data Sources: USDA SNAP county-level enrollment data (1990-2007), BLS Local Area Unemployment Statistics (1990-2007)
Notes: These figures show raw trends in SNAP receipt rates and unemployment rates for counties exposed to the automated system and counties never automated by IBM (excluding Marion and Lake counties). The rate for each group is calculated as the average rate (weighted by county population in September 2007) across each of the group's counties. Rates are plotted for January and July of every year, and recession periods are shaded in gray.

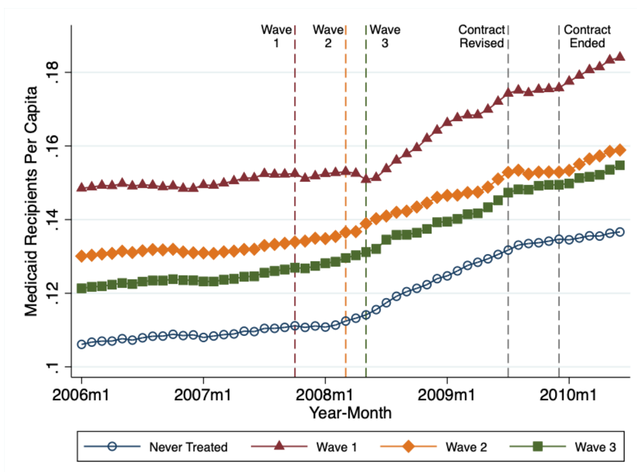
Figure 3. Raw Trends in Welfare Receipt Rates



(a) SNAP



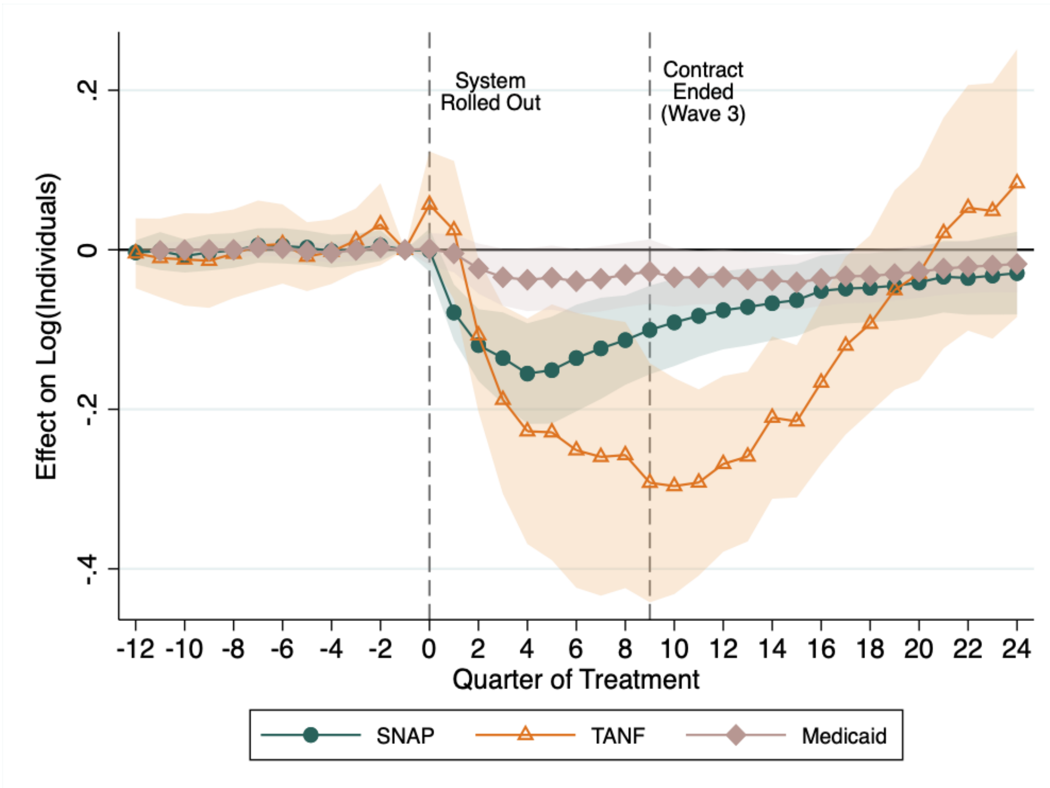
(b) TANF



(c) Medicaid

Data Sources (public-use): County-level SNAP, TANF, and Medicaid program records (2006-2010), Census population estimates (2006-2010)
Notes: These figures show raw trends in monthly receipt rates for SNAP, TANF, and Medicaid - calculated as the share of the population receiving each program - for counties exposed to the automated system (categorized by rollout wave) and counties never automated by IBM. The receipt rate for each group is calculated as the average of receipt rates (weighted by county population in September 2007) across each of the group's counties. In each of the panels, the dashed vertical lines correspond to the timing of major events, including the rollout of the automated system in Waves 1, 2, and 3, the undertaking by IBM of the "Corrective Action Plan" (in July 2009), and the termination of the IBM contract (in December 2009).

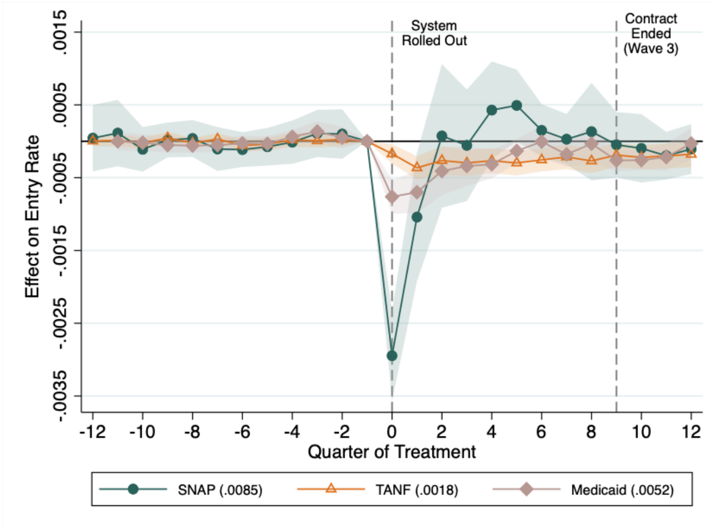
Figure 4. Dynamic Treatment Effects on Overall Enrollment



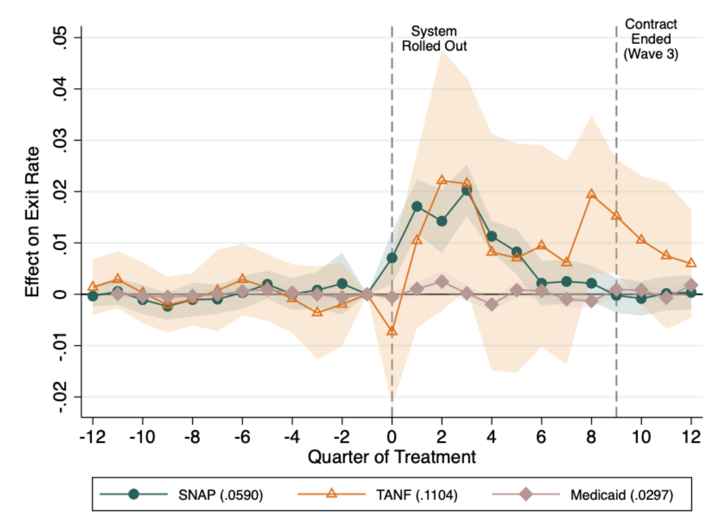
Data Sources: Administrative SNAP, TANF, and Medicaid records for Indiana (2004-2014), Census population estimates (2004-2014)

Notes: These figures show regression estimates of log total individuals on binary indicators corresponding to the event-quarter relative to receiving IBM automation, controlling for county- and month-fixed effects as well as county- and year-varying covariates that include log total population and population subgroups by race, gender, and age. Observations are at the county-month level and are weighted by the county-specific enrollment volume in September 2007 (month prior to initial IBM rollout). Marion and Lake counties (which are untreated) are excluded from the sample given that they are outliers on a number of dimensions. Standard errors are clustered at the county level. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization number: CBDRB-FY2021-CES005-021.

Figure 5. Dynamic Treatment Effects on Monthly Entry and Exit Rates



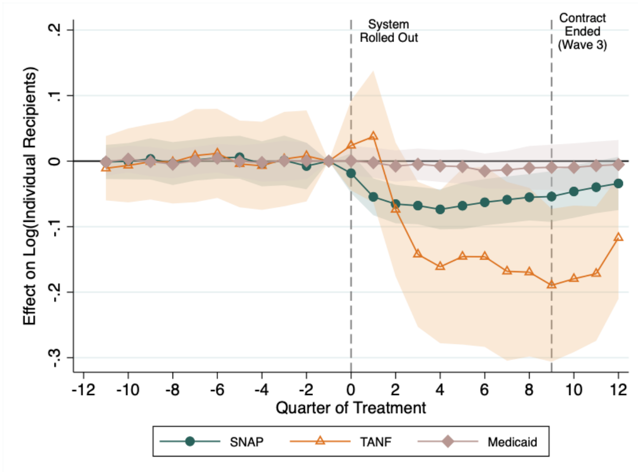
(a) Entry Rates



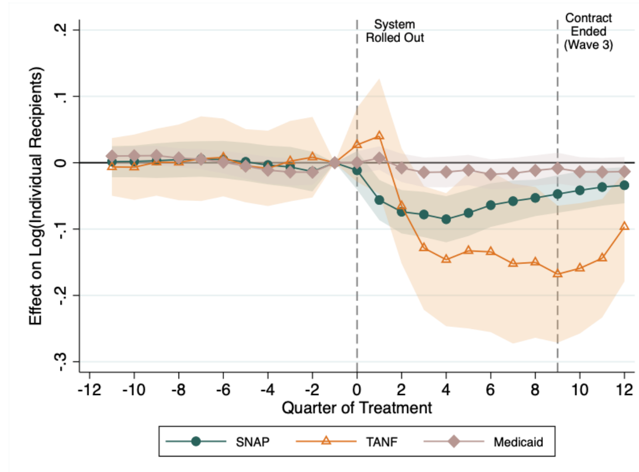
(b) Exit Rates

Data Sources: Administrative SNAP, TANF, and Medicaid records for Indiana (2004-2014), Census population estimates (2004-2014)
Notes: These figures show regression estimates of monthly entry and exit rates on binary indicators corresponding to the event-quarter relative to receiving IBM automation, controlling for county- and month-fixed effects as well as a set of county- and year-varying covariates. Regression estimates of entry rates control for the mean shares of race/gender/age groups among non-recipients and weight counties by the number of non-recipients in September 2007. Regression estimates of exit rates control for the mean shares of race/gender/age groups among recipients and weight counties by the number of recipients in September 2007. Marion and Lake counties (which are untreated) are excluded from the sample given that they are outliers on a number of dimensions. Standard errors are clustered at the county level. Baseline entry and exit rates for treated counties (in September 2007) are reported in parentheses in the legend. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization number: CBDRB-FY2021-CES005-021.

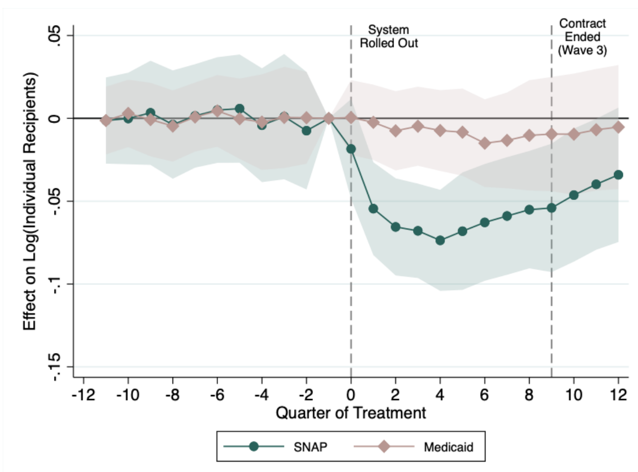
Figure 6. Dynamic Treatment Effects on Enrollment for Existing Recipients



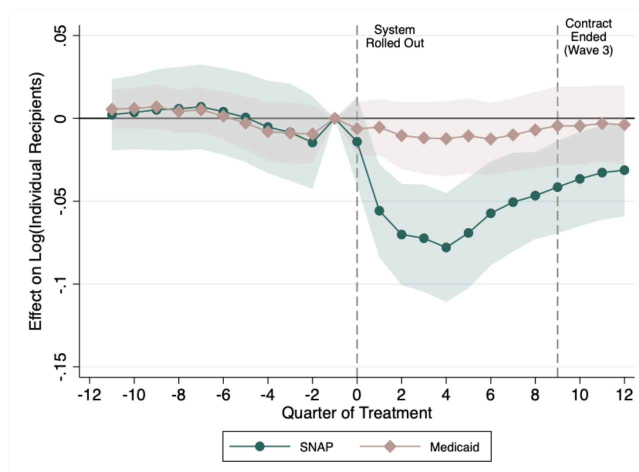
(a) Recipients of Each Individual Program



(b) Recipients of All Three Programs



(c) Recipients of Each Individual Program



(d) Recipients of Both Programs

Data Sources: Administrative SNAP, TANF, and Medicaid records for Indiana (2005-2011), Census population estimates (2004-2014)
Notes: These figures show regression estimates of log total individuals receiving SNAP, TANF, or Medicaid (conditional on receiving some or all of these programs in September 2007) on binary indicators corresponding to the event-quarter relative to receiving IBM automation and being in the post-treatment period, controlling for county- and month-fixed effects as well as a series of county- and year-varying covariates that include log total population and population subgroups by race, gender, and age. Observations are at the county-month level and are weighted by the number of individuals receiving any program in September 2007. Marion and Lake counties (which are untreated) are excluded from the sample given that they are outliers on a number of dimensions. Panels A and C shows estimates conditional on receiving each individual program (regardless of multiple program receipt) in September 2007, Panel B shows estimates conditional on receiving all three programs together (SNAP, TANF, and Medicaid) in September 2007, and Panel D shows estimates conditional on receiving SNAP and Medicaid together in September 2007. Standard errors are clustered at the county level. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization number: CBDRB-FY2021-CES005-021.

Table 1. Characteristics of Untreated and Treated Counties (2007)

Characteristics	Untreated Counties		Treated Counties			
	All	Less Pop.	All	Wave 1	Wave 2	Wave 3
	(1)	(2)	(3)	(4)	(5)	(6)
Welfare Receipt						
SNAP (%)	9.6	7.0	9.0	11.2	8.7	8.0
TANF (%)	2.0	1.1	1.3	1.7	1.1	1.2
Medicaid (%)	13.8	11.1	13.5	15.2	13.4	12.6
Population						
Rural (%)	20.2	31.2	40.4	35.1	49.3	35.7
White (%)	83.6	92.2	92.8	92.2	94.5	91.7
Black (%)	12.7	4.2	4.4	5.3	2.8	5.4
Hispanic (%)	7.2	5.1	3.0	3.1	2.0	3.9
Ages 0-17 (%)	26.0	26.2	24.2	23.2	23.1	25.7
Ages 18-64 (%)	62.2	61.8	62.3	61.8	63.8	61.2
Ages 65+ (%)	11.8	12.0	13.5	15.0	13.1	13.1
Employment and Industry						
Manufacturing (%)	17.4	20.5	21.9	21.0	19.1	24.8
Construction (%)	5.9	5.7	4.8	4.1	4.7	5.3
Wholesale and Retail Trade (%)	15.8	16.4	14.7	15.2	14.0	15.1
Profess. and Adm. Services (%)	15.9	13.9	11.2	10.8	9.9	12.6
Education &and Health (%)	19.9	19.6	19.3	24.0	18.0	18.0
Entertainment and Hospitality (%)	10.4	10.2	9.4	10.3	9.1	9.1
Public Administration (%)	4.7	4.4	5.3	6.4	6.4	3.8
Unemployment Rate (%)	4.0	3.8	4.4	5.3	4.2	4.1
Income and Poverty						
Median Household Income ($)	51,403	54,624	45,186	42,507	43,851	47,739
Poverty Rate (%)	12.1	10.1	12.7	13.8	14.2	10.9
Average County Population	109,413	72,053	46,932	51,050	36,806	58,131
Number of Counties	33	31	59	12	27	20

Data Sources (public-use): County-level program records (2007), Census population and SAIPE estimates (2007), BLS Local Area Unemployment Statistics (2007), BLS Quarterly Census of Empl. and Wages (2007)
Notes: This table shows average pre-treatment demographic and economic characteristics of counties in Indiana classified based on whether they are treated (i.e., received IBM's automated system) or untreated. Column 1 shows averages for all untreated counties, and Column 2 shows averages for a subset of the untreated counties that excludes the two largest counties (Marion and Lake counties). Column 3 shows averages for all treated counties, and Columns 4-6 divide treated counties based on the wave during which they receive treatment. Characteristics measured at the monthly level (i.e., SNAP, TANF, Medicaid, and unemployment rates) correspond to September 2007 values, while characteristics measured at the annual level (all other variables) correspond to 2007. All county-level averages (with the exception of total population) are weighted by county population in 2007.

Table 2. Treatment Effects on Overall Program Enrollment

Outcomes	Post-Treatment Window: 3 Years		Post-Treatment Window: 6 Years	
	Point Estimate	Standard Error	Point Estimate	Standard Error
	(1)	(2)	(3)	(4)
SNAP				
Log Individuals	-0.1014***	(0.0261)	-0.0745***	(0.0212)
Log Cases	-0.1034***	(0.0234)	-0.0800***	(0.0181)
Log Dollars	-0.0880***	(0.0272)	-0.0646***	(0.0212)
TANF				
All Recipients				
Log Individuals	-0.1895***	(0.0532)	-0.1321**	(0.0546)
Log Cases	-0.1745***	(0.0445)	-0.1053**	(0.0485)
Log Dollars	-0.1502***	(0.0483)	-0.0853*	(0.0512)
Child-Only Cases				
Log Individuals	-0.1481***	(0.0512)	-0.0747	(0.0616)
Log Cases	-0.1222***	(0.0456)	-0.0526	(0.0541)
Log Dollars	-0.1063**	(0.0469)	-0.0326	(0.0555)
Cases with Adult				
Log Individuals	-0.2119***	(0.0622)	-0.2085***	(0.0610)
Log Cases	-0.2129***	(0.0526)	-0.1985***	(0.0550)
Log Dollars	-0.1741***	(0.0576)	-0.1712***	(0.0616)
Medicaid				
Log Individuals	-0.0281*	(0.0153)	-0.0271*	(0.0140)
Log Cases	-0.0318***	(0.0113)	-0.0334***	(0.0108)
County-Months	7,200		10,500	

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

Data Sources: Administrative SNAP, TANF, and Medicaid records for Indiana (2004-2014), Census population estimates (2004-2014)

Notes: This table shows regression estimates of various log enrollment measures (total cases, individuals, or benefit dollars) on a binary indicator for receiving IBM automation and being in the post-treatment period, controlling for county- and month-fixed effects as well as county- and year-varying covariates that include log total population and population subgroups by race, gender, and age. Observations are at the county-month level and are weighted by the county-specific enrollment volume in September 2007. Marion and Lake counties (which are untreated) are excluded from the sample given that they are outliers on a number of dimensions. All regressions use a pre-treatment window of 12 quarters before automation. Columns 1 and 2 show regression estimates using a post-treatment window of 12 quarters after automation, and Columns 3 and 4 show regression estimates using a post-treatment window of 24 quarters after automation. Standard errors are clustered at the county level. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization number: CBDRB-FY2021-CES005-021.

Table 3. Treatment Effects on Monthly Entry and Exit Rates

Outcomes	Treatment Effects		Pre-Treatment Mean	Ratio of
	Point Estimate	Standard Error	for Treated Counties	(1) to (3)
	(1)	(2)	(3)	(4)
SNAP				
Entry Rate	-0.00055***	(0.00018)	0.0085	-0.0642
Exit Rate	0.0074***	(0.0014)	0.0590	0.1249
TANF				
All Recipients				
Entry Rate	-0.00029***	(0.00005)	0.0018	-0.1625
Exit Rate	0.0105*	(0.0061)	0.1104	0.0408
Child-Only Cases				
Entry Rate	-0.00005***	(0.00001)	0.0003	-0.1446
Exit Rate	-0.0089	(0.0074)	0.0870	-0.1019
Cases with Adult				
Entry Rate	-0.00026***	(0.00005)	0.0015	-0.1804
Exit Rate	0.0175**	(0.0072)	0.1211	0.1443
Medicaid				
Entry Rate	-0.00036***	(0.00006)	0.00522	-0.0697
Exit Rate	0.0004	(0.0004)	0.02968	0.0143
County-Months		7,200		

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

Data Sources: Administrative SNAP, TANF, and Medicaid records for Indiana (2004-2011), Census population estimates (2004-2014)

Notes: This table shows regression estimates of monthly entry and exit rates on a binary indicator for receiving IBM automation and being in the post-treatment period, controlling for county- and month-fixed effects as well as a set of county- and year-varying covariates. The entry rate in month t is defined as the number of recipients entering between month t and month $t+1$ as a share of relevant non-recipients (total population in month $t+1$ minus recipients in month t). The exit rate in month t is defined as the number of recipients exiting between month t and month $t+1$ as a share of recipients in month t. Observations are at the county-month level. Regression estimates when entry rate is the outcome control for the mean shares of race/gender/age groups among non-recipients and weight counties by their number of non-recipients in September 2007. Regression estimates when exit rate is the outcome control for the mean shares of race/gender/age groups among recipients and weight counties by their number of recipients in September 2007. Marion and Lake counties (which are untreated) are excluded from the sample given that they are outliers on a number of dimensions. Columns 1 and 2 show the main treatment effects (with regressions using a pre-treatment window of 12 quarters before automation and a post-treatment window of 12 quarters after automation). Column 3 shows the baseline average (weighted) entry and exit rates for treated counties, measured prior to treatment (September 2007). Standard errors are clustered at the county level. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization number: CBDRB-FY2021-CES005-021.

Table 4. Treatment Effects on Enrollment Among Existing Recipients

Outcomes	Treatment Effects: Point Estimate (1)	Treatment Effects: Standard Error (2)	Total Treated Recipients (Sep. '07) (3)
	A: Recipients of Each Individual Program		
Log SNAP Enrollment	-0.0418***	(0.0153)	251,000
Log TANF Enrollment	-0.1147***	(0.0385)	36,000
Log Medicaid Enrollment	-0.0002	(0.0126)	352,000
	B: Recipients of SNAP, TANF, and Medicaid		
Log SNAP Enrollment	-0.0451***	(0.0134)	
Log TANF Enrollment	-0.1007***	(0.0359)	31,500
Log Medicaid Enrollment	-0.0047	(0.0085)	
	C: Recipients of SNAP and Medicaid		
Log SNAP Enrollment	-0.0420***	(0.0128)	194,000
Log Medicaid Enrollment	-0.0035	(0.0087)	
County-Months		7,200	

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

Data Sources: Administrative SNAP, TANF, and Medicaid records for Indiana (2005-2011), Census population estimates (2004-2014)

Notes: This table shows regression estimates of log total individuals receiving SNAP, TANF, or Medicaid (conditional on receiving some or all of these programs in September 2007) on a binary indicator for receiving IBM automation and being in the post-treatment period, controlling for county- and month-fixed effects as well as a series of county- and year-varying covariates that include log total population and population subgroups by race, gender, and age. Observations are at the county-month level and are weighted by the number of individuals receiving any program in September 2007. Marion and Lake counties (which are untreated) are excluded from the sample given that they are outliers on a number of dimensions. Panel A shows estimates conditional on receiving each individual program (regardless of multiple program receipt) in September 2007, Panel B shows estimates conditional on receiving all three programs together (SNAP, TANF, and Medicaid) in September 2007, and Panel C shows estimates conditional on receiving SNAP and Medicaid together in September 2007. All regressions use a pre-treatment window of 12 quarters before automation and a post-treatment window of 12 quarters after automation. Column 3 shows the number of unique individuals underlying the samples for each panel in treated counties. Standard errors are clustered at the county level. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization number: CBDRB-FY2021-CES005-021.

Table 5a. Treatment Effects on the Characteristics of SNAP Recipients (Targeting)

Outcomes	All Remaining Recipients				Entrants				Exiters			
	Point Estimate	Std. Error	Baseline Mean		Point Estimate	Std. Error	Baseline Mean		Point Estimate	Std. Error	Baseline Mean	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Log Benefit $s/Person	0.0149***	(0.0034)	103	+	0.0814***	(0.0097)	90	+	0.0355***	(0.0050)	100	−
Spell Length (mos.)					1.8740***	(0.2311)	15.1	+	1.1250***	(0.1369)	12.1	−
Log 3-Year Tax Income[1]	-0.0277**	(0.0135)	38,620	+	-0.0892***	(0.0170)	65,150	+	-0.0789***	(0.0133)	58,870	−
Log Tax Income[1]	-0.0335**	(0.0153)	14,900	+	-0.0915***	(0.0201)	25,200	+	-0.0807***	(0.0153)	24,540	−
Log Wages (W-2)[1]	-0.0444***	(0.0152)	6,735	+	-0.1112***	(0.0191)	12,160	+	-0.1017***	(0.0162)	12,310	−
Has Earnings[1]	-0.0067**	(0.0028)	0.794	+	-0.0138***	(0.0036)	0.869	+	-0.0109***	(0.0023)	0.850	−
Has Asset Income[1]	-0.0007	(0.0012)	0.074	+	-0.0072***	(0.0019)	0.101	+	-0.0046***	(0.0018)	0.086	−
Years of Education	-0.0871***	(0.0268)	10.9	+	-0.2259***	(0.0426)	11.1	+	-0.0918***	(0.0269)	11.1	−
Has Elderly Member	0.0061***	(0.0016)	0.105	+	0.0046***	(0.0014)	0.033	+	0.0030*	(0.0016)	0.046	−
Has Disabled Member	0.0078***	(0.0027)	0.316	+	0.0114***	(0.0027)	0.169	+	0.0107***	(0.0023)	0.211	−
Single Parent	0.0076**	(0.0033)	0.328	+	0.0254***	(0.0051)	0.337	+	0.0162***	(0.0044)	0.333	−
Multiple Parents	-0.0075***	(0.0027)	0.161	+	-0.0108***	(0.0035)	0.188	+	-0.0044	(0.0028)	0.197	−
Deprivation Index (SIPP)	-0.0002	(0.0002)	0.195	−	0.0015***	(0.0003)	0.191	+	0.0023***	(0.0003)	0.185	−
Non-White	0.0035	(0.0046)	0.178	+	0.0050	(0.0038)	0.168	+	0.0061**	(0.0028)	0.173	−
County-Months		7,200				7,200				7,200		

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

[1] All tax income measures are calculated for 2007 (the year before treatment was fully rolled out), and 3-year tax income is calculated for 2005-2007

Data Sources: Administrative SNAP records for Indiana (2004-2014), Census population estimates (2004-2014), IRS Forms 1040, W-2, and 1099-R (2005-2007), Survey of Income and Program Participation (2004 Panel, Wave 5 and 2008 Panel, Waves 6 and 9)

Notes: This table shows regression estimates of average SNAP recipient characteristics on a binary indicator for receiving IBM automation and being in the post-treatment period, controlling for county- and month-fixed effects as well as a set of county- and year-varying covariates. Observations are at the county-month level. Regression estimates for all remaining recipients (Columns 1-3) control for the mean shares of race/gender/age groups for the entire population and weight counties by the number of recipients in September 2007. Regression estimates for entrants control for the mean shares of race/gender/age groups among non-recipients and weight counties by the number of non-recipients in September 2007. Regression estimates for exiters control for the mean shares of race/gender/age groups among recipients and weight counties by their number of recipients in September 2007. The continuous income outcomes are equivalized using the NAS equivalence scale to reflect the number of children and adults in the assistance unit. For income/education outcomes, regression estimates additionally control for average age and age-squared of the case head. Marion and Lake counties (which are untreated) are excluded from the sample given that they are outliers on a number of dimensions. Columns 1-2, 5-6, and 9-10 show the main treatment effects for all remaining recipients, entrants, and exiters, respectively (with regressions using a pre-treatment window of 12 quarters before automation and a post-treatment window of 12 quarters). Standard errors are clustered at the county level. Columns 3, 7, and 11 show the (weighted) baseline means for each outcome among all recipients, entrants, and exiters (respectively) in treated counties in September 2007. Columns 4, 8, and 12 show indicators for whether IBM automation improves (+) or worsens (−) targeting efficiency. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization number: CBDRB-FY2021-CES005-021.

Table 5b. Treatment Effects on the Characteristics of TANF Recipients (Targeting)

	All Remaining Recipients				Entrants				Exiters			
Outcomes	Point Estimate	Std. Error	Baseline Mean		Point Estimate	Std. Error	Baseline Mean		Point Estimate	Std. Error	Baseline Mean	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Log Benefit $s/Person	0.0421***	(0.0060)	97	+	0.1537***	(0.0123)	107	+	0.0327***	(0.0077)	93	−
Spell Length (mos.)					0.1300	(0.2103)	7.9	+	0.7540***	(0.2251)	8.7	−
Log 3-Year Tax Income[1]	-0.0443	(0.0384)	21,230	+	-0.0598	(0.0430)	33,370	+	-0.0249	(0.0253)	24,780	−
Log Tax Income[1]	-0.0474	(0.0344)	6,368	+	-0.0679	(0.0436)	10,030	+	-0.0411	(0.0347)	8,369	−
Log Wages (W-2)[1]	-0.0738*	(0.0400)	3,243	+	-0.0645	(0.0534)	6,093	+	-0.0461	(0.0308)	4,941	−
Has Earnings[1]	-0.0294***	(0.0101)	0.531	+	-0.0170	(0.0125)	0.731	+	-0.0052	(0.0054)	0.618	−
Has Asset Income[1]	-0.0011	(0.0018)	0.040	+	-0.0014	(0.0035)	0.062	+	0.0011	(0.0021)	0.049	+
Years of Education	-0.3366**	(0.1526)	11.1	+	-0.2402**	(0.1184)	11.3	+	-0.0686**	(0.0315)	11.1	−
Has Disabled Member	0.0040***	(0.0010)	0.024	+	-0.0010	(0.0012)	0.018	−	0.0011	(0.0018)	0.037	−
No Adults	0.0267***	(0.0095)	0.416	+	0.0037	(0.0080)	0.224	+	-0.0207***	(0.0080)	0.375	+
Deprivation Index (SIPP)	-0.0010**	(0.0004)	0.193	−	-0.0008	(0.0005)	0.197	−	-0.0002	(0.0003)	0.187	+
Non-White	0.0117	(0.0084)	0.271	+	0.0082	(0.0057)	0.206	+	0.0008	(0.0028)	0.266	−
County-Months		7,200				7,200				7,200		

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

[1] All tax income measures are calculated for 2007 (the year before treatment was fully rolled out), and 3-year tax income is calculated for 2005-2007

Data Sources: Administrative SNAP records for Indiana (2004-2014), Census population estimates (2004-2014), IRS Forms 1040, W-2, and 1099-R (2005-2007), Survey of Income and Program Participation (2004 Panel, Wave 5 and 2008 Panel, Waves 6 and 9)

Notes: This table shows regression estimates of average TANF recipient characteristics on a binary indicator for receiving IBM automation and being in the post-treatment period, controlling for county- and month-fixed effects as well as a set of county- and year-varying covariates. Observations are at the county-month level. Regression estimates for all remaining recipients (Columns 1-3) control for the mean shares of race/gender/age groups for the entire population and weight counties by the number of recipients in September 2007. Regression estimates for entrants control for the mean shares of race/gender/age groups among non-recipients and weight counties by the number of non-recipients in September 2007. Regression estimates for exiters control for the mean shares of race/gender/age groups among recipients and weight counties by their number of recipients in September 2007. The continuous income outcomes are equivalized using the NAS equivalence scale to reflect the number of children and adults in the assistance unit. For income/education outcomes, regression estimates additionally control for average age and age-squared of the case head. Marion and Lake counties (which are untreated) are excluded from the sample given that they are outliers on a number of dimensions. Columns 1-2, 5-6, and 9-10 show the main treatment effects for all remaining recipients, entrants, and exiters, respectively (with regressions using a pre-treatment window of 12 quarters before automation and a post-treatment window of 12 quarters). Standard errors are clustered at the county level. Columns 3, 7, and 11 show the (weighted) baseline means for each outcome among all recipients, entrants, and exiters (respectively) in treated counties in September 2007. Columns 4, 8, and 12 show indicators for whether IBM automation improves (+) or worsens (−) targeting efficiency. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization number: CBDRB-FY2021-CES005-021.

Table 5c. Treatment Effects on the Characteristics of Medicaid Recipients (Targeting)

Outcomes	All Remaining Recipients				Entrants				Exiters			
	Point Estimate	Std. Error	Baseline Mean		Point Estimate	Std. Error	Baseline Mean		Point Estimate	Std. Error	Baseline Mean	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Spell Length (mos.)					1.7710***	(0.3027)	31.2	+	1.3430***	(0.2426)	20.1	–
Log 3-Year Tax Income[1]	-0.0171	(0.0133)	21,520	+	-0.0128	(0.0161)	40,540	+	-0.0344**	(0.0155)	25,200	–
Log Tax Income[1]	-0.0241*	(0.0135)	6,812	+	-0.0131	(0.0182)	12,700	+	-0.0537***	(0.0179)	8,803	–
Log Wages (W-2)[1]	-0.0209	(0.0193)	2,998	+	-0.0083	(0.0217)	7,416	+	-0.0372*	(0.0227)	4,460	–
Has Earnings[1]	0.0017	(0.0031)	0.576	–	0.0099***	(0.0044)	0.622	–	0.0104***	(0.0036)	0.476	+
Has Asset Income[1]	-0.0034***	(0.0011)	0.078	+	-0.0039	(0.0026)	0.118	+	-0.0044***	(0.0021)	0.095	–
Has Elderly Member	-0.0021	(0.0021)	0.161	–	-0.0030	(0.0030)	0.124	–	0.0106***	(0.0027)	0.120	–
Has Disabled Member	-0.0062***	(0.0020)	0.300	–	-0.0103***	(0.0037)	0.122	–	0.0045	(0.0029)	0.173	–
Single Parent	0.0088***	(0.0030)	0.221	+	0.0054	(0.0055)	0.187	+	0.0123***	(0.0026)	0.159	–
Multiple Parents	-0.0007	(0.0011)	0.047	+	0.0015	(0.0016)	0.035	–	0.0012	(0.0010)	0.023	+
Deprivation Index (SIPP)	-0.0002	(0.0001)	0.154	–	-0.0001	(0.0002)	0.149	–	0.0001	(0.0001)	0.150	–
Non-White	0.0061*	(0.0031)	0.171	+	0.0140***	(0.0033)	0.170	+	0.0011	(0.0019)	0.156	–
Receives Home Care	0.0002	(0.0008)	0.021	+	0.0006***	(0.0002)	0.0005	+	0.0009***	(0.0003)	0.002	–
County-Months		7,200				7,200				7,200		

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

[1] All tax income measures are calculated for 2007 (the year before treatment was fully rolled out), and 3-year tax income is calculated for 2005-2007

Data Sources: Administrative Medicaid records for Indiana (2004-2014), Census population estimates (2004-2014), IRS Forms 1040, W-2, and 1099-R (2005-2007), Survey of Income and Program Participation (2004 Panel, Wave 5 and 2008 Panel, Waves 6 and 9)

Notes: This table shows regression estimates of average Medicaid recipient characteristics on a binary indicator for receiving IBM automation and being in the post-treatment period, controlling for county- and month-fixed effects as well as a set of county- and year-varying covariates. Observations are at the county-month level. Regression estimates for all remaining recipients (Columns 1-3) control for the mean shares of race/gender/age groups for the entire population and weight counties by the number of recipients in September 2007. Regression estimates for entrants control for the mean shares of race/gender/age groups among non-recipients and weight counties by the number of non-recipients in September 2007. Regression estimates for exiters control for the mean shares of race/gender/age groups among recipients and weight counties by their number of recipients in September 2007. The continuous income outcomes are equivalized using the NAS equivalence scale to reflect the number of children and adults in the assistance unit. For income outcomes, regression estimates additionally control for average age and age-squared of the case head. Marion and Lake counties (which are untreated) are excluded from the sample given that they are outliers on a number of dimensions. Columns 1-2, 5-6, and 9-10 show the main treatment effects for all remaining recipients, entrants, and exiters, respectively (with regressions using a pre-treatment window of 12 quarters before automation and a post-treatment window of 12 quarters). Standard errors are clustered at the county level. Columns 3, 7, and 11 show the (weighted) baseline means for each outcome among all recipients, entrants, and exiters (respectively) in treated counties in September 2007. Columns 4, 8, and 12 show indicators for whether IBM automation improves (+) or worsens (–) targeting efficiency. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization number: CBDRB-FY2021-CES005-021.

Table 6. Treatment Effects on Enrollment by County-Level Characteristics

Regressors	Post-Treatment Window: 3 Years			Post-Treatment Window: 6 Years		
	SNAP	TANF	Medicaid	SNAP	TANF	Medicaid
	(1)	(2)	(3)	(4)	(5)	(6)
D_{ct}	-0.1624***	-0.3085***	-0.0582**	-0.0950**	-0.1845	-0.0420
	(0.0478)	(0.0851)	(0.0240)	(0.0476)	(0.1148)	(0.0256)
$D_{ct} \times (High\ Unemp)_c$	0.0827**	0.0251	0.0478**	0.0715*	0.0543	0.0490**
	(0.0349)	(0.0508)	(0.0197)	(0.0392)	(0.0736)	(0.0227)
$D_{ct} \times (Small\ Pop)_c$	-0.0395	-0.0289	-0.0218	-0.0445	-0.0455	-0.0196
	(0.0270)	(0.0487)	(0.0137)	(0.0282)	(0.0604)	(0.0142)
$D_{ct} \times (High\ Poverty)_c$	-0.0827***	-0.1397***	-0.0181	-0.0821***	-0.1580**	-0.0206
	(0.0267)	(0.0459)	(0.0143)	(0.0303)	(0.0642)	(0.0162)
$D_{ct} \times (High\ NonWhite)_c$	-0.0180	0.0458	-0.0113	-0.0358	-0.0177	-0.0140
	(0.0277)	(0.0539)	(0.0129)	(0.0296)	(0.0719)	(0.0144)
$D_{ct} \times (Wave\ 2)_c$	0.0881***	0.3399***	-0.0015	0.0705**	0.2841***	-0.0182
	(0.0297)	(0.0501)	(0.0155)	(0.0327)	(0.0639)	(0.0173)
$D_{ct} \times (Wave\ 3)_c$	0.1299***	0.1697***	0.0453**	0.0964***	0.1488**	0.0243
	(0.0310)	(0.0606)	(0.0188)	(0.0336)	(0.0723)	(0.0204)
Observations		7,200			10,500	

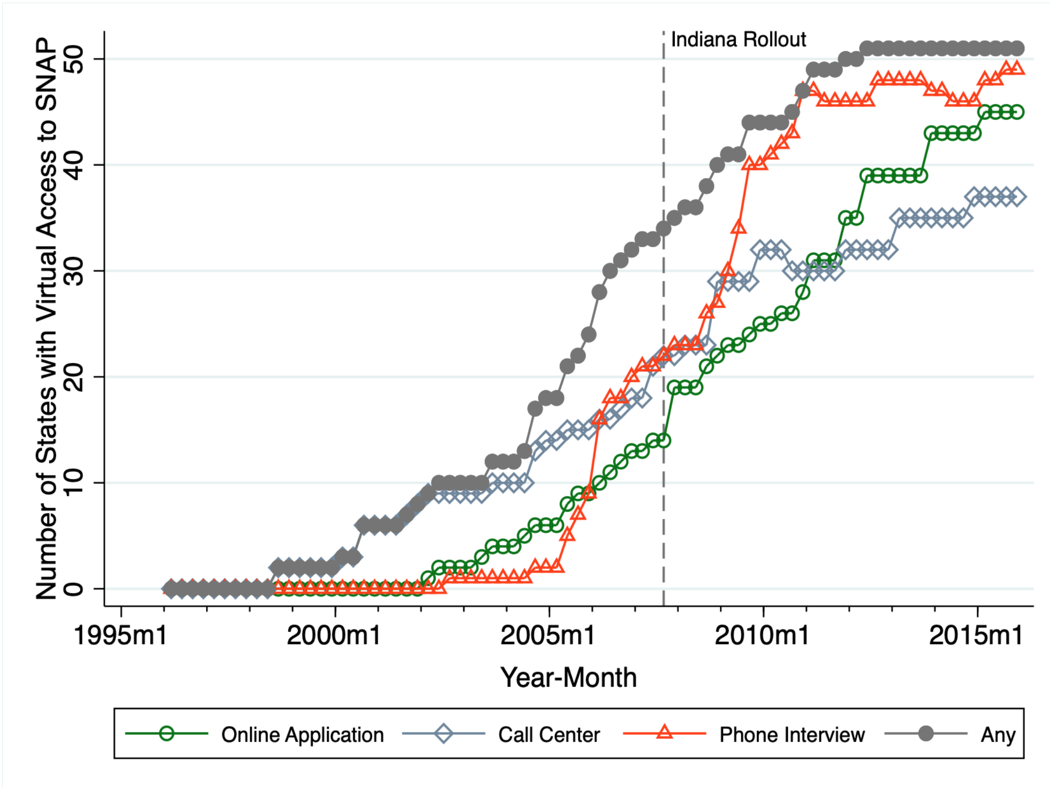
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Data Sources (public-use): County-level SNAP, TANF, and Medicaid program records (2004-2014), Census population estimates (2004-2014), Census SAIPE estimates (2007), BLS Local Area Unemployment Statistics (2007)

Notes: This table shows regression estimates of log individual recipients of SNAP, TANF, or Medicaid on a binary indicator for receiving IBM automation and being in the post-treatment period as well as this binary indicator interacted with various binary county characteristics (having above-median unemployment rate, below-median population size, above-median poverty rate, above-median share of non-white individuals, treated in Wave 2, and treated in Wave 3) measured in September 2007. Estimates control for county- and month-fixed effects as well as county- and year-varying covariates that include log total population and population subgroups by race, gender, and age. Observations are at the county-month level and are weighted by the county-specific enrollment volume in September 2007. Marion and Lake counties (which are untreated) are excluded from the sample given that they are outliers on a number of dimensions. All regressions use a pre-treatment window of 12 quarters before automation. Columns 1, 2, and 3 show regression estimates using a post-treatment window of 12 quarters after automation, and Columns 4, 5, and 6 show regression estimates using a post-treatment window of 24 quarters after automation. Standard errors are clustered at the county level.

Appendix Figures and Tables

Figure A1. Trends in Number of States with Virtual Access to SNAP



Data Sources (public-use): USDA SNAP Policy Database
Notes: This figure displays trends in the number of states adopting various policies expanding virtual access to SNAP (allowing online applications, setting up call centers to facilitate applications and customer service, allowing individuals to conduct interviews over the phone rather than face-to-face, and a composite indicator for having any of these three policies) between 1995 and 2016.

Figure A2. Supporting Documents for Medicaid Application in Indiana

Identity

- ☐ Driver's License
- ☐ State Photo ID Card
- ☐ Student Photo ID

Social Security Number

- ☐ Social Security Card
- ☐ Proof of Application for Social Security Card

US Citizenship / Immigration Status

- ☐ Alien Registration Card
- ☐ Baptismal Certificate
- ☐ Birth Certificate
- ☐ Bureau for Citizenship & Immigration Svcs. Document
- ☐ Hospital Birth Certificate
- ☐ Passport
- ☐ Permanent Resident Card

Money Received / Income

- ☐ Child Support – Proof of Payment Received
- ☐ Copy of Paychecks

Money Received (con't)

- ☐ Disability Payments
- ☐ Employer Statement
- ☐ Employer Statement of Termination
- ☐ Pay Stubs
- ☐ Pension Statements / Stubs
- ☐ Railroad Retirement Benefits
- ☐ Self Employment Records
- ☐ Sick Benefits
- ☐ Social Security / SSI Award Letter
- ☐ Statement of Loan, Gift or Contribution
- ☐ Unemployment Benefits
- ☐ Veteran's Benefits
- ☐ Worker's Compensation

Resources

- ☐ Annuity Contract
- ☐ Bank/ Credit Union Statement
- ☐ Real Estate, Oil, Gas or Mineral Rights Deed/Document
- ☐ Statement of Vehicle Value from Licensed dealer

Resources (con't)

- ☐ Stock / Bond Statement or Certificate
- ☐ Trust Statement
- ☐ Vehicle Registration

Insurance

- ☐ Insurance Card
- ☐ Life / Burial / Health Insurance Policy
- ☐ Statement from Insurance Provider

Expenses

- ☐ Cancelled Rent Check
- ☐ Homeowner's Insurance Statement
- ☐ Lease Agreement
- ☐ Proof of Energy Assistance Received
- ☐ Proof of Public Housing Assistance
- ☐ Property Tax Statement
- ☐ Rent Receipt
- ☐ Landlord or Mortgage Lender Statement
- ☐ Utility Bill

Child Care / Child Support Expenses

- ☐ County Clerk Record for Child Support
- ☐ Proof of Child Support You Pay
- ☐ Receipt / Copy of Check for Child Care that You Pay
- ☐ Statement from Child Care Provider

Medical

- ☐ Medical Bill / Receipt
- ☐ Medical Statement
- ☐ Medical Statement of Pregnancy / Due Date
- ☐ Prescription Receipt or Printout

Legal

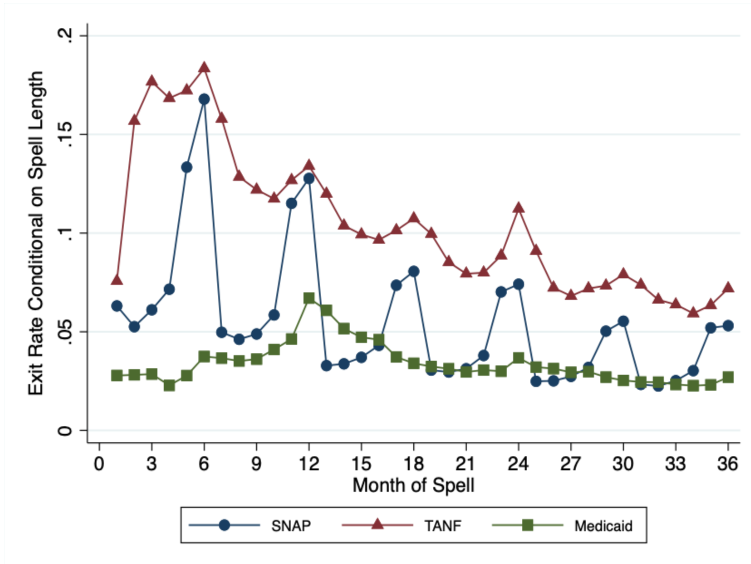
- ☐ Divorce Decree
- ☐ Guardianship Order
- ☐ Marriage Certificate
- ☐ Paternity record
- ☐ Power of Attorney

Other Documents

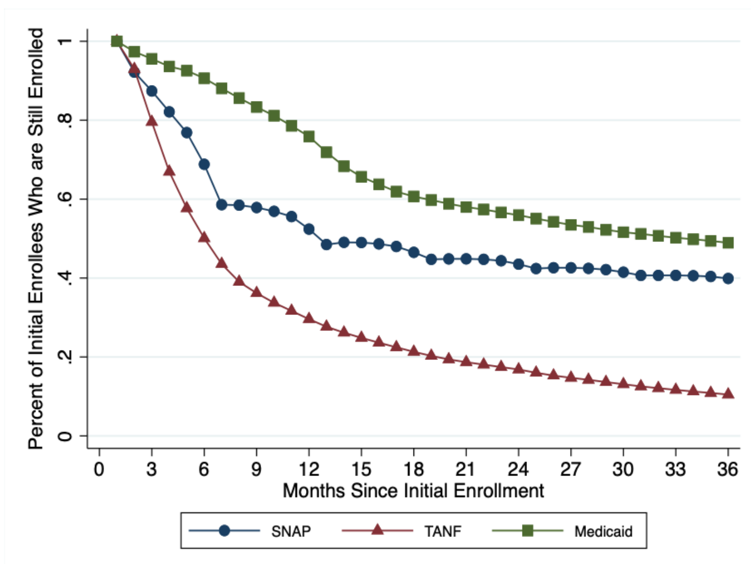
- ☐ Other(s): ______________________________

Notes: This figure is taken from a present-day Medicaid application in Indiana, detailing the various types of supporting documentation that may need to be provided to verify the incomes and expenses listed in one's application.

Figure A3. Baseline Hazard Rates and Survival Probabilities by Spell Length



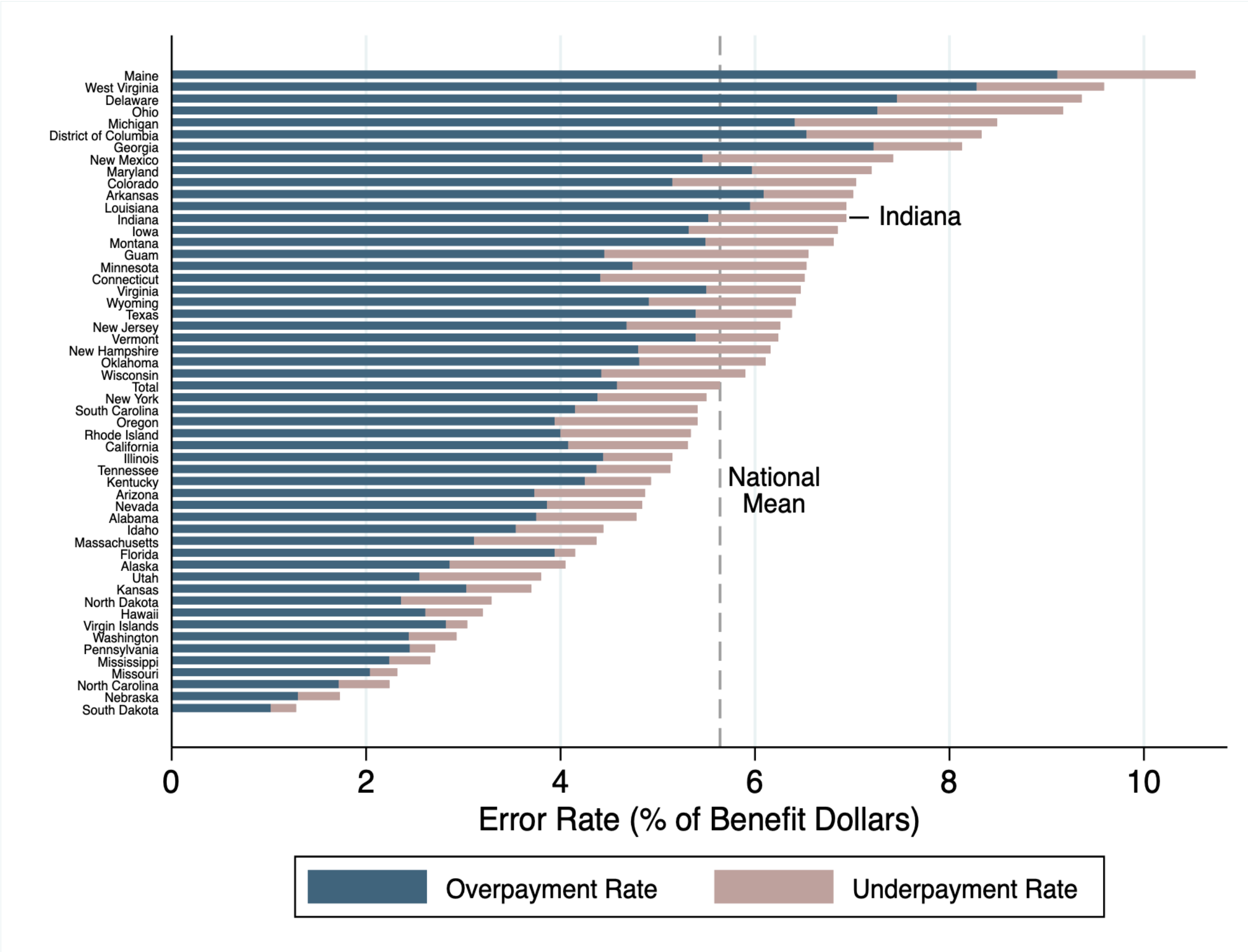
(a) Hazard Rates



(b) Survival Probabilities

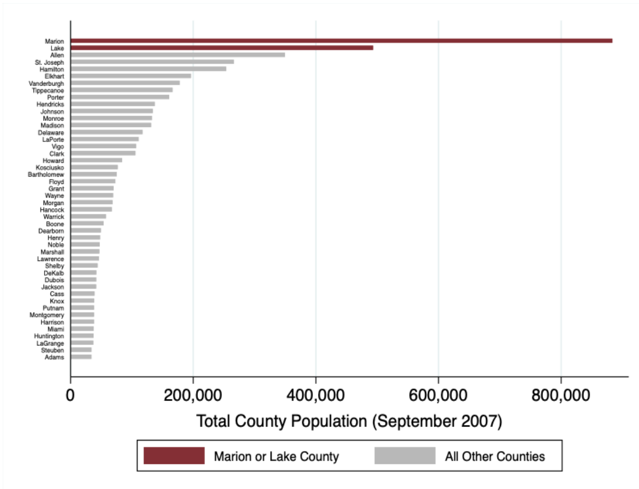
Data Sources: Administrative SNAP, TANF, and Medicaid records for Indiana (2005-2014)
Notes: These figures show baseline hazard rates and survival probabilities calculated over all individual recipients of SNAP, TANF, or Medicaid in Indiana. Panel A shows hazard rates (average exit rates conditional on a given length of one's receipt spell) by program. Panel B provides a transformation of Panel A by showing the share of recipients (by program) that are enrolled at each month relative to their entry month. The samples consist of all recipients who initially enroll between February 2005 and October 2011. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization number: CBDRB-FY2021-CES005-021.

Figure A4. Error Rates for SNAP Payments by State (FY 2007)

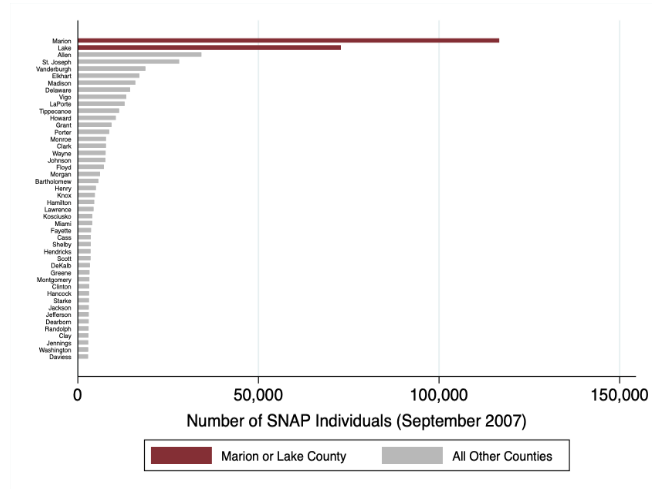


Data Sources (public-use): USDA-Mathematica SNAP Quality Control File (2007)
Notes: This bar graph shows combined SNAP error rates (split into overpayment rates and underpayment rates) for each state during fiscal year 2007. The average combined SNAP error rate for the U.S. was 5.64% (represented by the dashed line). States are ordered from highest combined error rate to lowest combined error rate.

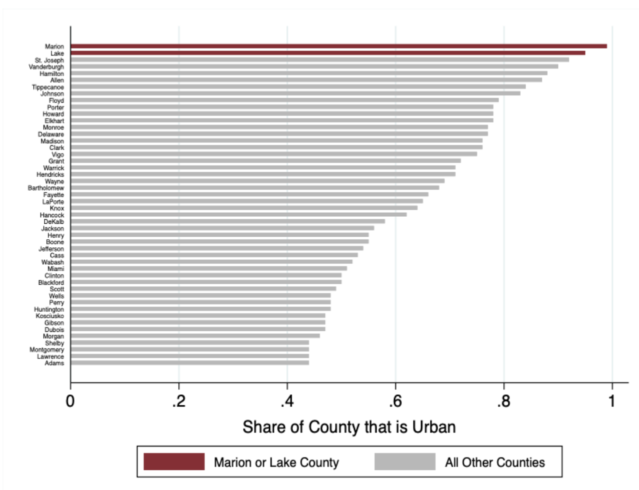
Figure A5. Distribution of County Characteristics (Marion/Lake vs. All Other)



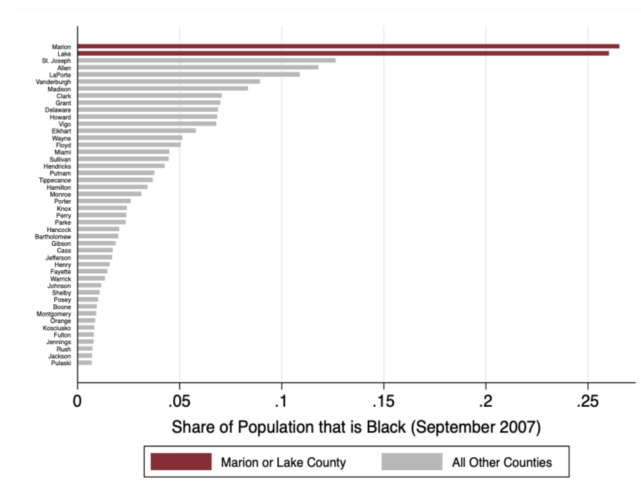
(a) Total Population



(b) Number of SNAP Recipients



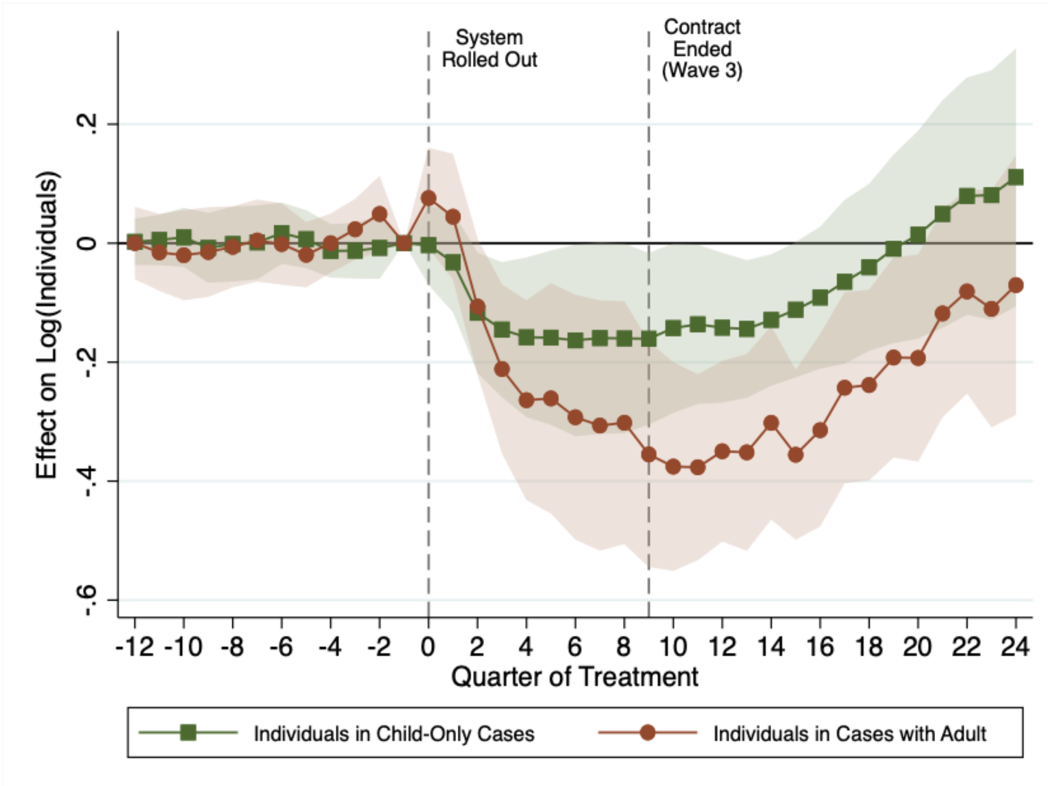
(c) Urban Share



(d) Black Share of Population

Data Sources (public-use): Census population estimates (2007), County-level SNAP program records (2007)
Notes: These bar graphs show the distribution of various county-level characteristics (total population, number of SNAP recipients, urban share, and black share of population) measured in September 2007 among all counties in Indiana in the top 50% of each category. Marion and Lake counties are highlighted in maroon, while all other counties are in gray.

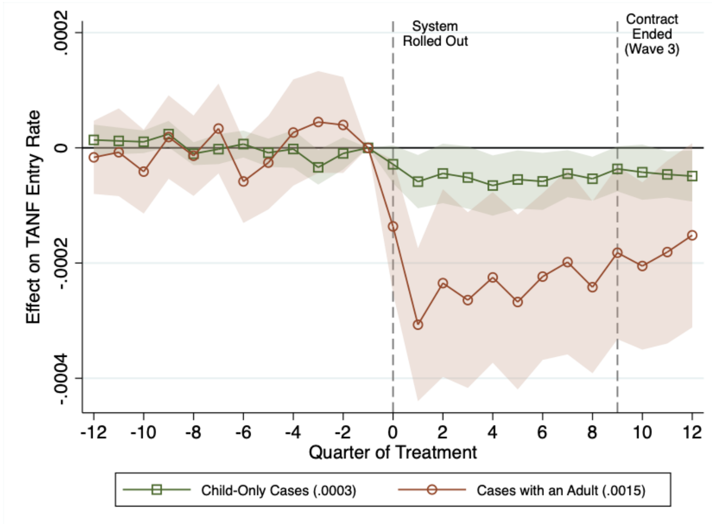
Figure A6. Dynamic Treatment Effects on TANF Enrollment



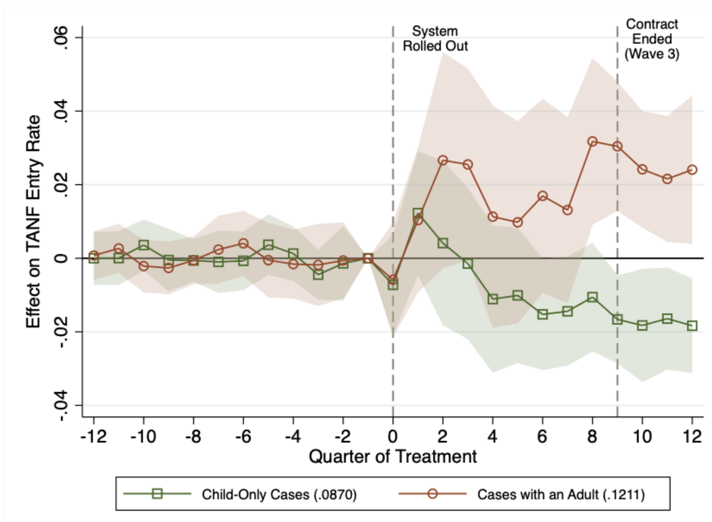
Data Sources: Administrative TANF records for Indiana (2004-2014), Census population estimates (2004-2014)

Notes: This figure shows regression estimates of log TANF recipients (subdivided into individuals who are part of child-only cases and individuals who are part of cases with an adult) on binary indicators corresponding to the event-quarter relative to receiving IBM automation, controlling for county- and month-fixed effects as well as county- and year-varying covariates that include log total population and population subgroups by race, gender, and age. Observations are at the county-month level and are weighted by the county-specific enrollment volume in September 2007 (month prior to initial IBM rollout). Marion and Lake counties (which are untreated) are excluded from the sample given that they are outliers on a number of dimensions. Standard errors are clustered at the county level. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization number: CBDRB-FY2021-CES005-021.

Figure A7. Dynamic Treatment Effects on Monthly TANF Entry and Exit Rates



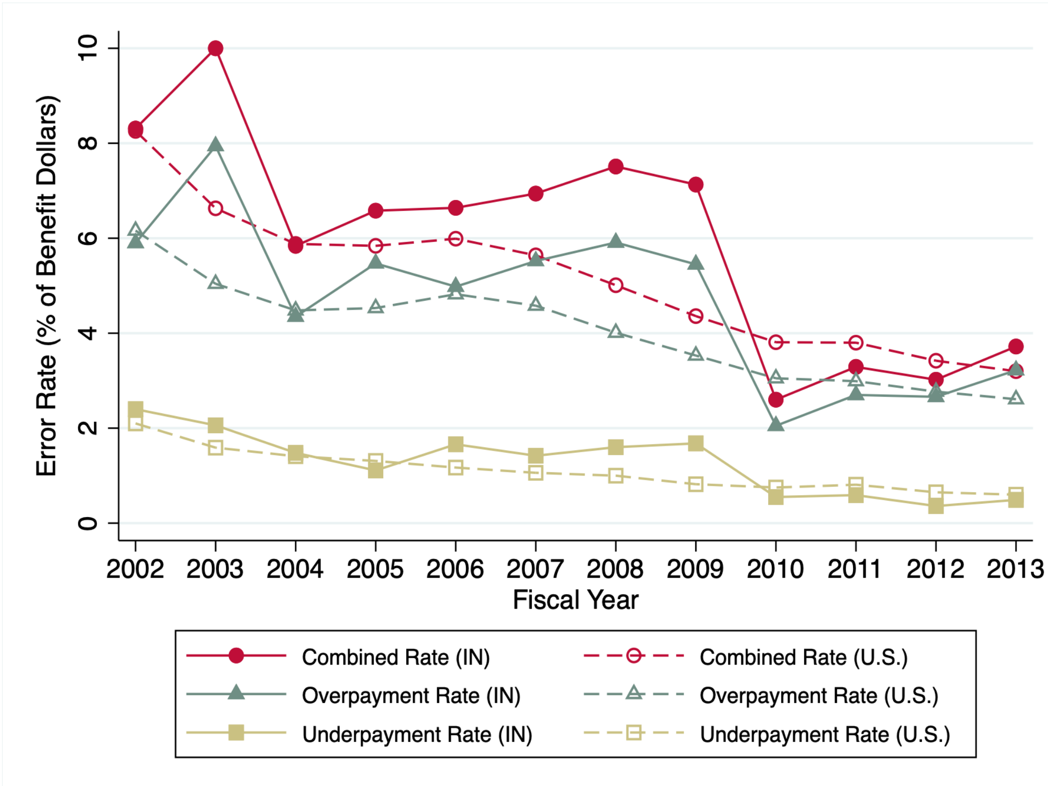
(a) Entry Rates



(b) Exit Rates

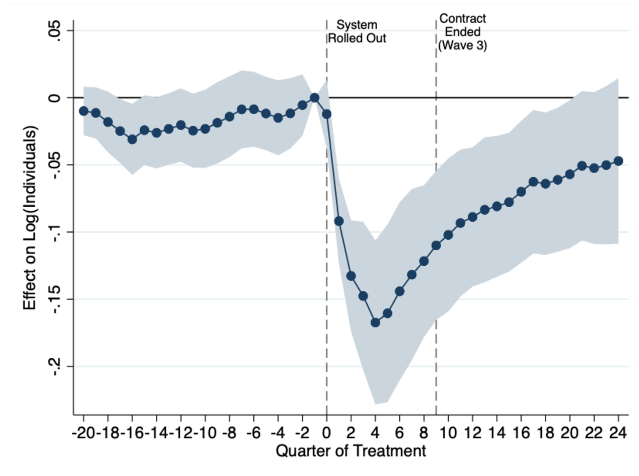
Data Sources: Administrative TANF records for Indiana (2004-2014), Census pop. estimates (2004-2014)
Notes: These figures show regression estimates of monthly TANF entry and exit rates (separately for individuals who are part of child-only cases and individuals who are part of cases with an adult) on binary indicators corresponding to the event-quarter relative to receiving IBM automation, controlling for county- and month-fixed effects as well as a set of county- and year-varying covariates. Regression estimates of entry rates control for the mean shares of race/gender/age groups among non-recipients and weight counties by the number of non-recipients in September 2007. Regression estimates of exit rates control for the mean shares of race/gender/age groups among recipients and weight counties by the number of recipients in September 2007. Marion and Lake counties (which are untreated) are excluded from the sample given that they are outliers on a number of dimensions. Standard errors are clustered at the county level. Baseline entry and exit rates for treated counties (in September 2007) are reported in parentheses in the legend. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization number: CBDRB-FY2021-CES005-021.

Figure A8. Trends in SNAP Payment Error Rates: Indiana vs. U.S.

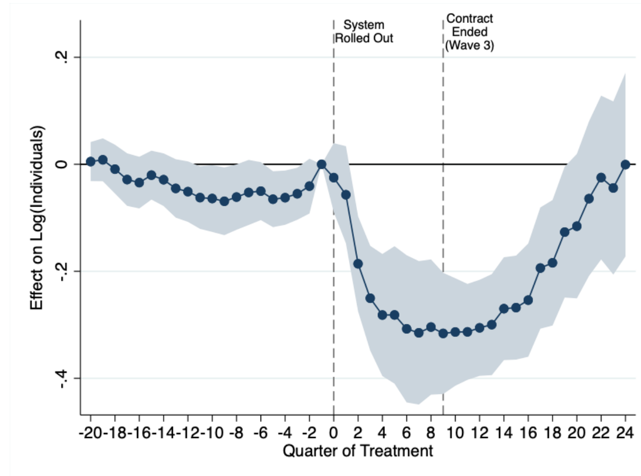


Data Sources (public-use): USDA-Mathematica SNAP Quality Control Files (2002-2013)
Notes: This figure shows trends in SNAP error rates (calculated from the U.S. Department of Agriculture's Quality Control files) for Indiana and the U.S. between fiscal years 2002 and 2013. Underpayment rates reflect the share of cases receiving too few SNAP benefits in error, overpayment rates reflect the share of cases receiving too many SNAP benefits in error, and combined rates reflect the sum of underpayment and overpayment rates. Solid markers correspond to rates for Indiana, and hollow markers correspond to rates for the U.S.

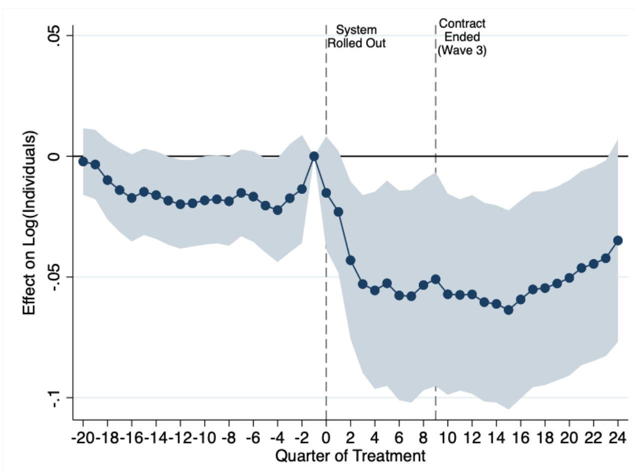
Figure A9. Dynamic Treatment Effects on Enrollment with Longer Pre-Trends



(a) SNAP



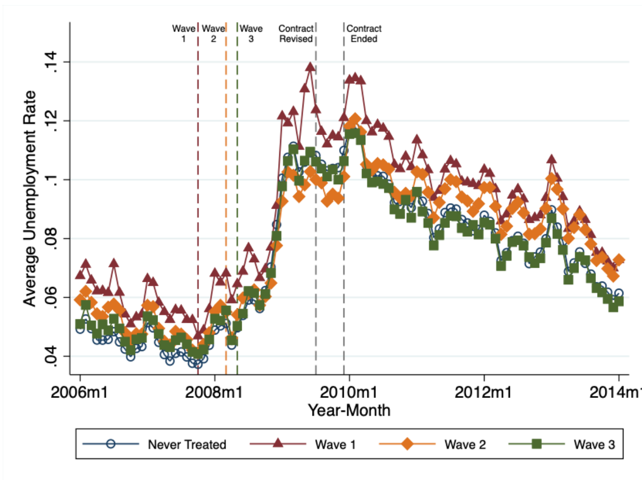
(b) TANF



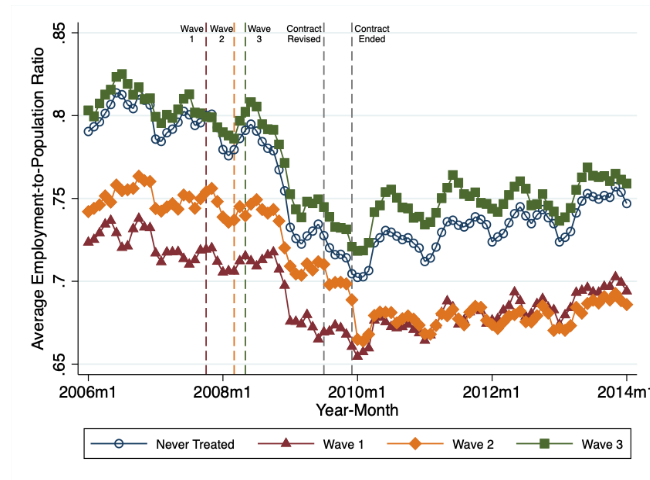
(c) Medicaid

Data Sources (public-use): County-level SNAP, TANF, and Medicaid program records (2002-2014), Census population estimates (2002-2014)
Notes: These figures show regression estimates of log total individuals on binary indicators corresponding to the event-quarter relative to receiving IBM automation, controlling for county- and month-fixed effects as well as county- and year-varying covariates that include log total population and population subgroups by race, gender, and age. Observations are at the county-month level and are weighted by the county-specific enrollment volume in September 2007 (month prior to initial IBM rollout). Marion and Lake counties (which are untreated) are excluded from the sample given that they are outliers on a number of dimensions. Standard errors are clustered at the county level.

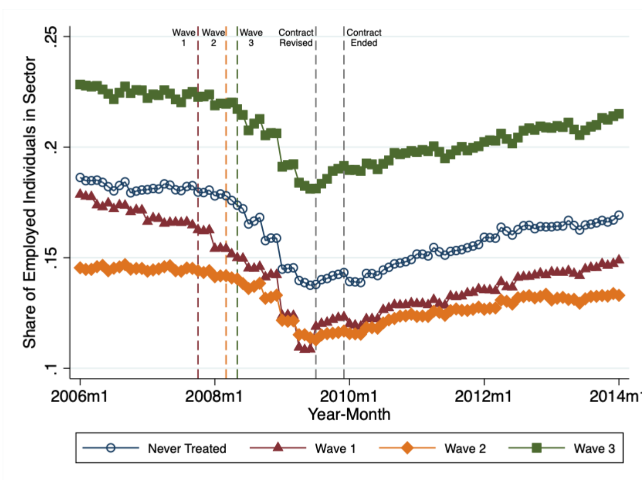
Figure A10. Raw Trends in Economic Characteristics



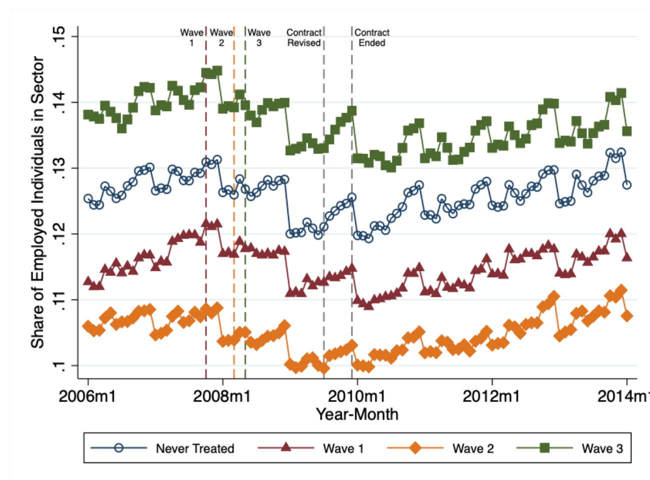
(a) Unemployment Rate



(b) Employment-to-Population Ratio



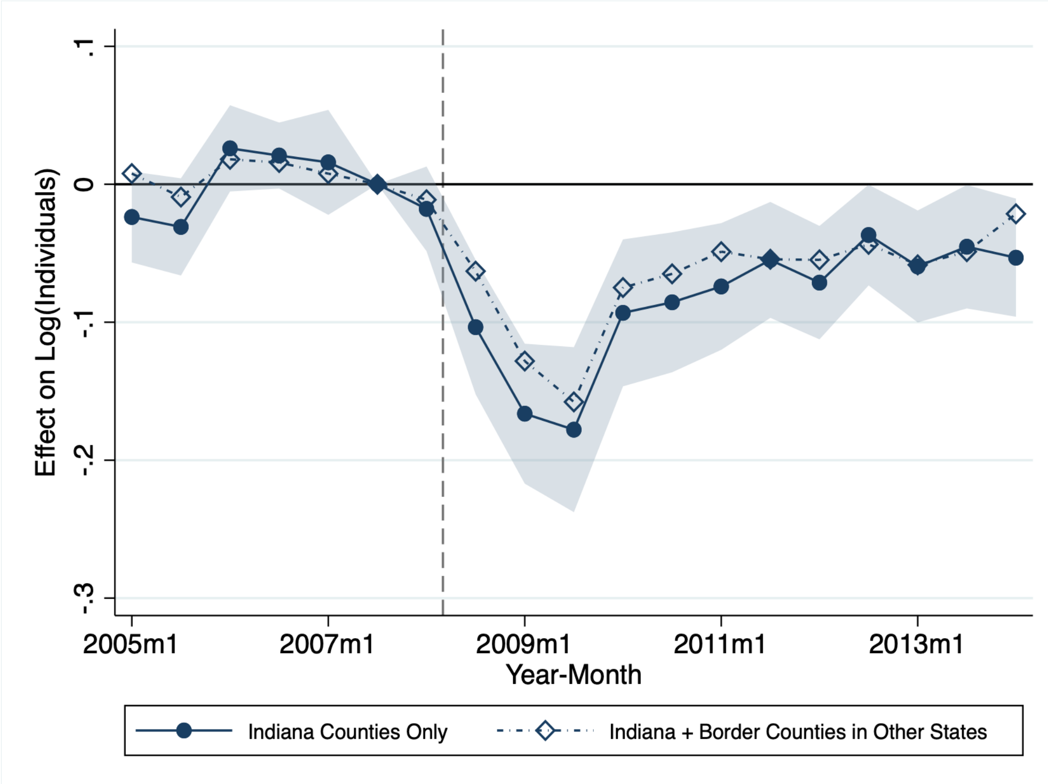
(c) Share of Employed in Manufacturing



(d) Share of Employed in Wholesale/Retail Trade

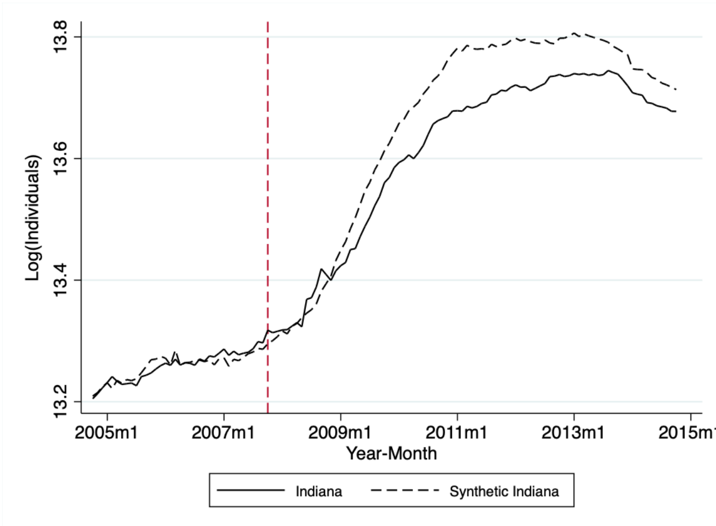
Data Sources (public-use): Census population estimates (2006-2014), BLS Local Area Unemployment Statistics (2006-2014), BLS Quarterly Census of Employment and Wages (2006-2014)
Notes: These figures show raw trends in monthly unemployment rates, monthly employment-to-population ratios (where the population consists of adults aged 18-64), and quarterly shares of employed individuals in each of two key economic sectors in Indiana (manufacturing and wholesale and retail trade) for counties exposed to the automated system (categorized by rollout wave) and counties never automated by IBM. Note that county-level unemployment rates are imputed by the Bureau of Labor Statistics based on state-level employment rates and county-level Unemployment Insurance records. The receipt rate for each group is calculated as the average of receipt rates (weighted by county population in September 2007) across each of the group's counties. In each of the panels, the dashed vertical lines correspond to the timing of major events, including the rollout of the automated system in Waves 1, 2, and 3, the undertaking by IBM of the "Corrective Action Plan" (in July 2009), and the termination of the IBM contract (in December 2009).

Figure A11. Effects on SNAP Enrollment vs. Border Counties in Other States

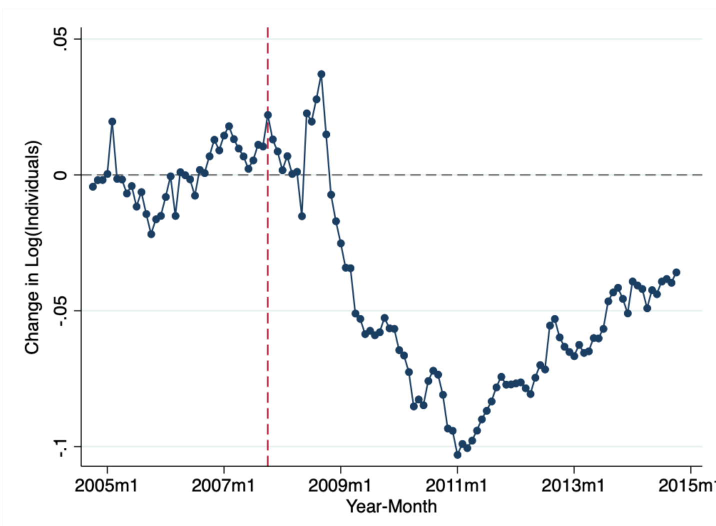


Data Sources (public-use): USDA SNAP county-level enrollment data (2005-2014), Census population estimates (2005-2014)
Notes: This figure shows regression estimates of log SNAP individuals on binary indicators corresponding to the interaction of year-month (either January or September) and being treated, controlling for county- and month-fixed effects as well as county- and year-varying covariates that include log total population and population subgroups by race, gender, and age. IBM automation (treatment) is defined as starting in January 2008, and data are only available for January and July of every year (due to data availability issues). The hollow markers correspond to estimates comparing treated counties in Indiana to untreated counties in Indiana, while the solid markers correspond to estimates comparing treated counties in Indiana to both untreated counties in Indiana and bordering counties in neighboring states (i.e., Illinois, Kentucky, Ohio, and Michigan) that are by construction untreated. Observations are at the county-month level and are weighted by the county-specific enrollment volume in September 2007 (month prior to initial IBM rollout). Marion and Lake counties (which are untreated) are excluded from the sample given that they are outliers on a number of dimensions. Standard errors are clustered at the county level.

Figure A12. Synthetic Control Estimates for Log SNAP Receipt



(a) Indiana vs. Synthetic Indiana Trends



(b) Difference in Trends

Data Sources (public-use): USDA SNAP state-level enrollment data (2004-2015), BLS Unemployment Statistics (2004-2015), Census population and income estimates (2004-2015), USDA SNAP Policy Database (2004-2015)

Notes: These figures use the synthetic control method to compare changes in the number of individuals receiving SNAP in Indiana to changes in other states. Panel (a) compares the monthly trends in log SNAP recipients between Indiana and synthetic Indiana, with the synthetic control group constructed based on matching to Indiana on the following set of pre-treatment covariates: SNAP recipients for each of the 12 quarters preceding treatment, population cuts by gender, race, and age, median income, unemployment rate, and the number of SNAP policies adopted by a state during a particular month. Panel (b) plots the monthly differences in log SNAP recipients between Indiana and synthetic Indiana.

Table A1. Characteristics of SNAP, TANF, and Medicaid Recipients (2005-2007)

Characteristics	United States				Indiana			
	All	SNAP	TANF	Medicaid	All	SNAP	TANF	Medicaid
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CPS (2005-2007)								
Has Children (%)	38.5	62.7	82.3	64.1	37.1	63.4	88.9	70.1
Single Parent (%)	3.8	20.7	32.2	11.6	4.0	25.9	44.7	16.9
Has Elderly Member (%)	22.1	15.3	7.8	19.3	19.1	9.9	2.0	10.0
Has Disabled Member (%)	15.9	41.1	37.0	34.4	13.9	45.2	38.0	35.0
Average Income ($)*	105,798	25,628	29,280	50,848	96,466	28,372	19,975	47,902
Average Earnings ($)*	85,096	14,779	17,089	36,534	78,736	17,304	10,436	36,021
Average Asset Income ($)*	5,285	147	533	1,496	3,757	82	69	546
Move in Last Year (%)	11.9	25.1	28.5	17.5	10.3	34.8	41.3	21.5
Homeowner (%)	72.4	30.2	25.8	48.9	78.4	23.2	17.4	46.0
Receives Hous. Sub. (%)	3.1	25.1	29.0	12.6	3.7	32.5	45.5	17.9
White (non-Hispanic) (%)	72.3	48.4	43.0	52.8	88.0	69.0	56.2	73.2
Black (non-Hispanic) (%)	10.4	26.5	29.8	17.5	7.0	23.7	30.2	15.6
Hispanic (%)	11.5	19.7	20.7	23.2	3.6	4.6	9.1	9.1
Has HS Diploma (%)	86.9	63.5	66.3	70.9	88.1	59.3	58.8	69.2
Citizen (%)	92.2	90.0	88.7	85.4	97.4	98.3	97.7	94.5
Unemployed (%)	2.6	8.4	11.7	5.0	3.2	13.5	18.5	7.9
Sample Size	361,692	21,249	5,039	60,359	5,511	400	99	786
SIPP (2005)								
Material Hardships (of 8)	0.52	1.53	1.60	1.05	0.46	1.41	1.34	0.90
Unowned Appliances (of 8)	1.35	2.67	2.79	2.17	1.31	2.31	2.63	1.86
Home Problems (of 7)	0.24	0.53	0.69	0.42	0.16	0.37	0.46	0.30
Food Problems (of 8)	0.37	1.34	1.46	0.91	0.32	1.20	1.29	0.81
Any Health Problem (%)	22.1	48.1	45.0	39.8	20.6	38.2	33.0	33.0
Sample Size	37,368	3,315	537	7,869	1,332	116	21	240
NLSY97 (2005-2007)								
AFQT Score (Pctile)	51.1	32.9	25.6	31.0				
AFQT < Army Min. (%)	30.0	55.4	67.8	59.4				
Sample Size	5,990	1,058	256	835				

* Equivalized for a 2-adult, 2-child family

Data Sources (public-use): Current Population Survey Annual Social Economic Supplement (2006-2008), Survey of Income and Program Participation (2004 Panel, Wave 5), Nat'l Longitudinal Survey of Youth 1997

Notes: This table shows average characteristics of the entire population and SNAP, TANF, and Medicaid recipients in the United States and in Indiana, using data for reference years 2005-2007. Columns 1, 2, 3, and 4 show estimates for the entire population and for SNAP, TANF, and Medicaid recipients (respectively) in the entire nation, and Columns 4, 5, 6, and 7 show estimates for the entire population and for SNAP, TANF, and Medicaid recipients (respectively) in Indiana. CPS characteristics are calculated for households and/or household heads pooled over reference years 2005-2007 (weighted using survey household weights), SIPP characteristics are calculated for households in reference year 2005 (weighted using survey household weights), and NLSY characteristics are calculated for individuals in reference years 2005-2007 (weighted using survey individual weights for 2007).

Table A2. Characteristics of SNAP Recipients in Indiana vs. U.S. (2000-2007)

Characteristics	Indiana (1)	U.S. (2)	P-Value for Difference (3)
Number of Persons in Assistance Unit	2.34	2.30	0.010
Number of Adults in Assistance Unit	1.18	1.14	0.000
Number of Children in Assistance Unit	1.16	1.15	0.573
Elderly (%)	15.31	18.30	0.000
Single Parent (non-elderly) (%)	37.53	34.28	0.000
Multiple Parents (non-elderly) (%)	13.44	12.28	0.002
Single Individual (non-elderly) (%)	28.85	28.70	0.759
Multiple Childless Individuals (non-elderly) (%)	4.86	6.44	0.000
Has Disability (%)	28.77	24.36	0.000
Has Non-Citizen (%)	1.11	5.96	0.000
Income as Share of Poverty Line	59.81	59.85	0.918
Gross Income (equivalized) ($)	609	601	0.310
Net Income (equivalized) ($)	316	329	0.016
Net Earnings (equivalized) ($)	283	268	0.022
Observations	8,573	380,095	

Data Sources (public-use): USDA-Mathematica SNAP Quality Control Files (2000-2007)
Notes: This table shows average characteristics of SNAP assistance units in Indiana (Column 1) compared to those in the U.S. as a whole (Column 2), calculated using the SNAP Quality Control Files from the U.S. Department of Agriculture for fiscal years 2000 through 2007. Column 3 shows the p-values corresponding to tests of the null hypothesis that Indiana and the rest of the U.S. have equivalent average values for a given characteristic.

Table A3. Treatment Effects After Clustering Standard Errors at Varying Levels

	Cluster SEs by County			Cluster SEs by Region	
Outcomes	Point Est.	Std. Error	P-Value	P-Value (Reg.)	P-Val. (Boot.)
	(1)	(2)	(3)	(4)	(5)
			A: Post-Treatment Window: 3 Years		
SNAP					
Log Individuals	-0.0990***	(0.0258)	[0.000]	[0.038]	[0.059]
Log Cases	-0.0980***	(0.0232)	[0.000]	[0.037]	[0.045]
Log Dollars	-0.0699***	(0.0257)	[0.008]	[0.083]	[0.101]
TANF					
Log Individuals	-0.1851***	(0.0403)	[0.000]	[0.029]	[0.071]
Log Cases	-0.1641***	(0.0351)	[0.000]	[0.038]	[0.095]
Log Dollars	-0.1432***	(0.0358)	[0.000]	[0.062]	[0.108]
Medicaid					
Log Individuals	-0.0297*	(0.0156)	[0.062]	[0.166]	[0.264]
County-Months			7,200		
			B: Post-Treatment Window: 6 Years		
SNAP					
Log Individuals	-0.0730***	(0.0209)	[0.001]	[0.066]	[0.097]
Log Cases	-0.0766***	(0.0179)	[0.000]	[0.047]	[0.061]
Log Dollars	-0.0539**	(0.0205)	[0.010]	[0.105]	[0.138]
TANF					
Log Individuals	-0.1306**	(0.0509)	[0.012]	[0.148]	[0.129]
Log Cases	-0.0997**	(0.0443)	[0.028]	[0.245]	[0.249]
Log Dollars	-0.0805*	(0.0458)	[0.083]	[0.356]	[0.409]
Medicaid					
Log Individuals	-0.0310**	(0.0150)	[0.042]	[0.171]	[0.281]
County-Months			10,500		

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

Data Sources (public-use): County-level SNAP, TANF, and Medicaid program records (2004-2014), Census population estimates (2004-2014)

Notes: This table shows regression estimates of various log enrollment measures (total cases, individuals, or benefit dollars) on a binary indicator for receiving IBM automation and being in the post-treatment period, controlling for county- and month-fixed effects as well as county- and year-varying covariates that include log total population and population subgroups by race, gender, and age. Observations are at the county-month level and are weighted by the county-specific enrollment volume in September 2007. Marion and Lake counties (which are untreated) are excluded from the sample given that they are outliers on a number of dimensions. All regressions use a pre-treatment window of 12 quarters before automation. Panel A shows regression estimates using a post-treatment window of 12 quarters after automation, and Panel B shows regression estimates using a post-treatment window of 24 quarters after automation. Column 1 shows the main point estimates, and Columns 2 and 3 show the standard errors and p-values associated with those estimates when clustering at the county level. Column 4 shows p-values after clustering at the region level, and Column 5 shows p-values after clustering at the region level using the wild cluster bootstrap.

Table A4. Effects of IBM Automation on Additional Characteristics of Program Recipients (Targeting)

Outcomes	All Remaining Recipients				Entrants				Exiters			
	Point Estimate	Std. Error	Baseline Mean		Point Estimate	Std. Error	Baseline Mean		Point Estimate	Std. Error	Baseline Mean	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
SNAP												
Log Earnings	0.0347***	(0.0117)	532	−	-0.0672***	(0.0133)	677	+	-0.0868***	(0.0172)	1,009	−
Log Unearned Income	0.0136***	(0.0053)	800	−	-0.0074	(0.0122)	442	+	-0.0505***	(0.0108)	589	−
Log Total Income	0.0220***	(0.0052)	1,332	−	-0.0378***	(0.0098)	1,119	+	-0.0662***	(0.0112)	1,597	−
Log 3-Year Tax Income	-0.0131	(0.0106)	38,620	+	-0.0710***	(0.0120)	65,150	+	-0.0844***	(0.0095)	58,870	−
Log Tax Income	0.0051	(0.0119)	14,900	−	-0.0747***	(0.0137)	25,200	+	-0.0746***	(0.0111)	24,540	−
Born in Non-Border State	0.0040**	(0.0020)	0.123	−	-0.0003	(0.0022)	0.146	+	0.0011	(0.0023)	0.141	+
Urban	0.0009	(0.0011)	0.617	−	0.0004	(0.0017)	0.585	−	0.0015	(0.0019)	0.620	+
TANF												
Log Earnings	0.1402***	(0.0302)	307	−	0.0592	(0.0687)	303	−	-0.0235	(0.0323)	500	−
Log 3-Year Tax Income	-0.0265	(0.0311)	21,230	+	-0.0412	(0.0315)	33,370	+	-0.0233	(0.0164)	24,780	−
Log Tax Income	0.0304	(0.0393)	6,368	−	-0.0519*	(0.0294)	10,030	+	-0.0456*	(0.0264)	8,369	−
Born in Non-Border State	0.0231*	(0.0125)	0.106	−	0.0108	(0.0090)	0.116	−	0.0073**	(0.0033)	0.111	+
Urban	0.0040**	(0.0019)	0.686	−	0.0010	(0.0023)	0.585	−	0.0023	(0.0026)	0.688	+
Medicaid												
Dual Recipient	-0.0016	(0.0020)	0.173	−	0.0029	(0.0019)	0.075	−	0.0165***	(0.0016)	0.085	+
Log 3-Year Tax Income	-0.0061	(0.0099)	21,520	+	0.0038	(0.0140)	40,540	−	-0.0213	(0.0141)	25,200	−
Log Tax Income	0.0111	(0.0119)	6,812	−	0.0244	(0.0221)	12,700	−	-0.0059	(0.0141)	8,803	−
Born in Non-Border State	0.0032**	(0.0016)	0.111	−	-0.0013	(0.0031)	0.145	+	-0.0014	(0.0021)	0.138	−
Urban	0.0021	(0.0015)	0.588	−	-0.0008	(0.0023)	0.582	+	0.0009	(0.0027)	0.589	+
County-Months		7,200				7,200				7,200		

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

Data Sources: Administrative SNAP, TANF, and Medicaid records for Indiana (2004-2014)

Notes: This table shows regression estimates of average SNAP recipient characteristics on a binary indicator for receiving IBM automation and being in the post-treatment period, controlling for county- and month-fixed effects as well as a set of county- and year-varying covariates. Observations are at the county-month level. Regression estimates for all remaining recipients (Columns 1-3) control for the mean shares of race/gender/age groups for the entire population and weight counties by the number of recipients in September 2007. Regression estimates for entrants control for the mean shares of race/gender/age groups among non-recipients and weight counties by the number of non-recipients in September 2007. Regression estimates for exiters control for the mean shares of race/gender/age groups among recipients and weight counties by their number of recipients in September 2007. The continuous income outcomes are equivalized using the NAS equivalence scale to reflect the number of children and adults in the assistance unit. For income/education outcomes, regression estimates additionally control for average age and age-squared of the case head. Marion and Lake counties (which are untreated) are excluded from the sample given that they are outliers on a number of dimensions. The Census Bureau has reviewed this data product for unauthorized disclosure of confidential information and has approved the disclosure avoidance practices applied to this release, authorization number: CBDRB-FY2021-CES005-021.

Table A5. Treatment Effects on Program Enrollment (Different Specifications)

Outcomes	Main Estimate (Public-Use) (1)	Main Estimate (Restricted-Use) (2)	No Controls (3)	Population Controls Only (4)	Pop. + Demog. + Unemp. Controls (5)	Exclude Biggest Counties (6)	Exclude Flooded Counties (7)
			A: Post-Treatment Window: 3 Years				
Log SNAP Individuals	-0.0990***	-0.1014***	-0.1085***	-0.0953***	-0.0656***	-0.0995***	-0.1049***
	(0.0258)	(0.0261)	(0.0310)	(0.0302)	(0.0184)	(0.0166)	(0.0331)
Log TANF Individuals	-0.1851***	-0.1895***	-0.1856**	-0.1712**	-0.1583***	-0.1739***	-0.2109***
	(0.0403)	(0.0532)	(0.0714)	(0.0739)	(0.0360)	(0.0394)	(0.0549)
Log Medicaid Individuals	-0.0297*	-0.0281*	-0.0440**	-0.0281	-0.0152	-0.0432***	-0.0226
	(0.0156)	(0.0153)	(0.0214)	(0.0198)	(0.0137)	(0.0097)	(0.0188)
County-Months	7,200	7,200	7,200	7,200	7,200	6,800	5,200
			B: Post-Treatment Window: 6 Years				
Log SNAP Individuals	-0.0730***	-0.0745***	-0.0881***	-0.0721***	-0.0531***	-0.0809***	-0.0761***
	(0.0209)	(0.0212)	(0.0281)	(0.0254)	(0.0185)	(0.0182)	(0.0267)
Log TANF Individuals	-0.1306**	-0.1321**	-0.1375*	-0.1247	-0.1209**	-0.1697***	-0.1491**
	(0.0509)	(0.0546)	(0.0774)	(0.0793)	(0.0513)	(0.0475)	(0.0698)
Log Medicaid Individuals	-0.0310**	-0.0271*	-0.0496**	-0.0306*	-0.0234	-0.0490***	-0.0234
	(0.0150)	(0.0140)	(0.0225)	(0.0183)	(0.0146)	(0.0113)	(0.0181)
Observations	10,500	10,500	10,500	10,500	10,500	9,800	7,500

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

Data Sources: Administrative restricted-use SNAP, TANF, and Medicaid records for Indiana (2004-2014), County-level public-use SNAP, TANF, and Medicaid program records (2004-2014), Census population estimates (2004-2014)

Notes: This table shows regression estimates of log total individuals on a binary indicator for receiving IBM automation and being in the post-treatment period, controlling for county- and month-fixed effects as well as county- and year-varying covariates. Observations are at the county-month level and are weighted by the county-specific enrollment volume in September 2007. Marion and Lake counties (which are untreated) are excluded from the sample given that they are outliers on a number of dimensions. All regressions use a pre-treatment window of 12 quarters before automation. Columns 1 and 2 show the primary estimates from the public- and restricted-use data sources, respectively, which control for log total population and subgroups by race, gender, and age. Column 3 shows estimates without any controls, Column 4 controls only for log total population, and Column 5 controls for log total population (along with subgroups by race gender, and age) and unemployment rate. Column 6 shows estimates after removing the five largest counties in Indiana (after Marion and Lake counties): Allen, St. Joseph, Hamilton, Vanderburgh, and Elkhart counties. Column 7 shows estimates that exclude the 26 Indiana counties that were severely impacted by floods in September 2008. Standard errors are clustered at the county level. Results have been approved for release by the U.S. Census Bureau, authorization number CBDRB-FY2021-CES005-021.

Table A6. Treatment Effects for Automated vs. Non-Automated Programs

Outcomes	Point Estimate (1)	Standard Error (2)
Automated Programs		
Log SNAP Individuals	-0.1001***	(0.0287)
Log TANF Individuals	-0.2115***	(0.0511)
Log Medicaid Individuals	-0.0308*	(0.0173)
Other Programs		
Log Social Security Individuals	-0.0031	(0.0041)
Log SSI Individuals	-0.0283**	(0.0127)
Log Medicare Individuals	-0.0017	(0.0037)
Log Free and Reduced Meals Individuals	-0.0289	(0.0194)
County-Years	630	

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

Data Sources (public-use): County-level public-use SNAP, TANF, and Medicaid program records (2005-2011), SSA annual estimates of Social Security and SSI recipients (2005-2011), CMS Medicare enrollment counts (2007-2011), Department of Education CCD Farm counts (2005-2011), Census population estimates (2005-2011)

Notes: This table shows regression estimates of log total individuals receiving various programs - including SNAP, TANF, and Medicaid (which are automated) and Social Security, SSI, Medicare, and free and reduced meals (which are not automated) - on a binary indicator for receiving IBM automation and being in the post-treatment period, controlling for county- and month-fixed effects as well as county- and year-varying covariates that include log total population and population subgroups by race, gender, and age. Annual individuals for SNAP, TANF, and Medicaid are averaged across months in a calendar year. Annual individuals for Social Security and SSI are measured by the number of recipients in December of a given calendar year. Annual individuals receiving free and reduced meals are measured by the number of recipients in a given school year. The calendar year corresponding to the implementation of the automated system is set to 2008. All regressions also use a pre-treatment window of 2005-2007 (except for Medicare, which uses a pre-treatment window of only 2007 given data availability issues) and a post-treatment window of 2008-2011. Observations are at the county-month level and are weighted by the county-specific enrollment volume in September 2007. Marion and Lake counties (which are untreated) are excluded from the sample given that they are outliers on a number of dimensions. Standard errors are clustered at the county level.

Table A7. Treatment Effects Using Alternative Diff.-in-Diff. Estimators

Outcomes	Two-Way Fixed Effects (1)	Two-Way Fixed Effects (2)	Stacked Diff-in-Diff (3)	Callaway-Sant'anna Estimator (4)
		A: Post-Treatment Window: 3 Years		
Log SNAP Individuals	-0.0990***	-0.0989***	-0.1145***	-0.1242***
	(0.0258)	(0.0257)	(0.0286)	(0.0279)
Log TANF Individuals	-0.1851***	-0.1857***	-0.2195***	-0.2177***
	(0.0403)	(0.0400)	(0.0492)	(0.0690)
Log Medicaid Individuals	-0.0297*	-0.0303*	-0.0335**	-0.0365***
	(0.0156)	(0.0156)	(0.0159)	(0.0108)
County-Months	7,200	7,400	11,400	7,400
		B: Post-Treatment Window: 6 Years		
Log SNAP Individuals	-0.0730***	-0.0726***	-0.0816***	-0.0995***
	(0.0209)	(0.0209)	(0.0222)	(0.0307)
Log TANF Individuals	-0.1306**	-0.1285**	-0.1635***	-0.1490
	(0.0509)	(0.0515)	(0.0585)	(0.1060)
Log Medicaid Individuals	-0.0310**	-0.0309**	-0.0326**	-0.0380***
	(0.0150)	(0.0149)	(0.0144)	(0.0143)
County-Months	10,500	10,500	17,000	10,500
Balanced On	Event Time	Calendar Time	Event Time	Calendar Time

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

Data Sources (public-use): County-level SNAP, TANF, and Medicaid program records (2004-2014), Census population estimates (2004-2014)

Notes: This table shows regression estimates of log total individuals receiving SNAP, TANF, or Medicaid on a binary indicator for receiving IBM automation and being in the post-treatment period, controlling for county- and month-fixed effects as well as county- and year-varying covariates that include log total population and population subgroups by race, gender, and age. Observations are at the county-month level and are weighted by the county-specific enrollment volume in September 2007. Marion and Lake counties (which are untreated) are excluded from the sample given that they are outliers on a number of dimensions. All regressions use a pre-treatment window of 12 quarters before automation. Panel A shows regression estimates using a post-treatment window of 12 quarters after automation, and Panel B shows regression estimates using a post-treatment window of 24 quarters after automation. Column 1 shows standard two-way fixed effects estimates with panel observations balanced on event time (12 quarters before treatment and 12 or 28 quarters after treatment). Column 2 shows two-way fixed effects estimates with panel observations balanced on calendar time (from October 2004 to July 2011 for Panel A and from October 2004 to July 2014 for Panel B). Column 3 shows stacked difference-in-differences estimates where the counties affected by each treatment wave are compared against never-treated counties (balanced on event time), and estimates are obtained after stacking these comparisons. Column 4 shows aggregated difference-in-differences effects from the Callaway-Sant'anna (2021) estimator that is robust to settings with more than two time periods and variation in treatment timing. Standard errors are clustered at the county level.

Table A8. Goodman-Bacon (2021) Decomposition of Diff.-in-Diff. Estimates

	Coefficient (1)	Total Weight (2)
	A: Post-Treatment Window: 3 Years	
SNAP Individuals		
Main TWFE Estimate	-0.0989	
Timing Group Comparisons	0.0178	0.1065
Never Treated vs. Timing Group Comparisons	-0.1264	0.8281
Within-Group Variation from Covariates	0.0589	0.0654
TANF Individuals		
Main TWFE Estimate	-0.1857	
Timing Group Comparisons	0.1457	0.1022
Never Treated vs. Timing Group Comparisons	-0.2253	0.8429
Within-Group Variation from Covariates	-0.1937	0.0549
Medicaid Individuals		
Main TWFE Estimate	-0.0303	
Timing Group Comparisons	0.0027	0.0991
Never Treated vs. Timing Group Comparisons	-0.0511	0.8252
Within-Group Variation from Covariates	0.1533	0.0757
	B: Post-Treatment Window: 6 Years	
SNAP Individuals		
Main TWFE Estimate	-0.0726	
Timing Group Comparisons	0.0145	0.0891
Never Treated vs. Timing Group Comparisons	-0.1007	0.8563
Within-Group Variation from Covariates	0.2256	0.0546
TANF Individuals		
Main TWFE Estimate	-0.1285	
Timing Group Comparisons	0.1182	0.0859
Never Treated vs. Timing Group Comparisons	-0.1659	0.8658
Within-Group Variation from Covariates	0.1034	0.0484
Medicaid Individuals		
Main TWFE Estimate	-0.0309	
Timing Group Comparisons	-0.0011	0.0826
Never Treated vs. Timing Group Comparisons	-0.0555	0.8545
Within-Group Variation from Covariates	0.2638	0.0630

Data Sources (public-use): County-level SNAP, TANF, and Medicaid program records (2004-2014), Census population estimates (2004-2014)

Notes: This table shows estimates from the difference-in-differences decomposition described in Goodman-Bacon (2021). The decomposition shows comparisons among treatment groups (based on timing of treatment wave), comparisons of treatment groups to never-treated counties, and the component of the effect due to within-group variation from covariates. Outcomes are log SNAP individuals, TANF individuals, and Medicaid individuals, and regressions control for county-fixed effects, month-fixed effects, and county- and year-varying covariates that include log total population and population subgroups by race, gender, and age. Observations are at the county-month level and are weighted by the county-specific enrollment volume in September 2007. Marion and Lake counties (which are untreated) are excluded from the sample given that they are outliers on a number of dimensions. Observations are balanced in calendar time (from October 2004 to July 2011 for Panel A and from October 2004 to July 2014 for Panel B).

Appendix Model of Program Participation and Costs

Set-Up and Program Participation Decision

This section lays out a simple model of program participation that attempts to parameterize program enrollment costs in a realistic fashion (embedding the characteristics of some of the most important means-tested transfers in the U.S.). Specifically, this entails allowing enrollment costs to vary not only across applicant types but also across programs. Later in this section, enrollment costs are also allowed to vary across application stages. These features contrast with those of the models in Deshpande and Li (2019) and Finkelstein and Notowidigdo (2019), which allow costs to vary only across applicant types. This model will then be used to clarify the channels through which changes in enrollment costs can affect program enrollment and targeting.

Suppose there are two types of individuals indexed by j: those who are "low-skill" (L) and those who are "high-skill" (H). Each type can decide whether or not to enroll in a program $b \in \{1, ..., \mathcal{B}\}$. For simplicity, the enrollment decision (and thus the associated benefits and costs of enrollment) for a given program is assumed to be independent from the enrollment decision for another program. Furthermore, in the static version of this model, individuals are assumed to make their enrollment decisions at some hypothetical (unspecified) stage of the application process. In the dynamic version of this model, individuals are assumed to decide whether to enter or exit a program depending on the application stage (initial application or recertification) at which they appear.

We first describe the program participation decision in the static version of this model. An individual of type $j \in \{L, H\}$ who decides to participate in program b receives utility u_b^j, while the same individual who decides not to participate in program b receives utility u_{nb}^j. Enrolling in a program incurs some cost c_b^j, which is parameterized as $c_b^j = \bar{\eta}\theta_b\kappa^j$. Note that this enrollment cost is made up of three components. The first is a constant parameter $\bar{\eta}$ that is set by the government and is common across programs and applicant types. For example, the welfare agency may require that all applicants to all programs submit documentation verifying their income sources and expenses. The second term is a program-specific cost shifter θ_b, which reflects how $\bar{\eta}$ varies across programs. Even though a single welfare agency may administer multiple programs, programs may have different rules and application procedures that lead to different costs. The third parameter (κ^j) reflects how enrollment costs vary across applicant types and also across application stages. Neoclassical models that typically assume higher opportunity costs of time for high-skill applicants suggest that $\kappa^H > \kappa^L$ (Nichols and Zeckhauser 1982). Conversely, models that emphasize cognitive costs concentrated among low-skill applicants suggest that $\kappa^L > \kappa^H$ (consistent with behavioral economics models of scarcity).

Given this setup, individuals enroll in program b if and only if the total payoff from enrolling is greater than or equal to the total payoff from not enrolling:

$$u_b^j - \left(\bar{\eta}\theta_b\kappa^j + c\right) \geq u_{nb}^j, \tag{1}$$

where c is some idiosyncratic cost parameter that is distributed according to $f_j(c)$ with support $[\underline{c}, \bar{c}]$. Equation (1) implies that there is some threshold c_b^{j*} defined as follows:

$$c_b^{j*} = u_b^j - \bar{\eta}\theta_b\kappa^j - u_{nb}^j, \tag{2}$$

for which individuals would enroll if $c \leq c_b^{j*}$ and not enroll otherwise. "Applying" and "enrolling" can be thought of interchangeably, with the difference being some constant probability of rejection captured within either the utility term u_b^j or the cost term c_b^j.

Let p_b^L (p_b^H) designate the share of the population that is eligible for program b and of type L (H). The share of the population enrolling in program b, decomposed by type j, can be expressed as follows:

$$\begin{aligned}
A_b &= p_b^L \ \Pr(\text{enroll}|j = L) + p_b^H \ \Pr(\text{enroll}|j = H) \\
&= p_b^L \int_{\underline{c}}^{c_b^{L*}} f_L(c)dc + p_b^H \int_{\underline{c}}^{c_b^{H*}} f_H(c)dc \\
&= p_b^L \left(\frac{c_b^{L*} - \underline{c}}{\bar{c} - \underline{c}}\right) + p_b^H \left(\frac{c_b^{H*} - \underline{c}}{\bar{c} - \underline{c}}\right),
\end{aligned} \tag{3}$$

where the final expression follows from the simplifying assumption that $c \sim \text{Uniform}[\underline{c}, \bar{c}]$ for types L and H (see Deshpande and Li 2019).

Static Effects of Application Costs on Program Enrollment

Suppose in the static model that the welfare agency decides to change $\bar{\eta}$ (the constant component of the application cost) by imposing additional administrative burdens that apply to all programs under its purview.

Proposition 1. *The change in the share of total enrollees in program b following a change in the enrollment cost $\bar{\eta}$ is given by:*

$$\frac{\partial A_b}{\partial \bar{\eta}} = -\left(\frac{\theta_b}{\bar{c} - \underline{c}}\right)\left(p_b^L \kappa^L + p_b^H \kappa^H\right). \tag{4}$$

A program experiences a larger decline in enrollment if it has a higher enrollment cost (θ_b) or if more of its applicants are of a given type that is associated with higher costs.

To obtain the expression in equation (4), one simply substitutes equation (2) into equation (3) and takes the partial derivative of equation (3) with respect to $\bar{\eta}$.

Equation (4) illuminates the channels through which a common set of administrative burdens may yield differential effects on enrollment across programs. The role of the program-specific cost shifter θ_b is obvious; a program with a higher value of θ_b (e.g., because it has a more complicated application form) will experience a larger reduction in enrollment, *ceteris paribus*. Differences across programs in the composition of applicants can also play a role if costs vary across applicant types. Assuming for example that low-skill individuals face higher costs than high-skill individuals ($\kappa^L > \kappa^H$), then a program with more low-skill applicants will experience a larger reduction in enrollment than a program with fewer low-skill applicants (and vice-versa). It is an empirical question as to whether low- or high-skill applicants face higher costs.

Dynamic Effects of Application Costs on Program Enrollment

We now outline a dynamic version of the model using a Markov Chain representation of the program caseload (see, e.g., Haider and Klerman 2002, Klerman and Haider 2004). This enables an analysis of the effects of changes in application costs on changes in enrollment along the entry and exit margins. Suppose that individuals can be in one of two "states" in a given period: either receiving a program or not receiving a program. For simplicity, individuals are all assumed to be of some unspecified type, although this framework can easily be extended to accommodate multiple types (as in the static version). The share of the population receiving a program b in period t is given by $A_{b,t}$. The Markov chain model of the program caseload can thus be represented as:

$$\begin{bmatrix} A_{b,t} \\ 1 - A_{b,t} \end{bmatrix} = \begin{bmatrix} 1 - \lambda_{b,t}^{exit} & \lambda_{b,t}^{enter} \\ \lambda_{b,t}^{exit} & 1 - \lambda_{b,t}^{enter} \end{bmatrix} \begin{bmatrix} A_{b,t-1} \\ 1 - A_{b,t-1} \end{bmatrix}, \tag{5}$$

where $\lambda_{b,t}^{enter}$ and $\lambda_{b,t}^{exit}$ are the transition probabilities of entering a program conditional on not previously being enrolled (i.e., the entry rate) and exiting a program conditional on previously being enrolled (i.e., the exit rate), respectively.

From equation (5), one can derive the following equality:

$$A_{b,t} = \left(1 - \lambda_{b,t}^{exit}\right) A_{b,t-1} + \lambda_{b,t}^{enter} \left(1 - A_{b,t-1}\right), \tag{6}$$

where the total share of recipients in a given period is the sum of those continuing to receive a program from the prior period and those entering a program in the current period. The steady state a_b (the value such that $A_{b,t} = A_{b,t-1}$) can be calculated from Equation (6) as:

$$a_b = \frac{\lambda_b^{enter}}{\lambda_b^{exit} + \lambda_b^{enter}} = \frac{\lambda_b^{enter}}{\omega_b \lambda_b^{exit/pd} + \lambda_b^{enter}}. \tag{7}$$

The last equality follows from $\lambda_b^{exit} = \omega_b \lambda_b^{exit/pd}$, where ω_b reflects the frequency of recertifications (or interactions with the welfare agency) for program b between time periods $t-1$ and t and $\lambda_b^{exit/pd}$ reflects the exit rate during a given recertification.

Drawing from the original framework in equation (1), suppose that individuals who are not yet enrolled decide to enter program b if the payoff from entering is greater than or equal to the payoff from not entering: $u_b^{enter} - (\bar{\eta}\theta_b^{enter} + c) \geq u_{nb}^{enter}$. Similarly, individuals who are enrolled decide to exit program b if the payoff from exiting is greater than the payoff from remaining enrolled: $u_{nb}^{cont} > u_b^{cont} - (\bar{\eta}\theta_b^{cont} + c)$. Under the assumption that $c \sim \text{Uniform}[\underline{c}, \bar{c}]$, the expression for the steady state enrollment in equation (7) can then be rewritten as:

$$a_b = \frac{\left(\frac{u_b^{enter} - \bar{\eta}\theta_b^{enter} - u_{nb}^{enter} - \underline{c}}{\bar{c} - \underline{c}}\right)}{\omega_b \left(1 - \frac{u_{nb}^{cont} + \bar{\eta}\theta_b^{cont} - u_b^{cont}}{\bar{c} - \underline{c}}\right) + \left(\frac{u_b^{enter} - \bar{\eta}\theta_b^{enter} - u_{nb}^{enter} - \underline{c}}{\bar{c} - \underline{c}}\right)}. \tag{8}$$

Proposition 2. *The changes in the entry rate, exit rate, and steady state enrollment in program b following a change in the enrollment cost $\bar{\eta}$ are respectively given by:*

$$\frac{\partial \lambda_b^{enter}}{\partial \bar{\eta}} = -\frac{\theta_b^{enter}}{\bar{c} - \underline{c}}, \tag{9}$$

$$\frac{\partial \lambda_b^{exit}}{\partial \bar{\eta}} = -\frac{\omega_b \theta_b^{cont}}{\bar{c} - \underline{c}}, \tag{10}$$

$$\frac{\partial a_b}{\partial \bar{\eta}} = -\left(\frac{\theta_b^{enter}}{\bar{c} - \underline{c}}\right)\left(\frac{1}{\omega_b \lambda_b^{exit/pd} + \lambda_b^{enter}} - \lambda_b^{enter}\right) - \left(\frac{\theta_b^{cont}}{\bar{c} - \underline{c}}\right)\frac{\omega_b}{\lambda_b^{enter}}. \tag{11}$$

A program experiences a larger decline in the entry rate if it has a higher enrollment cost at entry (θ_b^{entry}), while it experiences a larger increase in the exit rate if it has more frequent recertifications (ω_b) and/or a higher recertification cost (θ_b^{cont}). The corresponding decline in the steady state enrollment is a function not only of the aforementioned parameters but also the baseline entry and exit rates λ_b^{enter} and $\lambda_b^{exit/pd}$.

To obtain the expressions in equations (9), (10), and (11), one simply takes the partial derivative of the full expression in equation (8) - or separate terms from the numerator or denominator - with respect to $\bar{\eta}$. Note that all of these expressions are unambiguously negative, meaning that an increase in the enrollment cost $\bar{\eta}$ necessarily leads to declines in the entry rate, exit rate, and steady state enrollment level.

Effects of Application Costs on Program Targeting

Finally, we discuss how this model can provide some simple theoretical context for the channels through which the "targeting" effects of administrative burdens may operate. Recall from equation (4) that the change in total enrollment in response to a change in application costs can be expressed as a weighted sum of the enrollment changes for low-skill and high-skill individuals (multiplied by some program-specific constant). Using this framework, an increase in the application cost $\bar{\eta}$ would worsen targeting if $\kappa^L > \kappa^H$ (as low-skill individuals are screened out more than high-skill individuals), while it would improve targeting if $\kappa^H > \kappa^L$ (as high-skill individuals are screened out more than low-skill individuals).

Note that this framework accounts only for mean differences in type-specific enrollment costs. However, focusing only on mean differences can mask more subtle differences in the distribution of type-specific enrollment costs. To see this, suppose that high-skill individuals H can be separated into two groups: type H_1 with a higher opportunity cost of time (who can be thought of as "more" high-skill) and type H_2 with a lower opportunity cost of time (who can be thought of as "less" high-skill). Furthermore, suppose that low-skill individuals L can be separated into two groups: type L_1 with lower cognitive ability (who can be thought of as "more" low-skill) and type L_2 with greater cognitive ability (who can be thought of as "less" low-skill).

Breaking up low- and high-skill individuals into these four sub-types, one can then rewrite the change in total enrollees following a change in the enrollment cost - previously given in equation (4) - as follows:

$$\frac{\partial A_b}{\partial \bar{\eta}} = -\left(\frac{\theta_b}{\bar{c}-\underline{c}}\right) \sum_{j=L,H} p_b^j \underbrace{\left[\frac{p_b^{j_1}}{p_b^j}\kappa^{j_1} + \left(1 - \frac{p_b^{j_1}}{p_b^j}\right)\kappa^{j_2}\right]}_{\text{Enrollment Change for Type } j}, \tag{12}$$

where $p_b^{L_1}$ ($p_b^{H_1}$) denotes the share of the population that is eligible for program b and of type L_1 (H_1). Thus, a comparison of the overall enrollment responses for types L and H (given a change in $\bar{\eta}$) amounts to comparing the expression in brackets for types L and H.

Proposition 3. *An increase in the application cost $\bar{\eta}$ improves targeting overall if the* *weighted combination of cost shifters for high-skill individuals exceeds the weighted combination of cost shifters for low-skill individuals:*

$$\frac{p_b^{H_1}}{p_b^{H}}\kappa^{H_1} + \left(1 - \frac{p_b^{H_1}}{p_b^{H}}\right)\kappa^{H_2} \geq \frac{p_b^{L_1}}{p_b^{L}}\kappa^{L_1} + \left(1 - \frac{p_b^{L_1}}{p_b^{L}}\right)\kappa^{L_2}, \tag{13}$$

where the weights are the shares of individuals of a given sub-type (conditional on being of a given type).

Suppose that both the neoclassical and behavioral models of targeting hold, implying that enrollment costs are highest for both the most high-skill (H_1) and most low-skill (L_1) individuals. In other words, there may be non-monotonic targeting effects along the ability spectrum. Formally, this is given by $\min\left\{\kappa^{H_1}, \kappa^{L_1}\right\} > \max\left\{\kappa^{H_2}, \kappa^{L_2}\right\}$. If this is the case, then equation (13) - which compares overall weighted means - may reveal no differences in overall targeting effects, despite certain types of individuals clearly being screened out more than others.

One can extend this framework to analyzing targeting effects of administrative burdens separately at entry and exit. Specifically, let $p_b^{j,enter}$ denote the share of the population that is of type j and appears at the initial application stage for program b. Similarly, let $p_b^{j,cont}$ denote the share of the population that is of type j and appears at the recertification stage for program b. Then, following the set-up in equation (13), an increase in the application cost $\bar{\eta}$ will improve targeting at a given stage s if:

$$\frac{p_b^{H_1,s}}{p_b^{H,s}}\kappa^{H_1} + \left(1 - \frac{p_b^{H_1,s}}{p_b^{H,s}}\right)\kappa^{H_2} \geq \frac{p_b^{L_1,s}}{p_b^{L,s}}\kappa^{L_1} + \left(1 - \frac{p_b^{L_1,s}}{p_b^{L,s}}\right)\kappa^{L_2}. \tag{14}$$

It is reasonable to assume that higher-skill are more likely to appear at initial application, while lower-skill individuals are more likely to appear at recertification. Formally, this assumes that $p_b^{H_1.entry}/p_b^{H,.entry} > p_b^{L_1.entry}/p_b^{L,.entry}$ and $p_b^{H_1.exit}/p_b^{H,.exit} < p_b^{L_1.exit}/p_b^{L,.exit}$. Given our prior assumption that enrollment costs are highest for the most high-skill and most low-skill individuals, this suggests that the differential selection of certain types into a given application stage may lead to improved targeting at entry but worse targeting at exit.