Title

ESSAYS IN PUBLIC ECONOMICS AND WEALTH MOBILITY

Author

Lin, Jou Chun

Publication Date

2024

ESSAYS IN PUBLIC ECONOMICS AND WEALTH MOBILITY

By

JOU-CHUN LIN
DISSERTATION

Submitted in partial satisfaction of the requirements for the degree of

DOCTOR OF PHILOSOPHY

in

ECONOMICS

in the

OFFICE OF GRADUATE STUDIES

of the

UNIVERSITY OF CALIFORNIA

DAVIS

Approved:

Marianne Bitler, Chair

Monica Singhal

Jenna Stearns

Committee in Charge

2024

Contents

Abstract

My dissertation falls under the broad interest of redistribution of economic resources. The first chapter examines the efficiency of eligibility criteria for transfer programs in the United States, specifically in balancing between screening purposes and administrative burdens. The second chapter investigates the rationale behind states' decisions to adopt different policies for transfer programs. The third chapter studies the multi-generational wealth correlations among the Taiwanese population.

Welfare programs in the United States aim to target beneficiaries and combat fraud through means-testing approaches. Chapter 1 evaluates the efficiency of income and asset limits in the Supplemental Nutrition Assistance Program (SNAP) in the United States, with a focus on the state option "Broad-Based Categorical Eligibility (BBCE)." BBCE allows states to eliminate asset limits and raise income thresholds to broaden eligible populations. Leveraging state variations from 1996 to 2007, I find that the states adopting BBCE reduced SNAP administration costs by 15-25% without an increase in fraud cases. Moreover, the eligible population only increased by about 2%, implying that 25% of the costs were spent to rule out 2% of the eligible population. Additionally, there is suggestive evidence of increased program take-up among households already eligible under previous rules, potentially driven by the simplified requirements. These findings indicate that existing asset limits and income thresholds impose unnecessary restrictions, incurring high costs for government agencies and deterring participation without effectively targeting or preventing fraud.

In Chapter 2, I attempt to understand states' decision-making behaviors through the lens of office-seeking agents' perspectives. State variations in SNAP policies have been widely utilized as sources of quasi-experiments, yet little rationalization behind such variations has been done. I test factors including voter preferences, political ideologies, fiscal constraints, administrative incentives, and economic circumstances. I examine five SNAP policies: the Broad-Based Categorical Eligibility (BBCE), vehicle exemptions, face-to-face interview waiver, simplified reporting system, and transitional benefits. Leveraging the exogenous change of federal regulations that made these

policies available, I find that BBCE and vehicle exemptions were most utilized by states as economic stabilizers. While Democratic party controls and voters' liberal-leaning preferences affect BBCE, vehicle exemptions, simplified reporting, and transitional benefits, the magnitude appears to be secondary to economic factors or small. Notably, the SNAP error rates continue to emerge in explaining policy adoptions. In sum, SNAP policies appear to be a blend of political, economic, and administrative considerations, depending on the specific function served by each policy.

In Chapter 3, we use millions of records from a public registry in Taiwan to estimate the wealth correlations among Taiwanese kinship members. For a long time, social scientists have used correlations in social status, measured by such characteristics as schooling, income, or occupation, across family members to capture family resemblance in social status. We measure the wealth correlations from the closest parent–child pairing to the farthest kinship ties, with only 1/32 genetic relatedness. Based on this wealth correlation, we present a complete picture of economic similarity among kin members. These correlations give us a better grasp of the hitherto obscure Chinese family structure than that of mechanical genetic relatedness. We obtain statistical evidence to support the following hypotheses: Family members' wealth resemblance to male egos is stronger than to female egos, wealth correlations are larger along patrilineal lines than along matrilineal counterparts, wealthy families have larger correlations within the nuclear family members but smaller correlations outside it, and adopted children have weaker wealth resemblance with close relatives.

Acknowledgments

I would like to thank my Ph.D. advisors Marianne Bitler, Monica Singhal, Jenna Stearns, and Marianne Page for their dedication to my success in pursuing this degree. Marianne Bitler is the best primary advisor I could ever hope for. She is not only very knowledgeable but also caring. I always felt safe sharing any thoughts or concerns, and never once did she make me feel unheard. I cannot count how many times I have told others that Professor Bitler is an angel and how lucky I am to have her as my advisor.

Monica Singhal is the person I go to when I need someone to cut through the weeds of thoughts in my brain and lift me out of chaos. I am also grateful that she organized a study group among her advisees. The tips and know-how she shared with the group were incredibly useful.

Jenna Stearns is insightful in pinpointing research questions and suggesting analytical techniques. Her comments always bring a breath of fresh air, providing new perspectives on my project.

Last but not least, Marianne Page provided invaluable opportunities for me to practice communicating my research. I cannot express enough appreciation for her reading through my job talk slides and sending me page-by-page comments, which were incredibly helpful and made me feel well-supported.

I would also like to thank all the faculty members of the UC Davis Economics Department. I enjoyed the courses I took (yes, including the prelim ones). I loved how much the faculties engaged in seminars and brown bags, with special thanks to those who attended my practice job talk. I am grateful to have been pursuing my degree in such an open-minded and positive department.

I thank my social safety net study group members — Eunju Lee, Seojung Oh, and Chang Jae Lee. I am glad I found them just when I was about to give up forming any type of group. They showed me that with the right people, it is possible to not feel alone even when working on a solo project.

I also thank my co-authors C.Y. Cyrus Chu and Kamhon Kan, who generously agreed to let me use our collaborative work for my dissertation. I especially thank C.Y. Cyrus Chu, who was also my master's advisor, for his mentorship and for offering me the privilege of accessing the Taiwan

Income and Wealth Tax database. It was such a rare opportunity and I still feel very blessed to have that.

I thank my parents for taking good care of themselves physically and financially, so I never had to worry about them while being abroad. I appreciate my father for not judging my decision to pursue a Ph.D., contrary to what his family might have done. I thank my mother for regularly checking in with me and trying her best to understand what I am doing. I would also like to thank my eldest sister, Pei-Chun, for keeping my parents company when two of their three daughters are away. I thank my second sister, Pei-Ju, for bringing joy to the family with her lovely children.

Before coming here, I never thought that gratitude would be the biggest takeaway from this program. No matter how much I love doing research or how pleasant it is to live in California, this program is hard. At some point, the experience surpassed being just a personal pursuit and became about how much help you could get. This has been perhaps the most positive experience of my entire life, and I thank all the wonderful people I have encountered who have helped me accomplish this.

Chapter 1

Designing Efficient Welfare Programs: Evidence from SNAP's BBCE Expansion

1.1 Introduction

Transfer programs in the United States are often designed with complex requirements and rules. Policy-makers use such designs as a screening mechanism to target those who would benefit the most from income redistribution. This is rationalized in economic theories as optimal program designs, which suggest imposing certain restrictions to transfer programs for targeting efficiency (Kleven & Kopczuk, 2011; Nichols & Zeckhauser, 1982). On the other hand, theories also emphasize that these restrictions should balance the costs that come with them, as it takes resources for government agencies to enforce the rules and for potential beneficiaries to comply. However, theories do not provide precise guidelines for achieving this balance, leaving us with only glimpses of insight through empirically examining the rules we currently implement.

Several empirical studies have examined different aspects of program designs. For instance, recertification processes are consistently found to be inefficient because most beneficiaries who drop out during the process remain eligible (Gray, 2019; Homonoff, Rino, & Somerville, 2022; Unrath, 2021). It is also found that the current program designs create information barriers and difficulties in navigating the application processes for eligible individuals (Currie, 2006; Ko & Moffitt, 2022).

Nevertheless, the extent to which we should remove the program restrictions and their consequences remains to be determined.

This paper evaluates how relaxing the income and asset limits would affect program administrative costs, fraud incidents, and program take-up. Income and asset limits are the most ubiquitous requirements for transfer programs. While the limits are set to screen out more well-off households and prevent fraud, they constitute a large portion of the administrative burdens. Potential beneficiaries are asked to provide various documentation of their income and assets. Completing the paperwork requires physical time and effort and creates mental stress and confusion. In addition, caseworkers spend a tremendous amount of time verifying the information, costing government resources as well. Due to a lack of variation over time, these income and asset limits are rarely examined in terms of achieving their goals and collateral costs. This paper is one of the first to provide such analyses utilizing a state option to expand eligibility for the Supplemental Nutrition Assistance Program (SNAP), formerly the Food Stamp Program.

Authorized by the federal government in 2000, the "Broad-Based Categorical Eligibility (BBCE)" allows states to relax the income and asset limits for a more general set of low-income households to be qualified for SNAP. States also choose to qualify any household through this category, including those already eligible. I focus on the states that eliminated asset limits, raised income thresholds, and applied them to every household. Under such changes, admitting a household to the program is much simpler than traditional eligibility rules. I measure the changes in administrative costs, the number of fraud cases, the size of the newly eligible households and their characteristics, and the take-up behaviors of households who were already eligible before BBCE.

My research design exploits the variation across states and years in BBCE adoptions. Observation periods range from 1996 to 2007. During this period, thirteen states adopted any BBCE policies, and six adopted the most generous form that reduced three income and asset limits to only one income limit. The comparison group consists of states that adopted BBCE between 2008 and 2012, which includes 28 states[1]. I employ an event study specification using the interaction-

[1] The rest of the states only adopted after 2015 or have never adopted, suggesting a very different behavior from most states. I choose the states most likely to satisfy the parallel trends assumption, formally justified in later sections.

weighted estimator proposed by Sun and Abraham (2021).

To analyze changes in eligible households, I use a micro-simulation dataset that includes simulated eligibility for various programs on the sample of the Current Population Survey — Annual Social and Economic Supplement (CPS-ASEC). The data is published as the Transfer Income Model, Version 3 (TRIM3)[2], maintained by the Urban Institute. In Appendix A.2, I show that this dataset performs well in correcting the under-reporting issue of program participation in CPS (Meyer, Mok, & Sullivan, 2009, 2015), and captures similar profiles of SNAP participants with the administrative data[3]. Building on the SNAP eligibility provided by TRIM3, I identify the already-eligible households by predicting the likelihood of a household satisfying the traditional rules of SNAP based on pre-expansion characteristics. This way, I resolve the problem that households might change their behaviors once BBCE is implemented; that is, their observed income post-BBCE does not represent how they would behave had BBCE not been implemented. From here on, I will call these households "already-eligible" or "always-eligible" interchangeably.

Results show that adopting the most generous form of BBCE reduced state administrative costs by 15-25%, saving over 100 million dollars annually. Meanwhile, there was no indication of an increase in eligibility fraud. Moreover, the size of households eligible only through BBCE constitutes only about 2% of all eligible households, implying that the reduced administrative costs were targeted at these 2% households. Their characteristics show that these are larger-sized households and are more likely to have children — not necessarily more "well-off" households. I also find suggestive evidence of increases in program take-up of the already-eligible households, most likely driven by the streamlined administrative processes. The increase in take-up is particularly observed among those eligible for short-term periods, spanning one to six months in the year. These households are more prone to income fluctuations and could benefit from consumption smoothing utility by enrolling in SNAP. Across all outcomes, the effects from the most generous form of BBCE are more prominent than any BBCE, further suggesting that the effects stem from changes in rules

[2]Information presented here is derived in part from the Transfer Income Model, Version 3 (TRIM3) and associated databases. TRIM3 requires users to input assumptions and/or interpretations about economic behavior and the rules governing federal programs. Therefore, the conclusions presented here are attributable only to the author of this paper.

[3]SNAP Quality Control Dataset published by USDA

rather than other channels, such as raising awareness due to new policies.

This study contributes to three strands of the literature. Firstly, within the optimal program design literature, I provide one of the first empirical evidence showcasing the generally efficiency-improving outcomes resulting from the relaxation of income and asset limits. Studies on this strand rely on structural models and calibrations (Golosov & Tsyvinski, 2006; Wellschmied, 2021), and rarely leverage quasi-experiments. Thus, this research acts as a bridge between program design in theory and its real-world implementation. Secondly, within the incomplete take-up literature, the study provides another piece of evidence illustrating how administrative burdens can potentially hinder program take-up. The term "administrative burdens" generally refers to the costs of applying to a public program, including the learning and psychological costs (Herd & Moynihan, 2018). Causal evidence on how administrative burdens affect take-up is manifold. For example, lack of information or assistance (Aizer, 2003, 2007; Finkelstein & Notowidigdo, 2019), in-person interviews (Homonoff & Somerville, 2021), attitudes of caseworkers (Cook & East, 2023), and physical distance from local offices (Deshpande & Li, 2019) are all found to affect take up. I measure the effects of the income and asset limits on take-up, which are the most common burdens across programs yet have not been widely studied. Lastly, in the policy evaluation papers of BBCE, I conduct a thorough causal estimation of the most general population. I also differentiate between the already and newly eligible populations, a departure from prior studies that predominantly focused on very poor households defined by observed income. Additionally, this paper comprehensively examines the determinants of BBCE adoptions across states and establishes the quasi-random nature of BBCE variations. The findings align with previous research by not rejecting an increase in take-up, reinforcing consistency with prior empirical observations (Anders & Rafkin, 2022; Dickert-Conlin, Fitzpatrick, Stacy, & Tiehen, 2021; Jones, Courtemanche, Denteh, Marton, & Tchernis, 2021; Klerman & Danielson, 2011; Ratcliffe, Mckernan, & Finegold, 2008).

The paper proceeds as follows. Section 2 discusses the institution of SNAP eligibility and BBCE. Section 3 introduces the data and how the sample is constructed. Section 4 describes the identification strategy. Section 5 presents the results. The final section concludes.

1.2 Broad-Based Categorical Eligibility (BBCE)

The Supplemental Nutrition Assistance Program (SNAP), or the Food Stamp Program, is the second largest in-kind transfer program in the United States (following Medicaid). The eligibility criteria for SNAP, particularly the income and asset limits, have been established in federal law since 1980. Despite the scale of the program, the same set of income and asset limits have never received any evaluation on their effectiveness or efficiency other than adjusting for inflation over the past 50 years. The Broad-Based Categorical Eligibility (BBCE), an optional state policy started in 2000, provides a rare opportunity for researchers to examine the federal limits by leveraging the states' changes in these rules. In this section, I introduce the SNAP eligibility rules before and after BBCE, and discuss the outcomes of interest that imply the effectiveness and efficiency of eligibility rules.

1.2.1 Federal eligibility rules for SNAP

As per federal law, citizens of the United States have two pathways to qualify for SNAP. The first is to pass the "income and asset tests", meaning that the household has income and assets that fall below the specified thresholds under the federal rules. For a household without any elderly (60 years old) or disabled members, two separate income tests apply — the gross and net income tests. Gross income is the sum of earned and unearned income, including cash benefits from other public programs. Net income is gross income minus allowed deductions such as child care, shelter/housing, and medical expenses. The federal limit for gross income is 130% times the federal poverty guideline (FPL) and 100% of FPL for net income. A household with elderly or disabled members only has the net income limit to satisfy.

On top of the income limits, the federal law also sets limits for countable resources (referred to as "assets"), which includes cash and bank accounts[4], at $2,750 for households without elderly or disabled members and $4,650 for households with elderly or disabled members.

The second way to qualify for SNAP is to be "categorically eligible", which means being au-

[4] The federal law also sets a vehicle limit at $4,650, but almost all states set a higher vehicle limit nowadays because the federal limit is considered outdated and too restrictive.

tomatically eligible if already qualified for another cash assistance program. More specifically, categorical eligibility applies to households with all members eligible for cash assistance from other means-tested programs, which include the Supplemental Security Income (SSI), General Assistance (GA), Aid to Families with Dependent Children (AFDC)/Temporary Assistance for Needy Families (TANF), and state maintenance-of-effort (MOE) programs. These households are deemed eligible because they have already undergone more restrictive income and asset tests for these other programs[5].

These eligibility requirements only apply to U.S. citizens. Non-citizens can be eligible by satisfying other criteria, mostly regarding their immigration circumstances. Due to the complexity of these criteria, this paper excludes households with only non-citizen members (about 4% of the population) from the analyses.

1.2.2 Changes to eligibility by BBCE

The origin of BBCE traces back to The Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) of 1996, also known as the Welfare Reform. The reform replaced a major welfare program — the Aid to Families with Dependent Children (AFDC) with Temporary Assistance for Needy Families (TANF), which does not cover as many households as AFDC[6]. This change affected the food stamp households who were categorically eligible through AFDC. Those not eligible for TANF had to re-apply for food stamps by passing the federal income and asset tests. Those eligible for TANF may still need to take the first pathway because AFDC was primarily cash benefits, but TANF benefits are at the state's discretion and can be noncash. Noncash TANF benefits were not conferred to categorical eligibility for SNAP in the federal law. However, in 1996, there was no modification in the Food Stamp law except for simply substituting the term AFDC with TANF. As a result, many households were no longer automatically eligible for food stamps and had to go through income and asset tests, which also created burdens for the state agencies.

[5]Note that the food stamp benefit is a function of the aforementioned net income; therefore, the categorically eligible households could still end up with no benefits if their net income is too high.

[6]AFDC was an entitlement program that any qualified families are guaranteed to receive benefits, while TANF is a block grant, which is a fixed amount of federal funds to states.

It was not until 2000 that the USDA amended the regulation[7] on how states can extend categorical eligibility to those qualified for noncash TANF/MOE benefits. In such "broad-based categorical eligibility"[8] options, states can be as generous as deeming a household with one member qualified to receive a brochure printed by a sufficient portion of TANF/MOE funds to be automatically eligible for SNAP. This implies that by designing the eligibility rules for TANF/MOE noncash benefits, states also decide whom to extend SNAP eligibility. The eligibility requirements for these noncash benefits are then generalized as BBCE policies for SNAP.

BBCE policies are much less restrictive than the federal income and asset limits. According to the SNAP Policy Database published by the Economic Research Service (ERS), USDA, and a USDA-commissioned report by Laird and Trippe (2014), most states set higher gross income limits than 130% of the FPL, and some even exempt the net income and asset tests. However, while some states apply BBCE to all households, others only apply BBCE to a subset of households, such as those with dependents or senior/disabled members. I group the states that adopted the most generous form of BBCE — setting a gross income limit higher than the 130% FPL, eliminating the net income and asset limits, and apply them to all households — and name them "BBCE Max". I expect BBCE Max to have larger effects than generally adopting any BBCE policies.

1.2.3 Expected effects on state administration

By imposing a simpler set of eligibility rules, BBCE can reduce states' expenditures on SNAP administration, which are mostly spent on eligibility certification. This expectation is based on previous studies. During site visits to 5 states and 18 local SNAP offices by the U.S. Government Accountability Office, the staff pointed out that verifying assets is especially time-consuming because it requires cooperation from banks, who sometimes would even charge fees for documentation (GAO, 2012). In the quantitative study of Geller, Zic, Isaacs, and Braga (2019), BBCE was associated with lower SNAP administrative costs by 7 percent during FY1999-FY2016. Therefore, this paper aims to identify the causal effects of adopting BBCE on SNAP administrative costs. If the

[7] A letter was first issued on July 14th, 1999, but the regulations were promulgated in 2000 (7 CFR §273.2(j)).

[8] The term "broad-based categorical eligibility" was made official in a USDA policy guidance — "Improving Access to SNAP through Broad-Based Categorical Eligibility" issued on Sep 30th, 2009.

general BBCE reduces administrative costs, BBCE Max should reduce more.

On the other hand, one of the purposes of imposing three tests is to screen and combat fraud, and simplifying them may induce more fraud. However, the effect of BBCE on fraud is ambiguous. While simpler rules may reduce the difficulty of falsifying information, they could also increase the capacity for caseworkers to detect fraud. Moreover, it is unclear whether BBCE increases incentives for fraud large enough to induce more fraud because the benefits are still determined by the same formula, which requires the same information for net income, and food stamp fraud is considered a felony in federal law. With these considerations, this paper will mainly look for evidence of an increase in fraud, and results that suggest otherwise will not be further rationalized in the scope of this paper.

1.2.4 Expected effects on households

Undoubtedly, more households could become eligible under the simplified rules of BBCE. Since the major purpose of the three-tests system is to target those whose welfare would improve the most from SNAP benefits, it is natural to suspect that administration efficiency comes at the expense of targeting efficiency. However, it appears that the expansion of the eligible population by BBCE is minimal. In a study based on 2014 observations, participants who are eligible solely through BBCE accounted for only 8 percent of all SNAP participants and 1.2 percent of all benefits issued (Cunnyngham, 2016). Analyzing the size of eligible populations, I find that BBCE expanded them by approximately only 2%. The newly eligible population possesses some characteristics that suggest more well-off families, such as having higher income and education, but also some others that may suggest otherwise, such as having more children. Moreover, characteristics may change in response to the new rules. For example, eliminating asset limits is associated with increases in savings (Ratcliffe et al., 2016), which definitely increases welfare but more assets are categorized as less "deserving". Thus, other than separating them from the already eligible population, this paper will not draw any conclusion on the deservingness of the newly eligible households.

For those who were already eligible without BBCE, the change of eligibility rules per se does not affect them. However, the streamlined application process can increase their incentive to take up the benefits. First, less documentation is required for the application, especially if the state eliminates asset tests, which would free them from providing asset information and have about half to one page less of the forms to fill. Second, simplified eligibility requirements make it easier for households to comprehend and assess their eligibility. This argument is corroborated by Anders and Rafkin (2022), who found that each ten percentage points of FPL higher gross income limits lead to roughly a 1 percent increase in take-up rates, and the effects are primarily driven by reducing information barriers. Third, higher income thresholds and less paperwork are thought to be associated with lower stigma and make the beneficiaries feel less stigmatized (Currie, 2006), lowering the psychological barrier to applying. The reduction in transaction costs, information barriers, and stigma can be generalized as a reduction in "administrative burdens" for households who are already eligible, and this is the channel of how BBCE could affect their take-up rate.

1.3 Data

Data for this study are collected from multiple sources. In this section, I present these data sources by state- and household-level observations. For state adoptions of BBCE, I use the SNAP Policy Database, which collects states' adoption of various SNAP policies for 50 states and DC from 1996 through 2015, and supplement with Laird and Trippe (2014) for net income limits. The main state-level outcomes of interest are state administrative costs for SNAP and eligibility fraud cases obtained from administrative publications. The household-level outcomes are SNAP eligibility, program take-up, and earnings derived from the Transfer Income Model, Version 3 (TRIM3) sample, as described below.

1.3.1 State Adoptions of BBCE

BBCE has been gaining more popularity over the years. As of July 2023, 44 states and the District of Columbia (DC) have adopted some form of BBCE, 41 of them adopted before 2015. Figure 1.1 shows that 28 states adopted between 2009 and 2012 during the Great Recession. However, studying the effects of BBCE during the Great Recession could be challenging, as the economy expanded the eligible population, and the American Recovery and Reinvestment Act of

2009 raised SNAP benefits along with many other non-SNAP stimulus measures. Therefore, I focus on 1996 to 2007, during which 13 states adopted BBCE, and 6 adopted BBCE Max. South Carolina was the first state to adopt BBCE (Max) in October 2000, followed by Oregon, Maryland, Delaware, Michigan, and North Dakota. From the geographical distribution of adoption timing in Figure 1.3, no apparent correlation exists within regions or adjacent states that constitute the ones adopted during 1996-2007. Another reason to focus on this period is that the treatment was monotonous within states. The states never repealed or modified their BBCE policies once adopted, while many states that adopted them during the Great Recession made multiple changes soon after adoption.

As mentioned in the previous section, I group the states that adopted the most generous form of BBCE: setting a gross income limit higher than 130% FPL while eliminating the net income and asset test, as "BBCE Max". Although analyzing the income and asset tests separately would be ideal, these policies were almost always implemented together. Figure 1.2 shows that only one state solely raised gross income limits, and only one state solely eliminated asset tests (net income was always eliminated with asset tests). Nevertheless, BBCE Max represents the most salient change in program rules, and grouping them captures the synergy of these three modifications.

1.3.2 State-Level Outcomes

Administrative Costs

States report their SNAP administrative costs to the federal government for reimbursement. As per federal law, the reimbursement rates are set at 50%. Some special items are reimbursed at more than 50%; e.g., 100% for employment and training programs, but these account for relatively small portions of total administrative costs. State agencies submit the standardized Form SF-269[9], to the U.S. Department of Agriculture (USDA), which publishes this information in the State Activity Report for each fiscal year.

According to the State Activity Report FY1996-FY2007, over half of the administrative costs are from certification-related activities, namely the collection and verification of eligibility informa-

[9] After 2012, Form SF-269 is replaced by Form SF-425.

tion. The rest of the costs come from various activities such as automatic data processing, benefit issuance (paper vouchers and electronic cards), and fraud control, each constituting no more than 7% of the total costs. This structure of the costs remains similar throughout the years. However, in a study by Geller et al. (2019), the authors raise caution about using the categorized costs for analyses due to potential inconsistencies in how states allocate costs for each activity, though this does not affect the relative portions of each cost item. Therefore, this paper uses total administrative costs as the primary outcome, with the assumption that the certification-related costs are the channel of effects from BBCE.

A minor note is that both the total administrative cost and the federal share of the costs are available in the State Activity Report. Since the structure of the federal share may differ from the overall structure due to different reimbursement rates, I focus on total administrative costs.

Eligibility Fraud

As a part of the administrative costs, states also report their fraud control efforts to the federal government. The State Activity Report provides the total number of investigations and the number of confirmed eligibility fraud cases. Although these reported data only represent detected fraud, we can infer the change of true fraud incidence by the following exercise.

Suppose the true fraud incidence rate is $P(F)$, and the probability of a case being subject to fraud investigation is $P(I)$, or the investigation rate. The conditional probability of a true fraud being investigated is $P(I|F)$, or the fraud detection rate. The conditional probability of an investigation on a true fraud incidence is $P(F|I)$, or the investigation accuracy rate. By Bayes' theorem:

$$P(F) = \frac{P(F|I)}{P(I|F)} P(I)$$

That is, changes in the true fraud incidence rate can be inferred from changes in the investigation accuracy rate, the investigation rate, and the fraud detection rate. $P(F|I)$ can be calculated by dividing the number of confirmed fraud cases by the total number of investigations reported in the State Activity Report. Changes in $P(I)$ can be tested by analyzing the total number of fraud investigations. As for $P(I|F)$, its changes can be inferred from the number of confirmed pre-certification fraud cases. A pre-certification fraud case means the investigation is conducted before certification is completed, meaning the fraud is detected upon application. Contrarily, a post-certification case has completed the certification and received benefits before being investigated. Since $P(I|F)$ measures states' fraud detection rate, more confirmed pre-certification cases indicate a higher ability to catch fraud conditional on the same fraud incidence, which is a likely effect from BBCE since the simplified eligibility rules could also make it easier to detect fraud upon application. In sum, although the true fraud incidence is not observable, its changes could be implied from the reported data.

1.3.3 Household-Level Observations: Transfer Income Model, Version 3 (TRIM3)

TRIM3 is a microsimulation model the Urban Institute maintains under primary funding from the Department of Health and Human Services. Based on the Current Population Survey – The Annual Social and Economic Supplement (CPS-ASEC), TRIM3 simulates eligibility and participation for several federal transfer programs, including SNAP, Medicaid, Supplemental Security Income (SSI), and Temporary Assistance for Needy Families (TANF).

The advantage of using samples from TRIM3 is manifold. A major strength of TRIM3 is its detailed modeling of program rules. This includes state-varying policies and cross-program interactions since it models multiple programs simultaneously. Moreover, TRIM3 performs well in correcting the under-reporting issue with CPS-ASEC by matching external data sources, including administrative data. Figure A.2.1 compares the counts of the total number of SNAP participants between TRIM3, CPS-ASEC, and the administrative data published by USDA. TRIM3 fits the administrative counts much better than the raw CPS-ASEC and thus measures the participation more accurately.

To evaluate how well TRIM3 performs the simulation, we can compare the participants captured by TRIM3 to the administrative data — the SNAP Quality Control Database (QC)[10]. However-

[10] In each month, the state agencies randomly select a sample of SNAP participating units to USDA for quality control review. Approximately 50,000 of these reviewed cases will be published as the SNAP Quality Control Data (QC) for public use. According to the 2016 Technical Documentation of the SNAP Quality Control Data, units determined as eligible and received benefits of at least $1 will be made into the SNAP Quality Control Data, which Mathematica then edits for public use.

ever, although the administrative data collect precise eligibility and benefit-related information, other characteristics not directly required are somewhat non-randomly missing, such as education and race/ethnicity. Therefore, I select some more reliable characteristics in QC, and compare the means in Table A.2.1. Most characteristics are similar across the two datasets except for gross income, which might raise concerns about TRIM3 capturing a more well-off population. Nevertheless, based on the benefit levels being so similar at the mean, the high gross income in TRIM3 may result from wider variation in income measurement rather than an inconsistent population. To test this, I conduct an exercise in which I calculate the eligible benefits using TRIM3 income and QC income, respectively[11]. Figure A.2.2 plots my calculated distributions of eligible benefits for SNAP recipients. TRIM3 renders an almost identical distribution of benefits with QC. The higher income at the mean for TRIM3 appears to be led by a longer right tail in income instead of a mispositioned center, which reassures the population captured in TRIM3 data.

Sample - the always-eligible households

With the SNAP-eligible household sample from TRIM3, I separate those who are newly eligible through BBCE from those who are always eligible regardless of BBCE. The purpose is to measure how much BBCE expands the eligible population and focus on the always eligible households regarding their take-up behavior.

To determine who is always eligible, I construct a model to predict the likelihood of a household passing the three federal income and asset tests. By definition, households with income and assets below the federal thresholds are always eligible regardless of BBCE. However, simply looking at the observed income and assets may not be the best approach for finding them because households may not have the same observed income and assets had there not been BBCE. For example, eliminating asset limits could encourage more savings, resulting in households having higher savings with BBCE than without it. To resolve these potential behavioral shifts, I use the observations from the states and years in which BBCE was not adopted as the training sample to acquire fitted coefficients.

[11] I construct a benefits calculator following the federal formula — Benefits = maximum allotment - 0.3 $\times$ unit net income. The maximum allotment is issued by USDA each year and increases with household size. I input the income data of TRIM3 recipients and QC samples in the calculator and estimate the eligible benefits.

The predictors include virtually fixed household characteristics and state characteristics, which are not subject to household choices:

$$pass^{m}_{ist} = \alpha + \mathbf{head_{ist}} + \mathbf{unit_{ist}} + \mathbf{economy_{st}} + \mathbf{policy_{st}} + \theta_s + \sigma_t + \epsilon_{ist} \tag{1.1}$$

$pass^{m}_{ist}$ is a binary indicator of whether household i in state s passes the federal income and asset tests[12] for m months in year t, $m \in \{\geq 1, 1 \leq m \leq 6, 12\}$. While CPS-ASEC collects annual-level observations, TRIM3 provides monthly levels of simulation[13]. I utilize this monthly information to distinguish households eligible for at least 1 month, 1-6 months, and 12 months. The first group defines the most general always-eligible households, thus capturing the average effects. The latter two groups could examine potential heterogeneity effects since the 6-month group is eligible for a shorter period and the 12-month group is eligible for a longer term, reducing the administrative burden could mean more to the 6-month group, considering the burdens are essentially a fixed participation cost.

$\mathbf{head_{ist}}$ is a vector of household head characteristics, including age, age^2, female, race, education, unemployment, marital status, and disabilities status. The individual disability status is not collected in CPS-ASEC until 2008. Although TIRM3 imputes whether a household contains disabled members, it does not identify individual disability status. I follow the algorithm provided in the USDA's SNAP Quality Control Technical Documentation 2015. This algorithm utilizes the information from individuals' age, SSI participation, worker's compensation, working hours, and labor force participation. In Figure A.3.2, my imputation acquires consistent counts of households with disabled members with TRIM3 and QC. Also, it counts a very similar total number of disabled participants with QC.

[12] I construct some of the income and asset measures in the years that they are missing: household asset for all years, household gross income before 2005, and household net income before 2005. I followed the documentation of TRIM3 and constructed the variables using their approach. Figure A.3.1 compares my imputation to TIRM3 data in available years (2005-2015). I can construct mostly consistent distributions with TRIM3. Although for gross income, there is a small interval between \$500 to \$1,000 that shows some deviation, my imputation appears to allocate the differences to nearby income intervals, which does not cross the federal eligibility threshold (\$1,265 for one member household in 2015).

[13] Earned income is allocated into each month by matching the reported total number of weeks worked in the past year with the monthly employment published by the Bureau of Labor Statistics (BLS). Unearned income is allocated evenly in each month.

$\mathbf{unit_{ist}}$ is a vector of household characteristics, including household size, have members who are children (0-4 years old and 5-17 years old, respectively) and proportions of children, have members who are elderly (more than 60 years old) and proportions of elderly members, have members who have disabilities and proportions of such members, have members who are non-citizens and proportions of such members, have able-bodied adults without dependents and proportions of such members, whether the households are receiving SSI/AFDC/TANF cash assistance, and the decile rank of unearned income among national distribution in year t.

$\mathbf{economy_{st}}$ is the BLS published state unemployment rate in year t, $t-1$, $t-2$, and $t-3$. $\mathbf{policy_{st}}$ include states adopting other food stamp policies relevant to eligibility rules in year t. Such policies include vehicle limits, non-citizen eligibility, and certification periods measured by the share of participants in year t and $t-1$ who are only certified for 1-3 months of benefits and need to re-certify afterward. I also include the federal eligibility parameters that vary by year and household size: the standard deduction for income and the maximum shelter deduction. In addition to food stamp policies, I also include the state minimum wage rate and states' welfare expenditure per capita in year t; both are likely to affect the income and assets of low-income households.

The prediction is made on the entire population who can qualify for the food stamp programs based on their income and asset levels. Therefore, households exclusively comprised of children members and households exclusively comprised of noncitizen members are excluded from the analysis, as their eligibility may involve other criteria. Also, to include more pre-BBCE observations as some states adopted as early as 2000, I include the 1993 to 1995 TRIM3 samples and controls for the 1996 welfare reform waivers.

I define the "always-eligible" households as those who are identified to likely pass the income and asset tests by equation (1.1) and are determined to be eligible in TRIM3. The likelihood thresholds are selected to match the mean of the training sample. The "newly-eligible" households are defined as those who are predicted to not likely pass the income and asset tests while also observed to be eligible.

The prediction by equation (1.1) is fairly precise. It can identify the correct status of $pass_{ist}^{m}$ for 96.9% of the training sample for the at least 1-month always-eligible group. The model is less precise for the 12-month eligible group and the 1-6 month group but is correct for the majority of the sample — 77.9% for the 12-month eligible group and 83.5% for the 6-month group.

Aside from using the ordinary least-squared estimator, I also used Lasso estimators to cross-check with the OLS samples in case of overfitting. Over 96-99% of the Lasso sample overlaps with the OLS sample. The two approaches also have very similar adjusted R-squared, around 0.25-0.26. Appendix Table A.3.1 and A.3.2 present mean characteristics of the Lasso sample, which are almost identical to those of the OLS sample in Table 1.1 and 1.2. In the event study analyses introduced in the following sections, the Lasso sample shows almost identical treatment effects to the OLS sample.

Sample Characteristics

The first two columns of Table 1.1 report the mean characteristics of the at least 1-month always-eligible household by the comparison ("Pre BBCE") and treated ("Post BBCE") states-years. Although some characteristics are statistically different post-treatment, they describe virtually the same population if compared to the newly eligible group in column (3). The characteristics of the newly eligible households are very similar to previous studies (GAO, 2012; Laird & Trippe, 2014): they tend to have larger household sizes, are more likely to have children, and are eligible for lower benefits. The share of newly eligible households is about 2.12% among all eligible households. This small proportion is also consistent with other studies using later years of data, implying that the new rules only expand the eligibility to a small population with distinct characteristics that are not necessarily less needy.

The differences among the always-eligible households between the pre-and post-BBCE state years could be troublesome for identification, as a shifted population confounds the effects of change in decisions and decision-makers. However, the mean differences could come from fixed differences between states or years, which can be controlled for in empirical strategy. To examine this, I run a state and year fixed effect regression on the always eligible households. The dependent variable is whether the state has adopted BBCE/BBCE Max in the year, and the independent variables are some population characteristics. This specification examines if there is a within-state-year difference in population composition by adopting BBCE/BBCE max. In Table 1.3, most characteristics are not statistically different or different in a very small magnitude except for having more household heads with disabilities in BBCE states. BBCE Max states seem to have a much more consistent population, yet the joint test for the 6-month group is significant at 5%. In the event studies conducted in later sections, these population shifts likely happened after 5-7 years of BBCE/BBCE Max adoptions (Figure 1.8), leaving the first five post-event periods as the main focus.

1.4 Empirical Strategy: Sun & Abraham (2021) Event Study Estimator

The identification strategy builds on the variation of BBCE/BBCE Max adoptions across states and years. To estimate the dynamic effects of BBCE while accounting for the differential adoption timing, I use the interaction-weighted estimator (IW estimator) developed by Sun and Abraham (2021). Their approach resolves the issues of contaminated weights in the traditional two-way fixed effect specification (Callaway & Sant'Anna, 2021; De Chaisemartin & D'haultfoeuille, 2018; Goodman-Bacon, 2021), is robust under treatment effect heterogeneity, and allows for time-varying covariates. The estimator is a weighted average of cohort-specific treatment effects. Each cohort represents a treatment timing group, whose treatment effect in each event time is estimated by interacting the cohort dummy with the event time dummy. I then use the sample share of cohorts as weights to calculate the weighted average event coefficients. Equation (1.2) estimates the cohort-specific event study coefficients for the already-eligible households.

$$y_{ist} = \alpha + \sum_{c \in C} \sum_{k \neq -1} \pi_{c,k} 1(\tau_{st} = k) \cdot cohort_s^c + \theta_s + \sigma_t + \mathbf{X_{ist}} \boldsymbol{\Gamma} + \mathbf{W_{st}} \boldsymbol{\Phi} + e_{ist} \tag{1.2}$$

y_{ist} represents the outcome of interest, including take-up and average weekly earnings. Program take-up is measured by whether household i in state s receives food stamp benefits in year t. Average weekly earning is measured by dividing the annual earnings by the total number of weeks worked in year t.

$1(\tau_{st} = k)$ is the event indicator, which equals 1 if state s in year t is k years apart from the first adoption year. $cohort_s^c$ is the cohort indicator of whether state s first adopted BBCE/BBCE Max in year c. The event study coefficient $\pi_{c,k}$ estimates the difference in outcome for cohort c between event time k and the base year ($k = -1$) and comparison states. Each $\pi_{c,k}$ is then weighted by cohort c's sample share in event time k to calculate a single event time coefficient π_k. θ_s and σ_t represent state and year fixed effect, respectively.

$\mathbf{X_{ist}}$ controls for household characteristics: household size, indicators and proportions of household composition, including elderly members, disabled members, children, able-bodied adults without dependents (ABAWD)[14], and non-citizen members. I include whether the household receives SSI/TANF cash benefits to control for categorical eligibility. $\mathbf{X_{ist}}$ also includes household head characteristics: age, citizenship, disability status, ABAWD status, gender, race/ethnicity, education, and marital status.

$\mathbf{W_{st}}$ controls for state-varying factors, including other food stamp policies such as vehicle limits, application aids[15], duration of each certification (t and $t-1$), outreach spending (t and $t-1$), and electronic benefit issuance[16]. I also control for the federally determined standard deduction for income and maximum benefit levels, which vary by household size and years. Aside from food stamp policies, I include states' minimum wage rates and seasonally adjusted unemployment rates[17] (t and $t-1$).

The same approach is also used for state-level outcomes, which include state administrative costs for SNAP and fraud cases. Equation (1.2) is modified to control for other SNAP policies, state, and year-fixed effects.

1.4.1 Comparison group: States that adopt BBCE between 2008 and 2012

The identification strategy relies on the parallel trend assumption, which requires the comparison states to represent the counterfactual trends of the treatment states. The treatment group in

[14] ABAWD members are subject to additional work requirements and time-limit of eligibility as opposed to the general work requirement for households with working age adults.

[15] Application aids include waiving face-to-face interviews, waiving reporting of changes if not related to eligibility change, joining the federally-initiated Combined Application Project to simply application for SSI recipients, operating call centers, and having online application portals.

[16] In 1996, the federal government mandated a change of benefit issuance from paper vouchers to electronic cards by 2002. The transition was completed in 2004.

[17] Unemployment rates are extracted from the U.S. Bureau of Labor Statistics. State minimum wage rates are from the U.S. Department of Labor.

this paper is the 13 states that adopted BBCE/BBCE Max before 2008. This makes the rest of the states "never-treated" during the observation period from 1996 to 2007. However, ten states adopted later than 2015 or never adopted as of July 2023, while 28 states adopted between 2008 and 2012, i.e., during the Great Recession periods. From Figure 1.3, these ten states, marked in gray, have some geographical similarities and relatively small populations. It is also peculiar for states not to utilize BBCE during major economic downturns such as the Great Recession. Expanding welfare programs serves as an economic stabilizer, and BBCE could also alleviate the pressure of demand surge on state administration. With such systematic differences, I chose the 28 states (including DC) that adopted BBCE from 2008 to 2012 as my comparison group.

To examine if the comparison group satisfies the parallel trend assumption, I plot the time series of the outcome variables by treatment and control groups in Figure 1.4. Before 2000, when the BBCE option became an option for the states, total administrative costs, number of fraud cases, take-up rate, and average weekly earnings demonstrated the same directional change and similar slopes across the treatment and comparison states. On the other hand, the total number of investigations and pre-certification fraud cases show some opposite directional changes between the two groups from 1996 to 1999. However, in the longer term, both show decreasing trends over the years. In the following event study results, none of the pre-event differences are statistically significant and are all small in magnitude.

1.4.2 Correlates of BBCE adoptions

Although there is no pre-trend and many relevant factors are controlled for, the estimates could still be biased if the treatment states are systematically selected and thus may have a different counterfactual post-trend from the comparison group. To establish the quasi-random nature of states' BBCE adoption, I try to predict states' adoption of BBCE by some potential rationale for their policy choices. More specifically, four hypotheses are tested: 1) political factors, 2) economic circumstances, 3) fiscal incentives, and 4) SNAP administration. 1) The political factor refers to the electoral benefits of public policies discussed in the political economy literature that governments choose policies to gain more popularity among voters (Afzal, Alesina, & Amarante, 2011; Clinton

& Sances, 2018; Kogan, 2021; Pop-Eleches & Pop-Eleches, 2012). In this case, voters' preferences determine not only states' BBCE adoptions but also how the food stamp program is operated and the take-up rate. I measure the political factor by voters' preferences for welfare programs and voters' negative attitudes toward Black Americans, which is highly associated with political ideology, gathered from The General Social Survey as described in Appendix A.2. 2) Economic circumstances refer to the common practice of governments to expand public programs during recessions. If BBCE was adopted due to the economy, the states could also implement many other uncontrolled policies, and households could be affected by the economy as well. I measure the economic circumstances by the unemployment rate and median household income. 3) Fiscal incentives come from SNAP being a 100% federally-funded program in terms of benefits. States can substitute expenditures from their own revenues with expanding federal programs such as SNAP. I measure states' fiscal incentives by the share of total expenditures over their own sources of revenue. The lower the share, the more incentives there may be to expand federal programs and the more unobserved measures the states could be taking. 4) SNAP administration could be improved by adopting BBCE, specifically by lowering administrative costs and error rates. The federal government periodically evaluates error rates, representing the share of recipients for whom the state falsely determined their eligibility or amount of benefits. The states will receive a penalty if the error rates exceed a certain level for two consecutive years. High SNAP administration costs and error rates are also correlated with the general efficiency of the state government, which could affect the outcome of interests as well.

I test these hypotheses through two specifications. In the first specification, I use the state characteristics averaged between 1996 and 1999 to predict states' adoption of BBCE before 2008:

$$adopt_s = \alpha + \mathbf{X_s}\gamma + u_s \tag{1.3}$$

$adopt_s$ is an indicator of whether the state adopted BBCE before 2008. X_s includes the aforementioned factors, state demographics, the SNAP take-up rate, and the share of the SNAP-eligible population, all averaged between 1996 and 1999. Observations are weighted by the average size of the eligible population. Table 1.4 shows that none of the factors individually predicts adoptions, nor do they jointly explain the adoption of BBCE, which implies that in terms of pre-2000 characteristics, there is no systematic differences between the treatment and comparison states for both BBCE and BBCE Max.

The second specification accounts for unobserved state fixed effects and allows time-varying factors using the state-month level of observations starting from January 2000:

$$bbce_{sym} = \alpha + \mathbf{X_{s,y-1,m}}\gamma + \theta_s + \sigma_y + u_{sym} \tag{1.4}$$

$bbce_{sym}$ indicates whether the state s adopted BBCE in month m of year y. The predictors are the same variables as equation (1.3), but are time-varying and observed in the same month in the previous year or the previous year if observed annually. State and year fixed effects are represented by θ_s and σ_y, respectively. Weights are the same as the last specification. With much more power, Table A.1.1 shows similar results as Table 1.4 — the treated states do not seem to be selected in a particular way.

Another confounding factor is that the states simultaneously implemented other policies not captured by the tested factors and are affecting state administration and household take-up. I find no change in state welfare expenditures (Figure A.4.8). There are also small correlations between between BBCE and other SNAP policies. The most correlated policy was eliminating vehicle limits, which had a Pearson's correlation coefficient of 0.48 with BBCE, and 0.37 with BBCE Max (while others range from 0.04 to 0.28). However, eliminating vehicle limits is very common — 32 states adopted it before 2008, and it is controlled for in the analyses.

1.5 Results

Figure 1.5 shows that both BBCE and BBCE Max significantly reduced state administrative costs. The null pre-period differences show that BBCE/BBCE Max adoptions were not correlated with trends in total administrative costs. As expected, BBCE Max reduced the administrative costs more than general BBCE. In Table 1.5, the aggregate effects[18] in event time 3 to 5 are 3.7 dollars

[18] Simple average of linear combinations of the event coefficients.

(15%) for BBCE and 6 dollars (25%) reduction for BBCE Max. Note that all of the event times (11 years in pre-periods and 7 years in post-periods) are included in equation (1.2), and the full event study plots can be found in Appendix A.4. Because the data for 1996 is unavailable for state total administrative costs, I use the federal share of SNAP administrative costs as an alternative outcome in Figure A.4.3. The mean and treatment effects of the federal share are almost exactly half of those from total administrative costs, corresponding to the fact that the federal government shares 50% of states' administrative costs in most cases. This also implies that BBCE/BBCE Max reduces the type of costs that apply 50% of reimbursement, which are majorly certification-related.

Meanwhile, I find no evidence suggesting an increase in fraud. In Figure 1.6, the total number of fraud investigations is not statistically significantly different for BBCE or BBCE Max. However, the point estimates for BBCE Max are large in magnitude, suggesting an about 57% increase in fraud investigations from events 3 to 5. In the data section, I discussed a positive association between the investigations and the true incidence of fraud. On the other hand, in the lower panel of Figure 1.6, the number of confirmed fraud cases decreased, leading to a slight decrease of 5% in events 3 to 5 for the investigation accuracy rate (Figure 1.7), which is positively associated with the incidence of fraud. The third element is the fraud detection rate, which is negatively associated with the true fraud incidence rate. I measure the states' ability to detect fraud by the share of pre-certification cases among confirmed cases, which, in the lower panel of Figure 1.7, increased by about 31% from event 3 to 5. In sum, though there could be a net increase in true fraud incidence, none of the measures are statistically significant, not rejecting the hypothesis of null effects in fraud.

To interpret the results for the already-eligible households, we need to confirm if the eligible population in the post-treatment periods is the same as in the pre-treatment periods. In the data section, I find slight shifts in the characteristics of the always-eligible households in the general BBCE states. If the populations differ, the results could be led by the selection of households instead of changes in household decisions. Figure 1.8 formally tests this by using equation (1.2) with the always-eligible indicators as the dependent variables and all households nationwide (except for children and noncitizen-only households). The specification examines whether BBCE/BBCE Max systematically changed the likelihood of the same households being always eligible by controlling for household characteristics and relevant state factors. Figure 1.8 shows no clear trend and very small point estimates between the pre- and post-adoption periods for the general always-eligible group (at least 1 month) and the two subgroups.

Figure 1.9 suggests that BBCE Max might have increased take-up rates for the general always-eligible households. After one year of BBCE Max adoption, the take-up rate started to rise and increased by up to 9 percentage points (or about 18%) in the second year. Though the differences dropped in the fourth year, it is likely because the comparison group caught up, as there was a general increasing trend of take-up in Figure 1.4. The aggregate effects of BBCE Max from event 0 to 3 is about 5-6 percentage points increase (about 10%) for the at least 1-month eligible group. In Figure 1.10, the increase was majorly led by the short-term eligible group (1-6 months), whose aggregate effect from event time 0 to 3 was 9-10 percentage points (almost 20%), while no significant increase for the 12-month group was observed. This implies that simplifying program rules is more meaningful for those with income varying around the thresholds. Perhaps due to the unpredictable income or an expectation for only temporary needs, it was too costly for these households to apply for the program under the three-tests system. In Table 1.7, I compare the mean characteristics of the households who have taken up the program between the treatment and control states-years. In the fourth column, the 6-month group who newly took up appeared to have lower income, were less married but had more children, and were more white than the control group. An implication of this profile could be that these households tend to be under temporary income shock but can recover and have less capacity for administrative burdens by having children and being less married.

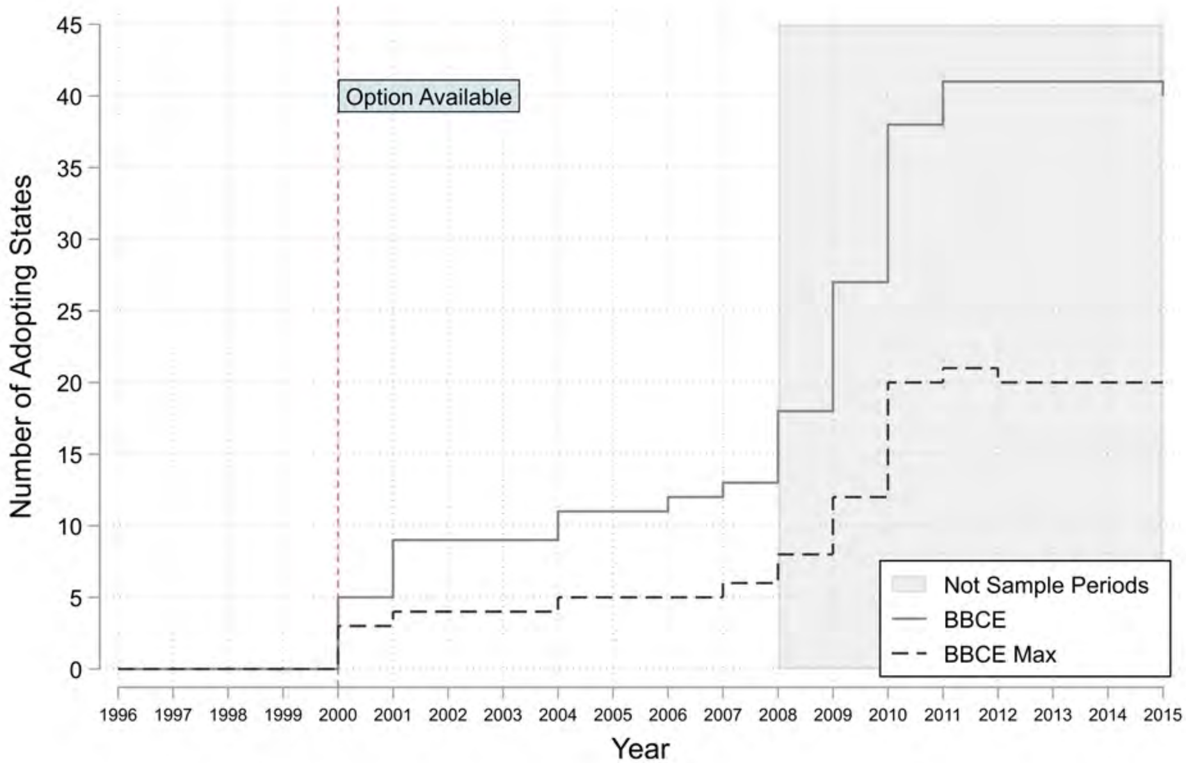
Besides reducing administrative burdens, BBCE Max also increased average weekly earnings. In Figure 1.11, both the 6-month and the 12-month always eligible groups increased earnings, with the 12-month group increasing more persistently. In Table 1.6, the aggregate effects in event time 0 to 5 for the 12-month group increased about 45 dollars per week (or 15%), and 133 dollars (or 17%) for the 6-month group. These are virtually the same households changing their labor supply decisions as the always-eligible populations did not change. A potential explanation is that the higher gross income thresholds and the elimination of the net income test allow more flexibility for households to work more without losing eligibility. A suggestive evidence of this is in Table 1.7, column (2), where those on the program have higher income and are more likely to have earnings in BBCE Max states than in the control group.

Appendix A.4 provides balanced panel analyses in which the composition of the treated states are the same across all event times. The effects are similar or larger from the balanced panel than from the unbalanced panel, as shown in Table A.4.1, Table 1.6, Figure A.4.2, A.4.6, A.4.11, and A.4.14.

1.6 Conclusion

BBCE was a solution provided by the federal government for states to transition former AFDC beneficiaries to SNAP in the post-welfare reform era. As intended, a group of households with distinct characteristics left out by the three-test system had gained eligibility. However, expanding eligibility should not be the only reason for states to consider simplifying the eligibility rules. The findings of this paper suggest that we can target essentially the same group of households with much less administrative burden while increasing incentives to work more. The saved government resources could be re-purposed for better use, and more low-income households could benefit from nutrition assistance. Although concerns for fraud may not be entirely dispelled in this paper, with more data and understanding of welfare fraud behaviors, future research could explore and further refine eligibility criteria.

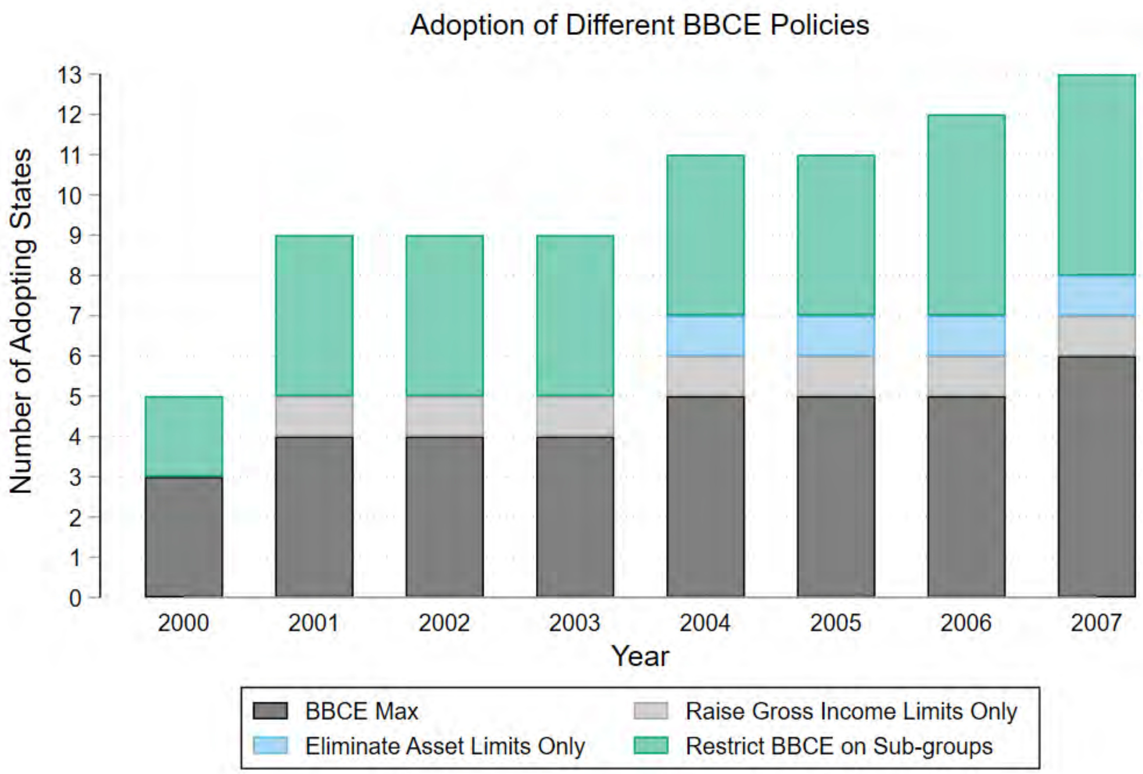
Figure 1.1: Number of States Adopting BBCE 1996 to 2015



BBCE indicates whether the states adopt any form of BBCE policies. BBCE Max indicates the states adopt the most generous form of BBCE: gross income limits above 130% FPL and eliminating net income and asset tests.

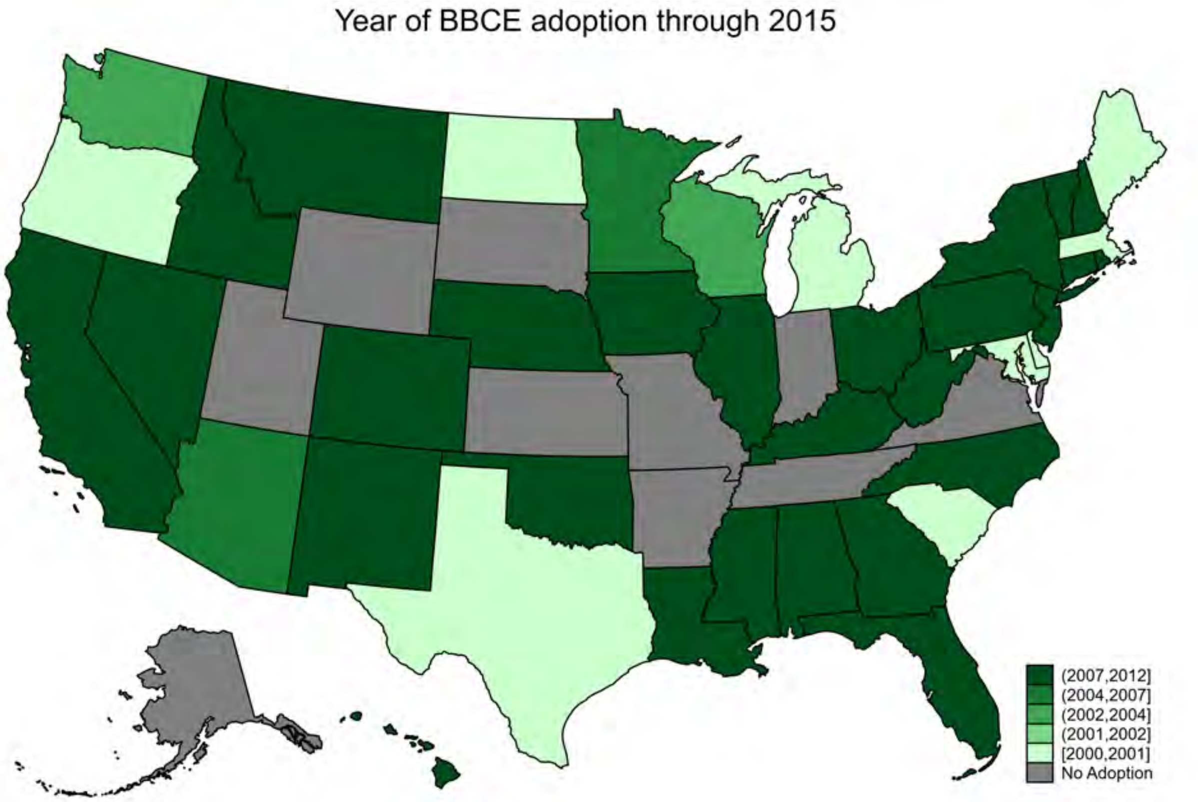
By the end of 2015, a total of 41 states had adopted BBCE. 13 of them adopted before the Great Recession (between 2000 and 2007). This paper studies the effects between 1996 and 2007 because during the Great Recession the effects are likely confounded with other factors, such as other stimulus policies. States that adopted BBCE post-2008 also made changes or reversed their BBCE policies within a few years, while none of the states that adopted before 2008 made any changes.

Figure 1.2: Types of BBCE Policies Adopted by States



Almost all states that adopt high gross income limits (above 130% FPL) also eliminate their asset limits except for two states. Texas only adopted a high gross income limit, and Washington only adopted no asset tests. The "More Restrictive BBCE" states adopted BBCE conditional on households with elderly members or dependents. By the year 2007, a total of 13 states adopted any BBCE, and 6 of them adopted BBCE Max.

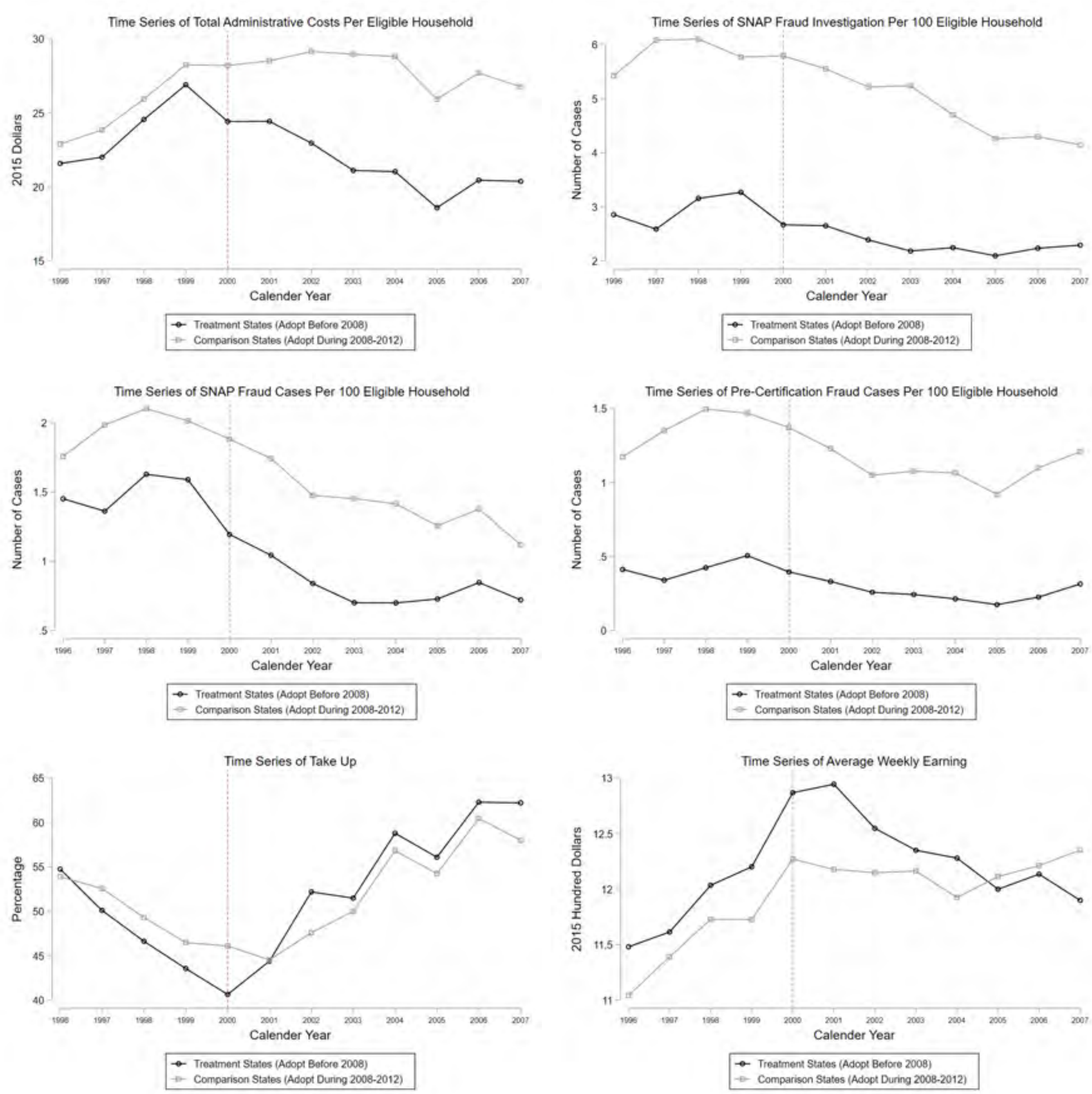
Figure 1.3: Geographical Distribution of BBCE Adoption Timing



Data source: SNAP Policy Database and Laird and Trippe (2014).

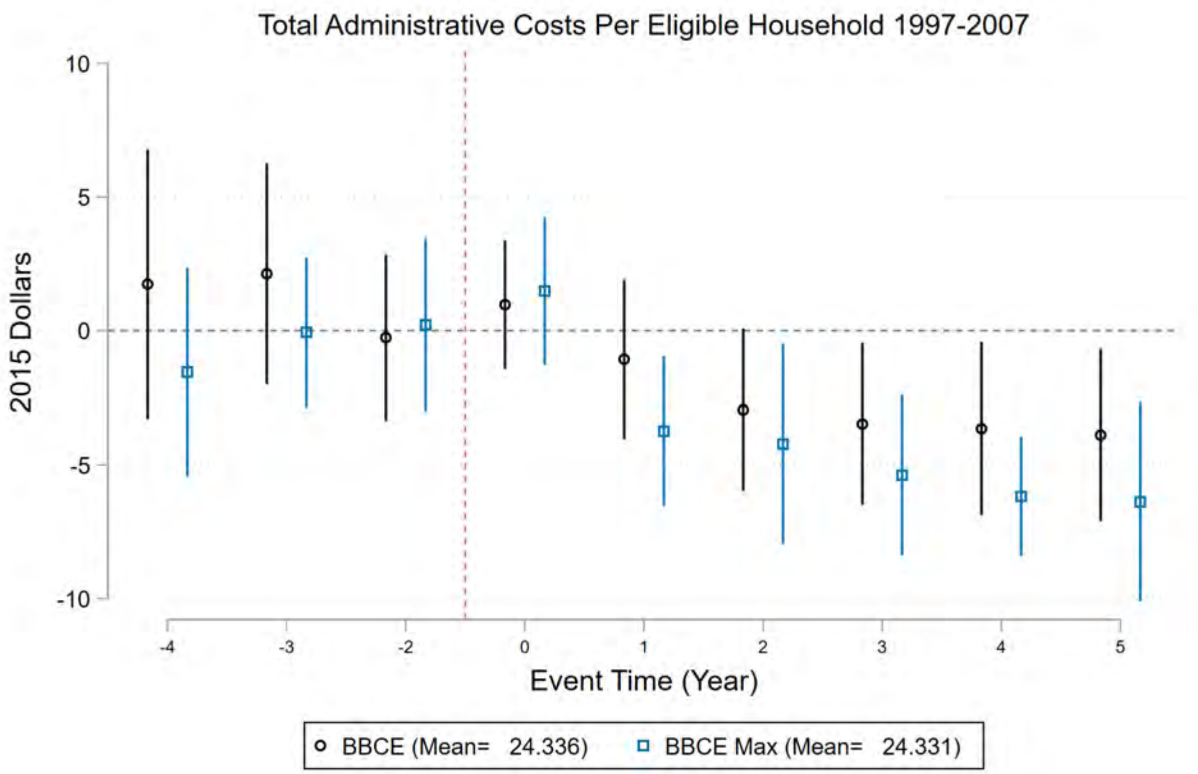
The first three lighter shades of green are the treatment states of this paper. There does not seem to be apparent geographical correlations in the timing of adoption. The darkest shade of green represents the comparison group of this paper — those who did not adopt BBCE from 2000 to 2007 but adopted it from 2008 to 2012. The gray states that did not adopt BBCE by 2015 are not studied in this paper.

Figure 1.4: Time Series of Outcomes by Treatment and Comparison States



Annual average of outcomes weighted by size of always-eligible households. This figure examines the parallel trends of raw series of outcomes between the treatment and control states. Treatment states are composed of 13 states that adopted BBCE between 2000 and 2007. Control states have 27 states and DC that adopted between 2008 and 2012.

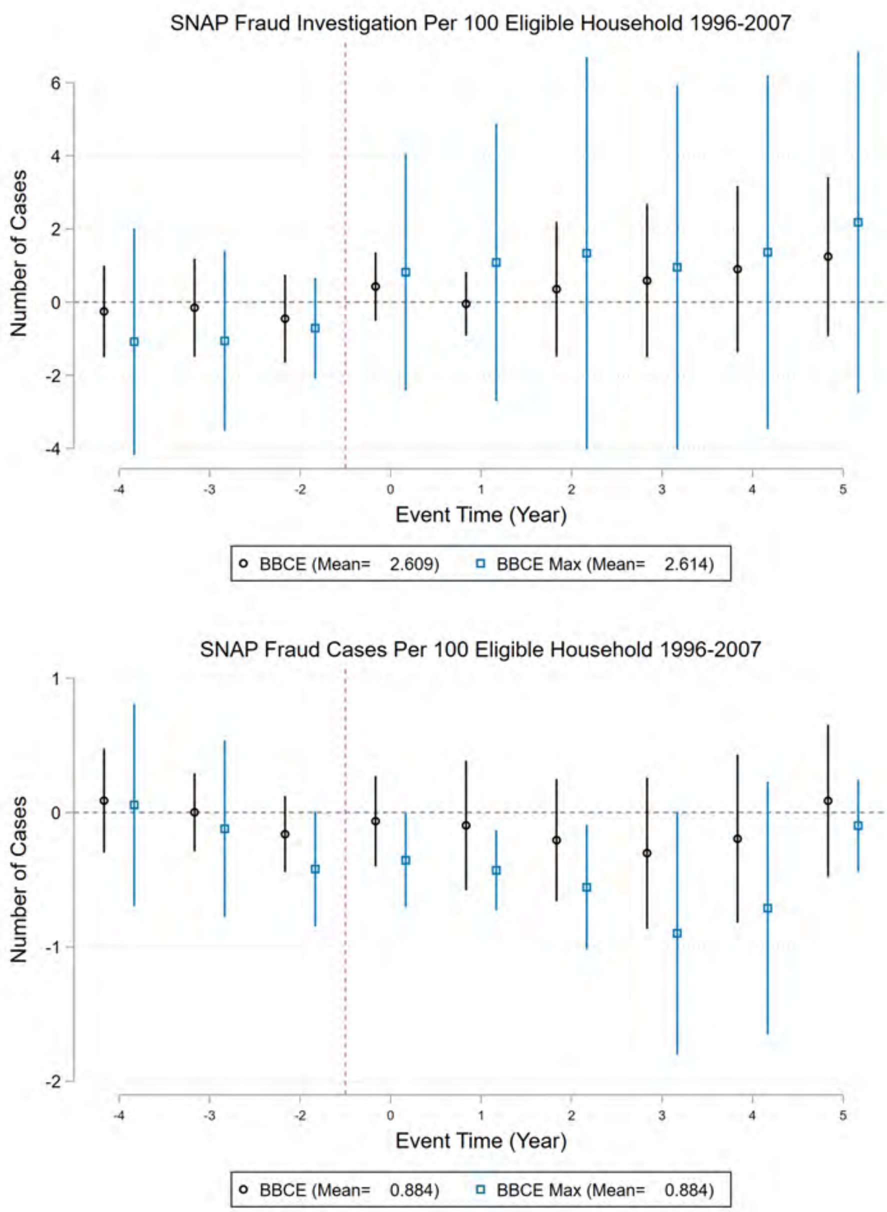
Figure 1.5: Event Study Estimates: State Total Administrative Costs for SNAP



Sun & Abraham (2021) estimators using equation 1.2, state-level version. Standard errors clustered at the state level. The dependent variable is the total SNAP administrative costs divided by the number of always eligible (for at least one month in the year) households. All monetary values are adjusted to the December 2015 consumer price index. The observations are weighted by states' always eligible (for at least one month in the year) populations. Event time ranges from -11 to 7. The complete vector of estimates is plotted in Appendix A.4. The base group is event time lead 1 and the comparison group. State total administrative costs in 1996 are not available.

This figure shows a major reduction in SNAP administrative costs by adopting BBCE, especially through BBCE Max. In event lag 3 to 5, state administrative costs decreased by 3.47-3.88 dollars (about 15%) for BBCE, and 5.38-6.38 dollars for BBCE Max (about 25%).

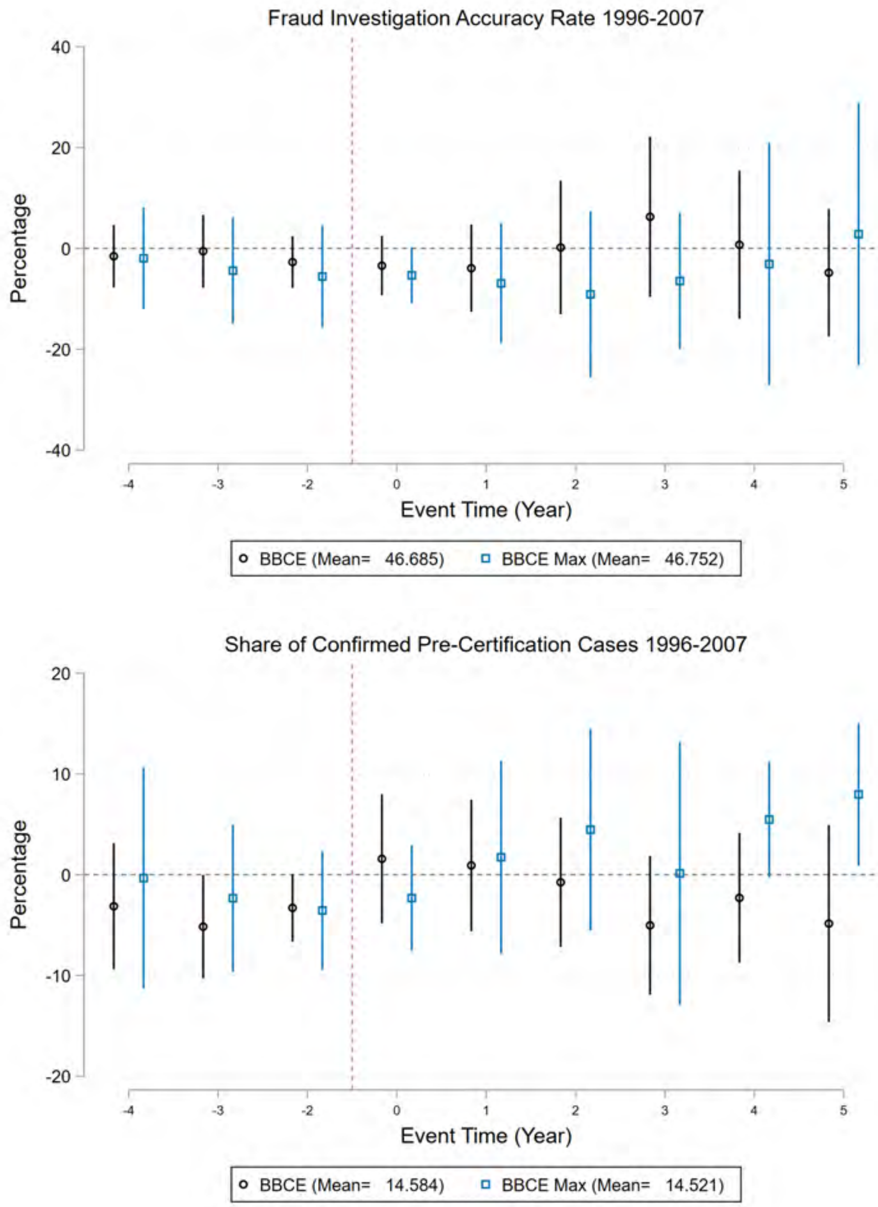
Figure 1.6: Event Study Estimates: SNAP Fraud Cases



Sun & Abraham (2021) estimators using equation 1.2, state-level version. Standard errors clustered at the state level. The dependent variables are the number of fraud investigations and confirmed fraud cases, both divided by the number of always eligible (for at least one month in the year) households. The observations are weighted by states' always eligible (for at least one month in the year) populations. Event time ranges from -11 to 7. The complete vector of estimates is plotted in Appendix A.4. The base group is event time lead 1 and the comparison group.

This figure examines whether BBCE/BBCE Max increased SNAP fraud incidence. The upper panel shows some increase in total investigations but not statistically significant. The lower panel shows a slight decrease for BBCE Max, but the trend likely began pre-event.

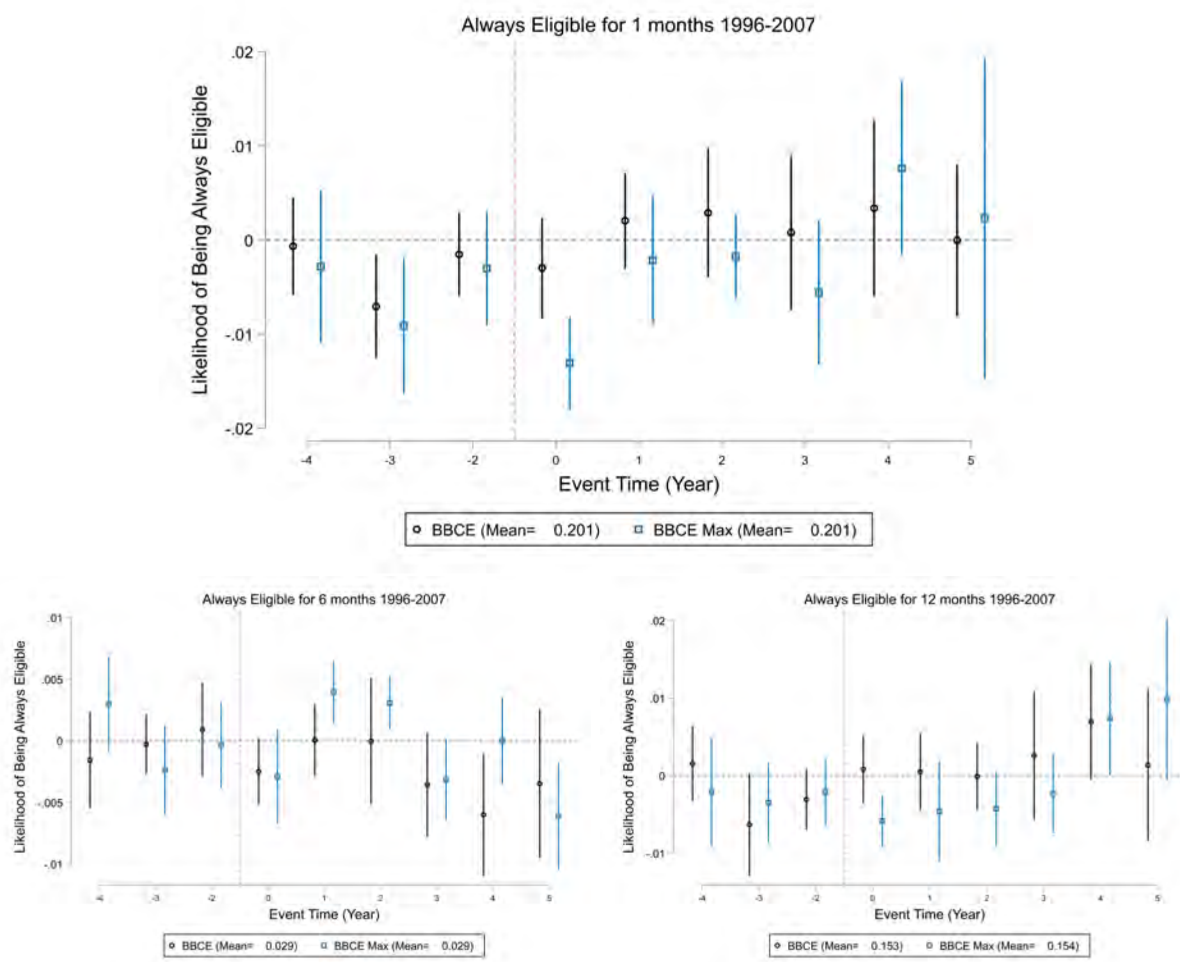
Figure 1.7: Event Study Estimates: SNAP Fraud Cases



Sun & Abraham (2021) estimators using equation 1.2, state-level version. Standard errors clustered at the state level. The dependent variables are the fraud accuracy rate, calculated by the number of confirmed fraud cases divided by total investigations, and the share of pre-certification fraud cases among confirmed fraud cases. The observations are weighted by states' always eligible (for at least one month in the year) populations. Event time ranges from -11 to 7. The complete vector of estimates is plotted in Appendix A.4. The base group is event time lead 1 and the comparison group.

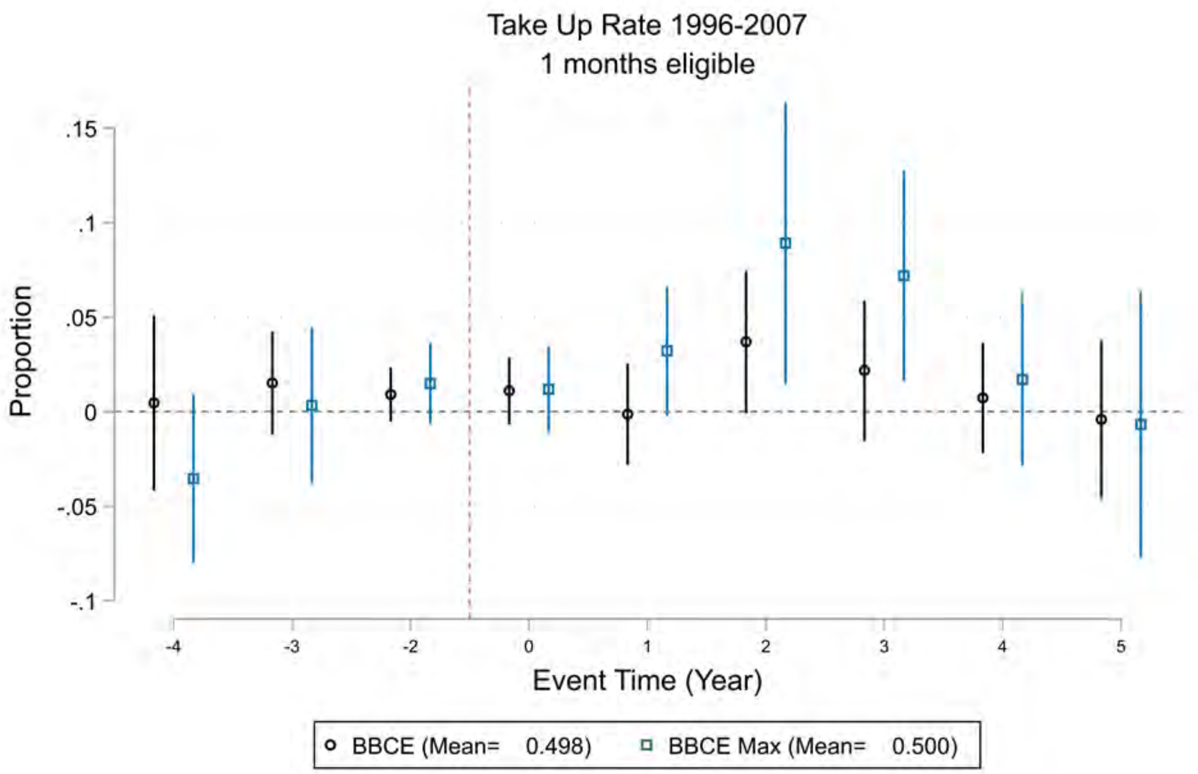
This figure examines whether BBCE/BBCE Max increased SNAP fraud incidence. The upper panel shows no change in fraud accuracy rates, and the lower panel shows no change or a slight increase in pre-certification cases. With no significant decrease in states' efforts and abilities in detecting fraud and no increase in detected fraud, BBCE/BBCE Max likely did not increase fraud incidence.

Figure 1.8: Event Study Estimates: Likelihood of Being Always-Eligible



Sun & Abraham (2021) estimators using equation 1.2. Observations weighted by household sampling weight. Standard errors clustered at the state level. Event time ranges from -11 to 7. The complete vector of estimates is plotted in Appendix A.4. The base group is event time lead 1 and the comparison group.
The dependent variables are indicators of being always eligible for at least one month, one to six months, and 12 months in the year. The sample includes all populations, eligible or not.
This figure shows no change in the size of the always-eligible population after BBCE/BBCE Max adoption, which implies no drastic or trending changes in the composition of the always-eligible population; otherwise, the probability of a household conditional on same characteristics being always eligible would change.

Figure 1.9: Event Study Estimates: Take-Up Among Always Eligible Households

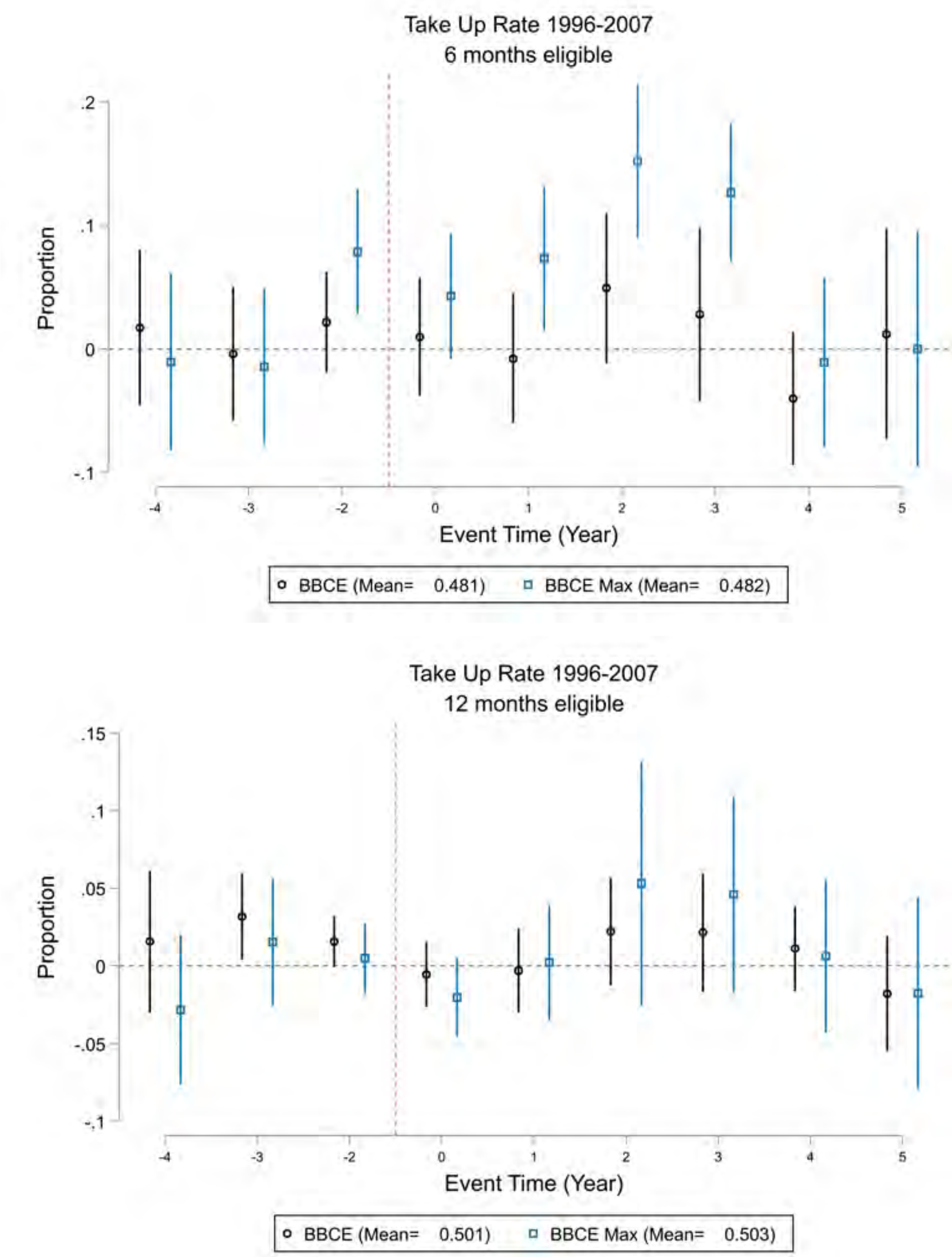


Sun & Abraham (2021) estimators using equation 1.2. Observations weighted by household sampling weight. Standard error clustered at the state level. Event time ranges from -11 to 7. The complete vector of estimates is plotted in Appendix A.4.

The dependent variable is whether or not the household is receiving SNAP benefits. The sample includes households that are always eligible for at least one month in the year.

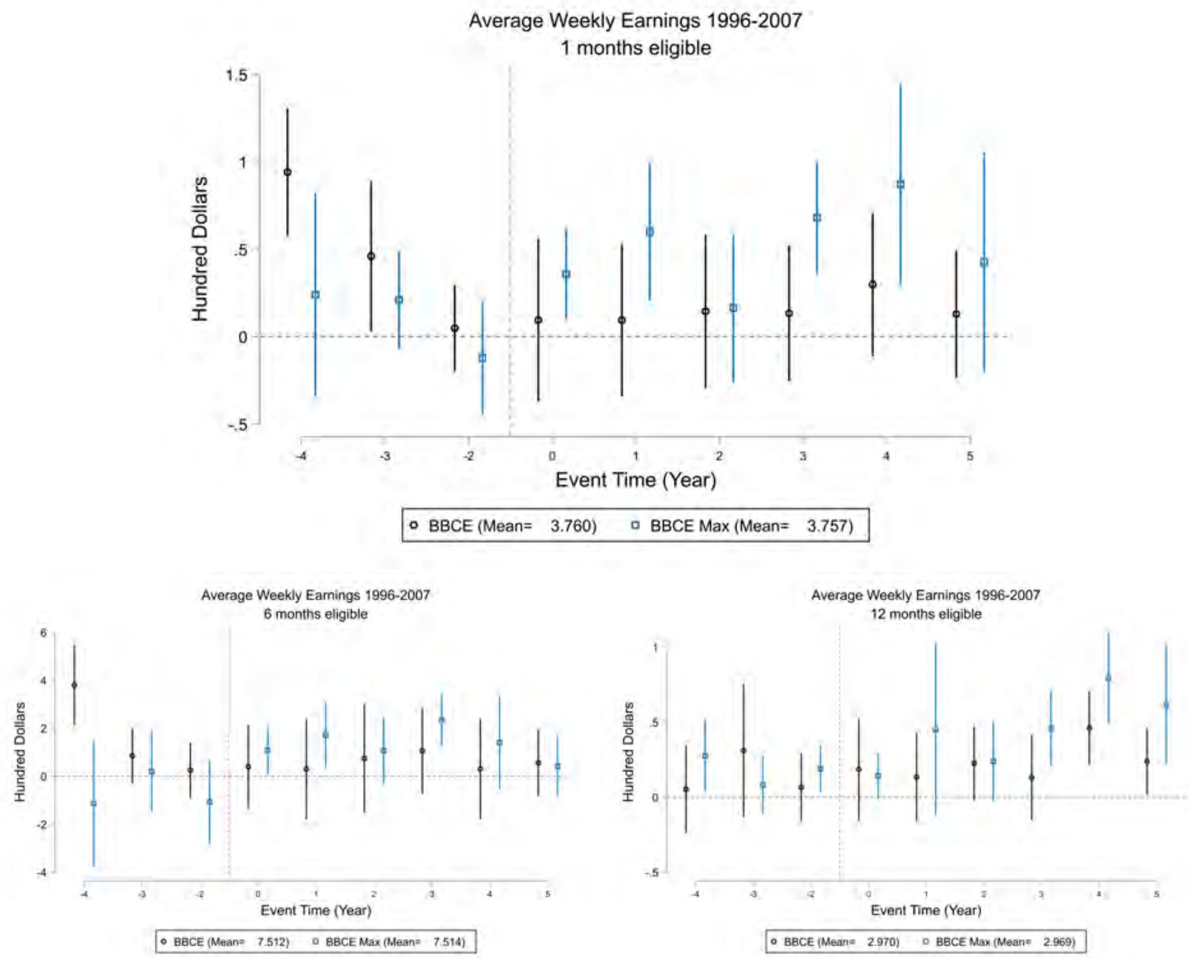
In event time 2 to 3, the take-up rate increases by about 2.2-3.7 percentage points (about 7%) for BBCE, and 7.2-8.9 percentage points (about 16%) for BBCE Max.

Figure 1.10: Event Study Estimates: Take-Up Among Subgroups



Sun & Abraham (2021) estimators using equation 1.2. Observations weighted by household sampling weight. Standard error clustered at the state level.
The dependent variable is whether or not the household is receiving SNAP benefits. The sample includes households always eligible for 1-6 months in the year and for every month in the year.
This figure analyzes heterogeneous responses by short-term and long-term eligible households. It appears that the short-term eligible groups are affected much more than the long-term eligible groups. In event time 2 to 3, BBCE Max increased the take-up rate of the 6-month group by 12-15 percentage points (about 30%), and 4.6-5.3 percentage points for the 12-month group (about 10%).

Figure 1.11: Event Study Estimates: Average Weekly Earnings



Sun & Abraham (2021) estimators using equation 1.2. Observations weighted by household sampling weight. Standard error clustered at the state level. Monetary value adjusted to the December 2015 consumer price index.

This figure analyzes whether BBCE/BBCE Max affects labor supply outcomes, and BBCE Max appears to increase earnings in general and for the two subgroups.

The average effects across events 0-5 are 52 dollars per week (about 14%) for the at least 1 month group, 133 dollars (about 17%) for the 6-months group, and 54 dollars for the 12-month group (about 18%).

Table 1.1: Mean Characteristics of SNAP Eligible Units: Always Eligible for At Least 1 month

	Pre BBCE	Post BBCE	
	Always-Eligible	Always-Eligible	Newly-Eligible
	(1)	(2)	(3)
Take-up Rate	0.509	0.569***	0.623*
	(0.492)	(0.486)	(0.480)
Gross Income Per Member	760.6	758.8	1378.0***
	(941.7)	(905.5)	(938.8)
Net Income Per Member	429.9	432.6*	840.8***
	(691.7)	(653.0)	(773.3)
Eligible Benefit Per Member	62.67	64.76***	44.67***
	(50.96)	(52.84)	(45.40)
Age of Head	46.49	46.88**	43.22***
	(19.21)	(19.07)	(15.50)
Head Female	0.616	0.620	0.726***
	(0.486)	(0.485)	(0.446)
Head White	0.710	0.741***	0.805***
	(0.454)	(0.438)	(0.396)
Head Black	0.238	0.214***	0.150***
	(0.426)	(0.410)	(0.357)
Head Hispanic	0.187	0.222***	0.0989***
	(0.390)	(0.416)	(0.299)
Head HS or below	0.707	0.681***	0.487***
	(0.455)	(0.466)	(0.500)
Head Unemployed	0.806	0.824***	0.846
	(0.396)	(0.381)	(0.361)
Head Married	0.364	0.350***	0.275***
	(0.481)	(0.477)	(0.447)
Head Disabled	0.0938	0.105***	0.0807
	(0.292)	(0.307)	(0.273)
Unit Size	2.295	2.220***	2.901***
	(1.541)	(1.501)	(1.620)
Have Earnings	0.584	0.580	0.610
	(0.493)	(0.494)	(0.488)
Has Disabled Member	0.150	0.154	0.117*
	(0.358)	(0.361)	(0.321)
Has Elderly Member	0.294	0.300	0.169***
	(0.455)	(0.458)	(0.375)
Has Children 0-4 y.o.	0.224	0.217*	0.197
	(0.417)	(0.412)	(0.398)
Has Children 5-17 y.o.	0.339	0.325***	0.547***
	(0.473)	(0.468)	(0.498)
Has Noncitizen Member	0.116	0.114	0.0113***
	(0.321)	(0.318)	(0.106)
Observations	125580	23563	506

Standard deviation in parentheses. Estimates weighted by household sampling weights. Column (2) marks the mean difference t-tests between column (1) versus (2), and column (3) marks the difference between (2) and (3): * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. "Pre BBCE" includes never adopting states and adopting states in pre-periods. "Post BBCE" includes adopting states in post-periods. "Always-eligible" is defined as likely to pass the federal income and asset limits for at least 1 month in the year. "Newly-eligible" is defined as eligible but not "Always-eligible". The weighted share of newly eligible among all eligible in the post period is 2.12%.

Table 1.2: Mean Characteristics of Always Eligible Units: Short- versus Long-term Eligible

	12 months		At most 6 months	
	Pre BBCE	Post BBCE	Pre BBCE	Post BBCE
	(1)	(2)	(3)	(4)
Take-up Rate	0.524	0.552***	0.465	0.604***
	(0.494)	(0.491)	(0.485)	(0.469)
Gross Income	1160.6	1143.7	2535.9	2275.6***
	(1331.9)	(1146.4)	(3342.8)	(2383.0)
Net Income	634.5	633.3	1638.8	1468.6***
	(797.4)	(720.7)	(2521.5)	(1842.9)
Eligible Benefit Per Member	65.86	67.60***	50.35	52.55*
	(51.40)	(53.45)	(47.03)	(48.77)
Age of Head	50.29	50.41	38.27	38.12
	(20.46)	(20.04)	(11.42)	(11.68)
Head Female	0.679	0.675	0.403	0.416
	(0.467)	(0.468)	(0.490)	(0.493)
Head White	0.679	0.716***	0.825	0.842*
	(0.467)	(0.451)	(0.380)	(0.365)
Head Black	0.268	0.238***	0.128	0.117
	(0.443)	(0.426)	(0.334)	(0.321)
Head Hispanic	0.225	0.268***	0.0662	0.0739
	(0.418)	(0.443)	(0.249)	(0.262)
Head HS or Below	0.789	0.767***	0.437	0.407***
	(0.408)	(0.423)	(0.496)	(0.491)
Head Unemployed	0.796	0.802	0.949	0.964***
	(0.403)	(0.398)	(0.219)	(0.187)
Head Married	0.342	0.341**	0.433	0.384***
	(0.474)	(0.474)	(0.495)	(0.486)
Head Disabled	0.129	0.143***	0.00719	0.00577
	(0.336)	(0.350)	(0.0845)	(0.0757)
Unit Size	2.245	2.202***	2.285	2.187***
	(1.511)	(1.512)	(1.560)	(1.464)
Unit Has Earnings	0.474	0.483*	0.869	0.856*
	(0.499)	(0.500)	(0.337)	(0.351)
Has Disabled Member	0.207	0.207	0.0129	0.0123
	(0.405)	(0.405)	(0.113)	(0.110)
Has Elderly Member	0.410	0.406	0.0111	0.0105
	(0.492)	(0.491)	(0.105)	(0.102)
Has Children 0-4 y.o.	0.240	0.233*	0.141	0.142
	(0.427)	(0.423)	(0.348)	(0.349)
Has Children 5-17 y.o.	0.349	0.333***	0.254	0.255
	(0.477)	(0.471)	(0.435)	(0.436)
Has Noncitizen Member	0.156	0.152	0.0130	0.0104
	(0.363)	(0.359)	(0.113)	(0.101)
Observations	94612	17557	18717	4049

Standard deviation in parentheses. Estimates weighted by household sampling weights. Columns (2) and (4) mark the mean difference t-test between pre- and post-BBCE periods: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. "12 months" represents always eligible for 12 months in the year. "At most 6 months" means always eligible for 1 to 6 months.

Table 1.3: Regression of BBCE/BBCE Max Implementations Over Characteristics of Always-Eligible Households

	At least 1 month		At most 6 months		12 months	
	BBCE	BBCE Max	BBCE	BBCE Max	BBCE	BBCE Max
Age of Head	-0.0000896	-0.0000291	-0.000531**	-0.000223	0.00000418	-0.00000767
	(0.0000625)	(0.0000510)	(0.000180)	(0.000153)	(0.0000564)	(0.0000437)
Head Female	0.000897	-0.000887	-0.00472*	-0.00399	0.00230	0.000613
	(0.00146)	(0.00144)	(0.00232)	(0.00266)	(0.00143)	(0.00145)
Head Black	0.00290	0.00142	0.00185	0.00467	0.00230	0.00113
	(0.00193)	(0.00158)	(0.00553)	(0.00387)	(0.00211)	(0.00199)
Head Hispanic	0.00131	0.0000121	0.000170	-0.00330	0.00119	-0.000369
	(0.00184)	(0.00145)	(0.00413)	(0.00423)	(0.00207)	(0.00164)
Head Others	-0.00307	-0.00165	-0.0107*	-0.0139*	-0.00360	0.0000686
	(0.00312)	(0.00260)	(0.00484)	(0.00645)	(0.00370)	(0.00284)
Head HS or Below	-0.000294	-0.0000270	-0.00558*	-0.00380	0.000595	0.00111
	(0.00103)	(0.000671)	(0.00243)	(0.00228)	(0.00138)	(0.00110)
Head Married	-0.00160	-0.000423	0.000755	-0.00156	-0.00123	0.000499
	(0.00107)	(0.000903)	(0.00278)	(0.00236)	(0.00139)	(0.00106)
Head Disabled	0.0106***	0.00684*	0.0135	0.0331*	0.0106**	0.00549*
	(0.00293)	(0.00257)	(0.0192)	(0.0149)	(0.00327)	(0.00230)
Head Unemployed	0.00217	0.000650	-0.000515	0.00262	0.00294	0.000507
	(0.00121)	(0.000934)	(0.00451)	(0.00341)	(0.00202)	(0.00106)
Unit Size	0.000480	-0.000374	-0.000251	0.0000344	0.00101	-0.000352
	(0.000630)	(0.000373)	(0.00150)	(0.00118)	(0.000757)	(0.000519)
Has Disabled Member	-0.00147	-0.00141	-0.0196	-0.0282	-0.00247	-0.000731
	(0.00217)	(0.00156)	(0.0146)	(0.0159)	(0.00240)	(0.00112)
Has ABAWD Member	0.000658	0.0000262	-0.00244	0.00125	-0.00208	-0.00238
	(0.00163)	(0.00147)	(0.00453)	(0.00322)	(0.00292)	(0.00218)
Has Elderly Member	0.00483*	0.00332	0.0119	-0.00566	-0.0000696	0.000908
	(0.00228)	(0.00205)	(0.0107)	(0.00849)	(0.00269)	(0.00162)
Has Children 0-4 y.o.	-0.0000920	-0.0000918	-0.00693	0.00118	-0.00134	-0.00249
	(0.00172)	(0.00139)	(0.00428)	(0.00266)	(0.00191)	(0.00155)
Has Children 5-17 y.o.	-0.000653	0.00229	0.00106	0.00354	-0.000755	0.00140
	(0.00219)	(0.00152)	(0.00478)	(0.00407)	(0.00218)	(0.00164)
Has Noncitizen Member	-0.000765	-0.00424	0.0184	0.0119	-0.00262	-0.00454
	(0.00314)	(0.00339)	(0.0126)	(0.0118)	(0.00349)	(0.00368)
Observations	149430	124375	22616	18720	111630	93306
Adjusted R^2	0.761	0.733	0.737	0.755	0.780	0.742
Prob > F	0.0818	0.3672	0.1124	0.0483	0.0648	0.6733
Mean	0.151	0.0715	0.159	0.0977	0.155	0.0674

State and year fixed effects regression estimates. Sample weighted by household sampling weight. Standard errors in parentheses and clustered at the state level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
The dependent variable is an indicator of BBCE/BBCE Max adoption. Independent variables are household characteristics, controlling for other SNAP policies and unemployment rates.
"Prob > F" conducts the joint test of the listed characteristics.

Table 1.4: Determinants of State Adoption of BBCE Before 2008: Fixed Pre-BBCE Option State Characteristics

	(1) BBCE	(2) BBCE Max
Take up rate (Percentage)	0.0174	0.0101
	(0.0233)	(0.0172)
Percentage of Eligible Population	0.0261	-0.123
	(0.221)	(0.163)
Percentage of Population Aged < 18	-0.237	-0.209
	(0.288)	(0.212)
Percentage of Population Aged > 64	-0.208	-0.129
	(0.175)	(0.129)
Percentage of Hispanic Population	0.00766	0.00995
	(0.0281)	(0.0206)
Percentage of Black Population	-0.0295	-0.00220
	(0.0200)	(0.0147)
Percentage of Other Race/Ethnicity Population	-0.0150	0.00711
	(0.0184)	(0.0136)
Percentage of HS or Below Education	-0.0425	-0.000654
	(0.0451)	(0.0332)
Percentage of Disabled Population	-0.413	-0.0466
	(0.292)	(0.215)
Percentage of Non-Disabled 18-49 Adult Without Dependent	-0.154	-0.134
	(0.206)	(0.152)
Percentage of Married Population	-0.112	-0.0744
	(0.0959)	(0.0706)
Percentage of Citizens	0.0252	0.0347
	(0.0568)	(0.0418)
Unemployment Rate (Percentage)	-0.142	-0.106
	(0.251)	(0.185)
Median Household Income (Thousand)	0.0125	-0.0270
	(0.0362)	(0.0266)
Percentage of Voters Who Support Welfare	0.0448	-0.0302
	(0.0676)	(0.0497)
Percentage of Voters Who Have Bias Against Black	0.0514	0.0110
	(0.0386)	(0.0284)
Percentage of State Expenditure Over Own-Source Revenue	0.000335	-0.0110
	(0.0201)	(0.0148)
SNAP Administrative Costs Per Case	0.00293	0.00834
	(0.0119)	(0.00878)
SNAP Error Rate (Percentage)	-0.0325	0.0420
	(0.0466)	(0.0343)
Observations	41	41
R^2	0.482	0.305
Prob > F	0.472	0.941
Mean	0.29	0.12

Ordinary least squared regression estimates weighted by eligible population size averaged from 1996 to 1999.
Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Dependent variable is an indicator of states ever adopting BBCE/BBCE Max before 2008, 0 if the states adopted BBCE between 2008 and 2012. Independent variables are the average of 1996 to 1999 levels of potential determinants of states' policy choices.
For sources of data, see Appendix A.1

Table 1.5: Aggregated Effects on State Outcomes

	Event -4 to -2		Event 3 to 5		Event 0 to 5	
	BBCE	BBCE Max	BBCE	BBCE Max	BBCE	BBCE Max
Total Administrative Costs						
	1.204	-0.461	-3.667*	-5.979***	-2.340*	-4.070***
	(1.878)	(1.368)	(1.449)	(1.110)	(1.155)	(1.004)
Number of Fraud Investigations						
	-0.286	-0.951	0.912	1.500	0.578	1.290
	(0.581)	(1.128)	(1.063)	(2.287)	(0.808)	(2.097)
Number of Confirmed Fraud Cases						
	-0.0238	-0.162	-0.137	-0.570	-0.129	-0.508*
	(0.125)	(0.220)	(0.215)	(0.317)	(0.161)	(0.216)
Fraud Accuracy Rate (Confirmed Cases/Total Investigations						
	-1.610	-3.988	0.727	-2.251	-0.830	-4.688
	(2.408)	(3.585)	(5.567)	(8.667)	(3.877)	(6.264)
Share of Pre-Certification Cases						
	-3.861*	-2.077	-4.060	4.527	-1.740	2.911
	(1.890)	(3.036)	(3.273)	(3.275)	(2.525)	(2.414)

Linear combinations of event study estimates. Standard error in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Balanced panel includes 11 out of the 13 states treated for BBCE (87% of the sample), and 5 out of the 6 states treated for BBCE Max (93% of the sample).

Table 1.6: Aggregated Effects on Always Eligible Household Outcomes

	Event -4 to -2		Event 0 to 3		Event 0 to 5	
	BBCE	BBCE Max	BBCE	BBCE Max	BBCE	BBCE Max
Take up						
Panel A - At least 1 month group						
All Observations	0.00958	-0.00582	0.0170	0.0512*	0.0119	0.0358
	(0.0134)	(0.0148)	(0.0127)	(0.0214)	(0.0129)	(0.0228)
Balanced Panel	0.0178	-0.0310	0.0352	0.0637*	0.0273	0.0441
	(0.0330)	(0.0209)	(0.0191)	(0.0276)	(0.0176)	(0.0263)
Panel B - 1-6 month group						
All Observations	0.0117	0.0178	0.0197	0.0985***	0.00852	0.0639**
	(0.0220)	(0.0231)	(0.0249)	(0.0205)	(0.0218)	(0.0245)
Balanced Panel	-0.0144	-0.0572	0.0480	0.111***	0.0322	0.0723**
	(0.0515)	(0.0296)	(0.0398)	(0.0260)	(0.0274)	(0.0272)
Panel C - 12 month group						
All Observations	0.0211	-0.00261	0.00887	0.0203	0.00484	0.0117
	(0.0141)	(0.0170)	(0.0129)	(0.0243)	(0.0129)	(0.0243)
Balanced Panel	0.0445	-0.0186	0.0327	0.0322	0.0283	0.0196
	(0.0342)	(0.0249)	(0.0211)	(0.0318)	(0.0206)	(0.0287)
Average Weekly Earnings						
Panel A - At least 1 month group						
All Observations	0.483***	0.117	0.450**	0.0512*	0.149	0.516*
	(0.132)	(0.180)	(0.202)	(0.163)	(0.185)	(0.202)
Balanced Panel	0.196	0.573*	0.293	0.687**	0.279	0.675**
	(0.249)	(0.241)	(0.265)	(0.227)	(0.244)	(0.241)
Panel B - 1-6 month group						
All Observations	1.629**	-0.671	0.620	1.549**	0.555	1.334**
	(0.556)	(0.979)	(0.959)	(0.511)	(0.871)	(0.512)
Balanced Panel	-0.564	0.351	0.266	1.983**	-0.131	1.624**
	(0.843)	(0.786)	(1.010)	(0.679)	(0.905)	(0.606)
Panel C - 12 month group						
All Observations	0.143	0.182*	0.169	0.323*	0.229*	0.451**
	(0.115)	(0.0885)	(0.114)	(0.138)	(0.107)	(0.145)
Balanced Panel	0.384*	0.692***	0.304	0.449**	0.407*	0.536***
	(0.186)	(0.126)	(0.173)	(0.166)	(0.165)	(0.162)

Linear combinations of event study estimates. Standard error in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Balanced panel includes the same set of states observed from event -4 to 5: eight out of the thirteen states treated for BBCE (80% of the sample), and four out of the six states treated for BBCE Max (92% of the sample).

Table 1.7: Mean Characteristics of Always Eligible SNAP Participants By BBCE Max Adoption

	At Least 1 Month		1-6 Months		12 Months	
	Control	Treated	Control	Treated	Control	Treated
	(1)	(2)	(3)	(4)	(5)	(6)
Gross Income	1250.2	1325.4***	2227.5	1914.7***	1065.4	1068.5
	(1455.6)	(1310.2)	(2709.6)	(1720.1)	(1495.4)	(1026.8)
Net Income	754.7	796.4**	1407.6	1218.8**	613.4	621.4
	(1067.6)	(937.3)	(2058.5)	(1323.2)	(847.4)	(649.1)
Eligible Benefit Per Member	75.33	74.84	59.46	61.28	78.98	79.87
	(50.15)	(53.33)	(49.07)	(51.61)	(49.29)	(52.99)
Age of Head	41.69	41.59	38.26	38.08	43.40	43.97
	(16.66)	(16.02)	(11.33)	(11.52)	(17.94)	(17.54)
Head Female	0.671	0.641***	0.431	0.425	0.732	0.721
	(0.470)	(0.480)	(0.495)	(0.495)	(0.443)	(0.448)
Head White	0.639	0.654*	0.799	0.839***	0.595	0.573**
	(0.480)	(0.476)	(0.401)	(0.368)	(0.491)	(0.495)
Head Black	0.309	0.306	0.154	0.136	0.351	0.379***
	(0.462)	(0.461)	(0.361)	(0.342)	(0.477)	(0.485)
Head Hispanic	0.166	0.0684***	0.0555	0.0260***	0.202	0.0897***
	(0.372)	(0.252)	(0.229)	(0.159)	(0.402)	(0.286)
Head HS or Below	0.717	0.660***	0.440	0.439	0.795	0.754***
	(0.451)	(0.474)	(0.496)	(0.496)	(0.404)	(0.430)
Head Unemployed	0.850	0.856	0.973	0.975	0.833	0.823
	(0.357)	(0.351)	(0.161)	(0.156)	(0.373)	(0.382)
Head Married	0.314	0.274***	0.373	0.332**	0.302	0.250***
	(0.464)	(0.446)	(0.484)	(0.471)	(0.459)	(0.433)
Head Disabled	0.146	0.157*	0.00611	0.00462	0.199	0.241***
	(0.353)	(0.364)	(0.0780)	(0.0678)	(0.399)	(0.428)
Unit Size	2.427	2.336***	2.090	2.134	2.508	2.393***
	(1.521)	(1.519)	(1.413)	(1.478)	(1.534)	(1.545)
Unit Has Earnings	0.578	0.600**	0.853	0.816***	0.489	0.490
	(0.494)	(0.490)	(0.354)	(0.388)	(0.500)	(0.500)
Has Disabled Member	0.226	0.215	0.0135	0.00978	0.307	0.329**
	(0.418)	(0.411)	(0.115)	(0.0985)	(0.461)	(0.470)
Has ABAWD Member	0.180	0.208***	0.576	0.502***	0.0665	0.0837***
	(0.384)	(0.406)	(0.494)	(0.500)	(0.249)	(0.277)
Has Elderly Member	0.170	0.152***	0.00362	0.00389	0.232	0.233
	(0.376)	(0.359)	(0.0601)	(0.0622)	(0.422)	(0.423)
Has Children 0-4 y.o.	0.269	0.236***	0.128	0.156**	0.302	0.260***
	(0.443)	(0.425)	(0.334)	(0.363)	(0.459)	(0.439)
Has Children 5-17 y.o.	0.408	0.374***	0.223	0.263**	0.451	0.406***
	(0.492)	(0.484)	(0.416)	(0.441)	(0.498)	(0.491)
Has Noncitizen Member	0.0859	0.0449***	0.00853	0.00579	0.115	0.0662***
	(0.280)	(0.207)	(0.0920)	(0.0759)	(0.319)	(0.249)
Observations	59813	5736	8694	1324	45413	3879

"Control" includes comparison states and BBCE Max adopting states in pre-periods. "Treated" includes BBCE Max adopting states in post-periods. "At Least 1 Month" is defined as always-eligible for at least 1 month in the year, similar for "1-6 Months" and "12 Months". Columns (2), (4), and (6) mark the differences between Control and Treated columns tested by simple linear regressions weighted by household sampling weights. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Chapter 2

Understanding State Variation in SNAP Policies

2.1 Introduction

The Supplemental Nutrition Assistance Program (SNAP), also known as the food stamps, is the second largest in-kind transfer program in the United States. While federal law regulates program eligibility and benefit levels, the state governments, as the administering agents, are given several options to tailor the program to local needs. This results in wide variations in the choice of these state options, which became valuable quasi-experiments for studying program participation and other outcomes. However, it is rarely discussed why states adopt different policies in the first place. Are states acting to maximize the welfare of their constituents? Can we perceive their choices as random? Understanding the reasons behind these choices is crucial not only to justify their quasi-experiment applications but also for optimizing the design of intergovernmental grants.

Previous research on state policy variations was predominantly on TANF (Temporary Assistance for Needy Families) and Medicaid. Studies on TANF indicate that conservative-leaning states tend to adopt more restrictive policies, such as stringent work requirements or shorter lifetime eligibility limits, than liberal-leaning states. Additionally, interracial relationships play a significant role in shaping public attitudes toward welfare programs, influencing policy decisions (Filindra, 2013; Rodgers, 2005; Rodgers & Tedin, 2006; Soss, Schram, Vartanian, & O'brien, 2001). Regarding Medicaid, state variations in expansion following the Affordable Care Act of 2010 are largely characterized by party polarization, with Democratic states generally expanding Medicaid while Republican states do not. Race and states' economic well-being show only a moderate or small association with Medicaid expansion decisions (Barrilleaux & Rainey, 2014; Filindra, 2013; Jacobs & Callaghan, 2013). Though not a transfer program, the state minimum wage laws are also investigated. The common finding is that because states can only increase, but not decrease, the minimum wage rates, it is less likely for them to change the law when the unemployment rate is high (Ford, Minor, & Owens, 2012; Lesica, 2018; Whitaker, Herian, Larimer, & Lang, 2012).

The explanatory variables in previous studies are often selected by pooling multiple stand-alone political science theories. The methodology primarily uses multivariate regressions and descriptive approaches. This paper aims to contribute to the literature in two aspects. The first is to offer a comprehensive framework aimed at systematically explaining state governments' behavior. More specifically, I consider an office-seeking agent's decision-making process. Such agents choose policies to maximize the probability of gaining office while being constrained by state budgets and SNAP administrative duties. The chosen explanatory variables from this framework would include voter preferences toward welfare, political ideologies, states' fiscal and economic circumstances, and their performance in administering the program. These factors are mostly consistent with the literature but interpreted within the framework of an office-seeking agent's maximization problem. The goal is to provide a cohesive rationale for states' behaviors and potentially predict their future behaviors.

The second contribution of this paper lies in employing a quasi-experimental design to ensure that the policy determinants are not reversely affected by policy adoptions. For instance, if a state with a higher proportion of low-income households adopted more inclusive policies, it is unclear whether the adoption is driven by the number of low-income households or if the inclusive policies themselves influence household behaviors. I utilize an exogenous variation from the institutional background that these policies were unavailable for states until the federal government changed regulations. The timing of when the federal government allowed these policies was not of the states' choice; therefore, the pre-federal-change levels of the explanatory factors were not under the influence of policy adoptions. I use two econometric specifications. The first approach predicts the policy adoption decisions and timing using the pre-federal timing levels of the determinants. I normalize the adoption and timing between zero and one, with zero representing no adoption as of 2015, and one means adoption in the first available year. This enables me to analyze the variation of not only whether the states adopted the policies but also how soon they did so. With only 51 observations in this approach, I introduce a machine learning approach, the best subset selection, to choose the determinants that best explain the adoptions. The second approach covers annual observations of policy adoptions from 1996 to 2015, interacting the lagged one-year levels of the determinants with a post-federal-timing indicator variable to predict adoption as a dummy variable. This approach complements the first one by examining time-varying levels of the determinants, capturing states' responses to current circumstances.

I focus on five heavily studied policy options: Broad-Based Categorical Eligibility (BBCE), vehicle limits exemption, face-to-face interview waivers, simplified reporting systems, and transitional benefits. Analyzing the total number of these five policies adopted, poverty rates, voters' political leanings toward liberalism, and SNAP error rates are the three most explanatory determinants. Policies expanding eligibility, including BBCE and vehicle exemptions, were mostly driven by economic circumstances, with BBCE also influenced by Democratic party controls. Simplified reporting was favored by states with higher error rates and influenced by voters' preferences toward welfare policies. However, face-to-face interview waivers seemed less aligned with the office-seeking agent model. SNAP error rates also shape transitional benefits provision, but the effects differ depending on long-term versus contemporaneous status. Overall, unlike TANF and Medicaid, which are predominantly influenced by political ideology, SNAP appears to function as a combination of political and economic tools. Notably, its administrative factors also non-trivially affect states' incentives and capabilities in adopting policies.

The paper proceeds as follows. Section 2 describes the institution of the SNAP policies studied in this paper. Section 3 explains the theories and measurement of the policy determinants. Section 4 presents the empirical methodology. Section 5 shows the results. Section 6 concludes.

2.2 State Options for SNAP

The 1964 Food Stamp Act established the division that the federal government is responsible for funding benefits while the states are in charge of issuance and certification. In the following Food and Agricultural Act of 1977, the program was further standardized regarding eligibility and benefit determination, leaving the states with limited autonomy in operating the program. After the 1996 welfare reform, more flexibility was given to state agencies. Below, I introduce five of these state options studied in this paper. Two policies are eligibility expansion, two are simplifying administrative processes, and one is offering extra benefits. Data of state adoptions are from the SNAP Policy Database compiled and published by the Economic Research Services (ERS) of the U.S. Department of Agriculture (USDA). This section describes the institution of these policies and states' adoptions between 1996 and 2015. Regardless of whether states adopt these options, the federal government remains responsible for all the SNAP benefits and shares 50% of the administrative costs with the states.

2.2.1 Broad-Based Categorical Eligibility (BBCE)

To qualify for SNAP, citizens of the United States can either pass the income and asset tests or fall into a certain "category" that is deemed eligible. As per federal law, categorical eligibility should at least be given to households with all members eligible for cash assistance from means-tested programs, including the Supplemental Security Income (SSI), General Assistance (GA), and Temporary Assistance for Needy Families (TANF) or state maintenance-of-effort (MOE). The Broad-Based Categorical Eligibility (BBCE) extends such categorical eligibility to those who are eligible for *noncash* benefits such as child care or a brochure funded by TANF/MOE. States usually set higher income limits for these noncash benefits than cash benefits. Therefore, BBCE is a form of eligibility expansion that the states can use to include more low-income households in SNAP.

Since BBCE was made possible by the block grant nature of TANF, it might be straightforward to consider the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) of 1996, which was the origin of TANF replacing the Aid to Families with Dependent Children

(AFDC), to be the starting year of this state option. However, in 1996, there were no further modifications of the categorical eligibility section of the Food Stamp law that incorporated the noncash benefits of TANF. It was not until July 1999 that USDA announced which noncash TANF/MOE benefits could be used for categorical eligibility, and the formal regulations were issued in 2000. Although the name BBCE was formalized in 2009 (USDA, 2009), the policy was made available in 2000.

Figure 2.1 shows the time trend of states' adoption of BBCE. Delaware was the first state that adopted BBCE as early as February 2000, followed by Maine, Michigan, North Dakota, and Oregon. Most states continued the policy once it was adopted except for Louisiana, which temporarily had BBCE from 2010 to 2014 to help families cope with losses from the 2010 BP Oil Spill[1]. Among the 41 states that have (ever) adopted BBCE, 22 of them adopted between 2009 and 2010. This could suggest that states see BBCE as a form of economic stabilizer, and economic circumstances could be a major determinant of adoption.

2.2.2 Vehicle Exemption

The SNAP "asset test" sets a limit on households' countable resources such as cash and bank accounts. The limit on vehicles was controversial for two main reasons: 1) vehicles serve more as one's ability to work than as assets, and 2) the federal vehicle limits had been fixed at $4650 since it was set in The Food Stamp Act of 1977. After TANF replaced AFDC, states started to establish more generous vehicle limits for TANF, so households who qualified for SNAP through AFDC could remain eligible through TANF. Recognizing the states' demand, the Agriculture Appropriations Act for fiscal year 2001 allowed states to apply their TANF vehicle policies to the Food Stamp Program. This option is not attached to adopting BBCE, and states can apply their new vehicle policies without deeming TANF noncash benefits eligible (Super & Dean, 2001). Therefore, states choosing BBCE versus vehicle exemptions could consider different reasons and target different groups.

With 2000 being the first year when vehicle policies were available for states, Figure 2.1 shows

[1]Source: https://www.labudget.org/2020/03/a-change-in-louisianas-snap-rules-could-keep-families-above-water/

that 17 out of 46 states that have ever exempted all vehicles adopted it within the first two years. The second wave was in 2009, when nine more states eliminated the vehicle limits during the Great Recession, though two states (Idaho and Pennsylvania) re-imposed some vehicle limits later in 2011 and 2012. Another state that reversed its decisions is Michigan, which excluded all vehicles in 2000 but set some limits again in 2012. For the remaining five states that have not fully removed vehicle tests, Alaska, Arkansas, South Dakota, and Texas have exempted some amount or at least one vehicle. Nebraska is the only state that has never made any vehicle policies.

2.2.3 Face-to-face Interview Waiver

The federal law used to require face-to-face interviews for all households who were applying for initial or continuing certification of the Food Stamp Program. Waivers could be granted for households with difficulties participating in such interviews, but only when the households requested so. In an amendment of PRWORA published in February 2000, USDA removed the interview requirements for re-certifications within 12 months and gave the states the discretion to determine when to replace in-person interviews with telephone interviews. The amendment took effect in January 2001[2]. However, it appeared that states found the implementation difficult because they were still required to document the exact "hardships" for the household to be interviewed in person. The documentation was required for quality control purposes, conducted by the USDA every year to ensure that the states issued benefits correctly[3]. A few states requested waivers of documenting the "hardships", and in October 2006, USDA announced that they would approve all such waiver requests for states who had no greater than national average error rates from the quality control evaluation last year[4]. The waiver allows for 50% of the initial certifications and all the re-certification cases to be waived in-person interviews without specified reasons.

I consider 2001 the first year the states could waive face-to-face interviews. Although it was harder to implement before 2006, some states still adopted it in these early years. Utah was the

[2]See 65 FR 70134: Food Stamp Program: Noncitizen Eligibility, and Certification Provisions of Pub. L. 104-193, as Amended by Public Laws 104-208, 105-33 and 105-185

[3]USDA conducts a quality control check by verifying the information and payment for a subsample of states' caseloads.

[4]Source: https://www.fns.usda.gov/snap/cert/face-face-interview-waiver-criteria

first state to waive face-to-face interviews in 2002, followed by Illinois, Connecticut, Delaware, North Carolina, Ohio, and Texas. Figure 2.1 shows a surge of adoption in 2006 corresponding to the aforementioned USDA announcement. Again, another wave of adoption was in 2009, during the Great Recession. North Dakota is the only state that has never waived in-person interviews. Most states kept the policy except for five states: Arkansas, Connecticut, Florida, Illinois, and Vermont. Arkansas, Florida, and Vermont re-implemented the policy within one and a half years. Considering the institutional background, the error rates could be a binding factor preventing states from waiving in-person interviews even if they would like to. However, the error rates would only make adoption difficult but not impossible; therefore, I treat the error rates as a predictor of adoption in the analyses.

2.2.4 Simplified Reporting

The simplified reporting option was also a part of the same amendment of PRWORA that allowed face-to-face interview waivers. The federal regulation required certified households to report virtually *any* changes of circumstances, including income, household composition, and residence address. If states adopt the simplified reporting option, only changes in income that would make households ineligible are required to be reported. The purpose of this option was to provide incentives for states to lengthen certification periods for households. Due to the quality control system, states trying to lower error rates used to set shorter certification periods so that households were forced to update their information frequently, or their benefits would be terminated. The option was meant to reduce the pressure on states to collect up-to-date information, so states would also reduce that on the households. Therefore, states adopting this option are likely being driven by administrative reasons.

Considering 2001 as the first year that states could use the simplified reporting option, Figure 2.1 shows that the option achieved 100% adoption by 2014. None of the states dropped out once they started using the option. 65% of the states started it within the first three years. The first seven states are Louisiana, Michigan, Missouri, New Hampshire, New York, Tennessee, and West Virginia. The last two states are California and Wyoming.

2.2.5 Transitional Benefits

The Transitional Benefits Alternative (TBA) was another option provided in the 2000 amendment of PRWORA, along with the in-person interview waivers and simplified reporting. This option freezes the food stamp benefits for households leaving TANF. Households can lose their TANF eligibility due to economic reasons, such as getting employed or increased earnings, and technical reasons, such as reaching their time limit. Either way, their food stamp eligibility and benefit levels will also be affected, and the transitional benefits could smooth their income for up to three months[5]. Despite a short period of time, states can still benefit fiscally since the transitional benefits are federally funded and perhaps also administratively because changes in household circumstances during this period are not subject to quality control checks.

Nevertheless, not as many states have adopted TBA as in the previous options, which makes the reasoning behind adoption even more intriguing. Figure 2.1 shows that only 21 states have provided transitional benefits. Colorado, Arizona, Maryland, Massachusetts, New York, and Pennsylvania were the first to adopt the policy, though Colorado only adopted it from 2002 to 2005. The rest of the states continued the policy through the observation periods.

2.3 Hypotheses Explaining State SNAP Policies Choices and Measurement

The hypotheses are based on the theory proposed by Downs (1957) that democratic governments function as political agents who "formulate policies and serve interest groups in order to gain office" (p. 137). In the context of choosing SNAP policies, I consider two goals of the state governments: maximizing the likelihood of votes for the incumbent and satisfying states' fiscal budget constraints. The first goal is consistent with the office-seeking agent theory and has been the main hypothesis in political economy literature. The second goal was often neglected in the studies of state policy variations. I incorporate this component considering the financial incentives from the program being an unlimited federal grant with states bearing the administrative costs. In

[5] It is later extended to five months by 2010.

the subsequent subsections, I specify the hypotheses regarding how the SNAP policies could affect voters' preferences or the budget constraint. Additionally, I will discuss the measurement and data sources of each element.

2.3.1 Maximizing Voter Preferences

A policy can win more votes by directly benefiting the voters or aligning with their preferences. It has been well-documented in the political economy literature that policy beneficiaries, including those who received cash transfers (Afzal et al., 2011; Bechtel & Hainmueller, 2011; Conover, Zárate, Camacho, & Baez, 2020; De La O, 2013), in-kind transfers (Kogan, 2021; Pop-Eleches & Pop-Eleches, 2012), and municipalities that received more inter-governmental transfers (Johansson, 2003; Kriner & Reeves, 2012; Levitt & Snyder, 1997), are more likely to be mobilized to vote in favor of the incumbent party. However, the targeted beneficiaries are not the only ones whom the incumbent would like to appeal to. General voters' preferences could also greatly influence transfer program policies since they represent a bigger population. Empirical evidence shows associations between the ideology represented in states' policies and the ideology of the voters (Caughey & Warshaw, 2018; Caughey, Warshaw, & Xu, 2017; Griffin & Newman, 2005; Soss et al., 2001). Higher liberalism generally correlates with more generous welfare programs, whereas conservatives tend to advocate for more restrictions. The mechanism may involve citizens selecting candidates who align with their ideology or elected officials adapting to citizen preferences. While distinguishing between these mechanisms is beyond the scope of this paper, the choice of measurement may vary depending on which scenario is presumed. The following illustrates the measurement of voter preferences for direct beneficiaries and general voters, respectively. Both will be tested to predict the adoption of SNAP policy.

Measurement: Direct Beneficiaries

If SNAP policies are enacted to appeal to the beneficiaries, the share of the SNAP-eligible population should be a good predictor for adopting more inclusive policies. However, for policies that expand eligible populations, such as BBCE and vehicle exemptions, it is those who are likely to become eligible through the policies that need to be captured. Consequently, I will use the *share of the population with household income below 180% of the Federal Poverty Guideline (FPL)* as the predictor representing direct beneficiaries. Another common approach in the literature is to use state demographics, such as the shares of children, elderly, below high school education population, and Latino and African American populations, though the racial groups are more complicated. In the studies of TANF, Soss et al. (2001) found that a higher share of the African American population is associated with more restrictive policies, and Rodgers and Tedin (2006) found less effort in state funding; both are opposite to the vote-seeking hypothesis. Considering the demographic groups are less representative of direct beneficiaries than income groups, I will incorporate voters' demographics as control variables rather than representing the voters' preference hypothesis.

To gather the income and demographics of voters, I use the Voting and Registration Supplement of the Current Population Survey (VRS) to collect potential voters. I predict the likelihood of voting or registering to vote using a set of individual characteristics, including age, sex, natural-born citizenship, education, race, marital status, household size, household income, state, and year indicators. This approach identifies a larger share of potential voters than using the self-reported voting status. The rationale is to imitate the perspectives of state governments that individual voters are aggregated into demographic groups, and their preferences are aggregated likewise.

Measurement: General Voters

If we consider politicians' ideology fixed, and whoever is elected will select policies according to their ideology, even if the voters' preferences have changed between elections, then the preferences of the elected officials or parties would be the main determinant of policies (the "selection" scenario). On the other hand, if elected officials adapt to voters' preferences, then the contemporaneous voters' preferences toward welfare policies should perform better in predicting policies (the "adaptation" scenario). These two scenarios need not be mutually exclusive, and my goal is to find which variation matters more.

Party controls, including the *governor's political party* and the *state legislature's party compo-*

sition, are commonly used as predictors of policy choices. Another common measure for political ideology is the voting structure for the last presidential election, specifically *the share of votes for the Democratic or Republican candidates.* Party variations can represent the selection scenario but imply a stronger assumption by considering political ideology as discrete. Several approaches have been made to measure ideology as a spectrum between liberal and conservative. Among those, Berry, Ringquist, Fording, and Hanson (1998) constructs annual observations of *government ideology* based on the voting behaviors of the members of Congress and weighted into state government ideology by the party composition of the governor and state legislators. Their ideology measure ranges between 1 and 100, allowing a broader spectrum of ideological positions.

If the adaptation scenario is truer, voters' opinions will be a more relevant variation for policy choices. I use the General Social Survey (GSS)[6] to construct a *welfare attitude index.* The index equals one if the respondent agrees that the government should be helping the poor or that the national expenditure on assistance to the poor/welfare is too little[7]. To ensure that the opinions are from potential voters, I use the GSS as the training sample and predict the likelihood of supporting welfare on the potential voters' sample from VRS.

An alternative measure of voter preferences for welfare policies is racial prejudice, specifically the prejudice that welfare recipients are mostly Black Americans — the so-called "race-coding" phenomenon. Such prejudice is found to be strongly associated with attitudes toward public welfare, and the more citizen racism against Blacks, the less likely that the government would be spending more on welfare (Gilens, 1995, 1996; Hunt, 2007; Peffley, Hurwitz, & Sniderman, 1997; Sanford F. Schram, Joe Soss, & Richard C. Fording, 2003; Soss et al., 2001). The GSS covers two beliefs behind race-coding: 1) blacks have innate inferiority, and 2) blacks lack motivation. I construct a *racism index* if the respondent answered yes to the question that "most (Negroes/Blacks/African-Americans) just don't have the motivation or willpower to pull themselves up out of poverty", or if they scored at least five out of seven in the scale of blacks' tendency to be lazy. For respondents who are not in the sampling of these two questions, I resort to the questions asking if they think

[6] NORC at the University of Chicago, `https://gss.norc.org/get-documentation`

[7] These two questions were rotated among subsamples within a survey year; therefore, choosing union instead of the intersection of the two questions retains the most sample size.

the government should help improve the conditions of Blacks or if they think the government is spending too much on assistance to Blacks. Similar to the welfare attitude index, I also predict the likelihood of racism among the VRS voters. As expected, the racism and welfare attitude indexes are negatively correlated, with Pearson's correlation coefficient around -0.166.

2.3.2 Fiscal Incentives

Even if policies for SNAP do not gain votes, states still have incentives to utilize the program due to its federal funding. Research has documented states' tendency to steer welfare recipients from partial to full federal-funded programs, such as from AFDC to SSI (Goodman-Bacon & Schmidt, 2020; Kubik, 2003; Schmidt & Sevak, 2004). The fiscal benefits from SNAP could be substantial, considering that the federal government fully funded more than 60 billion dollars of SNAP benefits annually, covering over 20 million households.

Using data from the Annual Survey of State and Local Government Finances, U.S. Census Bureau, I construct a measure capturing states' fiscal incentives — *the ratio of states' own source of revenues*[8] *to their total expenditure*. The lower the ratio, the higher the incentives for the states to seek more federal grants, especially considering the prevalent balanced budget requirements that limit states' options of leaving deficits.

SNAP Administration

Despite the fiscal gains, policies are not implemented without costs. Although the SNAP benefits are fully funded by the federal government, the administrative costs of operating the program are shared between the state and the federal government. The administrative costs vary widely across states. In 2015, the administrative cost of SNAP per case ranged from 81.37 dollars for Wyoming to 6.96 dollars for Florida. Higher administrative costs may create incentives for states to adopt policies that help reduce the costs, or contrarily, it might prevent the states from spending more on new policies. Although either direction is interesting, the latter is more likely to be true. According to a study on states' administrative costs from the USDA, 72% of the costs remained

[8] I calculate this by subtracting federal transfers from states' total revenue.

unexplained after accounting for caseloads, policies, demographics, and economic circumstances (Geller et al., 2019). The findings imply some unobserved differences in state administrations that persist over time, which may lead to differential capability to adopt new options. I gather the *SNAP administrative costs* per case from the State Activity Reports FY1994 to FY2015 published by the USDA.

In addition to the administrative costs, I incorporate state error rates as an administrative factor for policy adoption. States with error rates above 6% for two consecutive years will receive financial penalties and are subject to correction actions. This may motivate states to adopt policies that simplify the administration. On the other hand, high error rates may indicate low administrative efficiency, which could hinder the adoption of new policies. Either direction is acceptable.

2.3.3 Economic Circumstances

Economic circumstances are an important factor in both voters' preferences and fiscal constraints. Governments tend to expand transfer programs during economic downturns, and SNAP was one such program during the Great Recession and the COVID-19 pandemic when the federal government raised the benefit levels. Expanding the program could facilitate the mechanism of automatic stabilizers and secure votes while receiving more federal grants. I calculate states' annual *unemployment rate* and *employment-to-population ratio* by taking the average monthly rates published by the Bureau of Labor Statistics. In addition, I include the *poverty rate* (measured by income) and *median household income* to capture the states' general economic wellness aside from the business cycle.

2.4 Empirical Strategy

In the previous chapter, I have described the determinants and measures for state policy adoption decisions. The question remains of how to distinguish causal impacts from correlations. I formulate two econometric specifications. The first one uses the "pre-period" levels of the determinants to predict policy adoptions and timing in the subsequent years up to 2015. The second one utilizes the federal timing, which made the policies available, as an exogenous shock for an intensity-of-treatment style of estimation. I also integrate a machine learning approach, the best subset selection, to choose independent variables for the first specification.

2.4.1 “Pre-period” levels predicting policy adoptions and timing

For each policy, I normalize states’ adoptions and timing between 0 and 1, where 0 means no adoption as of 2015, and 1 means adoption in the first year when the policy became available. Any adoption timing in-between is subtracted from 2015 and divided by the total number of years when the policy was available up to 2015. For example, the starting year for BBCE was 2000. For a state adopting BBCE in 2003, the adoption measure renders 0.8. The following regression estimates what determines this adoption timing decision:

$$adopt_s = \alpha + \sum_k \beta_k factor_s^k + X_s \Gamma + u_s \tag{2.1}$$

$factor_s^k$ is one of the 16 policy determinants described in the previous chapter. X_s controls for voters’ demographics, including age groups (18-34 and over 60), female, race/ethnicity (Black and Hispanic), highest education (below high school and high school graduates), marital status, natural-born citizens, and average household size. I standardize all independent variables to compare their effects and adopt the heteroskedasticity-robust standard errors.

Suppose the first year when the policy became available is denoted as t_0, I examine two definitions of “pre-period”: one calculates the average from 1995 to $t_0 - 1$, while the other considers only the year $t_0 - 1$. For instance, if $t_0 = 2000$, the former method computes the average from 1995 to 1999, while the latter solely focuses on the year 1999. The former approach is smoothed across years, capturing a longer-term status of the states. The latter concept is used in Hoynes and Schanzenbach (2009) to explain county variation in the Food Stamp rollout, representing a more immediate snapshot of the states.

With 16 policy determinants, 10 control variables, and 51 observations, I introduce a data-driven method to reduce the number of policy determinants to enhance degrees of freedom and address potential multicollinearity concerns. The approach employs the best subset selection technique, which identifies a subset of predictors that best explains the variance in the dependent variable, relying on Akaike's information criterion (AIC) and the Bayesian information criterion (BIC). To facilitate this process efficiently, I employ the algorithm developed by Lindsey and Sheather (2015), which utilizes the leap-and-bound method, and generates recommended models from AIC and BIC, respectively.

There is no distinct preference between AIC and BIC in the context of small sample sizes. AIC tends to overfit, and BIC is often too parsimonious (Hastie, Tibshirani, & Friedman, 2009). My strategy is to rely on the full model for the magnitude of coefficients to avoid omitted variable bias. Then, I find the most important determinants by the choices of AIC and BIC and their statistical significance in the reduced model, which suffers less from multicollinearity-contaminated standard errors.

2.4.2 Intensity-of-treatment and time-varying determinants

One drawback of the first approach is that it could not capture the policy decisions due to contemporaneous circumstances. For example, if states had very low unemployment rates during the "pre-period" but in later years had a recession, which drove them to adopt the policy, then the first approach underestimates the effects of the economy. Figure 2.2, 2.3, and 2.4 examine the time variation of each factor. I group the states with a high average level of the factors (top 25%) and a low average level (bottom 25%) from 1995 to 1999, which are the intersection of pre-periods for all policies. Although the high and low states remain high and low in subsequent years, there appears to be non-trivial, if not drastic, within-state variations for all of the determinants. To explore such within-state variation and capture states' responses to contemporaneous circumstances, I add the second specification.

The second specification utilizes the timing of when the federal government made the state options available. It is an exogenous shock because the states cannot decide whether the policy will become an option or the timing of it. The shock had a different intensity of treatment across states, depending on the levels of the determinants. Unlike the first approach, the observations were made annually, and the determinants lagged one year:

$$adopt_{st} = \alpha + \sum_{k} \beta_k (factor^k_{s,t-1} \times Post_t) + X_{st-1}\Gamma + \theta_s + \lambda_t + u_{st} \tag{2.2}$$

where $adopt_{st}$ indicates whether state s adopts the policy in calendar year t, which ranges from 1996 to 2015. s includes 50 states and the District of Columbia. Each of the policy determinants $factor^k_{s,t-1}$ is lagged one year and interacts with $Post_t$, which is an indicator of whether year t is after the federal regulation changed. Suppose t_0 is the year when the policy option took effect, then $Post_t = 1$ if $t \geq t_0$, and $Post_t = 0$ if $t < t_0$. X_{st-1} is a vector of control variables lagged one year, including voters' demographics and whether the states had legislative sessions[9]. State fixed effects and year fixed effects are denoted by θ_s and λ_t. All independent variables are standardized in the regression, and I clustered standard errors at the state level. Table 2.1 displays summary statistics for the independent variables in columns (5) and (6) before standardization.

Equation (2.2) is effectively an intensity of treatment specification. The treatment $Post_t$ is imposed equally upon all states, but the treatment intensity varies with $factor^k_{s,t-1}$. States with higher/lower levels of the factors are more/less likely to adopt the policy than those with lower/higher ones. β_k estimates the difference of likelihood to adopt the policy by increasing one standard deviation of $factor^k_{s,t-1}$ in the post-year.

This identification strategy relies on the assumption that the timing of the federal change is not driven by states' adoption decisions, and neither is the lagged one-year level of the factors. States eager to adopt the policies might have expressed their demand to the USDA, but it was unlikely that the states could choose the specific timing of the change. For the policy adoption to reversely determine the factors in the prior year, it had to be that the state governments were implementing (some form of) the policies before the official adoption year. This was also unlikely because the states had to notify or even acquire permission from the federal government for these policies. This affects the criteria for their quality control evaluation. If they started the policy unofficially, they would end up with a high error rate and receive financial penalties.

[9]Some states only had legislative sessions every other year while others had it every year. Data source comes from The Book of the States Archive.

2.5 Results

Before delving into specific policies, I provide an overview of states' adoption of inclusive policies. I aggregate the total number of policies adopted by 2015 as the dependent variable for Equation (2.1), and the cumulative number of policies adopted to year t for Equation (2.2). Table 2.2 shows that the 95 to 99 average models have higher adjusted R^2 than 99 only. The poverty rate has the largest effect — one standard deviation (about 4 percentage points) increase in the poverty rate leads to 1.6 more inclusive policies adopted, which is 39% of the average number of policies (4.07) adopted. The share of voters supporting welfare policies also has a large effect, with 0.9 more policies adopted per 4.3 percentage points increase in more voters supporting welfare. The consistent selection of SNAP administrative costs and SNAP error rates across most models suggests that administrative factors could serve as robust predictors for the adoption of inclusive policies. While higher SNAP error rates may prompt states to adopt policies that streamline rules and procedures, higher administrative costs (or implied inefficiency) prohibit them from doing so. In Table 2.8, column (1), the contemporaneous SNAP administrative costs continue to have a negative effect on adopting policies by about 11%. A similar effect is observed for the share of voters potentially eligible for SNAP. Notably, the share of votes to the Democratic President in the last election also yields a comparable effect in magnitude by increasing about 11% more policies adopted. In sum, states appear to be most affected by their general economic well-being, specifically by higher poverty rates. Secondly, they respond to voters' preferences, and thirdly, they are motivated by their high error rates. However, contemporaneous surges in eligible populations and administrative costs might impede them from adopting these policies.

In Table 2.3, the two most influential predictors for BBCE are the share of voters supporting welfare and the share of Democrats in the state upper house. One standard deviation increase in these variables predicts the adoption of BBCE more than 7 years earlier, which is greater than the average. Both are statistically significant even in the full model. In Table 2.8, column (2), the contemporaneous unemployment rate is another important determinant for BBCE. One standard deviation increase in the unemployment rate leads to a 33% higher likelihood for a state to adopt

BBCE, aligning with observations from Figure 2.1. Overall, states that adopted BBCE during non-recession periods could be influenced by political preferences, while BBCE, in general, is perceived as a tool for stabilizing the economy during economic downturns.

In Table 2.4, the vehicle exemptions are better explained by the 1999 level of determinants than the 1995-1999 average. The employment-population ratio has the largest influence, delaying vehicle exemptions by 6.75 years per one standard deviation increase (about 4 percentage points). The share of Democrats in the lower house surprisingly predicts 4.5 years later in adopting the policy, but it appears that the political preference variables do not provide consistent prediction to vehicle exemptions but are rather noisy. Higher states' own revenue-expenditure ratio indicates less need for the policy, with every 10 percentage points more expenditure covered by self-provided revenue leading to a delay of 3.8 years in exempting vehicles. Taken together, despite being an expansive policy, vehicle exemptions are characterized quite differently from BBCE, with the economic and fiscal factors appearing to be more important drivers than political preferences, and none of the contemporaneous variations exert any particular influence.

Face-to-face interviews have fewer factors that have a pronounced influence. In Table 2.5, the share of voters having racism has the largest coefficient, but the direction is opposite to the theory. Vote shares for Democrats and Republicans in the last presidential election align with the expected direction, but the magnitudes are only about a 1 to 2-year difference in adoption timing. Conversely, in Table 2.8, column (4), the contemporaneous increase in voters eligible for SNAP leads to 30% lower likelihood for states to waive face-to-face interviews. It is generally challenging to rationalize states' adoption of in-person interview waivers. Perhaps the execution difficulties discussed in the previous chapters complicate states' decision-making processes. Whether it made the adoptions random requires further studies.

Table 2.6 shows that the political determinants have the largest effects in predicting simplified reporting. The share of votes for the Republican candidate in the last presidential election delayed the adoption by 2.8 years, which is 24% of the mean. On the other hand, having a Democratic governor and more voters supporting welfare led to earlier adoptions by approximately 2 years. Referring to Table 2.8, column (5), the contemporaneous variations of the votes for presidents, the welfare, and race attitudes continue to have effects, although not always aligning with theoretical expectations. Contrarily, Table 2.6 and 2.8 show consistent influences from the SNAP administrative costs and error rates. As a policy directly reduces states' administrative burden, states with higher error rates may use this policy to reduce the errors, but states with higher administrative costs may be less efficient in adopting new policies. As this reduced burden would be passed on to the households, voters' preferences also matter, although which preferences favor simplified reporting is unclear, perhaps due to its more administrative-oriented nature.

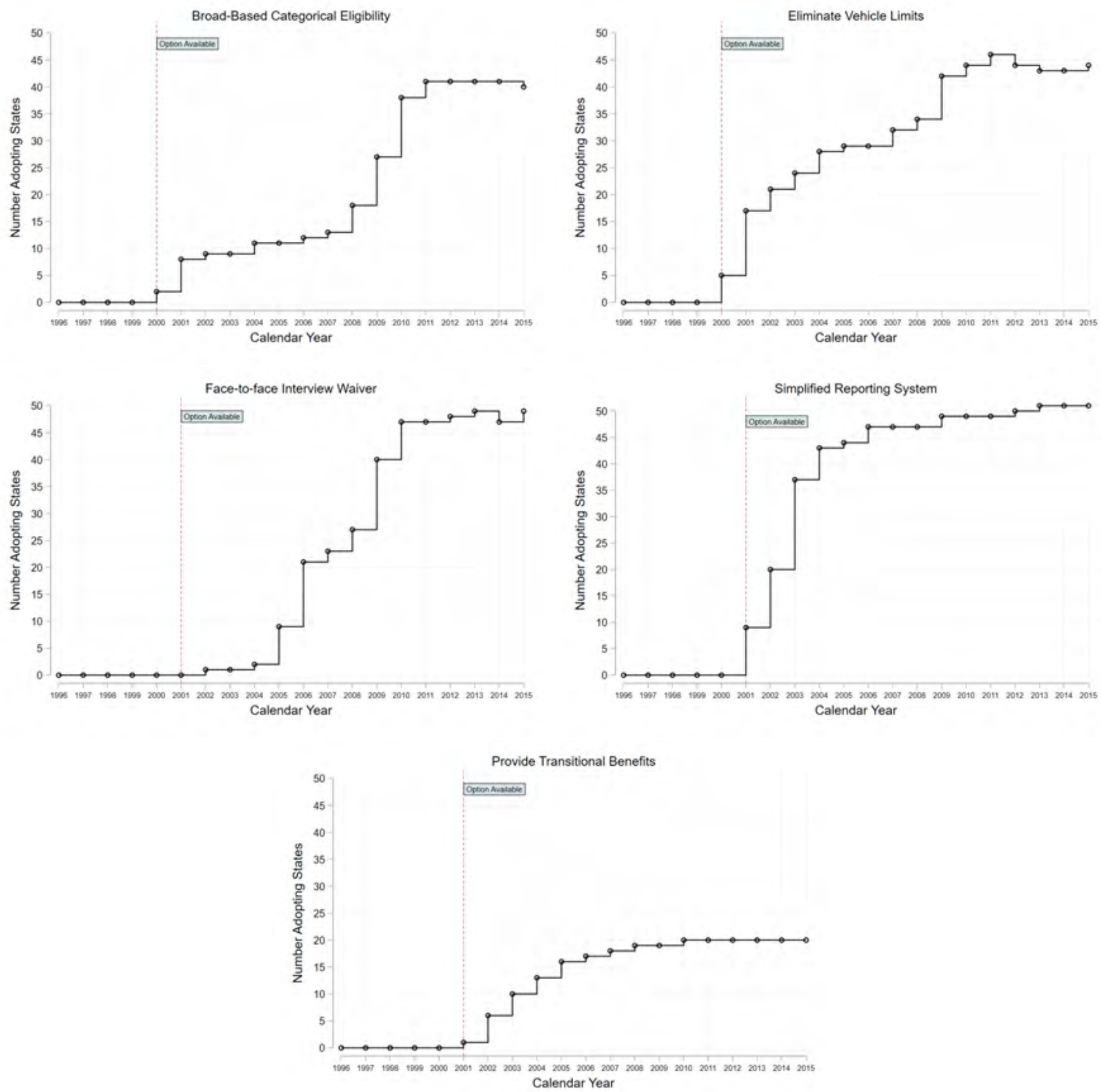
Most variations in adopting transitional benefits remain unexplained. As in Table 2.7, the adjusted R^2 is at most 0.1 using the best subset selection. Nevertheless, the democratic party controls and SNAP error rates stand out in explaining transitional benefits. In Table 2.7, the share of Democrats in the lower house and the SNAP error rates determine 1.5 to 2 years earlier adoption. In Table 2.8, column (6), having a democratic governor increased the likelihood of adoption by 26%. However, the contemporaneous SNAP error rates show effects opposite to those of the pre-period levels. While higher error rates being a long-term status predicts earlier adoption, contemporaneous error rates may diminish states' inclination to offer more services. Based on the magnitude, the Democratic party's control and the SNAP error rates have an equal influence on adopting transitional benefits.

2.6 Conclusion

In this paper, I examined the rationale behind states' SNAP policy choices through the lens of an office-seeking agent bounded by state budget constraints. Drawing from relevant literature, I tested various determinants of policy choices, including voter preferences for incumbents, fiscal incentives for federal grants, administrative efficiency improvements, and economic stabilization responsibilities. Looking at overall inclusive policy adoptions, poverty rates emerge as an important driver, followed by voters' political preferences and SNAP error rates. For policies that expand eligibility, BBCE is motivated by party controls from the Democrats and unemployment rates, while vehicle exemptions are deterred by states with better economic and fiscal circumstances. For policies that ease administrative processes, states with higher error rates favor the simplified reporting system. However, waiving face-to-face interviews appears less predictable. Error rates are also influential in transitional benefits provision, with differential effects based on them being a long-term status versus contemporaneous changes. In summary, while economic indicators and political preferences substantially influence policy adoption, administrative efficiency, and program-specific factors also play pivotal roles in the context of SNAP policies.

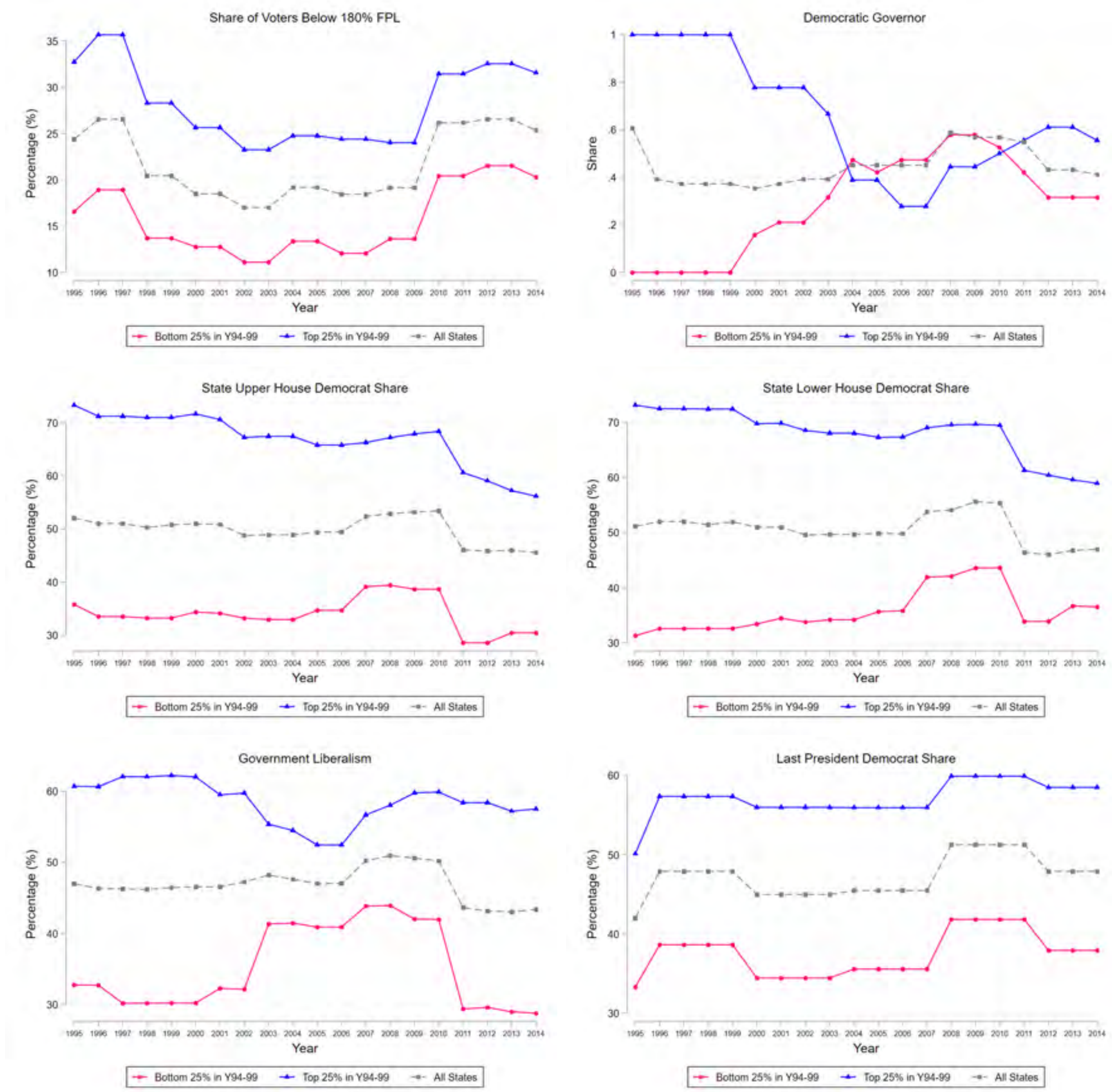
Though this paper is specific to SNAP policies, several findings are consistent with those of other programs. For example, liberal-leaning political preferences consistently play a role in TANF policies, Medicaid expansion, SNAP BBCE, simplified reporting, and transitional benefits, though the magnitude varies. Notably, unlike TANF and Medicaid, SNAP eligibility policies are leveraged as economic stabilizers, which hints at a well-designed fiscal federalism framework. However, this paper only considers the states as office-seeking agents without other motives. Despite the fact that I also include fiscal constraints and program-specific administrative factors, many of the variations remain unexplained. Different theories proposing alternative sets of factors or improved measurements for existing factors could continue to enhance our understanding of state policy variations.

Figure 2.1: Trends of State SNAP Policy Adoptions Y1996-Y2015



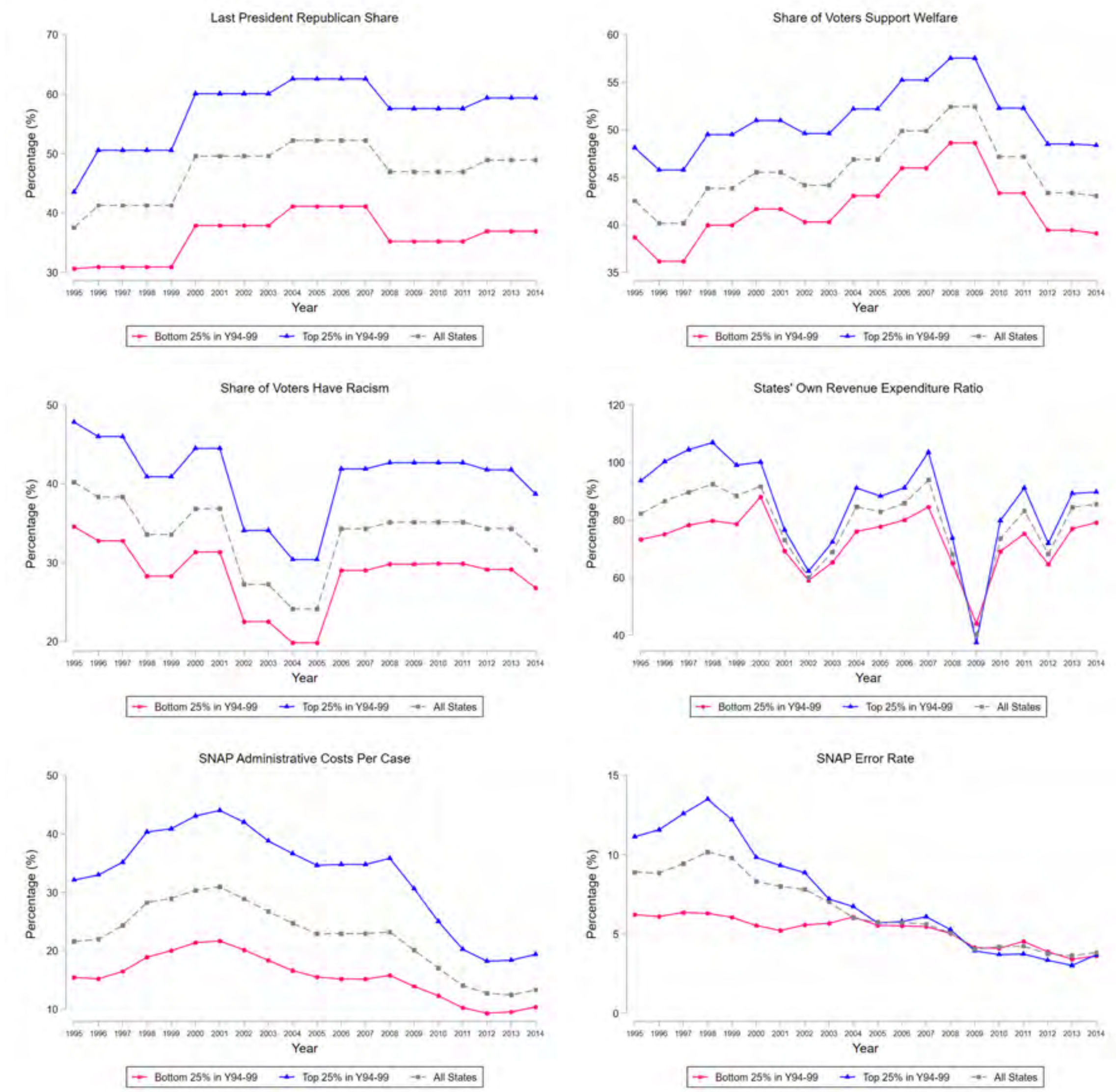
Note: The SNAP Policy Database has monthly entries. Annual adoptions are constructed by adopting for at least one month of the year. On average, states adopt 2.14 of these five policies annually from 2002 to 2015.

Figure 2.2: Variations of Policy Determinants Over Time



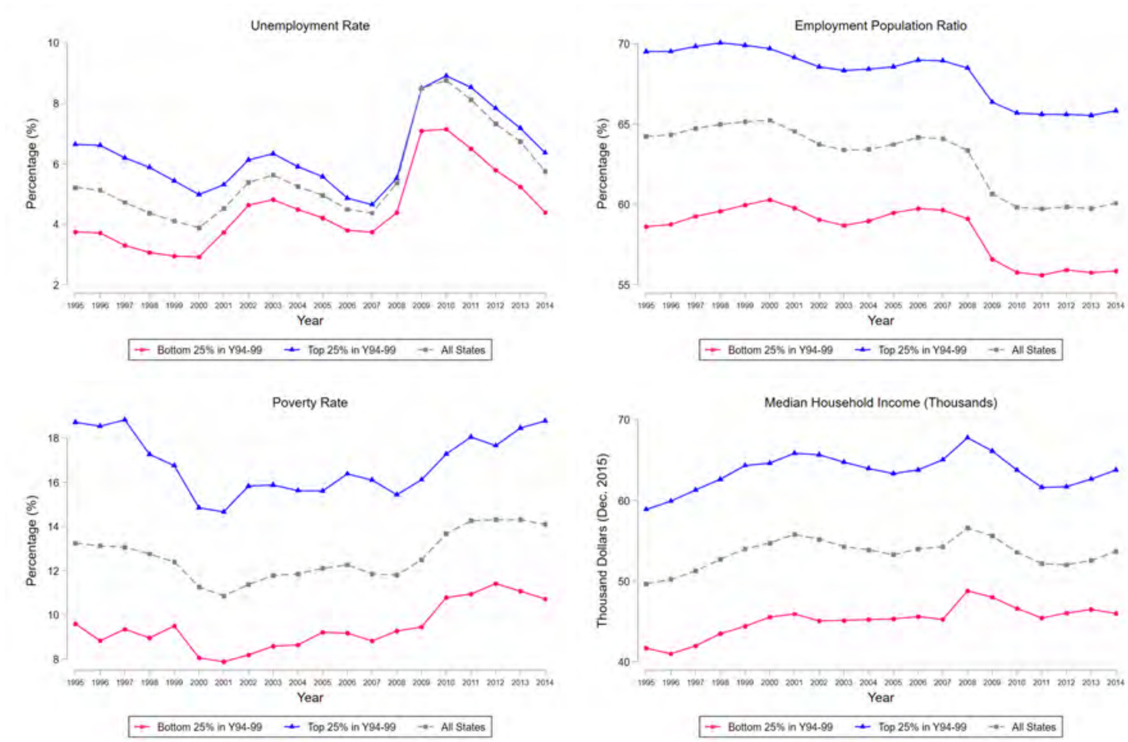
Note: This figure plots the annual variation of each policy determinant while also examining state variation. States having the top 25% and bottom 25% levels of the determinants during the "pre-period (years before the options became available)" years are grouped separately to observe their variations in subsequent years. Since the policies started at different years but most ranged from Y2000-Y2002, this figure uses Y2000 as the cutoff year. Generally, the between-state variations remain stable, while some within-state variations appear to be worth exploiting.

Figure 2.3: Variations of Policy Determinants Over Time - Cont'd



Note: This figure plots the annual variation of each policy determinant while also examining state variation. States having the top 25% and bottom 25% levels of the determinants during the "pre-period (years before the options became available)" years are grouped separately to observe their variations in subsequent years. Since the policies started at different years but most ranged from Y2000-Y2002, this figure uses Y2000 as the cutoff year. Generally, the between-state variations remain stable, while some within-state variations appear to be worth exploiting.

Figure 2.4: Variations of Policy Determinants Over Time - Cont'd



Note: This figure plots the annual variation of each policy determinant while also examining state variation. States having the top 25% and bottom 25% levels of the determinants during the "pre-period (years before the options became available)" years are grouped separately to observe their variations in subsequent years. Since the policies started at different years but most ranged from Y2000-Y2002, this figure uses Y2000 as the cutoff year. Generally, the between-state variations remain stable, while some within-state variations appear to be worth exploiting.

Table 2.1: Mean and Standard Deviations of Policy Determinants

	Y95-99 Average		Y99		Y95-14 Average	
	Mean	Std	Mean	Std	Mean	Std
	(1)	(2)	(3)	(4)	(5)	(6)
Policy Determinants						
Share of Voters Below 180% FPL	23.67	(6.913)	20.45	(5.814)	21.68	(6.669)
Democratic Governor	0.424	(0.495)	0.373	(0.488)	0.449	(0.498)
State Upper House Democrat Share	51.05	(15.57)	50.79	(15.53)	49.90	(16.55)
State Lower House Democrat Share	51.70	(15.94)	51.94	(15.74)	50.70	(15.70)
Government Liberalism	46.45	(12.76)	46.45	(14.07)	46.89	(14.81)
Last President Democrat Share	46.73	(8.743)	47.91	(8.487)	47.21	(11.49)
Last President Republican Share	40.53	(8.157)	41.28	(8.435)	47.22	(11.07)
Share of Voters Support Welfare	42.12	(4.284)	43.84	(3.957)	45.63	(5.071)
Share of Voters Have Racism	36.79	(5.985)	33.56	(5.205)	33.48	(6.774)
States' Own Revenue Expenditure Ratio	87.92	(12.07)	88.47	(9.806)	79.20	(16.87)
SNAP Admin Costs Per Case	25.00	(9.506)	28.93	(9.405)	22.40	(10.87)
SNAP Error Rate	9.430	(2.997)	9.783	(3.366)	6.504	(3.214)
Unemployment Rate	4.709	(1.244)	4.105	(1.057)	5.630	(1.963)
Employment Population Ratio	64.69	(4.205)	65.15	(4.017)	62.95	(4.615)
Poverty Rate	12.92	(3.894)	12.39	(3.418)	12.64	(3.531)
Median Household Income (Thousands)	51.57	(7.998)	53.99	(8.169)	53.48	(8.290)
Control Variables						
Share of Voters Age 18-34	26.10	(3.953)	24.32	(3.980)	25.31	(3.766)
Share of Voters Age > 60	23.46	(3.715)	23.78	(3.755)	24.41	(3.603)
Share of Voters Female	53.44	(1.471)	53.51	(1.529)	53.17	(1.311)
Share of Voters Race Black	10.41	(11.98)	10.30	(11.97)	10.57	(11.15)
Share of Voters Race Hispanic	3.596	(5.679)	3.754	(5.994)	5.111	(6.752)
Share of Voters Education < HS	11.33	(4.069)	10.29	(3.674)	8.972	(3.771)
Share of Voters Education HS	32.72	(4.365)	31.85	(4.309)	31.54	(4.699)
Share of Voters Natural Born Citizen	94.46	(4.289)	94.23	(4.409)	92.64	(5.761)
Share of Voters Married	64.12	(6.393)	64.28	(6.553)	61.17	(6.233)
Voters' Average Family Size	2.850	(0.155)	2.823	(0.161)	2.806	(0.162)
Observations	255		51		1020	

All shares and rates measured in percentages. Democratic governor is a dummy variable. Government liberalism is an index ranging from 0 to 100. All monetary value adjusted to the December 2015 Consumer Price Index. Data gathered from multiple sources: Integrated Public Use Microdata Series, Current Population Survey: Voting and Registration Supplement (VRS) and Annual Social and Economic Supplement (ASEC); United States Governors 1775-2020 by Jacob Kaplan, University of Pennsylvania; The Statistical Abstract of the United States; MIT Election Data and Science Lab; Citizen & Government Ideology Measure by Richard C. Fording; General Social Survey; Annual Survey of State and Local Government Finances; SNAP State Activity Reports; SNAP Quality Control Annual Reports; US Bureau of Labor Statistics.

Table 2.2: Best Subset Selection of Predictors for Number of Policies Adopted by 2015

	Y95 to Y99 Average			Y99		
	Full	AIC	BIC	Full	AIC	BIC
	(1)	(2)	(3)	(4)	(5)	(6)
Share of Voters Below 180% FPL	0.106			-0.225		
	(0.434)			(0.464)		
Democratic Governor	0.379			-0.00520		
	(0.292)			(0.223)		
State Upper House Democrat Share	0.710	0.444		0.626	0.517*	0.228
	(0.344)	(0.220)		(0.343)	(0.217)	(0.125)
State Lower House Democrat Share	-0.453	-0.418		-0.529	-0.418	
	(0.270)	(0.245)		(0.367)	(0.259)	
Government Liberalism	-0.393			0.00425		
	(0.447)			(0.446)		
Last President Democrat Share	-0.461			-0.176		
	(0.851)			(1.064)		
Last President Republican Share	-0.242			0.148		
	(0.746)			(1.016)		
Share of Voters Support Welfare	0.906	0.718**		0.111		
	(0.439)	(0.248)		(0.331)		
Share of Voters Have Racism	0.417	0.490		0.179		
	(0.403)	(0.284)		(0.272)		
States' Own Revenue Expenditure Ratio	-0.126			-0.186		
	(0.264)			(0.229)		
SNAP Admin Costs Per Case	-0.348	-0.361*	-0.287	-0.362	-0.335	
	(0.214)	(0.175)	(0.182)	(0.285)	(0.225)	
SNAP Error Rate	0.509*	0.433**	0.274	0.396*	0.309*	0.337**
	(0.191)	(0.151)	(0.138)	(0.181)	(0.124)	(0.116)
Unemployment Rate	-0.408	-0.266		-0.210		
	(0.425)	(0.252)		(0.307)		
Employment Population Ratio	-0.127			-0.201		
	(0.486)			(0.483)		
Poverty Rate	1.602*	1.172**		0.343		
	(0.621)	(0.426)		(0.499)		
Median Household Income	1.321	0.701		0.144		
	(0.657)	(0.408)		(0.756)		
Observations	51	51	51	51	51	51
Adjusted R^2	0.189	0.320	0.152	-0.020	0.223	0.188
Average Number of Policies Adopted = 4.07						

The dependent variable is the total number of policies adopted (five the maximum) by 2015. The first three columns used the average between 1996 and 1999 for the explanatory variables. The last three columns used observations in 1999 for the explanatory variables. All independent variables are standardized. Control variables include voters' demographics (age share, sex, education, race, married, natural-born citizens) and whether the state had legislative sessions during the observation year.
Heteroskedasticity-robust standard errors. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.3: Best Subset Selection of Predictors for Broad-Base Categorical Eligibility Adoption and Timing

	Y95 to (t_0 - 1) Average			t_0 - 1		
	Full	AIC	BIC	Full	AIC	BIC
	(1)	(2)	(3)	(4)	(5)	(6)
Share of Voters Below 180% FPL	-0.237	-0.301**	-0.139	-0.146	-0.101	-0.0970
	(0.183)	(0.0877)	(0.0754)	(0.147)	(0.0566)	(0.0538)
Democratic Governor	0.0744			0.0167		
	(0.101)			(0.0951)		
State Upper House Democrat Share	0.434**	0.358**	0.229*	0.341*	0.299**	0.215**
	(0.145)	(0.110)	(0.106)	(0.147)	(0.0924)	(0.0603)
State Lower House Democrat Share	-0.281	-0.257	-0.173	-0.299	-0.281*	-0.213*
	(0.154)	(0.138)	(0.131)	(0.155)	(0.124)	(0.0977)
Government Liberalism	-0.136			-0.0876	-0.0783	
	(0.125)			(0.170)	(0.0571)	
Last President Democrat Share	-0.366	-0.254**		-0.154		
	(0.284)	(0.0710)		(0.336)		
Last President Republican Share	-0.177			-0.0770		
	(0.243)			(0.332)		
Share of Voters Support Welfare	0.476**	0.470***	0.301***	0.132	0.139	
	(0.130)	(0.100)	(0.0804)	(0.116)	(0.0787)	
Share of Voters Have Racism	0.224	0.262*	0.137	0.0662	0.0885	
	(0.127)	(0.124)	(0.114)	(0.116)	(0.0745)	
States' Own Revenue Expenditure Ratio	0.0241			-0.0548		
	(0.0994)			(0.0638)		
SNAP Admin Costs Per Case	-0.125	-0.0960		-0.114	-0.124	
	(0.0742)	(0.0571)		(0.106)	(0.0637)	
SNAP Error Rate	0.0958	0.0719		0.109	0.110*	0.106*
	(0.0561)	(0.0469)		(0.0534)	(0.0405)	(0.0458)
Unemployment Rate	0.0685	0.113		0.00280		
	(0.141)	(0.0702)		(0.110)		
Employment Population Ratio	0.0163		-0.223*	-0.0473		
	(0.163)		(0.104)	(0.182)		
Poverty Rate	0.378	0.311**		0.0241		
	(0.216)	(0.0946)		(0.182)		
Median Household Income	0.127			0.0292		
	(0.339)			(0.253)		
Observations	51	51	51	51	51	51
Adjusted R^2	0.239	0.361	0.257	0.045	0.267	0.208
Dependent Variable Mean = 0.42						

The dependent variable is normalized adoption timing, with no adoption being 0 and adoption in the first year being 1. t_0 refers to the first year when the policy became available to the states. $t_0 = 2000$ for BBCE. The first three columns used the average between 1995 to 1999 for the explanatory variables. The last three columns used observations in 1999 for the explanatory variables. All independent variables are standardized. Control variables include voters' demographics (age share, sex, education, race, married, natural-born citizens) and whether the state had legislative sessions during the observation year.
Heteroskedasticity-robust standard errors. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.4: Best Subset Selection of Predictors for Vehicle Exemption Adoption and Timing

	Y95 to (t_0 - 1) Average			t_0 - 1		
	Full	AIC	BIC	Full	AIC	BIC
	(1)	(2)	(3)	(4)	(5)	(6)
Share of Voters Below 180% FPL	-0.440*	-0.455***	-0.194*	-0.139	-0.132	
	(0.173)	(0.124)	(0.0793)	(0.128)	(0.0983)	
Democratic Governor	-0.191	-0.179*		-0.108	-0.108	
	(0.159)	(0.0765)		(0.0833)	(0.0746)	
State Upper House Democrat Share	0.0837			0.180	0.180	
	(0.153)			(0.127)	(0.0938)	
State Lower House Democrat Share	-0.109			-0.308*	-0.317**	
	(0.139)			(0.123)	(0.101)	
Government Liberalism	0.220	0.220*		0.272	0.275*	
	(0.215)	(0.0879)		(0.137)	(0.114)	
Last President Democrat Share	0.245	0.299		-0.0465		
	(0.297)	(0.216)		(0.350)		
Last President Republican Share	0.363	0.410		0.213	0.263**	
	(0.263)	(0.213)		(0.292)	(0.0866)	
Share of Voters Support Welfare	-0.0507			-0.234*	-0.231*	
	(0.198)			(0.0989)	(0.0905)	
Share of Voters Have Racism	-0.00283			-0.140	-0.147	
	(0.165)			(0.139)	(0.122)	
States' Own Revenue/Expenditure	0.0000896			-0.253**	-0.253***	-0.106*
	(0.122)			(0.0723)	(0.0593)	(0.0520)
Federal Share SNAP Admin Costs	-0.0515			0.000825		
	(0.117)			(0.113)		
SNAP Error Rate	-0.00569			-0.0129		
	(0.101)			(0.0707)		
Unemployment Rate	0.0173			-0.104	-0.114	
	(0.163)			(0.0981)	(0.0841)	
Employment Population Ratio	-0.479*	-0.530***	-0.318***	-0.453**	-0.441**	-0.235**
	(0.216)	(0.126)	(0.0822)	(0.160)	(0.121)	(0.0811)
Poverty Rate	-0.358	-0.360		-0.0587		
	(0.290)	(0.177)		(0.177)		
Median Household Income	-0.628	-0.644**		0.117	0.154	0.240**
	(0.306)	(0.179)		(0.239)	(0.128)	(0.0789)
Observations	51	51	51	51	51	51
Adjusted R^2	-0.044	0.194	0.057	0.155	0.273	0.110
Dependent Variable Mean = 0.64						

The dependent variable is normalized adoption timing, with no adoption being 0 and adoption in the first year being 1. t_0 refers to the first year when the policy became available to the states. $t_0 = 2000$ for Vehicle Exemption. The first three columns used the average between 1995 to 1999 for the explanatory variables. The last three columns used observations in 1999 for the explanatory variables. All independent variables are standardized. Control variables include voters' demographics (age share, sex, education, race, married, natural-born citizens) and whether the state had legislative sessions during the observation year.
Heteroskedasticity-robust standard errors. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.5: Best Subset Selection of Predictors for In-Person Interview Waiver Adoption and Timing

	Y95 to (t_0 - 1) Average			t_0 - 1		
	Full	AIC	BIC	Full	AIC	BIC
	(1)	(2)	(3)	(4)	(5)	(6)
Share of Voters Below 180% FPL	-0.0592			-0.0619		
	(0.135)			(0.0754)		
Democratic Governor	-0.00630			0.00516		
	(0.0829)			(0.0401)		
State Upper House Democrat Share	-0.111			-0.0936		
	(0.109)			(0.0676)		
State Lower House Democrat Share	0.0840			0.0444		
	(0.0849)			(0.0694)		
Government Liberalism	0.0145			0.0359		
	(0.110)			(0.0551)		
Last President Democrat Share	0.0681		0.114	0.131	0.109*	0.122*
	(0.208)		(0.0573)	(0.226)	(0.0449)	(0.0485)
Last President Republican Share	-0.0681	-0.123*		-0.00730		
	(0.187)	(0.0501)		(0.194)		
Share of Voters Support Welfare	0.115			0.0635	0.0900	
	(0.102)			(0.0913)	(0.0493)	
Share of Voters Have Racism	0.164	0.0933		0.157	0.129*	
	(0.0855)	(0.0482)		(0.0867)	(0.0485)	
States' Own Revenue Expenditure Ratio	0.0663			0.0292		
	(0.0780)			(0.0395)		
SNAP Admin Costs Per Case	0.00233			-0.0175		
	(0.0531)			(0.0556)		
SNAP Error Rate	-0.0310			0.0155		
	(0.0440)			(0.0512)		
Unemployment Rate	-0.0415	-0.0564		-0.0333		
	(0.0910)	(0.0294)		(0.0563)		
Employment Population Ratio	-0.00256			-0.0308		
	(0.120)			(0.0856)		
Poverty Rate	0.0121			-0.0495		
	(0.160)			(0.0931)		
Median Household Income	-0.0722			-0.109		
	(0.240)			(0.117)		
Observations	51	51	51	51	51	51
Adjusted R^2	-0.043	0.212	0.143	-0.101	0.191	0.123
Dependent Variable Mean = 0.51						

The dependent variable is normalized adoption timing, with no adoption being 0 and adoption in the first year being 1. t_0 refers to the first year when the policy became available to the states. $t_0 = 2001$ for In-Person Interview Waiver. The first three columns used the average between 1995 to 2000 for the explanatory variables. The last three columns used observations in 2000 for the explanatory variables. All independent variables are standardized. Control variables include voters' demographics (age share, sex, education, race, married, natural-born citizens) and whether the state had legislative sessions during the observation year.
Heteroskedasticity-robust standard errors. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.6: Best Subset Selection of Predictors for Simplified Reporting Adoption and Timing

	Y95 to (t_0 - 1) Average			t_0 - 1		
	Full	AIC	BIC	Full	AIC	BIC
	(1)	(2)	(3)	(4)	(5)	(6)
Share of Voters Below 180% FPL	0.0993			-0.0565		
	(0.101)			(0.0645)		
Democratic Governor	0.147*	0.0847		0.0509	0.0423	
	(0.0686)	(0.0474)		(0.0379)	(0.0309)	
State Upper House Democrat Share	0.0765			-0.0612		
	(0.0639)			(0.0533)		
State Lower House Democrat Share	-0.0483			0.0611		
	(0.0648)			(0.0559)		
Government Liberalism	-0.183*	-0.0977		-0.0466	-0.0608	
	(0.0809)	(0.0509)		(0.0543)	(0.0379)	
Last President Democrat Share	-0.109			-0.111		
	(0.183)			(0.161)		
Last President Republican Share	-0.202	-0.0795		-0.132		
	(0.212)	(0.0481)		(0.166)		
Share of Voters Support Welfare	0.137	0.0745		0.1000	0.0988	
	(0.0959)	(0.0390)		(0.0687)	(0.0512)	
Share of Voters Have Racism	0.0197			0.120*	0.0920*	
	(0.0496)			(0.0483)	(0.0394)	
States' Own Revenue Expenditure Ratio	0.0149			-0.0264		
	(0.0641)			(0.0330)		
SNAP Administrative Costs Per Case	-0.0882	-0.0515*	-0.0549*	-0.0330	-0.0562*	
	(0.0469)	(0.0208)	(0.0220)	(0.0278)	(0.0243)	
SNAP Error Rate	0.0882	0.0663*	0.0737*	0.0475	0.0600*	0.0804**
	(0.0441)	(0.0301)	(0.0298)	(0.0317)	(0.0278)	(0.0268)
Unemployment Rate	-0.0373			0.00422		
	(0.0715)			(0.0491)		
Employment Population Ratio	0.0709			-0.0196		
	(0.0905)			(0.0589)		
Poverty Rate	0.211			-0.00866		
	(0.136)			(0.0764)		
Median Household Income	0.324	0.0879		0.0356	0.0641	
	(0.199)	(0.0557)		(0.0954)	(0.0405)	
Observations	51	51	51	51	51	51
Adjusted R^2	0.294	0.392	0.303	0.289	0.414	0.317
Dependent Variable Mean = 0.83						

The dependent variable is normalized adoption timing, with no adoption being 0 and adoption in the first year being 1. t_0 refers to the first year when the policy became available to the states. $t_0 = 2001$ for Simplified Reporting System. The first three columns used the average between 1995 to 2000 for the explanatory variables. The last three columns used observations in 2000 for the explanatory variables. All independent variables are standardized. Control variables include voters' demographics (age share, sex, education, race, married, natural-born citizens) and whether the state had legislative sessions during the observation year.
Heteroskedasticity-robust standard errors. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.7: Best Subset Selection of Predictors for Transitional Benefits Adoption and Timing

	Y95 to (t_0 - 1) Average			t_0 - 1		
	Full	AIC	BIC	Full	AIC	BIC
	(1)	(2)	(3)	(4)	(5)	(6)
Share of Voters Below 180% FPL	-0.00516			-0.0574	-0.134	
	(0.265)			(0.191)	(0.0765)	
Democratic Governor	-0.0361			-0.110		
	(0.143)			(0.0934)		
State Upper House Democrat Share	-0.0764			0.0497	0.0972	0.0912
	(0.169)			(0.185)	(0.0671)	(0.0737)
State Lower House Democrat Share	0.145	0.131*	0.131*	0.0273		
	(0.146)	(0.0640)	(0.0640)	(0.175)		
Government Liberalism	0.0981			0.102		
	(0.188)			(0.125)		
Last President Democrat Share	-0.0183			0.319		
	(0.588)			(0.580)		
Last President Republican Share	0.178			0.499		
	(0.554)			(0.565)		
Share of Voters Support Welfare	0.229			0.212		
	(0.211)			(0.193)		
Share of Voters Have Racism	0.230			0.144		
	(0.184)			(0.220)		
States' Own Revenue Expenditure Ratio	-0.115			-0.0669	-0.115	
	(0.156)			(0.124)	(0.0915)	
SNAP Administrative Costs Per Case	0.0357			-0.0187		
	(0.132)			(0.137)		
SNAP Error Rate	0.110	0.133*	0.133*	0.000877		
	(0.0906)	(0.0593)	(0.0593)	(0.0953)		
Unemployment Rate	-0.135			-0.0961		
	(0.198)			(0.145)		
Employment Population Ratio	-0.0647			-0.0559		
	(0.224)			(0.217)		
Poverty Rate	0.466			0.343		
	(0.333)			(0.227)		
Median Household Income (Thousands)	0.431			0.287		
	(0.473)			(0.293)		
Observations	51	51	51	51	51	51
Adjusted R^2	-0.141	0.100	0.100	-0.245	-0.001	-0.041
Dependent Variable Mean = 0.32						

The dependent variable is normalized adoption timing, with no adoption being 0 and adoption in the first year being 1. t_0 refers to the first year when the policy became available to the states. $t_0 = 2001$ for Simplified Reporting System. The first three columns used the average between 1995 to 2000 for the explanatory variables. The last three columns used observations in 2000 for the explanatory variables. All independent variables are standardized. Control variables include voters' demographics (age share, sex, education, race, married, natural-born citizens) and whether the state had legislative sessions during the observation year.
Heteroskedasticity-robust standard errors. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.8: Policy Adoption Over Lag 1 Interact With Federal Timing Regression Estimates

	Number of Policies	BBCE	Vehicle Exemption	In-Person Interview Waiver	Simplified Reporting	Transitional Benefit
	(1)	(2)	(3)	(4)	(5)	(6)
Share of Voters < 180% FPL	-0.231*	-0.00552	-0.0371	-0.122**	-0.0643*	-0.0319
	(0.0880)	(0.0413)	(0.0451)	(0.0363)	(0.0300)	(0.0426)
Democratic Governor	0.0904	0.00418	0.0275	-0.0143	0.00640	0.0614**
	(0.0483)	(0.0202)	(0.0237)	(0.0201)	(0.0147)	(0.0219)
State Upper House Democrat	0.0944	0.0430	0.00978	0.0259	-0.0246	0.0385
	(0.111)	(0.0467)	(0.0518)	(0.0335)	(0.0286)	(0.0516)
State Lower House Democrat	-0.117	-0.0454	-0.0102	-0.0186	0.0299	-0.0627
	(0.146)	(0.0639)	(0.0602)	(0.0344)	(0.0350)	(0.0671)
Last President Democrat	0.228***	0.0130	0.0596**	0.0158	0.112***	0.0295
	(0.0589)	(0.0174)	(0.0194)	(0.0235)	(0.0120)	(0.0279)
Last President Republican	-0.0276	-0.0578	-0.00367	0.0569	0.0593*	-0.0520
	(0.131)	(0.0567)	(0.0485)	(0.0394)	(0.0289)	(0.0762)
Government Liberalism	-0.0390	0.0308	-0.00318	0.00162	-0.0278	-0.0359
	(0.0725)	(0.0311)	(0.0369)	(0.0294)	(0.0205)	(0.0382)
Share of Voters Support Welfare	0.0646	0.0234	0.00634	0.0128	0.0607*	-0.0229
	(0.140)	(0.0555)	(0.0608)	(0.0340)	(0.0275)	(0.0715)
Share of Voters Have Racism	0.209	0.0658	0.0416	0.0326	0.0951**	-0.0249
	(0.169)	(0.0663)	(0.0780)	(0.0367)	(0.0312)	(0.0859)
States' Own Revenue/Expense	0.0761	0.0203	0.0359	-0.0124	0.0316*	-0.00211
	(0.0493)	(0.0181)	(0.0193)	(0.0227)	(0.0144)	(0.0162)
SNAP Admin Costs	-0.235*	-0.0443	-0.0799	-0.0167	-0.102*	0.00155
	(0.0965)	(0.0375)	(0.0437)	(0.0295)	(0.0466)	(0.0447)
SNAP Error Rate	0.00540	0.0203	0.0520	-0.0293	0.0397*	-0.0669**
	(0.0612)	(0.0267)	(0.0330)	(0.0224)	(0.0174)	(0.0244)
Unemployment Rate	0.181	0.116**	0.0927	0.0362	-0.0211	-0.0603
	(0.104)	(0.0393)	(0.0544)	(0.0378)	(0.0246)	(0.0458)
Employment Population Ratio	-0.0864	0.0545	0.00130	-0.0409	0.00786	-0.123
	(0.148)	(0.0534)	(0.0790)	(0.0507)	(0.0441)	(0.0849)
Poverty Rate	0.0544	-0.0107	0.0305	0.0293	0.00575	0.0259
	(0.0768)	(0.0341)	(0.0361)	(0.0359)	(0.0191)	(0.0468)
Median Household Income	0.210	0.0395	0.0396	0.0370	0.0198	0.0960
	(0.126)	(0.0512)	(0.0530)	(0.0311)	(0.0392)	(0.0575)
Observations	1020	1020	1020	1020	1020	1020
Adjusted R^2	0.866	0.605	0.591	0.747	0.826	0.359
Dependent Variable Mean	2.138	0.355	0.515	0.403	0.631	0.234

State fixed effects and year fixed effects regression. The dependent variable for column (1) is the number of policies (out of five) adopted in the year; columns (2)-(6) are the indicators of adopting the policy. Explanatory variables are lagged one year and interacted with an indicator of years after the option became available. All independent variables are standardized. Control variables include voters' demographics (age share, sex, education, race, married, natural-born citizens) and whether the state had legislative sessions during the observation year.
Clustered standard errors at the state level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Chapter 3

Variations of Wealth Resemblance by Family Relationship Types in Modern Chinese Families

Joint work with C.Y. Cyrus Chu and Kamhon Kan

Social scientists often use the correlation coefficient of family members' socioeconomic status to characterize the status resemblance between them using survey data; a larger correlation often reveals a closer family relationship in the background. Both the achievement indexes and the family members being studied are usually limited by the coverage of survey questionnaires (e.g., availability of measures of socioeconomic status and information on kin network members). These limitations may have constricted our understanding of the family structure in question.

Studies on family members' resemblance in economic outcomes focus on the parents–children correlation in early ones and correlations between siblings in later ones (Mazumder, 2008; Solon, Corcoran, Gordon, & Laren, 1991) and on correlations between multigenerational or extended family members in more recent ones (Jæger, 2012; Mare, 2011, 2015). The reasons why such correlation measures capture the idea of family members' connection may be due to material and affective support (Bengtson, 2001), cognitive and emotional interactions (Modin & Fritzell, 2009), experience and information sharing, or the heredity of intelligence or other traits. The general conceptual idea of the correlation analysis is similar both to that of Sahlins (2011), which involves capturing the nature of kinship as "mutuality of being," and to measuring the resemblance in performance among kinship members (Alvard, 2003; Kasper & Mulder, 2015). Because the family structure is time variant and has dynamic social contexts, it is important to see how such dynamic correlations change as modern Chinese family structure evolves (Chu & Yu, 2009).

The standard way of measuring genetic relatedness between any two family members was first proposed by Wright (1922) and has been broadly used by evolutionary anthropologists. Another time-invariant measure of relatedness is the degree of relationships defined by the Roman civil law. It is now adopted by most countries, including France, Germany, Japan, Switzerland, and Taiwan (Huang, Chen, & Kuo, 2009). The Roman law approach consists of counting from the ego up to the first common ancestor then down to the corresponding relative, and the sum of counts is the degree of relationship.

The advantage of the correlation measure, compared with the genetic relatedness or the Roman law measure, is that it captures the dynamics of complicated social institutions and forces. The disadvantage, however, as noted by Mare (2015), is that the data beyond grandparents are rarely available. In this paper, we try to analyze what are perhaps the best family network data in the world to study what are arguably among the closest family relationships in the world—Chinese families in Taiwan. Relations among members of Chinese families are known to be strong, and their interactions are frequent and affectionate. Their interactions often involve provision of emotional and material support to each other[1].

Our empirical analysis, along the lines of Solon (Solon, 1992, 2014) and Mare (Mare, 2011, 2015), sets out to calculate the correlation of family members' wealth while controlling their personal characteristics. Because of the huge number of observations available to us from the registry observations available to us from the registry records and our efforts in identifying the relatives from the records, we are able to trace an ego's family members, according to the Roman law criteria, from the most direct line, which is parents–children, to aunts/uncles, great aunts/uncles, first cousins, second cousins, etc. (Figure 3.1), and the farthest point of kinship in our observation can have only a 1/32 genetic relatedness with the ego. We therefore obtain correlation coefficients

[1] Evidence in previous studies (Chu & Yu, 2009; Gerardi & Tsai, 2014) shows that the intensity of interaction among family members in Taiwan is higher than in their Western counterparts. For example, while 72.4% of individuals in Taiwan gave cash to parents (Gerardi & Tsai, 2014), only 44.2% of black individuals and 28.5% of white individuals in the United States gave financial support to a family member (Sarkisian & Gerstel, 2004).

for almost the whole family network, which helps us to identify just how closely or how remotely family members resemble each other in economic status. Moreover, the correlation coefficients are estimated separately for paternal and maternal lines, males and females, etc., to test whether some traditional social science hypotheses on Chinese family patterns really can be supported by the data.

3.1 Data and Estimation Method

3.1.1 Data

Our analyses are based on de-identified administrative records of income and wealth in Taiwan provided by the Financial Information Agency (FIA), Ministry of Finance, Taiwan. These records are open to the public and available to all researchers, but any operations involving the data have to be undertaken in the FIA's on-site data center.

To calculate individual wealth, we use three sets of individual wealth records—stock shares, housing, and land. We also derive the total amount of savings deposits based on "interest income" in the income tax records. The FIA originally recorded stock shares at their face value; we measure their market price using the closing prices at the stocks' respective ex-right dates or, in the absence of an ex-right date for the companies in question, their closing price on July 31. For unlisted stocks, we use their net value or face value if their net values are unavailable. The value of housing and land is assessed by the registration authorities for tax purposes. The assessed value is understated by 40–60% compared with the market value. We adjust the land value by the yearly county-level percentage of undervaluation as assessed by the authorities. To avoid short-term fluctuations, we take the 3-y average wealth level for the period 2013–2015.

Taking those who were 25–45 years old in 2015 as egos, we trace their parents, grandparents, and great-grandparents from the FIA records [2]. It is then possible, from the lineal tree, to identify three generations of collateral relatives: the ego's parents, siblings, first cousins, second cousins,

[2] These egos were all born after 1970, long after the end of the Second World War, and were exposed to 9 years of compulsory education, which was launched in 1968, affecting people born after 1956. They have experienced the period when Taiwan's income inequality began to deteriorate (after 1980). Other things being equal, the wealth variations of these egos are likely to be larger and are more susceptible to the influence of their family background.

parents' siblings (aunts/uncles), parents' first cousins (first cousins once removed), and grandparents' siblings (great aunts/uncles). Here, great-grandparents are not included because most of them had passed away as of 2015.

We have a total of 7,682,140 egos; among them 5,147,035 had both parents alive in 2015, and 886,056 had both (paternal or maternal) grandparents alive. The average age of the ego sample was 35.27 years old in 2015. Their parents and grandparents, respectively, were 61.42 years old and 81 years old on average. Their fathers were on average 3.5 years older than their mothers, and grandfathers were about 2.64 years older than grandmothers.

As the paternal and maternal lines of relatives have different roles in the Chinese kinship system, in the estimation we first control the lineage fixed effects and then estimate a separate correlation coefficient with a relative for each lineage. To clarify the notations referring to different lineages, we start from the ego and put the closer lineage first; for example, for the paternal grandmother's siblings, we refer to them as paternal–maternal great aunts/uncles. Figure 3.1 shows the structure of the kinship members covered by our dataset, taking patrilineal descendants, only for the sake of simplicity.

3.1.2 Measurement of Resemblance

In the literature, the father–child wealth correlation or the coefficient of father's wealth in the regression model of children's wealth is the most commonly used indicator of intergenerational mobility. It is often interpreted as a measure of direct transmission of wealth from parents to their children, plus the indirect passage of other advantages, such as investment in their children's human capital (Solon, 2014) and provision of opportunities. Here we do not want to overinterpret the meaning of the correlation to imply any causality of kinship behavior toward the ego. Just as the sibling correlation is seen as the embodiment of family factors shared among siblings (Mazumder, 2008; Solon et al., 1991), one could imagine such correlations with his/her kinship members as being forged over time by a variety of different factors and through multiple channels. Thus, a static, contemporaneous wealth correlation could be seen as a useful way of capturing the pattern of egocentric similarity among kin members (Mare, 2015).

Following Chetty, Hendren, Kline, and Saez (2014) and Boserup, Kreiner, and Kopczuk (2014), we measure an individual's wealth using his/her relative position (i.e., his/her rank, in the range 0–1) in the sample instead of its level. To estimate wealth correlation β_k between an ego and his/her relatives belonging to kinship relationship k, we regress his/her wealth rank on that of his/her relatives while controlling for age (using a fourth-order polynomial) and whether the relative is a matrilineal or patrilineal one[3]. The following is the regression model,

$$\begin{aligned} R_i = &\alpha + \beta_k R_{ikj} + \sum_{a=1}^{4} \gamma_a^m A_{ikj}^a M_{ikj} + \sum_{a=1}^{4} \gamma_a^f A_{ikj}^a (1 - M_{ikj}) \\ &+ \sum_{a=1}^{4} \delta_a (A_i - 40)^a + \sum_{a=1}^{4} \theta_a R_{ikj} (A_i - 40)^a + \lambda P_{ikj} + \epsilon_{ikj} \end{aligned} \tag{3.1}$$

where R_i is ego i's wealth rank, R_{ikj} is the wealth rank of member j of i 's kinship relationship k, A_{ikj} and A_i denote the age, M_{ikj} is a binary variable indicating j is male, P_{ikj} is a binary variable indicating that j is a patrilineal relative, and ϵ_{ikj} is an error term. We allow age to have different effects for males and females because men and women may have distinct life-cycle paths for wealth. We follow Lee and Solon (2009) by normalizing an ego's age to 40.

3.1.3 Features of the Wealth Correlation

Figure 3.5 presents the estimation results. Results in column 1 show that the wealth correlation decreases with consanguinity. Parents and siblings are the closest relatives, as they share the most genetic and socioeconomic factors with the ego and are the most likely to have mutual influence on wealth accumulation. Grandparents come next for similar reasons. The fact that this correlation is larger than the square of the ego–parent correlation suggests that direct multigenerational transmission of wealth is nontrivial. Wealth correlations with aunts and uncles are lower than those with grandparents but higher than those with first cousins. The patterns of correlation

[3]It is noted that the regression coefficient β_k is not exactly equal to the correlation coefficient of wealth, but is close to it. It is identical to the correlation coefficient if the standard deviations of the ego's and his/her relatives' wealth ranks are the same after controlling for age and other factors in the regression model, which is likely to be the case.

with more distant relatives are similar and are not further discussed here. Results in columns 3–6 control for the gender of the relatives and the ego and show patterns similar to those in column 1. For male egos, the wealth correlation with male relatives is higher than that with female ones. For female egos, same-sex correlation is higher only for siblings. In general, the correlation declines with distance of relationship.

Note that for both the Roman law approach and the genetic relatedness approach, the degree of kinship relationship is established and counted through parents. For wealth correlations, kinship relationship going through parents implies that the correlation depends on parents' closeness with their relatives. An individual's correlation with his/her cousins, for instance, may be related to how the ego's parents interact with their own siblings. If the parents' siblings are not close in general, then the cousins in question tend to have a smaller correlation. Thus, the higher the Roman law kinship degree is, the larger is the variation in correlation. This explains why the wealth correlation declines as the distance in kinship of Roman law increases (Figure 3.5 and 3.2). However, an ego's siblings and his/her cousins are of the same generation as the ego, and their background and peer groups are likely similar, which increase their correlation. This explains why same-generation relatives (such as siblings in the second degree and first cousins in the fourth degree) generally have larger correlations than relatives of different-generation ones (such as grandparents and great aunts/uncles, who are also the second- and fourth-degree relatives). In fact, the ego's wealth correlation with the first cousin once removed is very close to that with great aunts/uncles, even though the great aunts/uncles are tighter in relationship to the ego in terms of genetic relatedness and the Roman law.

Figure 3.6 compares the patterns of wealth correlation and genetic overlap. The two measures decrease at a similar rate in the first three relationship categories: parents, siblings, and grandparents. However, genetic overlapping fails to account for the difference between grandparents and aunts/uncles, as well as first cousins and great aunts/uncles in our results. This highlights the fact that genetic relatedness cannot capture the top-to-bottom mode of relatedness outside of the nuclear family, and the Taiwanese family network does not work simply by consanguinity. These two indexes reach a similar near-zero level as the genetic relatedness reaches a very low level, though.

For instance, the wealth correlation between an ego and his/her kins of 1/32 genetic similarity (second cousins) is 0.0341, which is remarkably close to 1/32.

Overall, our results suggest that the family members' resemblance depicted by the wealth correlation is far more intricate than what the Roman law system and genetic relatedness predict. It provides the numeric distances, rather than the ordinal rankings, of each kinship member.

3.2 Wealth Resemblance of Chinese Family Networks

3.2.1 Patrilineal and Matrilineal Differences

We conduct a simple statistical test, which is illustrated shortly, on the differences between the paternal and maternal lineages. The test shows that the correlations with paternal (and paternal–paternal) relatives score consistently higher than those with the maternal counterparts (Figure 3.5, column 7). This result still stands even after we control for the gender of ego and that of his/her relatives[4]. In Figure 3.3 we separate the two lineages from parents, demonstrating the stronger relations with the paternal (and paternal–paternal) side. The exact numbers can be found in Figure 3.6, column 1. One may wonder whether the male/female difference is intertwined with the paternal/maternal difference. To disentangle these two factors we separate the groups and do a refined analysis. In Figure 3.4, we observe the particular characteristic of a patrilineal society in that males are more resembled to their paternal relatives and that there is little gender difference in relations with maternal relatives.

In rural societies, according to the anthropology literature, the existence of a distinctively patrilineal system is due to efficiency concerns. For instance, if males are more efficient in a hunting society, then a patrilineal system secures more returns and thus facilitates more family collaboration (Alvard, 2003; Strassmann & Kurapati, 2016). It is interesting to see that in a developed industrial society like Taiwan, where people are not reliant upon family cooperation for their living, the patrilineal tendency is still present.

Now we conduct some subgroup analyses to investigate further the characteristics of wealth

[4] The test is conducted using Eq. 2, where D_i is replaced with the paternal or paternal–paternal lineage dummy, conditional on the genders of ego and the corresponding relative.

resemblance of members of the Chinese family. We use an interaction term for a group dummy and the wealth rank of the kinship member to test for subgroup differences in wealth correlation. We let D_i be a group dummy and run the following regression:

$$R_i = \alpha + \delta D_i + \beta_k R_{ikj} + D_i \cdot \beta_k^D R_{ikj} + age\ controls + \epsilon_{ikj} \tag{3.2}$$

For group dummies, we consider gender, parental wealth (whether in the top 25% and whether in the top 1%), and whether the ego is an adoptee or not [5].

3.2.2 Gender Difference

The results of estimating Equation 3.2 are reported in Figure 3.6, columns 3–6. Column 3 contains the coefficient estimates of the male dummy. They are mostly positive and statistically significant for the paternal lineage relatives, while mostly smaller or insignificant for the maternal lineage relatives, which is consistent with the discussion in Patrilineal and Matrilineal Differences.

3.2.3 Difference by Parental Wealth Rank

A strong wealth correlation within a family network or a subset of this network is an indicator of economic exchange and offers potential insights into the social relations within a family. There is no consensus on how people may relate to their kinship differently as they become wealthier. Some studies find that an individual's wealth weakens reciprocal altruism with relatives, and thus the wealthier are less keen to help kinship members (Kasper & Mulder, 2015; Nolin, 2011), while others suggest that the wealthier people become, the more they hold onto family ideals (Santos, 2006). We frame our question as follows: "When people have wealthy parents, does their wealth correlation structure change?"

In Figure 3.6, column 4 we separate egos by whether their parents' wealth is in the top 25% among all parents in our sample. It appears that those who have top-25% parents do have a smaller wealth correlation with more distant kinship members and their parents' siblings, except for first

[5] We rely on income tax records to find gender, and therefore some observations are lost if they have never reported income taxes. Over 95% of observations have the gender information.

cousins and paternal second cousins. Column 5 shows that having extremely wealthy parents—in the top 1%—leads to a larger (smaller) correlation with members inside (outside) of the nuclear family. This pattern suggests that extremely wealthy families have tightly knitted "inner circles," but are more loosely linked with the distant kin members.

3.2.4 Difference Between Adopted and Biological Relations

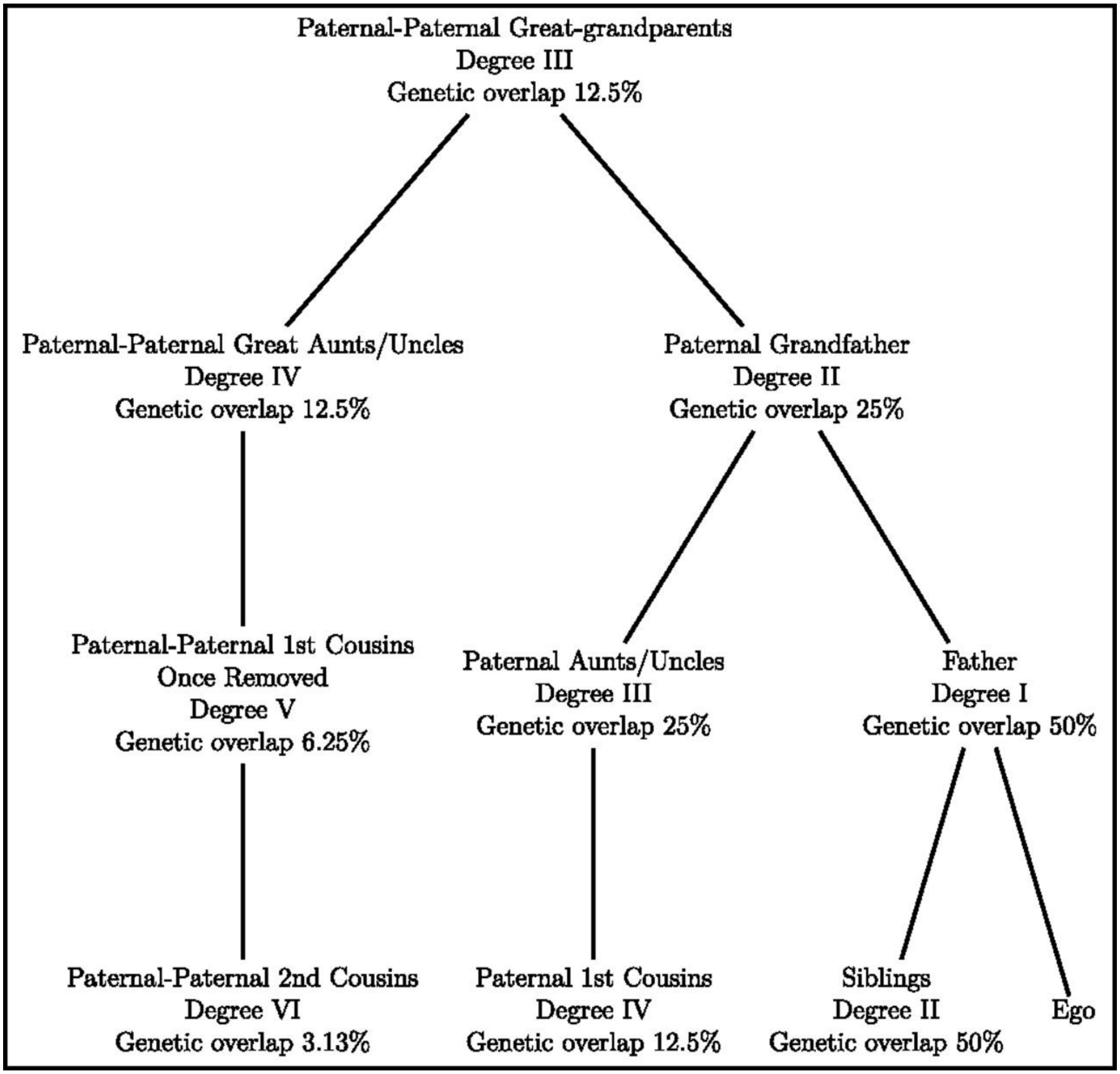
Taiwan's tax registration records show whether a child is an adoptee or not for tax purposes. Adoption, by definition, breaks the genetic correlations between the child and the parents, as well as the rest of the kinship. Therefore, we expect to see a weaker status correlation with relatives for adoptees, especially with closer relatives. This is exactly what we observe in Figure 3.6, column 6. But more distant relatives do not correlate in wealth with adoptees differently than with nonadoptees, likely because the resemblance among distant relatives is weak anyway.

3.3 Prospect

In this paper, we provide a measure of family members' resemblance based on analyses of wealth correlation. We find that in Taiwan, an industrialized society, gender gap and a tight patrilineal line still prevail, as indicated by the fact that male-line correlations are larger than their female-line counterparts. Furthermore, these correlations are also influenced by wealth status and adoptee status. Against the background of a time-invariant genetic relatedness measure, our measure using wealth correlation captures the various time-variant social contexts. Using this approach, we expect more interesting dynamics in this measure to be discovered in future studies.

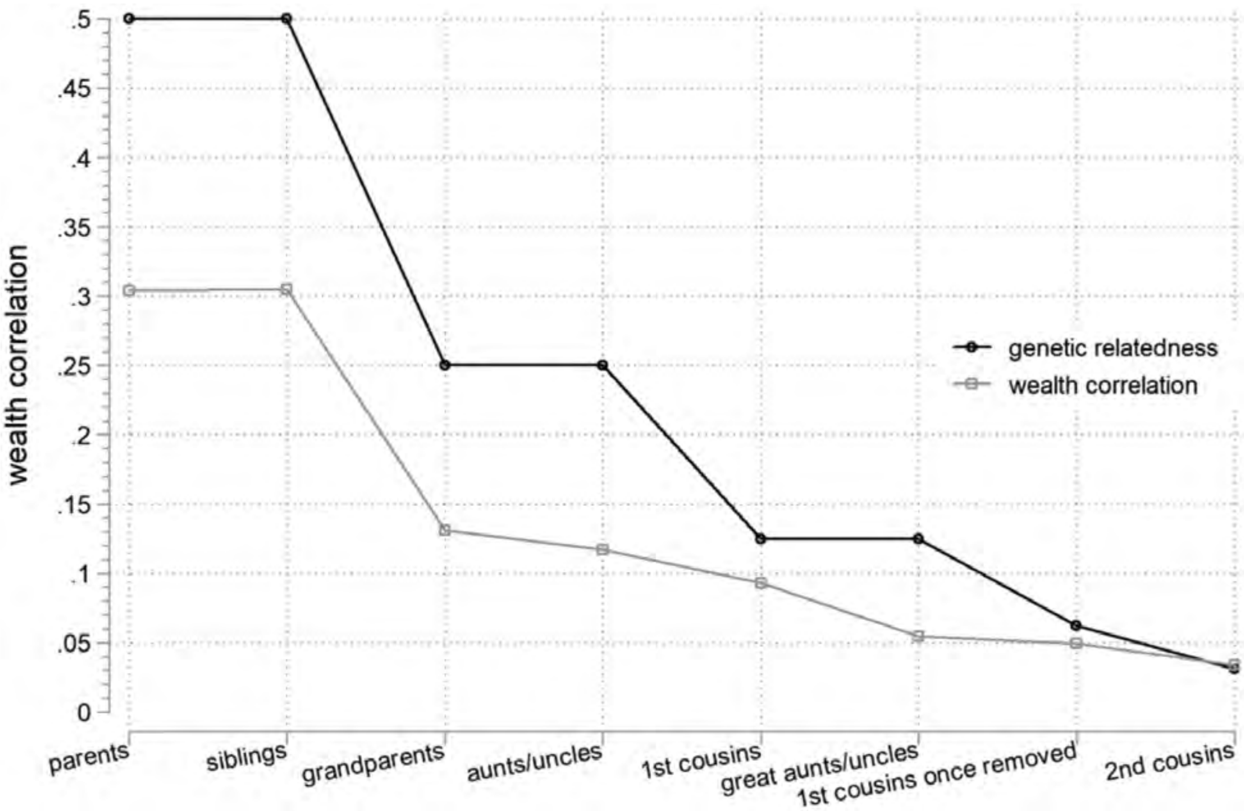
Compared with other countries, wealth correlations with parents in Taiwan are higher than those observed in Denmark (0.234 (Boserup et al., 2014), but lower than in the United States (0.371 (Pfeffer & Killewald, 2018). On the other hand, our wealth correlations with grandparents are lower than those of both Denmark and the United States (0.162 for the former (Boserup et al., 2014) and 0.194 for the latter (Pfeffer & Killewald, 2018)) We believe that further studies along this line would be useful, not only to compare the pattern of income or wealth mobility, but also to develop a cross-culture understanding of the determinants and behavioral patterns of family structure.

Figure 3.1



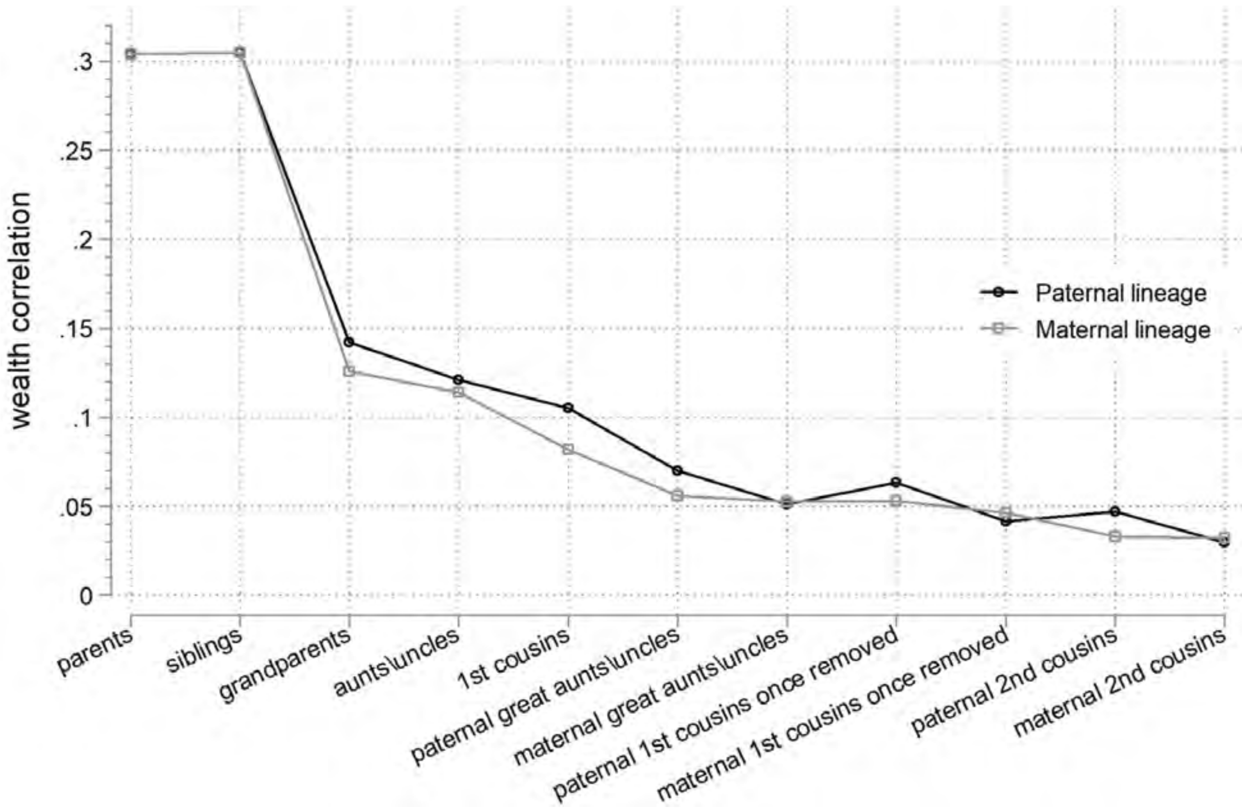
Note: Patrilineal descents covered in this study. This could also be applied to the paternal–maternal line by changing paternal grandfather to paternal grandmother and to maternal lines by changing father to mother and the corresponding relatives.

Figure 3.2



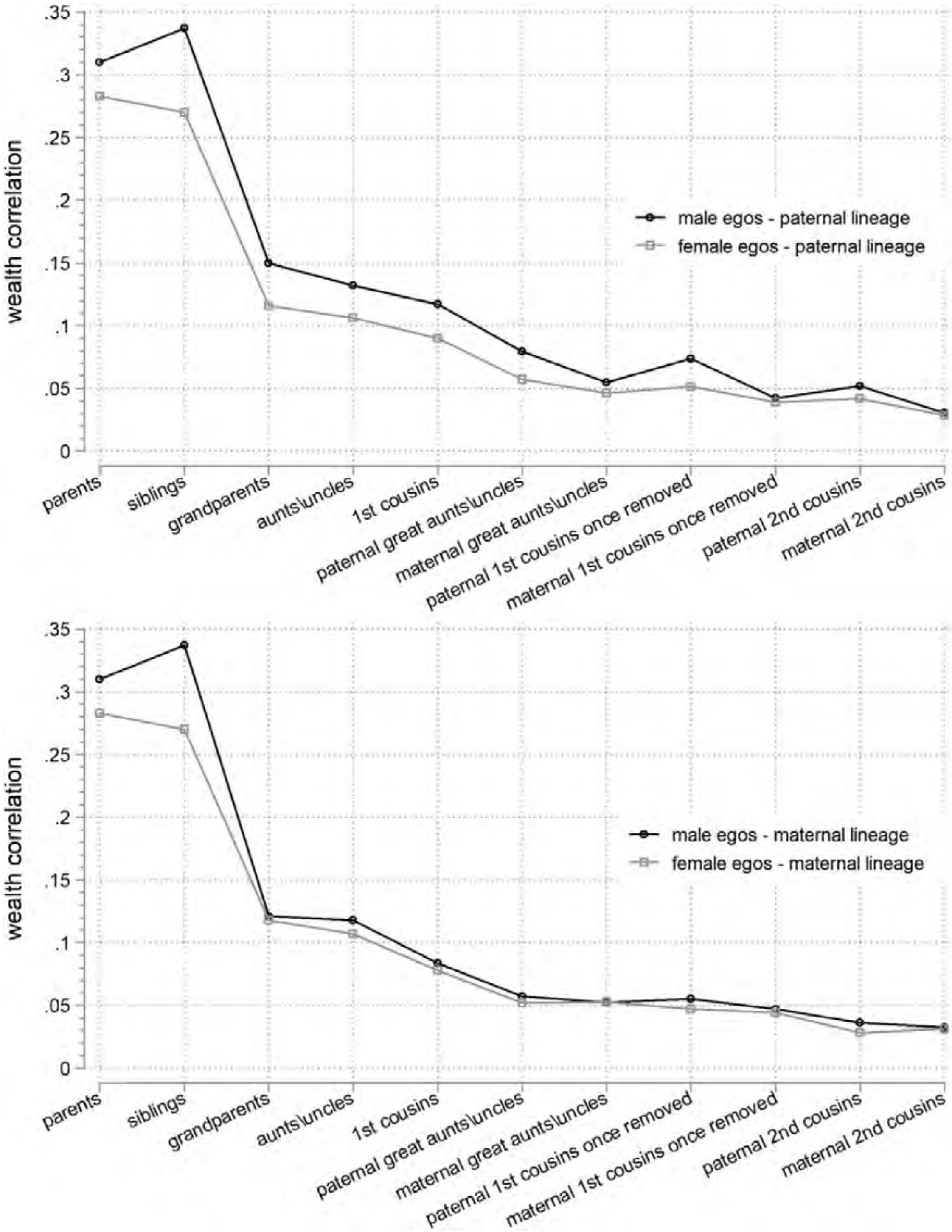
Note: Wealth correlation and genetic relatedness.

Figure 3.3



Note: Paternal and maternal lineages.

Figure 3.4



Note: Paternal and maternal lineages by gender.

Figure 3.5: Wealth Correlations

Relative degree	Genetic overlap	Kin type	All: 1) Wealth correlation	All: 2) Sample size	Male relatives: 3) Male egos	Male relatives: 4) Female egos	Female relatives: 5) Male egos	Female relatives: 6) Female egos	Difference: 7) Paternal vs. maternal
1	0.5	Parents	0.304***	5,147,035	—	—	—	—	—
2	0.5	Siblings	0.305***	13,667,901	0.403***	0.242***	0.275***	0.300***	—
2	0.25	Grandparents	0.131***	886,056	—	—	—	—	0.0160***
3	0.25	Aunts/uncles	0.117***	38,480,347	0.135***	0.106***	0.113***	0.107***	0.0118***
4	0.125	1st cousins	0.0931***	52,878,902	0.105***	0.0804***	0.0938***	0.0875***	0.0230***
4	0.125	Great aunts/uncles	0.0547***	7,604,802	0.0627***	0.0553***	0.0504***	0.0485***	0.00958***
5	0.0625	1st cousins once removed	0.0495***	26,556,522	0.0551***	0.0429***	0.0485***	0.0470***	0.0178***
6	0.0313	2nd cousins	0.0341***	31,174,165	0.0351***	0.0299***	0.0369***	0.0334***	0.0150***

Note: *,**, and *** denote statistical significance at the 10%, 5%, and 1% levels. Except for parents and siblings, the paternal/maternal lineage fixed effects are controlled in columns 1 and 3–6. Results in column 7 are for Equation 3.1, where the group dummy is for paternal/paternal–paternal lineage. Relative degrees follow those of the Roman law measures.

Figure 3.6: Group differences by paternal/maternal lineage: gender, parental wealth, and biological family

Relative degree	Genetic overlap	Kin type	Eq. 1: 1) Wealth correlation	Eq. 1: 2) Sample size	Eq. 2: group difference (β_k^D): 3) Male egos	4) Top 25% parental wealth	5) Top 1% parental wealth	6) Adoptee egos
1	0.5	Parents	0.304***	5,147,035	0.0170***	0.645***	8.686***	−0.0586***
2	0.5	Siblings	0.305***	13,667,901	0.0616***	0.125***	0.160***	−0.168***
2	0.25	Paternal grandparents	0.142***	363,917	0.0137***	0.00974**	−0.101***	−0.0154
2	0.25	Maternal grandparents	0.126***	522,139	−0.00412	0.00792**	−0.0609***	0.0171
3	0.25	Paternal aunts/uncles	0.121***	17,376,356	0.0192***	−0.0187***	−0.109***	−0.000727
3	0.25	Maternal aunts/uncles	0.114***	21,103,991	0.00445***	−0.00533***	−0.0892***	−0.0119***
4	0.125	Paternal 1st cousins	0.105***	25,223,873	0.0223***	0.0150***	−0.0407***	−0.0276***
4	0.125	Maternal 1st cousins	0.0820***	27,655,029	0.00247***	0.00166***	−0.0502***	−0.00728***
4	0.125	Paternal–paternal great aunts/uncles	0.0701***	1,066,118	0.0103***	−0.0160***	−0.0501***	0.0273
4	0.125	Paternal–maternal great aunts/uncles	0.0511***	2,121,057	0.00274*	−0.00359*	−0.0480***	0.00609
4	0.125	Maternal–paternal great aunts/uncles	0.0559***	1,513,377	−0.00137	−0.00272	−0.0441***	0.00189
4	0.125	Maternal–maternal great aunts/uncles	0.0528***	2,904,250	−0.00235*	−0.00474***	−0.0349***	0.0110
5	0.0625	Paternal–paternal 1st cousins o/r	0.0634***	4,594,690	0.0151***	−0.0133***	−0.0838***	−0.0100
5	0.0625	Paternal–maternal 1st cousins o/r	0.0415***	6,710,593	0.00276***	−0.0100***	−0.0691***	−0.000124
5	0.0625	Maternal–paternal 1st cousins o/r	0.0531***	6,282,853	0.000337	−0.00369***	−0.0528***	0.00558
5	0.0625	Maternal–maternal 1st cousins o/r	0.0465***	8,968,386	−0.000231	−0.00594***	−0.0480***	−0.00312
6	0.0313	Paternal–paternal 2nd cousins	0.0471***	5,425,001	0.0105***	0.00576***	−0.0393***	−0.00286
6	0.0313	Paternal–maternal 2nd cousins	0.0296***	8,368,383	0.0000877	−0.000704	−0.0440***	0.0143**
6	0.0313	Maternal–paternal 2nd cousins	0.0330***	7,004,836	0.000996	0.000260	−0.0185***	−0.00777
6	0.0313	Maternal–maternal 2nd cousins	0.0325***	10,375,945	−0.000593	−0.00159*	−0.0207***	−0.00567

Note: *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels. o/r, "once removed." In estimating results in columns 4 and 5 we include only egos who had both parents alive in 2015. About 67% of egos have both parents alive. About 1% of egos were adoptees.

References

Afzal, M., Alesina, A., & Amarante, V. (2011). Government Transfers and Political Support. *American Economic Journal: Applied Economics*, *3*, 1–28. Retrieved from http://www.aeaweb.org/articles.php?doi=10.1257/app.3.3.1http://www.aeaweb.org/articles.php?doi=10.1257/app.3.3.1. doi: 10.1257/app.3.3.1

Aizer, A. (2003). Low Take-Up in Medicaid: Does Outreach Matter and for Whom? *American Economic Review*, *93*(2), 238–241.

Aizer, A. (2007). Public Health Insurance, Program Take-Up, and Child Health. *The Review of Economics and Statistics*, *89*(3), 400–415.

Alvard, M. S. (2003). Kinship, lineage, and an evolutionary perspective on cooperative hunting groups in Indonesia. *Human Nature*, *14*(2), 129–163.

Anders, J., & Rafkin, C. (2022). The Welfare Effects of Eligibility Expansions: Theory and Evidence from SNAP.

Barrilleaux, C., & Rainey, C. (2014, 12). The politics of need: Examining governors' decisions to oppose the "obamacare" medicaid expansion. *State Politics and Policy Quarterly*, *14*(4), 437–460. doi: 10.1177/1532440014561644

Bechtel, M. M., & Hainmueller, J. (2011). How Lasting Is Voter Gratitude? An Analysis of the Short- and Long-Term Electoral Returns to Beneficial Policy. *American Journal of Political Science*, *55*(4), 852–868. doi: 10.1111/j.1540-5907.2011.00533.x

Bengtson, V. L. (2001, 2). Beyond the nuclear family: The increasing importance of multigenerational bonds. *Journal of Marriage and Family*, *63*(1), 1–16. doi: 10.1111/J.1741-3737.2001.00001.X

Berry, W., Ringquist, E., Fording, R., & Hanson, R. (1998). Measuring Citizen and Government Ideology in the American States, 1960-93. *American Journal of Political Science*, *42*(1), 327–348.

Boserup, S. H., Kreiner, C. T., & Kopczuk, W. (2014). Stability and persistence of intergenerational wealth formation : Evidence from Danish wealth records of three generations. *Working Paper*.

Callaway, B., & Sant'Anna, P. H. (2021, 12). Difference-in-Differences with multiple time periods. *Journal of Econometrics*, *225*(2), 200–230. Retrieved from https://doi.org/10.1016/j.jeconom.2020.12.001 doi: 10.1016/J.JECONOM.2020.12.001

Caughey, D., & Warshaw, C. (2018, 5). Policy preferences and policy change: Dynamic responsiveness in the American States, 1936-2014. *American Political Science Review*, *112*(2), 249–266. doi: 10.1017/S0003055417000533

Caughey, D., Warshaw, C., & Xu, Y. (2017, 10). Incremental democracy: The policy effects of partisan control of state government. *Journal of Politics*, *79*(4), 1342–1358. Retrieved from http://dx.doi.org/10.1086/692669 doi: 10.1086/692669

Chetty, R., Hendren, N., Kline, P., & Saez, E. (2014). Where Is The Land Of Opportunity? The Geography Of Intergenerational Mobility In The United States. *The Quarterly Journal of Economics*, *129*(4), 1553–1623. Retrieved from http://www.equality-of-opportunity.org. doi: 10.1093/qje/qju022

Chu, C. Y., & Yu, R. R. (2009). *Understanding Chinese Families: A Comparative Study of Taiwan and Southeast China*. Oxford University Press. Retrieved from https://academic.oup.com/book/3322 doi: 10.1093/ACPROF:OSO/9780199578092.001.0001

Clinton, J. D., & Sances, M. W. (2018). The Politics of Policy: The Initial Mass Political Effects of Medicaid Expansion in the States. *American Political Science Review*, *112*(1), 167–185. doi: 10.1017/S0003055417000430

Conover, E., Zárate, R. A., Camacho, A., & Baez, J. E. (2020, 1). Cash and ballots: Conditional transfers, political participation, and voting behavior. *Economic Development and Cultural Change*, *68*(2), 541–566. doi: 10.1086/701211

Cook, J., & East, C. N. (2023). The Effect of Means-Tested Transfers on Work: Evidence From Quasi-Randomly Assigned SNAP Caseworkers. *NBER Working Paper*. Retrieved from http://www.nber.org/papers/w31307

CRS. (2022). The Supplemental Nutrition Assistance Program (SNAP): Categorical Eligibility. *U.S. Congressional Research Service (R42054)*. Retrieved from https://crsreports.congress.gov

Cunnyngham, K. E. (2016). Estimates of State Supplemental Nutrition Assistance Program Participation Rates in 2018. *Mathematica Policy Research*, 1–8. Retrieved from http://www.fns.usda.gov/sites/default/files/ops/Reaching2013.pdf

Currie, J. (2006). The Take-Up of Social Benefits. In A. J. Auerbach, D. Card, & J. M. Quigley (Eds.), *Public policy and the income distribution* (pp. 80–148). New York: Russell Sage Foundation. doi: 10.7758/9781610440202.7

De Chaisemartin, C., & D'haultfoeuille, X. (2018). Fuzzy Differences-in-Differences. *Review of Economic Studies*, *85*, 999–1028. Retrieved from https://academic.oup.com/restud/article/85/2/999/4096388 doi: 10.1093/restud/rdx049

De La O, A. L. (2013). Do Conditional Cash Transfers Affect Electoral Behavior? Evidence from a Randomized Experiment in Mexico. *American Journal of Political Science*, *57*(1), 1–14. doi: 10.1111/J.1540-5907.2012.00617.X

Deshpande, M., & Li, Y. (2019). Who Is Screened Out? Application Costs and the Targeting of Disability Programs. *American Economic Journal: Economic Policy*, *11*(4), 213–248. doi: 10.1257/pol.20180076

Dickert-Conlin, S., Fitzpatrick, K., Stacy, B., & Tiehen, L. (2021). The Downs and Ups of the SNAP Caseload: What Matters? *Applied Economic Perspectives and Policy*, *43*(3), 1026–1050. doi: 10.1002/AEPP.13076

Downs, A. (1957). An Economic Theory of Political Action in a Democracy. *Journal of Political Economy*, *65*(2), 135–150. Retrieved from https://www.jstor.org/stable/1827369

Filindra, A. (2013, 3). Immigrant social policy in the american states: Race politics and state TANF and medicaid eligibility rules for legal permanent residents. *State Politics and Policy Quarterly*, *13*(1), 26–48. doi: 10.1177/1532440012454664

Finkelstein, A., & Notowidigdo, M. J. (2019). Take-up and Targeting: Experimental Evidence From SNAP. *The Quarterly Journal of Economics*, 1505–1556. Retrieved from https://academic.oup.com/qje/article/134/3/1505/5484907 doi: 10.1093/qje/qjz013

Flood, S., King, M., Rodgers, R., Ruggles, S., Warren, J. R., & Westberry, M. (2022). *Integrated Public Use Microdata Series, Current Population Survey: Version 10.0 [Voting and Registration Supplement].* Minneapolis, MN: IPUMS.

Ford, W. F., Minor, T., & Owens, M. F. (2012). State Minimum Wage Differences: Economic Factors or Political Inclinations? *Business Economics*, *47*, 57–67. doi: 10.1057/be.2011.37

GAO. (2012). Improved Oversight of State Eligibility Expansions Needed. *United States Government Accountability Office*.

Geller, D., Zic, B., Isaacs, J., & Braga, B. (2019, 2). *Exploring the Causes of State Variation in SNAP Administrative Costs* (Tech. Rep.). Prepared by Manhattan Strategy Group and the Urban Institute for the U.S. Department of Agriculture, Food and Nutrition Service. Retrieved from www.fns.usda.gov/research-and-analysis.

Gerardi, K., & Tsai, Y. (2014). The Effect of Social Entitlement Programmes on Private Transfers: New Evidence of Crowding Out. *Economica*, *81*(324), 721–746. doi: 10.1111/ECCA.12062

Gilens, M. (1995). Racial Attitudes and Opposition to Welfare. *The Journal of Politics*, *57*(4), 994–1014. Retrieved from https://about.jstor.org/terms

Gilens, M. (1996). "Race Coding" and White Opposition to Welfare. *American Political Science Review*, *90*(3), 593–604. Retrieved from https://doi.org/10.2307/2082611 doi: 10.2307/2082611

Golosov, M., & Tsyvinski, A. (2006). Designing Optimal Disability Insurance: A Case for Asset Testing. *Journal of Political Economy*, *114*(2).

Goodman-Bacon, A. (2021, 12). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, *225*(2), 254–277. Retrieved from https://doi.org/10.1016/

j.jeconom.2021.03.014 doi: 10.1016/J.JECONOM.2021.03.014

Goodman-Bacon, A., & Schmidt, L. (2020, 5). Federalizing benefits: The introduction of Supplemental Security Income and the size of the safety net. *Journal of Public Economics*, *185*. doi: 10.1016/J.JPUBECO.2020.104174

Gray, C. (2019, 11). Leaving benefits on the table: Evidence from SNAP. *Journal of Public Economics*, *179*, 104054. doi: 10.1016/j.jpubeco.2019.104054

Griffin, J. D., & Newman, B. (2005, 11). Are voters better represented? *Journal of Politics*, *67*(4), 1206–1227. doi: 10.1111/J.1468-2508.2005.00357.X

Hastie, T., Tibshirani, R., & Friedman, J. (2009). Model Assessment and Selection. In *The elements of statistical learning* (pp. 219–259). New York, NY: Springer Series in Statistics. doi: 10.1007/978-0-387-84858-7{_}7

Herd, P., & Moynihan, D. (2018). *Administrative Burden*. New York: Russell Sage Foundation.

Homonoff, T., Rino, G., & Somerville, J. (2022). Administrative Burden and Procedural Denials: Experimental Evidence from SNAP. *NBER Working Paper*.

Homonoff, T., & Somerville, J. (2021). Program Recertification Costs: Evidence from SNAP. *American Economic Journal: Economic Policy*, *13*(4), 271–298. Retrieved from https://doi.org/10.1257/pol.20190272 doi: 10.1257/pol.20190272

Hoynes, H., & Schanzenbach, D. (2009). Consumption Responses to In-Kind Transfers: Evidence from the Introduction of the Food Stamp Program. *American Economic Journal: Applied Economics*, *1*(4), 109–139. Retrieved from http://www.aeaweb.org/articles.php?doi=10.1257/app.1.4.109http://www.aeaweb.org/articles.php?doi=10.1257/app.1.4.109. doi: 10.1257/app.1.4.109

Huang, Z.-L., Chen, C.-Y., & Kuo, J.-K. (2009). *Civil Code: Family* (8th ed.). Taipei, Taiwan: San Min Book Corporation, Ltd.

Hunt, M. O. (2007). African American, Hispanic and white beliefs about black/white inequality, 1977-2004. *American Sociological Review*, *72*(3), 390–415. doi: 10.1177/000312240707200304

Jacobs, L. R., & Callaghan, T. (2013). Why States Expand Medicaid: Party, Resources, and

History. *Journal of Health Politics, Policy and Law*, *38*(5), 1023–1050. Retrieved from http://read.dukeupress.edu/jhppl/article-pdf/38/5/1023/434262/1023.pdf doi: 10.1215/03616878-2334889

Jæger, M. M. (2012, 12). The Extended Family and Children's Educational Success. *American Sociological Review*, *77*(6), 903–922. Retrieved from https://journals.sagepub.com/doi/full/10.1177/0003122412464040 doi: 10.1177/0003122412464040/ASSET/IMAGES/LARGE/10.1177{_}0003122412464040-FIG3.JPEG

Johansson, E. (2003, 5). Intergovernmental grants as a tactical instrument: empirical evidence from Swedish municipalities. *Journal of Public Economics*, *87*(5-6), 883–915. doi: 10.1016/S0047-2727(01)00148-7

Jones, J. W., Courtemanche, C. J., Denteh, A., Marton, J., & Tchernis, R. (2021). Do State SNAP Policies Influence Program Participation among Seniors? *NBER WORKING PAPER*. Retrieved from http://www.nber.org/papers/w29037

Kaplan, J. (2021). *United States Governors 1775-2020.* Ann Arbor: MI: Inter-university Consortium for Political and Social Research [distributor].

Kasper, C., & Mulder, M. B. (2015, 10). Who helps and why?: Cooperative networks in Mpimbwe. *Current Anthropology*, *56*(5), 701–732. Retrieved from https://www.journals.uchicago.edu/doi/10.1086/683024 doi: 10.1086/683024/ASSET/IMAGES/LARGE/FG2{_}ONLINE.JPEG

Klerman, J. A., & Danielson, C. (2011). The transformation of the Supplemental Nutrition Assistance Program. *Journal of Policy Analysis and Management*, *30*(4), 863–888. doi: 10.1002/PAM.20601

Kleven, H., & Kopczuk, W. (2011). Transfer Program Complexity and the Take-Up of Social Benefits. *American Economic Journal: Economic Policy*, *3*(1), 54–90. doi: 10.1257/pol.3.1.54

Ko, W., & Moffitt, R. A. (2022, 6). Take-up of Social Benefits. *NBER Working Paper 30148*. Retrieved from https://www.nber.org/papers/w30148 doi: 10.3386/W30148

Kogan, V. (2021, 1). Do welfare benefits pay electoral dividends? Evidence from the national food stamp program rollout. *Journal of Politics*, *83*(1), 20–70. Retrieved from https://doi.org/10.1086/708914. doi: 10.1086/708914/SUPPL{_}FILE/181891APPENDIX.PDF

Kriner, D. L., & Reeves, A. (2012). The Influence of Federal Spending on Presidential Elections. *The American Political Science Review*, *106*(2), 348–366. doi: 10.1017/S0003055412000159

Kubik, J. D. (2003). Fiscal federalism and welfare policy: The role of states in the growth of child SSI. *National Tax Journal*, *56*(1 I), 61–79. doi: 10.17310/NTJ.2003.1.04

Laird, E., & Trippe, C. (2014). Programs Conferring Categorical Eligibility for SNAP: State Policies and the Number and Characteristics of Households Affected. *Mathematica Policy Research*.

Lee, C. I., & Solon, G. (2009, 11). Trends in Intergenerational Income Mobility. *The Review of Economics and Statistics*, *91*(4), 766–772. Retrieved from https://dx.doi.org/10.1162/rest.91.4.766 doi: 10.1162/REST.91.4.766

Lesica, J. (2018, 10). Lobbying For Minimum Wages. *Economic Inquiry*, *56*(4), 2027–2057. doi: 10.1111/ECIN.12692

Levitt, S. D., & Snyder, J. M. (1997). The impact of federal spending on house election outcomes. *Journal of Political Economy*, *105*(1), 30–53. doi: 10.1086/262064

Lindsey, C., & Sheather, S. (2015). *Best subsets variable selection in nonnormal regression models* (Vol. 15; Tech. Rep. No. 4).

Mare, R. D. (2011). A Multigenerational View of Inequality. *Demography*, *48*(1), 1–23. Retrieved from http://read.dukeupress.edu/demography/article-pdf/48/1/1/880525/1mare.pdf doi: 10.1007/s13524-011-0014-7

Mare, R. D. (2015, 1). Measuring Networks beyond the Origin Family. *Annals of the American Academy of Political and Social Science*, *657*(1), 97–107. doi: 10.1177/0002716214548410

Mazumder, B. (2008, 7). Sibling similarities and economic inequality in the US. *Journal of Population Economics*, *21*(3), 685–701. Retrieved from https://link.springer.com/article/10.1007/s00148-006-0127-2 doi: 10.1007/S00148-006-0127-2/TABLES/7

Meyer, B., Mok, W., & Sullivan, J. (2009). The Under-Reporting Of Transfers In Household Surveys: Its Nature And Consequences. *NBER Working Paper 15181*. Retrieved from http://www.nber.org/papers/w15181

Meyer, B., Mok, W., & Sullivan, J. (2015, 9). Household surveys in crisis. *Journal of Economic Perspectives*, *29*(4), 199–226. doi: 10.1257/jep.29.4.199

Modin, B., & Fritzell, J. (2009, 6). The long arm of the family: are parental and grandparental earnings related to young men's body mass index and cognitive ability? *International Journal of Epidemiology*, *38*(3), 733–744. Retrieved from https://dx.doi.org/10.1093/ije/dyp001 doi: 10.1093/IJE/DYP001

Nichols, A. L., & Zeckhauser, R. J. (1982). Targeting Transfers through Restrictions on Recipients. *The American Economic Review: Papers and Proceedings*, *72*(2), 372–377.

Nolin, D. A. (2011, 7). Kin Preference and Partner Choice. *Human Nature*, *22*(1), 156–176. doi: 10.1007/s12110-011-9113-9

Peffley, M., Hurwitz, J., & Sniderman, P. M. (1997). Racial Stereotypes and Whites' Political Views of Blacks in the Context of Welfare and Crime. *American Journal of Political Science*, *41*(1), 30–60.

Pfeffer, F., & Killewald, A. (2018). Generations of Advantage: Multigenerational Correlations in Family Wealth. *Social Forces*, *96*(4), 1411–42.

Pop-Eleches, C., & Pop-Eleches, G. (2012). Targeted Government Spending and Political Preferences. *Quarterly Journal of Political Science*, *7*, 285–320. Retrieved from http://dx.doi.org/10.1561/100.00011017supp doi: 10.1561/100.00011017

Ratcliffe, C., Mckernan, S.-M., & Finegold, K. (2008). Effects of Food Stamp and TANF Policies on Food Stamp Receipt. *Social Service Review*, *82*(2), 291–334.

Ratcliffe, C., McKernan, S. M., Wheaton, L., Kalish, E., Ruggles, C., Armstrong, S., & Oberlin, C. (2016). Asset Limits , SNAP Participation , and Financial Stability. *Research Report for US Department of Agriculture*. Retrieved from https://fns-prod.azureedge.net/sites/default/files/ops/SNAPAssets.pdf

Rodgers, H. (2005, 5). State Welfare Options: Why Are Some States Tough? *Review of Policy Research*, *22*(3), 397–400. doi: 10.1111/J.1541-1338.2005.00143.X

Rodgers, H., & Tedin, K. (2006, 5). State TANF Spending: Predictors of State Tax Effort to Support Welfare Reform. *Review of Policy Research*, *23*(3), 745–759. doi: 10.1111/J.1541-1338.2006.00227.X

Sahlins, M. (2011, 3). What kinship is (part one). *Journal of the Royal Anthropological Institute*, *17*(1), 2–19. doi: 10.1111/J.1467-9655.2010.01666.X

Sanford F. Schram, Joe Soss, & Richard C. Fording. (2003). *Race and the Politics of Welfare Reform.* Ann Arbor, MI: University of Michigan Press. Retrieved from https://www.fulcrum.org/concern/monographs/5712m7555 doi: 10.3998/MPUB.11932

Santos, G. D. D. (2006). The Anthropology of Chinese Kinship. A Critical Overview. *European Journal of East Asian Studies*, *5*, 275–333.

Sarkisian, N., & Gerstel, N. (2004). Kin support among blacks and whites: Race and family organization. *American Sociological Review*, *69*(6), 812–837. doi: 10.1177/000312240406900604/SUPPL{_}FILE/ASR6900604{_}ONLINE{_}SUPPLEMENT.PDF

Schmidt, L., & Sevak, P. (2004). AFDC, SSI, and Welfare Reform Aggressiveness: Caseload Reductions versus Caseload Shifting. *The Journal of Human Resources*, *39*(3), 792–812.

SNAP policy database. (2016). *Economic Research Service (ERS), U.S. Department of Agriculture (USDA). https://www.ers.usda.gov/data-products/snap-policy-data-sets/*.

Solon, G. (1992). Intergenerational income mobility in the United States. *American Economic Review*, *82*(3), 393–408. doi: 10.2307/2117312

Solon, G. (2014, 3). Theoretical models of inequality transmission across multiple generations. *Research in Social Stratification and Mobility*, *35*, 13–18. doi: 10.1016/J.RSSM.2013.09.005

Solon, G., Corcoran, M., Gordon, R., & Laren, D. (1991). A Longitudinal Analysis of Sibling Correlations in Economic Status. *The Journal of Human Resources*, *26*(3), 509–534. Retrieved from https://www.jstor.org/stable/146023

Soss, J., Schram, S. F., Vartanian, T. P., & O'brien, E. (2001). Setting the Terms of Relief: Explaining State Policy Choices in the Devolution Revolution. *American Journal of Political Science*, *45*(2), 378–395.

Strassmann, B. I., & Kurapati, N. T. (2016, 6). What explains patrilineal cooperation? *Current Anthropology*, *57*, S118-S130. Retrieved from https://www.journals.uchicago.edu/doi/10

.1086/685762 doi: 10.1086/685762/ASSET/IMAGES/LARGE/FG5{_}ONLINE.JPEG

Sun, L., & Abraham, S. (2021, 12). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, *225*(2), 175–199. doi: 10.1016/j.jeconom.2020.09.006

Super, D., & Dean, S. (2001, 1). *New State Options To Improve The Food Stamp Vehicle Rule* (Tech. Rep.). Washington, DC: Center on Budget and Policy Priorities.

TRIM3. (n.d.). *TRIM3 project website, trim3.urban.org, downloaded on 7/19/2023.*

Unrath, M. (2021). Targeting, Screening, and Retention: Evidence from California's Food Stamps Program. *California Policy Lab Working Paper*.

USDA. (2009). *Improving Access to SNAP through Broad-Based Categorical Eligibility.*

Wellschmied, F. (2021). The welfare effects of asset mean-testing income support. *Quantitative Economics*, *12*(1), 217–249. doi: 10.3982/qe1241

Whitaker, E. A., Herian, M. N., Larimer, C. W., & Lang, M. (2012, 11). The Determinants of Policy Introduction and Bill Adoption: Examining Minimum Wage Increases in the American States, 1997-2006. *Policy Studies Journal*, *40*(4), 626–649. doi: 10.1111/J.1541-0072.2012.00467.X

Wright, S. (1922). Coefficients of Inbreeding and Relationship. *The American Naturalist*, *56*(645), 330–338. Retrieved from https://www.jstor.org/stable/2456273

Appendix A

Chapter 1 Appendix

A.1 Sources of factors explaining state BBCE adoption

- **Voters' Preference for Welfare:** the Voting and Registration Supplement of the Current Population Survey (VRS), the General Social Survey (GSS).
  - VRS: predict the likelihood to vote or register using a set of individual characteristics including age, sex, natural-born citizenship, education, race, marital status, household size, household income, region, and year indicators. The coefficients are used in the next step.
  - GSS: predict potential voters, construct a welfare attitude indicator according to the following questions (questions rotated):
    - if the government should be helping the poor
    - if the national expenditure on assistance to the poor/welfare is too little
  - predict the likelihood of supporting welfare among all the potential voters on TRIM3 samples. Collapse into state average (weighted by person sampling weights).
- **Voters' Having Racism:** same procedure and data as the welfare preference. Questions used are:
  - if agree to the statement"Most (Negroes/Blacks/African-Americans) just don't have the motivation or willpower to pull themselves up out of poverty"
  - if scored at least five out of seven on the tendency of blacks to be lazy
- **Unemployment rate:** monthly, seasonally adjusted. Current Employment Statistics, the Bureau of Labor Statistics
- **Median Household Income:** calculated by author using TRIM3 (based on CPS-ASEC)
- **State Finance:** Annual Survey of State Government Finances, US Census Bureau
- **SNAP Administrative Costs:** State Activity Reports, Food and Nutrition Services, USDA
- **SNAP Error Rates:** Quality Control Annual Report, Food and Nutrition Services, USDA

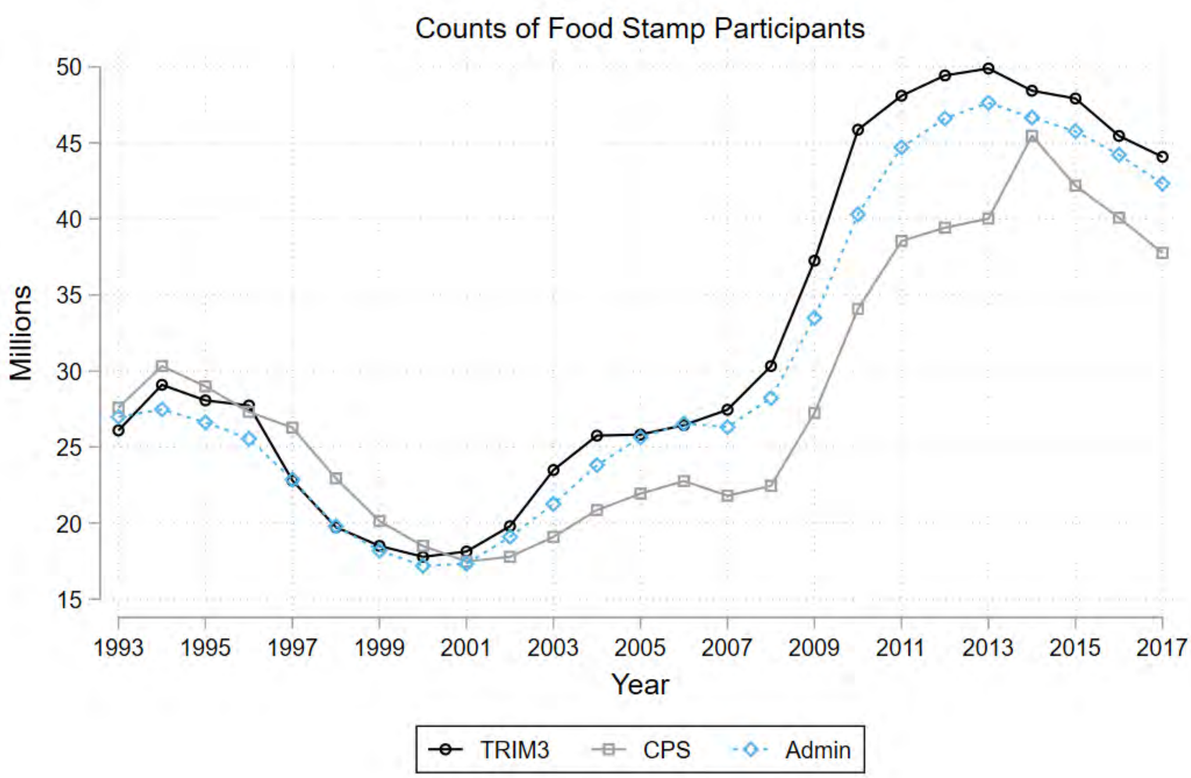
Table A.1.1: Determinants of State Adoption of BBCE Before 2008: Analysis of Time-Varying Characteristics from the Previous Year

	(1) BBCE	(2) BBCE Max
Take up rate (Percentage)	0.000507	0.0000411
	(0.00234)	(0.00121)
Percentage of Eligible Population	0.00895	0.00335
	(0.0123)	(0.00636)
Percentage of Population Aged < 18	0.00294	-0.00283
	(0.0211)	(0.00732)
Percentage of Population Aged > 64	-0.00609	-0.00147
	(0.0204)	(0.00607)
Percentage of Hispanic Population	0.0138	0.00711
	(0.0106)	(0.00547)
Percentage of Black Population	0.0245	-0.00509
	(0.0150)	(0.00557)
Percentage of Other Race/Ethnicity Population	-0.00510	-0.00749
	(0.0127)	(0.00415)
Percentage of HS or Below Education	0.000227	0.00105
	(0.00883)	(0.00220)
Percentage of Disabled Population	-0.00832	0.00927
	(0.0397)	(0.0212)
Percentage of Non-Disabled 18-49 Adult Without Dependent	0.0151	0.0126
	(0.0184)	(0.00773)
Percentage of Married Population	0.0284	0.0112
	(0.0152)	(0.00685)
Percentage of Citizens	0.000855	0.0127
	(0.0144)	(0.0102)
Unemployment Rate (Percentage)	0.0140	0.00882
	(0.0199)	(0.0163)
Median Household Income (Thousand)	-0.00847	-0.00159
	(0.00704)	(0.00266)
Percentage of Voters Who Support Welfare	0.0152	0.0158
	(0.0197)	(0.0133)
Percentage of Voters Who Have Bias Against Black	0.0341	-0.0132
	(0.0374)	(0.0235)
Percentage of State Expenditure Over Own-Source Revenue	0.000474	0.000300
	(0.00160)	(0.000875)
SNAP Administrative Costs Per Case	-0.00171	-0.000538
	(0.00135)	(0.000703)
SNAP Error Rate (Percentage)	0.00805	-0.0000333
	(0.00459)	(0.00207)
Observations	2802	3143
Adjusted R^2	0.557	0.607
P > F	0.4703	0.8871
Mean	0.047	0.014

Monthly observations of state adoption from January 2000 until the first adoption month. State and year fixed effect regression estimates. Observations weighted by eligible population size averaged from 1996 to 1999. Standard errors clustered at the state level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Dependent variable is an indicator of BBCE/BBCE Max adoption observed at the monthly level. Independent variables are observed in the same month in the previous year.

A.2 Comparing TRIM3 to Administrative Data

Figure A.2.1: Total Number of SNAP Participants: TRIM3 versus Administrative Data



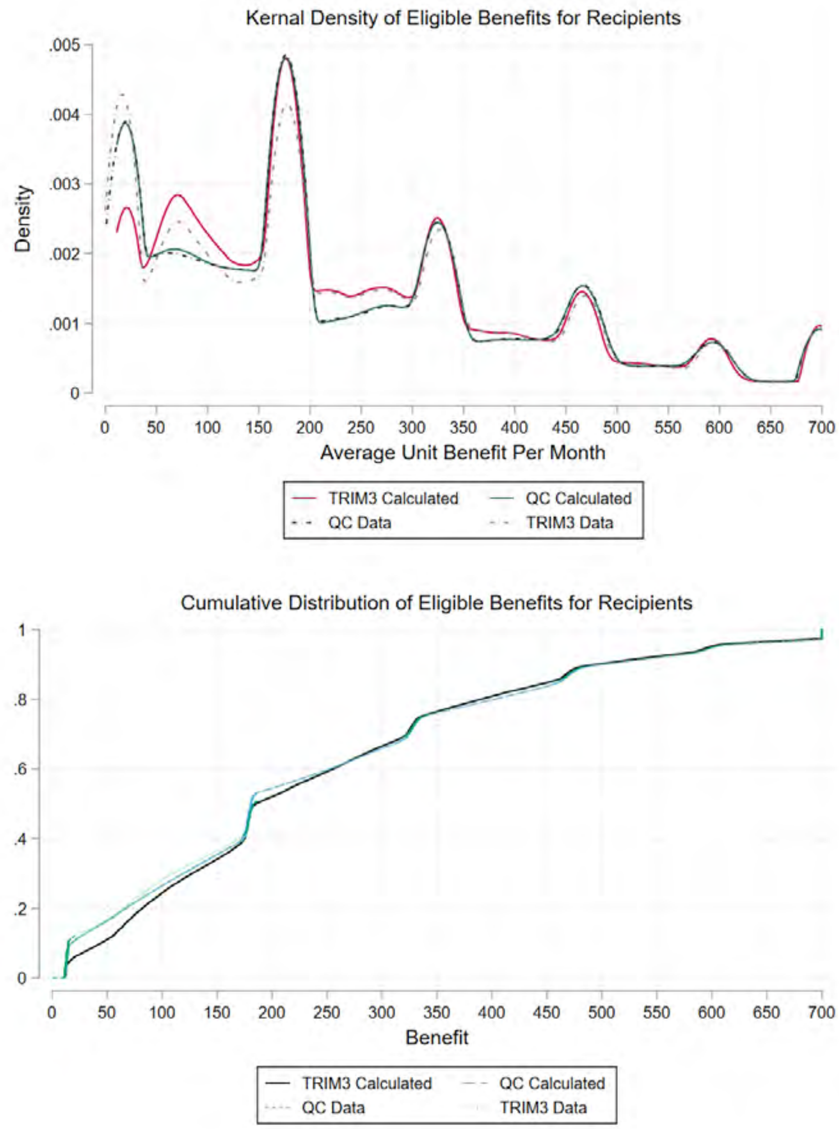
The figure plots the time series of SNAP total number of participants estimated using TRIM3 and CPS ASEC data. Official counts published by the Food and Nutrition Service, USDA is also plotted as a benchmark of the true values. It is apparent that TRIM3 counts are closer to the administrative counts in almost every year than using CPS ASEC directly.

Table A.2.1: Characteristics of SNAP Participating Households: TRIM3 versus QC (Admin Data)

	(1) TRIM Recipient	(2) QC
Monthly Benefits	231.5	241.7
	(188.7)	(185.3)
Gross Income	959.5	626.7
	(2691.7)	(448.7)
Net Income	375.6	328.1
	(475.4)	(351.8)
Unit Asset	151.8	133.6
	(4927.4)	(697.4)
Unit Size	2.463	2.335
	(1.548)	(1.536)
Unit with Elder Members	0.190	0.183
	(0.393)	(0.386)
Unit with Disabled Members	0.248	0.241
	(0.432)	(0.428)
Unit with Kids	0.576	0.547
	(0.494)	(0.498)
Age of Head	42.15	41.04
	(17.53)	(18.56)
Observations	968606	573856

Mean coefficients; sd in parentheses. Observations weighted by household sampling weights. All monetary values are adjusted to the December 2015 consumer price index.

Figure A.2.2: Distribution of Received Benefits Calculated by TRIM3 and QC Income



The eligible benefits calculated by TRIM3 income data and QC income data have very similar distributions. This means that the differences in income do not cause differences in SNAP recipients in terms of benefit levels.

A.3 Imputation of TRIM3 Data

- **Household Gross Income (pre-2005):** TRIM3 documentation https://boreas.urban.org/documentation/input/Concepts%20and%20Procedures/UsingIncomeVariables.php

  1. Sum up all monthly income sources of each household member. Income sources include asset income, earnings, unearned income, unemployment compensation, child support, workers' compensation, alimony, and public assistance (each item mutually exclusive).
  2. Divide by number of people in the household
  3. Sum up eligible household only — exclude cash-out individuals or non-citizens

- **Household Net Income (pre-2005):** Apply the benefits calculator

  Net Income = (Maximum allotment - eligible benefit)/0.3

  Maximum allotment varies by household size, which includes eligible members only.

- **Household Countable Asset:** Assume an asset return rate of 0.06

  1. Sum up the monthly asset income of all members of the household
  2. Sum over all months in the year
  3. Divide by 0.06 (following TRIM3 documentation)

- **Individual Disability Measure:** Follow SNAP QC Documentation FY 2015

  1. If the individual is nonelderly ($<$ 60 years old) and is an SSI recipient
  2. If the individual is nonelderly and is working $<$ 30 hrs a week and is receiving social security/worker's compensation/veteran's compensation or is coded as not working due to illness or disability
  3. If the household has medical expense deductions (modeled by TRIM3) and there is no elderly member in the unit, follow the steps until locating at least one member:
     (a) Coded as not working due to illness or disability
     (b) Work $<$ 30 hours per week and has social security, veteran's benefits, or worker compensation
     (c) Has social security, veteran's benefits, or worker compensation
     (d) Child work $<$ 30 hrs/week
     (e) Adult work $<$ 30 hrs/week
     (f) All individuals

Figure A.3.1: Imputation of Income and Asset in Unavailable Years and Comparison with Available Years

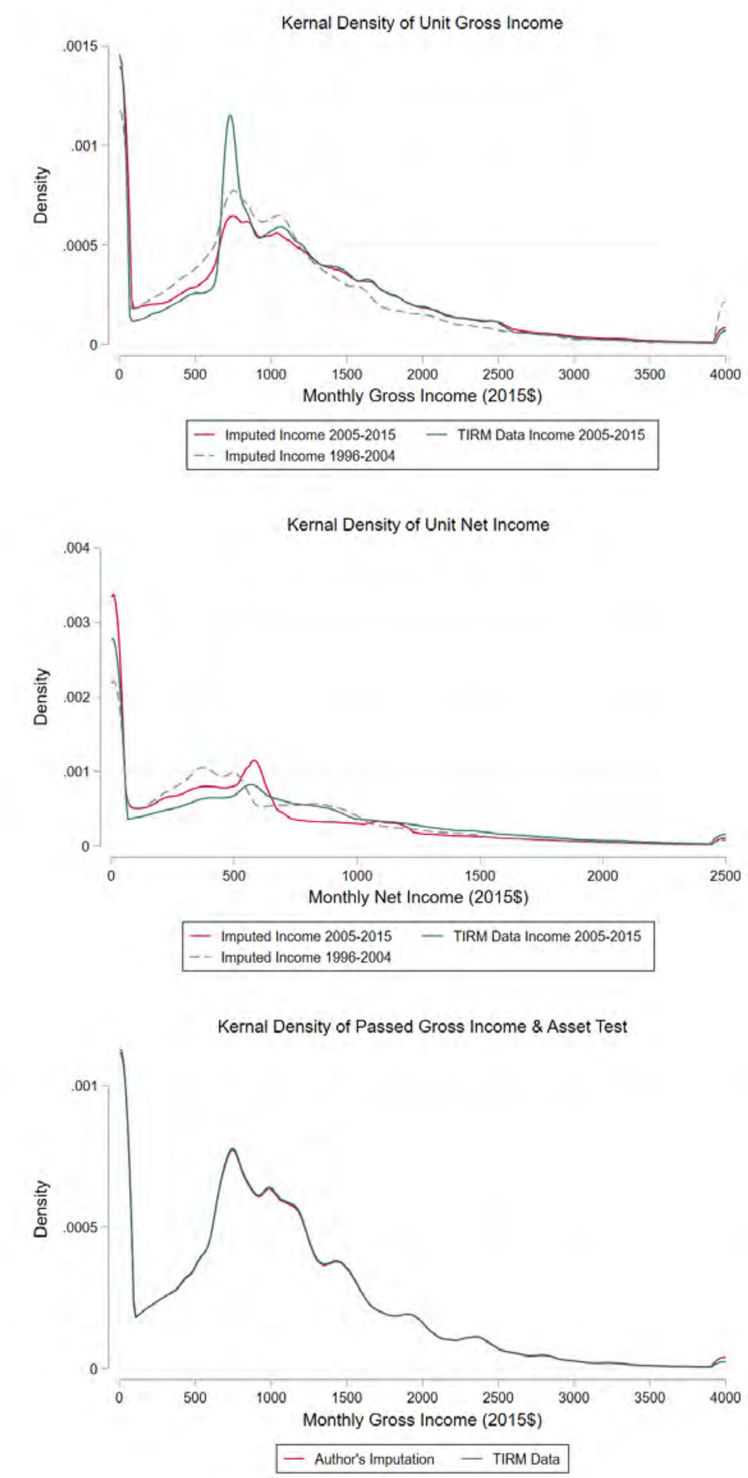
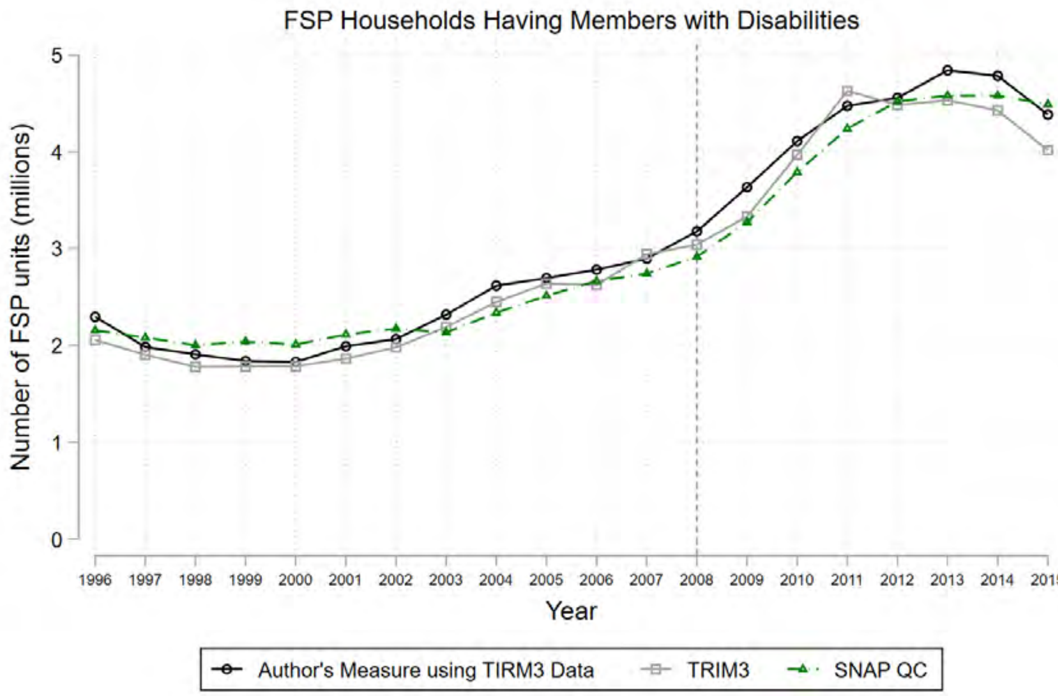
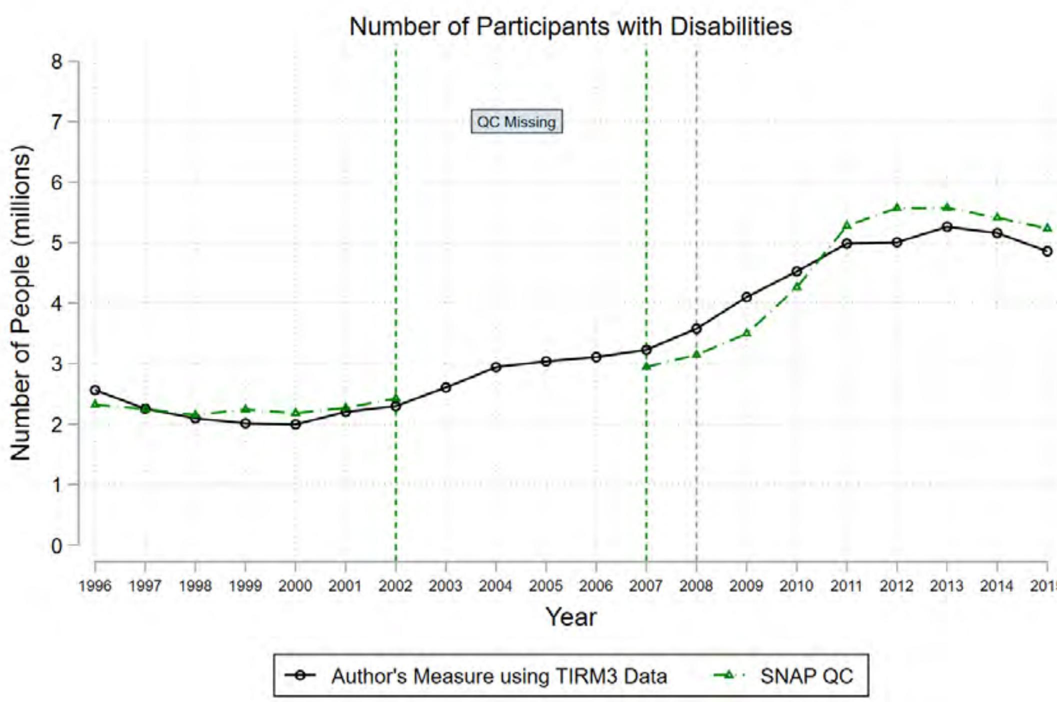


Figure A.3.2: Disability Measures





The author imputes individual disability status and verifies with the administrative data - the SNAP QC samples. The upper panel checks if the imputation renders consistent household characteristics provided in TRIM3 and QC. The lower panel checks the count of individuals with disabilities with the QC sample.

Table A.3.1: Lasso Sample Mean Characteristics: Always Eligible for At Least 1 month

	Pre BBCE	Post BBCE	
	Always-Eligible	Always-Eligible	Newly-Eligible
	(1)	(2)	(3)
Take-up Rate	0.508	0.569***	0.658***
	(0.492)	(0.486)	(0.469)
Gross Income	1415.8	1389.2*	3476.3***
	(1533.2)	(1512.7)	(2709.5)
Net Income	834.4	812.3**	2022.9***
	(1138.7)	(1075.4)	(2248.9)
Eligible Benefit Per Member	62.61	64.67***	46.79***
	(50.95)	(52.84)	(46.02)
Age of Head	46.51	46.88**	42.70***
	(19.21)	(19.08)	(14.55)
Head Female	0.616	0.620***	0.745
	(0.486)	(0.485)	(0.437)
Head White	0.710	0.741***	0.805**
	(0.454)	(0.438)	(0.397)
Head Black	0.237	0.214***	0.147***
	(0.426)	(0.410)	(0.355)
Head Hispanic	0.186	0.222***	0.0900***
	(0.389)	(0.416)	(0.286)
Head HS or Below	0.707	0.681***	0.498***
	(0.455)	(0.466)	(0.501)
Head Unemployed	0.806	0.824***	0.841
	(0.395)	(0.381)	(0.366)
Head Married	0.363	0.350***	0.289**
	(0.481)	(0.477)	(0.454)
Head Disabled	0.0935	0.105***	0.102
	(0.291)	(0.306)	(0.303)
Unit Size	2.292	2.219***	3.055***
	(1.538)	(1.498)	(1.690)
Unit Has Earnings	0.583	0.580	0.615
	(0.493)	(0.494)	(0.487)
Has Disabled Member	0.150	0.154*	0.145
	(0.357)	(0.360)	(0.353)
Has Elderly Member	0.294	0.300	0.159***
	(0.456)	(0.458)	(0.366)
Has Children 0-4 y.o.	0.224	0.217*	0.209
	(0.417)	(0.412)	(0.407)
Has Children 5-17 y.o.	0.338	0.325***	0.581***
	(0.473)	(0.468)	(0.494)
Has Noncitizen Member	0.116	0.114***	0.0121
	(0.320)	(0.317)	(0.109)
Observations	125651	23600	469

Lasso predicted sample. Standard deviation in parentheses. Estimates weighted by household sampling weights. Column (2) marks the mean difference t-tests between column (1) versus (2), and column (3) marks the difference between (2) and (3): * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. "Pre BBCE" includes never adopting states and adopting states in pre-periods. "Post BBCE" includes adopting states in post-periods. "Always-eligible" is defined as likely to pass the federal income and asset limits for at least 1 month in the year. "Newly-eligible" is defined as eligible but not "Always-eligible". The weighted share of newly eligible among all eligible in the post period is 1.91%.

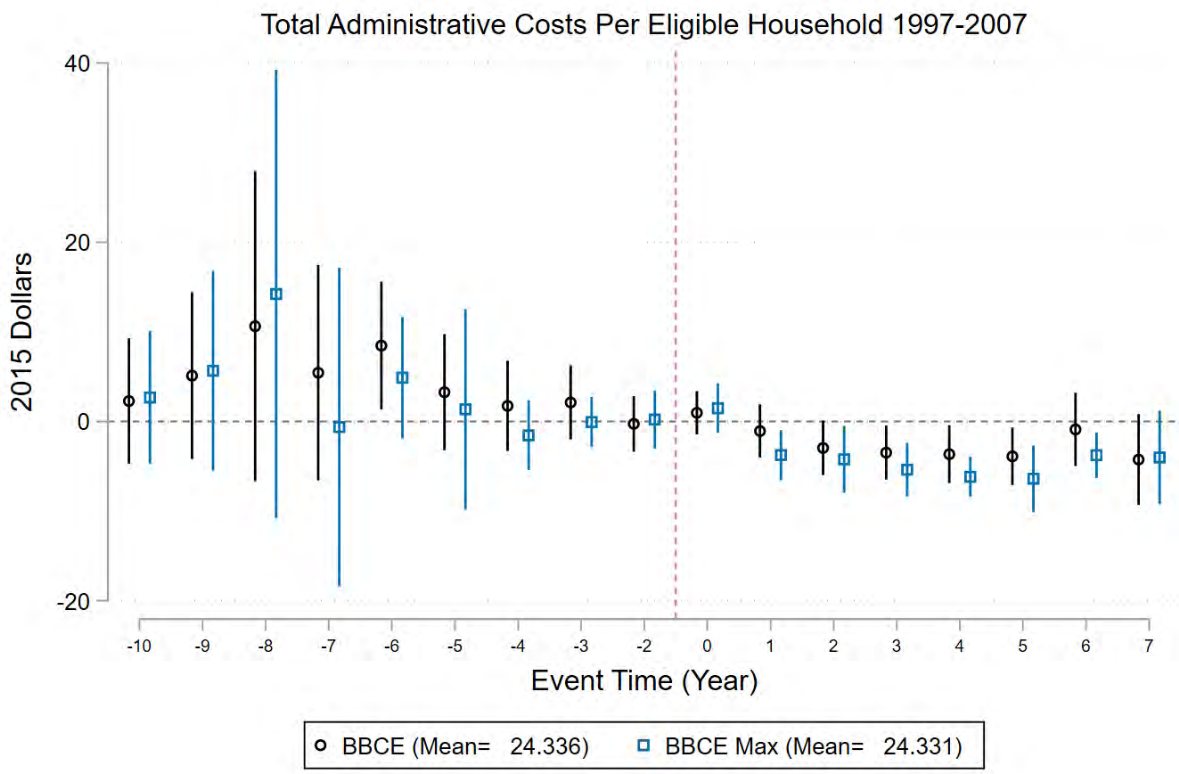
Table A.3.2: Mean Characteristics of Always Eligible Units: Short- versus Long-term Eligible

	12 months		At most 6 months	
	Pre BBCE	Post BBCE	Pre BBCE	Post BBCE
	(1)	(2)	(3)	(4)
Take-up Rate	0.523	0.555***	0.468	0.588***
	(0.494)	(0.491)	(0.485)	(0.472)
Gross Income	1163.3	1148.4	2515.9	2306.8***
	(1356.9)	(1148.0)	(3139.9)	(2432.6)
Net Income	635.0	636.1	1630.8	1494.8**
	(801.0)	(723.3)	(2398.1)	(1873.4)
Eligible Benefit Per Member	65.77	67.57***	50.57	51.73
	(51.36)	(53.44)	(47.32)	(48.03)
Age of Head	50.33	50.25	38.42	38.27
	(20.46)	(20.02)	(11.42)	(11.68)
Head Female	0.681	0.676	0.400	0.410
	(0.466)	(0.468)	(0.490)	(0.492)
Head White	0.679	0.713***	0.830	0.851**
	(0.467)	(0.453)	(0.375)	(0.356)
Head Black	0.269	0.241***	0.123	0.106**
	(0.443)	(0.428)	(0.329)	(0.308)
Head Hispanic	0.225	0.265***	0.0658	0.0850***
	(0.418)	(0.441)	(0.248)	(0.279)
Head HS or Below	0.786	0.762***	0.452	0.413***
	(0.410)	(0.426)	(0.498)	(0.492)
Head Unemployed	0.797	0.803	0.955	0.968***
	(0.402)	(0.398)	(0.207)	(0.175)
Head Married	0.339	0.338**	0.439	0.401***
	(0.473)	(0.473)	(0.496)	(0.490)
Head Disabled	0.130	0.141***	0.00805	0.00558
	(0.336)	(0.348)	(0.0893)	(0.0745)
Unit Size	2.239	2.201**	2.290	2.204**
	(1.506)	(1.509)	(1.563)	(1.479)
Unit Has Earnings	0.473	0.486**	0.869	0.861
	(0.499)	(0.500)	(0.338)	(0.346)
Has Disabled Member	0.209	0.205	0.0149	0.0120
	(0.406)	(0.404)	(0.121)	(0.109)
Has Elderly Member	0.411	0.402*	0.0113	0.00999
	(0.492)	(0.490)	(0.106)	(0.0995)
Has Children 0-4 y.o.	0.239	0.232*	0.141	0.142
	(0.426)	(0.422)	(0.348)	(0.349)
Has Children 5-17 y.o.	0.346	0.332***	0.256	0.252
	(0.476)	(0.471)	(0.437)	(0.434)
Has Noncitizen Member	0.157	0.150*	0.0128	0.0101
	(0.364)	(0.357)	(0.113)	(0.100)
Observations	94553	17752	18871	3961

Lasso predicted sample. Standard deviation in parentheses. Estimates weighted by household sampling weights. Columns (2) and (4) mark the mean difference t-test between pre- and post-BBCE periods: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. "12 months" represents always eligible for 12 months in the year. "At most 6 months" means always eligible for 1 to 6 months.

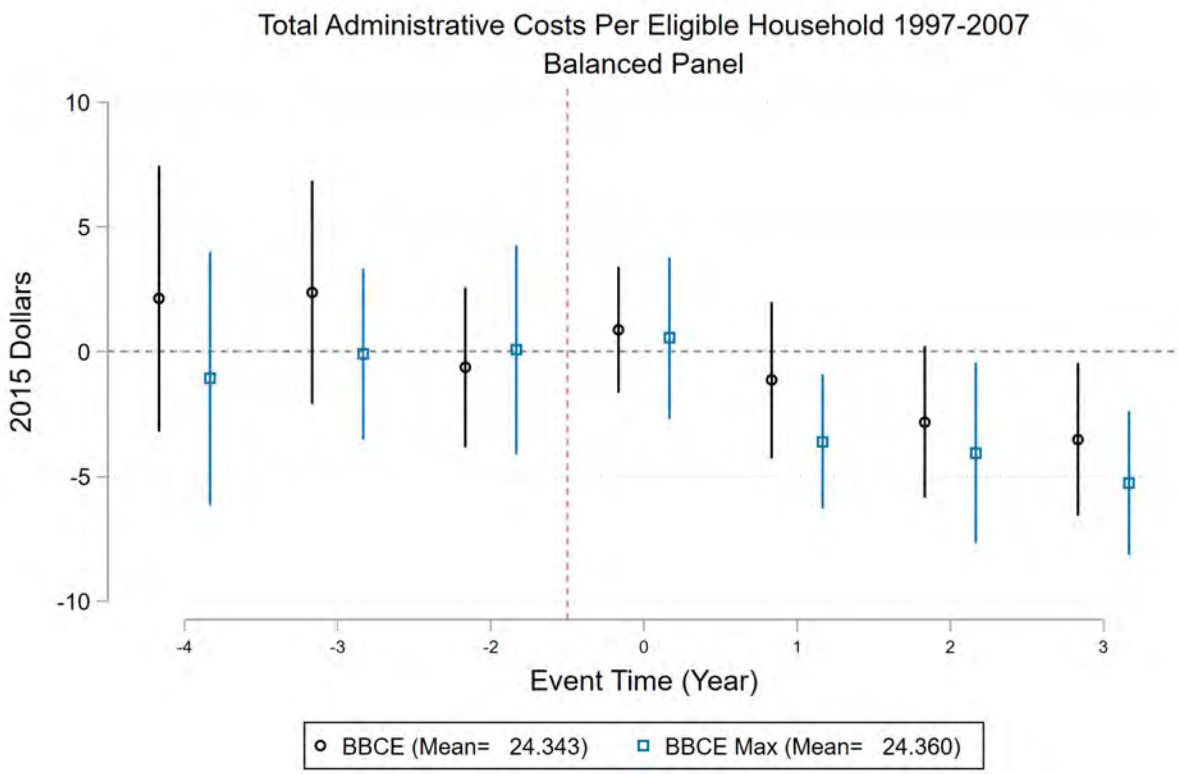
A.4 Result Appendix

Figure A.4.1: Total SNAP Administrative Costs: All Event Time Estimates



Sun & Abraham (2021) Event Study Estimates. Weighted by the size of always eligible (for any month) households. Clustered standard errors at the state level.

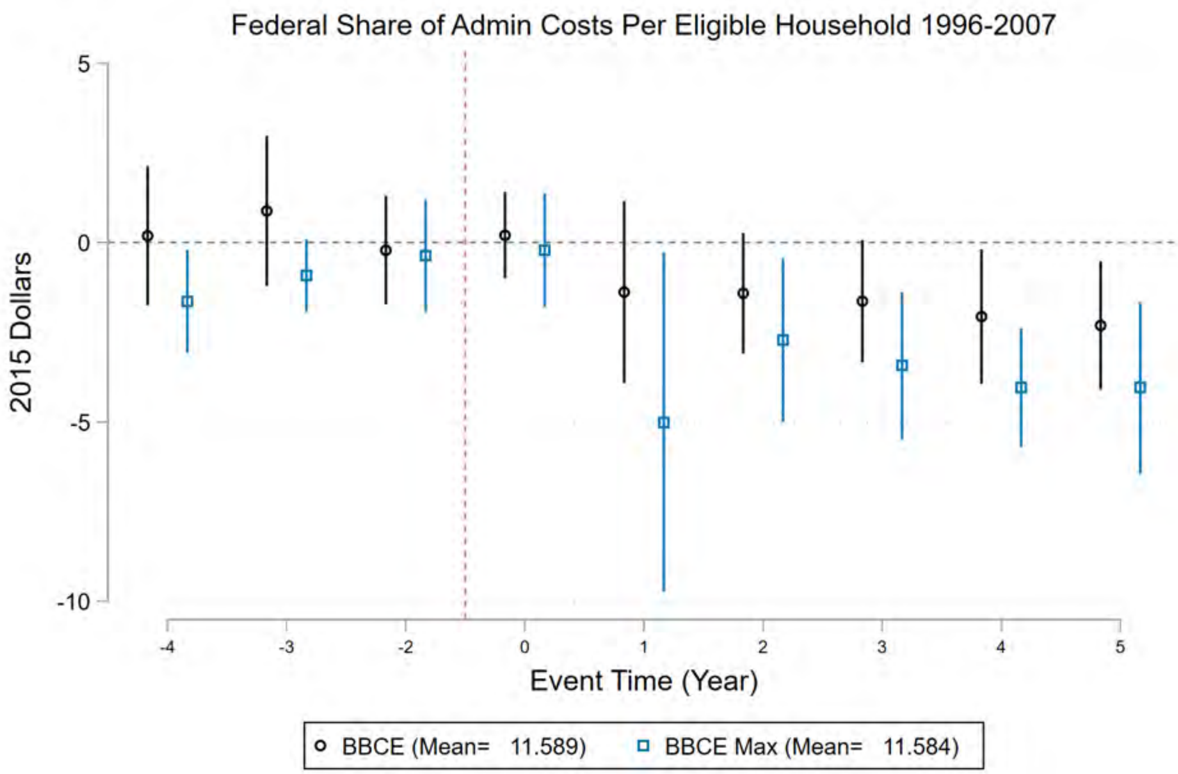
Figure A.4.2: Total SNAP Administrative Costs: Balanced Panel



Sun & Abraham (2021) Event Study Estimates. Weighted by the size of always eligible (for any month) households. Clustered standard errors at state level.

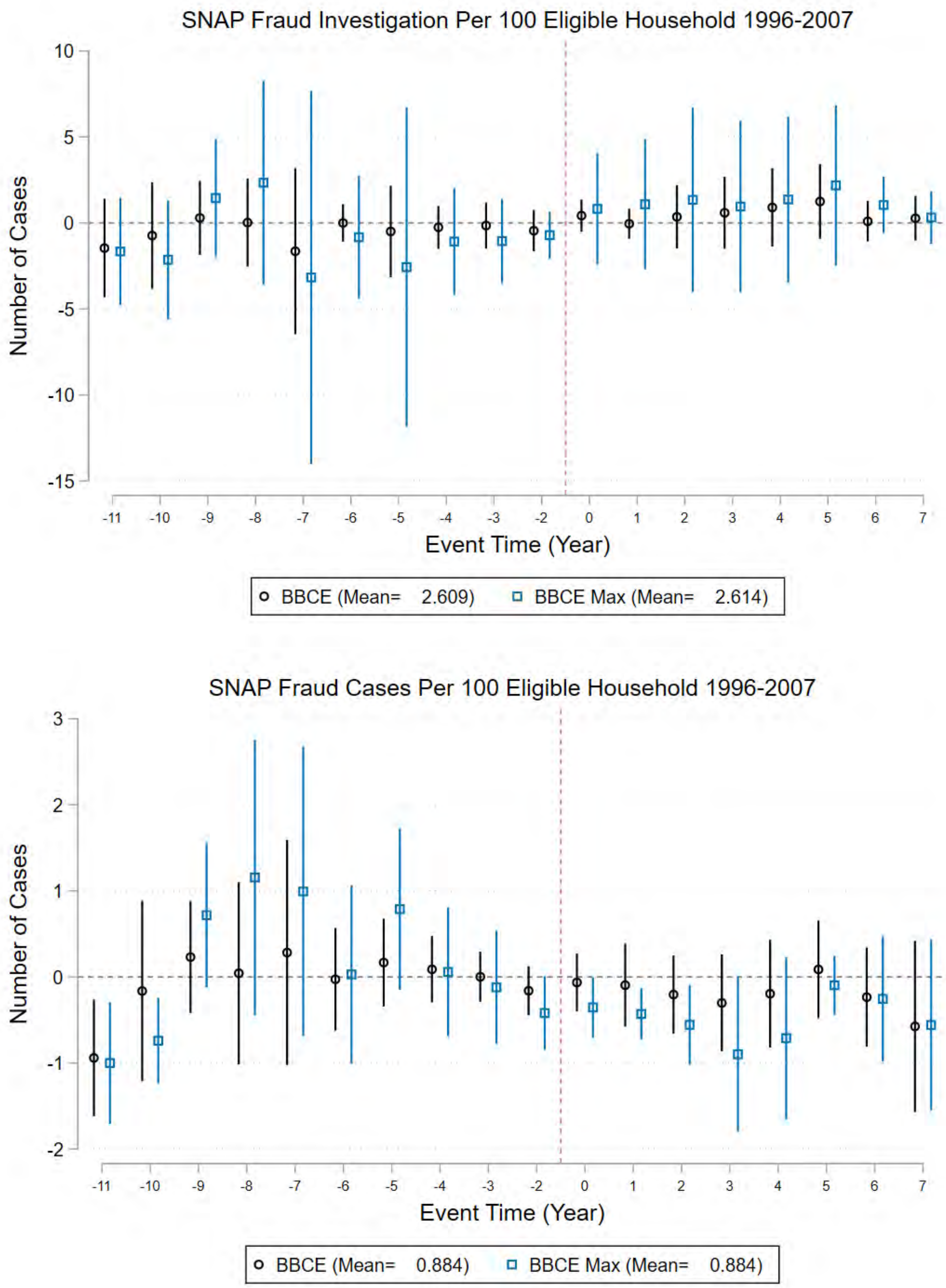
Eleven out of thirteen treated states (about 87% of the sample) are included in the balanced panel of BBCE. Six out of seven treated states (about 93% of the sample) are included in the balanced panel of BBCE Max.

Figure A.4.3: Federal Share of SNAP Administrative Costs



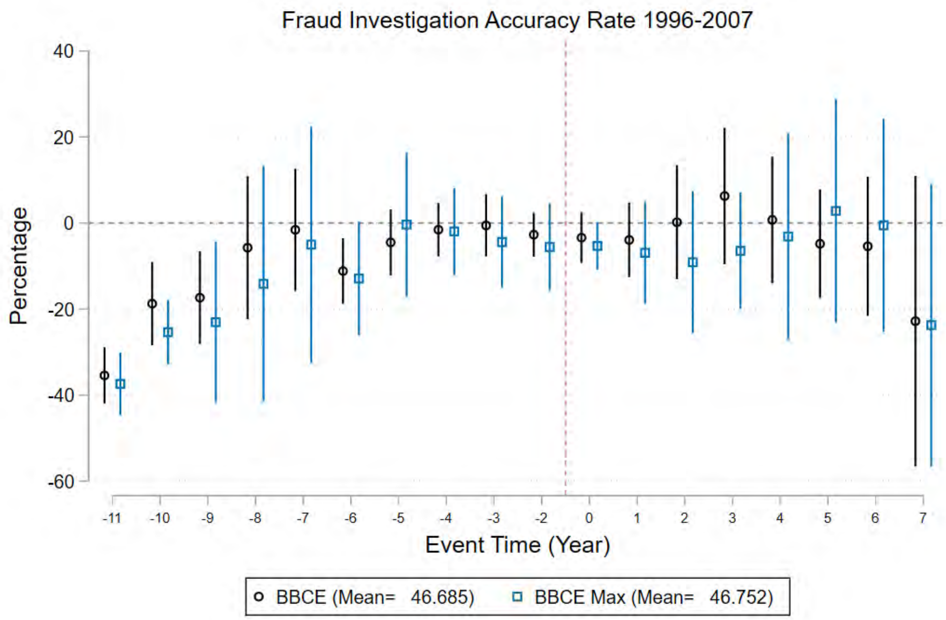
Sun & Abraham (2021) Event Study Estimates. Weighted by the size of always eligible (for any month) households. Clustered standard errors at the state level.

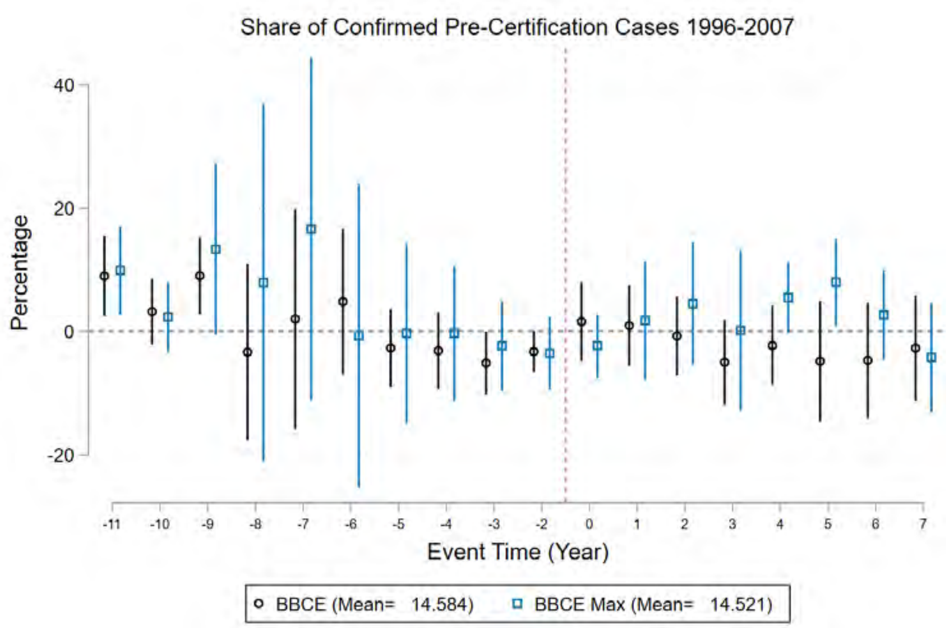
Figure A.4.4: SNAP Fraud Cases: All Event Time Estimates



Sun & Abraham (2021) Event Study Estimates. Weighted by the size of always eligible (for any month) households. Clustered standard errors at state level.

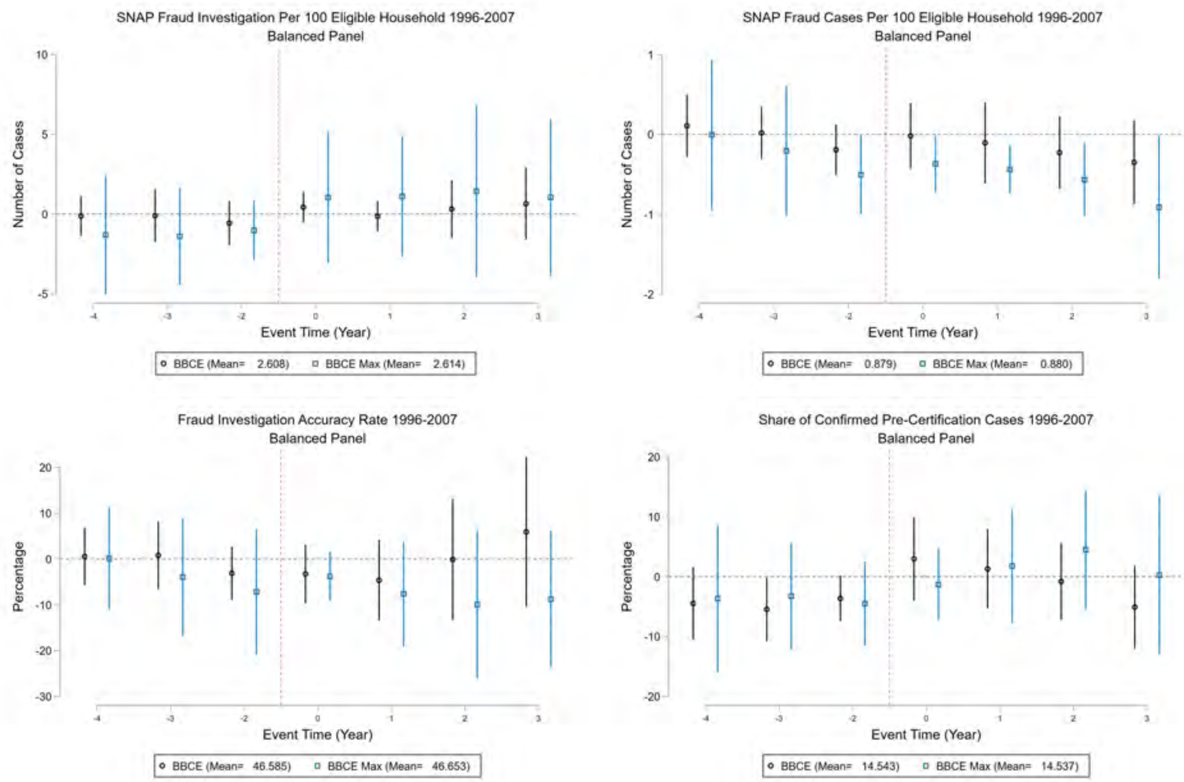
Figure A.4.5: SNAP Fraud Cases: All Event Time Estimates





Sun & Abraham (2021) Event Study Estimates. Weighted by the size of always eligible (for any month) households. Clustered standard errors at the state level.

Figure A.4.6: SNAP Fraud Cases: Balanced Panel



Sun & Abraham (2021) Event Study Estimates. Weighted by the size of always eligible (for any month) households. Clustered standard errors at state level.

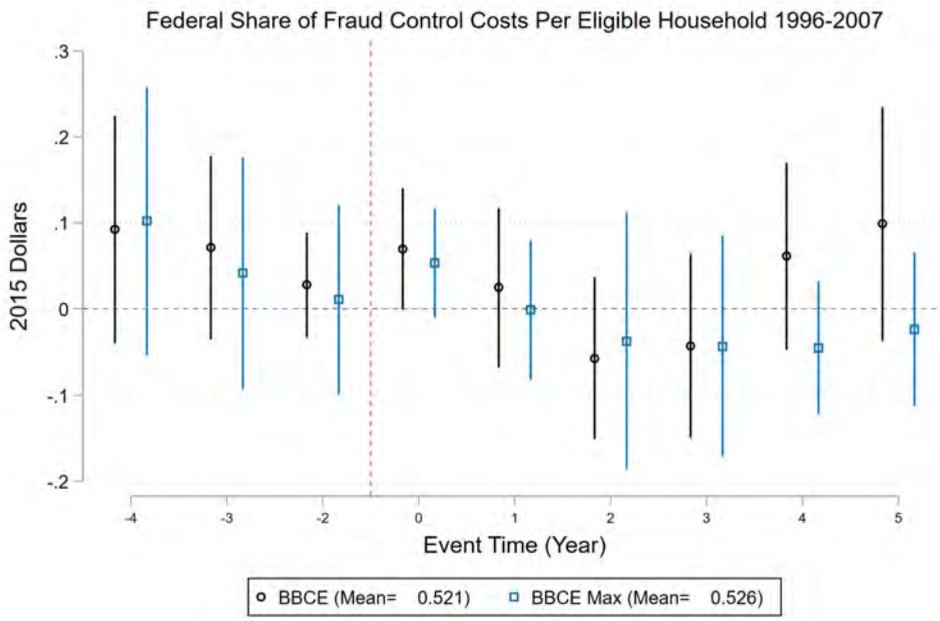
Eleven out of thirteen treated states (about 87% of the sample) are included in the balanced panel of BBCE. Six out of seven treated states (about 93% of the sample) are included in the balanced panel of BBCE Max.

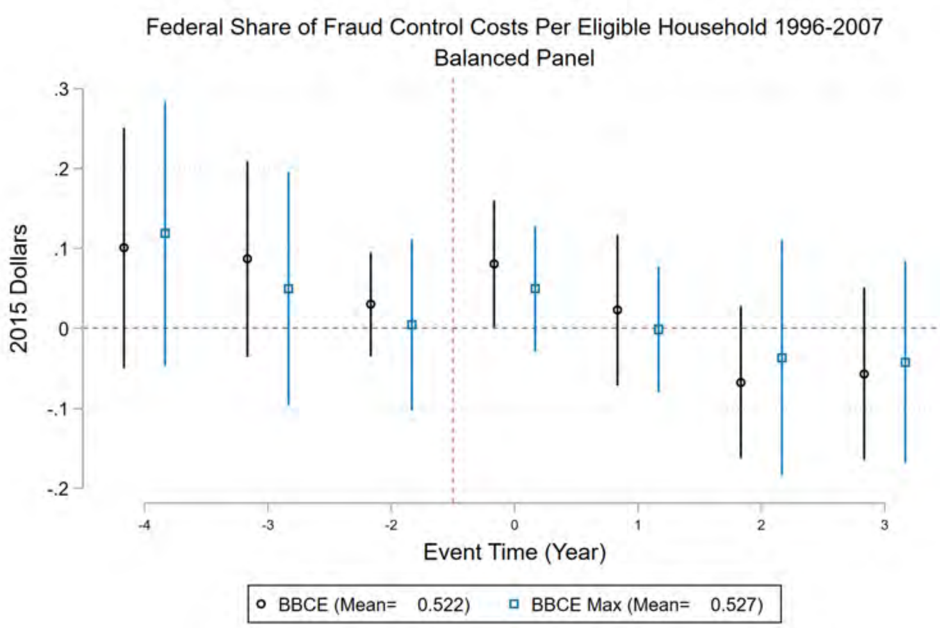
Table A.4.1: Aggregated Effects on State Outcomes

	Event -4 to -2		Event 0 to 3		Event 3	
	BBCE	BBCE Max	BBCE	BBCE Max	BBCE	BBCE Max
Total Administrative Costs						
All Observations	1.204	-0.461	-1.628	-2.965*	-3.473*	-5.382***
	(1.878)	(1.368)	(1.179)	(1.171)	(1.540)	(1.529)
Balanced Panel	1.284	-0.370	-1.651	-3.091**	-3.514*	-5.255***
	(1.859)	(1.357)	(1.219)	(1.184)	(1.557)	(1.478)
Number of Fraud Investigations						
All Observations	-0.286	-0.951	0.331	1.049	0.588	0.955
	(0.581)	(1.128)	(0.690)	(2.051)	(1.069)	(2.545)
Balanced Panel	-0.263	-1.241	0.313	1.156	0.648	1.051
	(0.644)	(1.423)	(0.709)	(2.157)	(1.159)	(2.514)
Number of Confirmed Fraud Cases						
All Observations	-0.0238	-0.162	-0.167	-0.560**	-0.302	-0.899
	(0.125)	(0.220)	(0.174)	(0.207)	(0.287)	(0.460)
Balanced Panel	-0.0213	-0.238	-0.175	-0.571**	-0.350	-0.912*
	(0.131)	(0.259)	(0.177)	(0.206)	(0.269)	(0.455)
Fraud Accuracy Rate (Confirmed Cases/Total Investigations)						
All Observations	-1.610	-3.988	-0.221	-6.959	6.277	-6.465
	(2.408)	(3.585)	(3.688)	(4.460)	(8.099)	(6.904)
Balanced Panel	-0.597	-3.668	-0.525	-7.529	5.920	-8.756
	(2.629)	(4.554)	(3.813)	(4.366)	(8.358)	(7.622)
Share of Pre-Certification Cases						
All Observations	-3.861*	-2.077	-0.822	1.005	-5.028	0.135
	(1.890)	(3.036)	(2.528)	(2.829)	(3.511)	(6.664)
Balanced Panel	-4.530*	-3.811	-0.411	1.300	-5.103	0.262
	(2.079)	(3.638)	(2.558)	(2.909)	(3.542)	(6.774)

Linear combinations of event study estimates. Standard error in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.
Balanced panel includes 11 out of the 13 states treated for BBCE (87% of the sample), and 5 out of the 6 states treated for BBCE Max (93% of the sample).

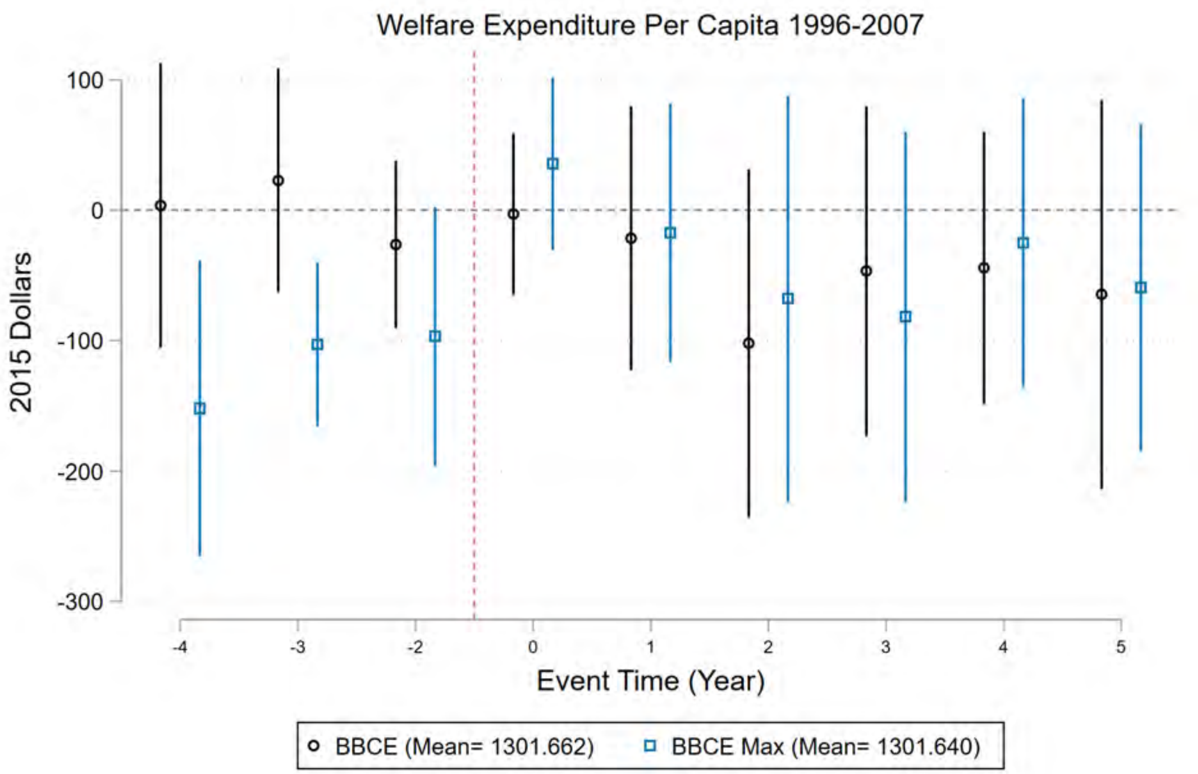
Figure A.4.7: Federal Share of States' SNAP Fraud Control Expenditure





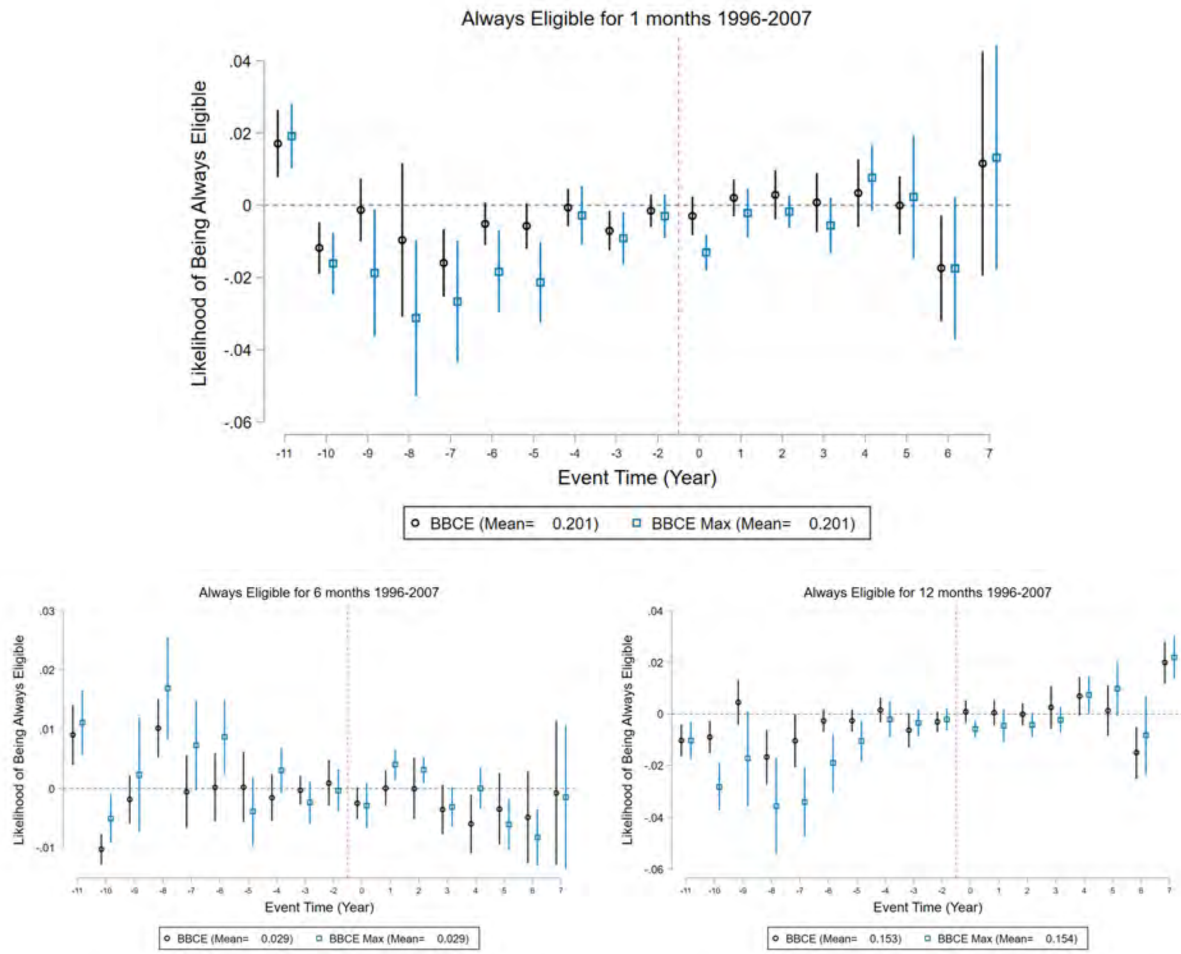
Sun & Abraham (2021) Event Study Estimates. Weighted by the size of always eligible (for any month) households. Clustered standard errors at the state level.

Figure A.4.8: State Welfare Expenditures Per Capita



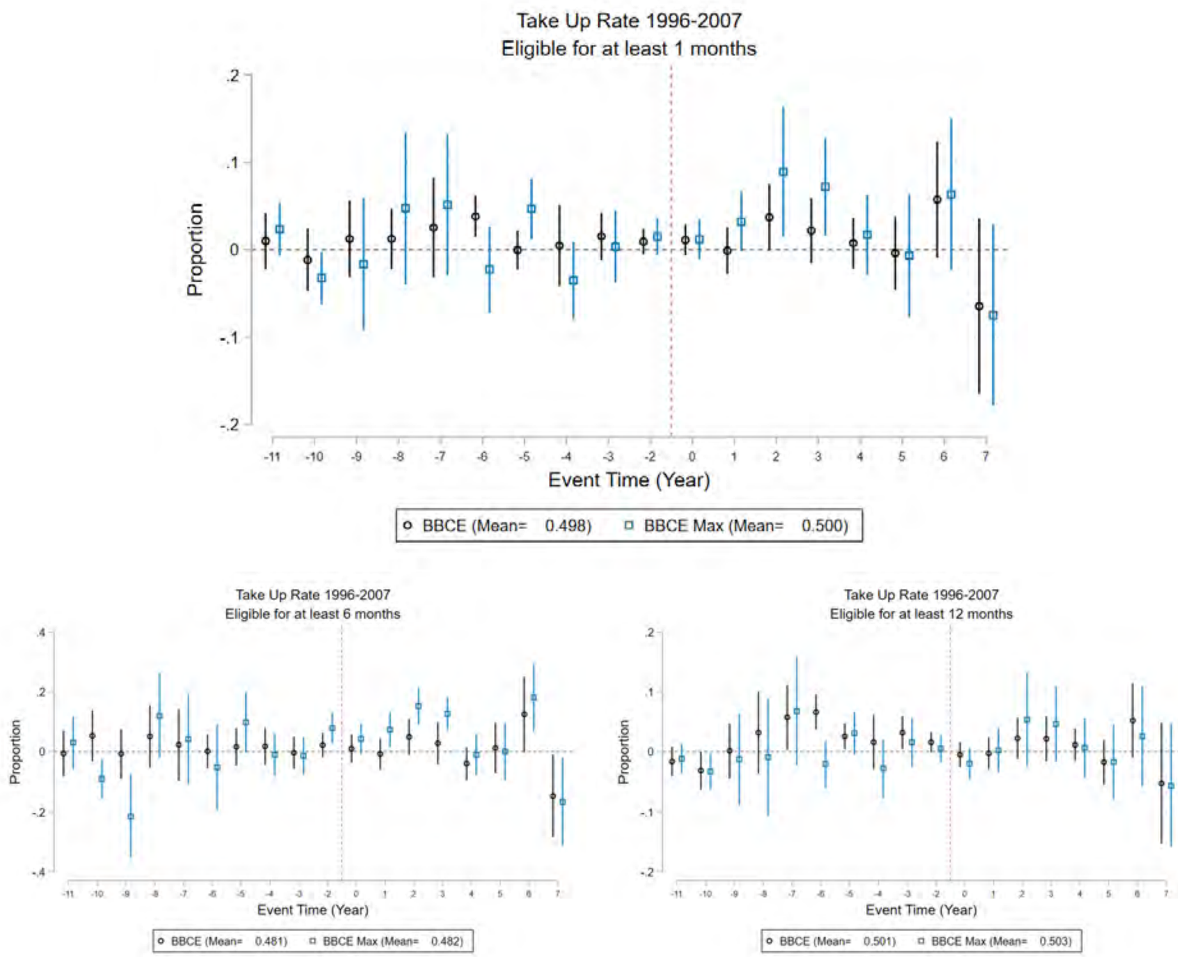
Sun & Abraham (2021) Event Study Estimates. Weighted by the size of always eligible (for any month) households. Clustered standard errors at the state level.

Figure A.4.9: Likelihood of Being Always-Eligible: All Event Time Estimates



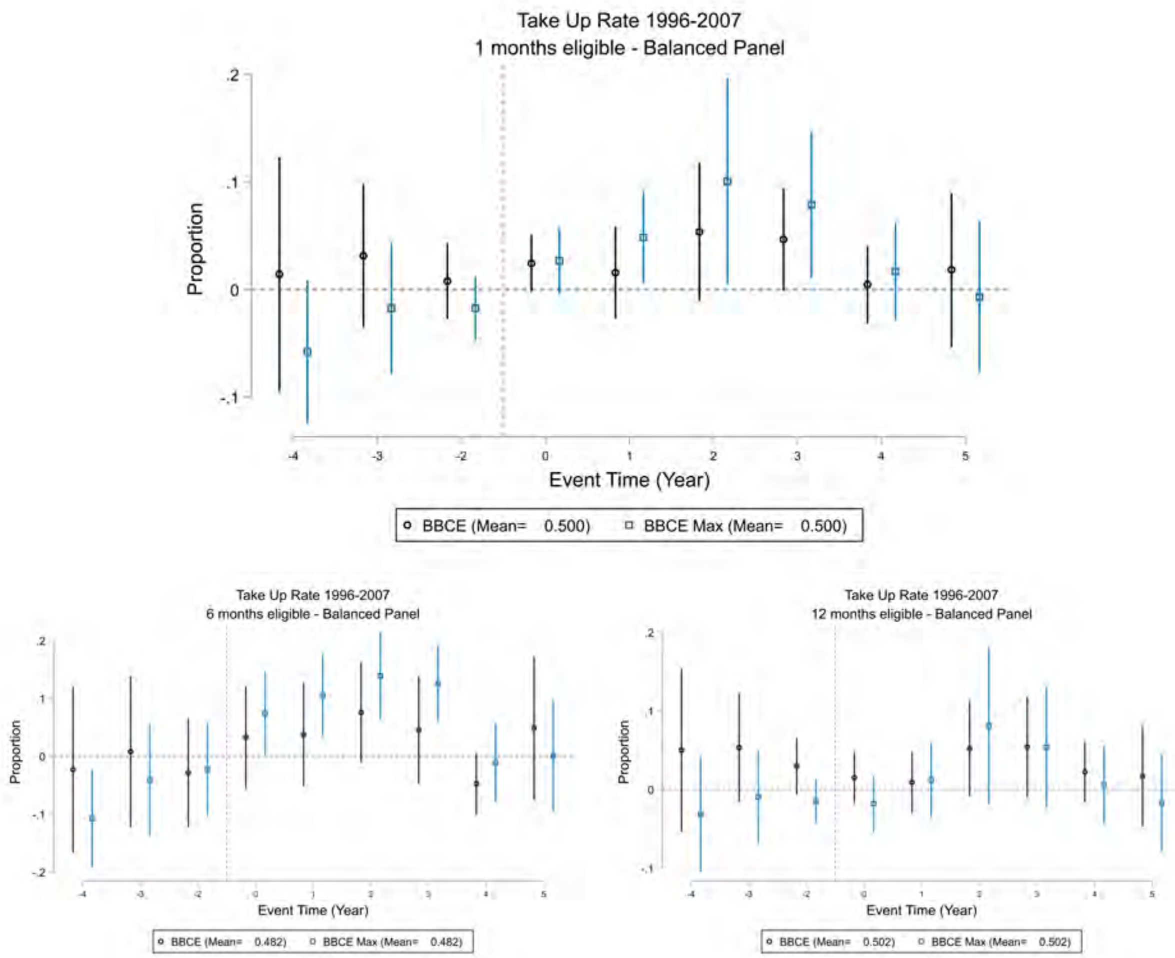
Sun & Abraham (2021) Event Study Estimates. Weighted by household sampling weights. Clustered standard errors at the state level. All covariates, as well as state and year fixed effects, are included.

Figure A.4.10: Take-Up Among Always-Eligible: All Event Time Estimates



Sun & Abraham (2021) Event Study Estimates. Weighted by household sampling weights. Clustered standard errors at the state level. All covariates, as well as state and year fixed effects included.

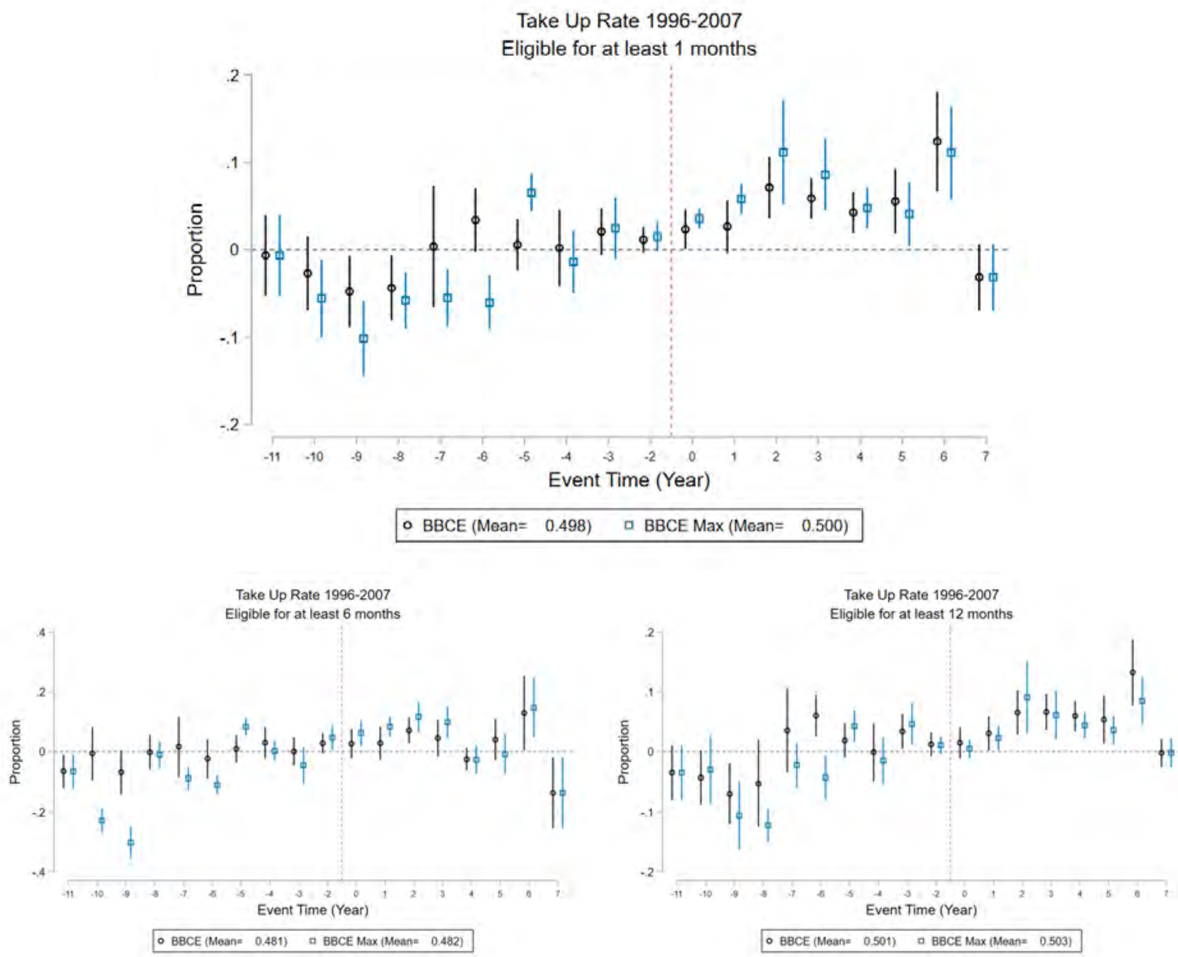
Figure A.4.11: Take-Up Among Always Eligible: Balanced Panel



Sun & Abraham (2021) Event Study Estimates. Weighted by household sampling weights. Clustered standard errors at the state level. All convariates as well as state and year fixed effects included.

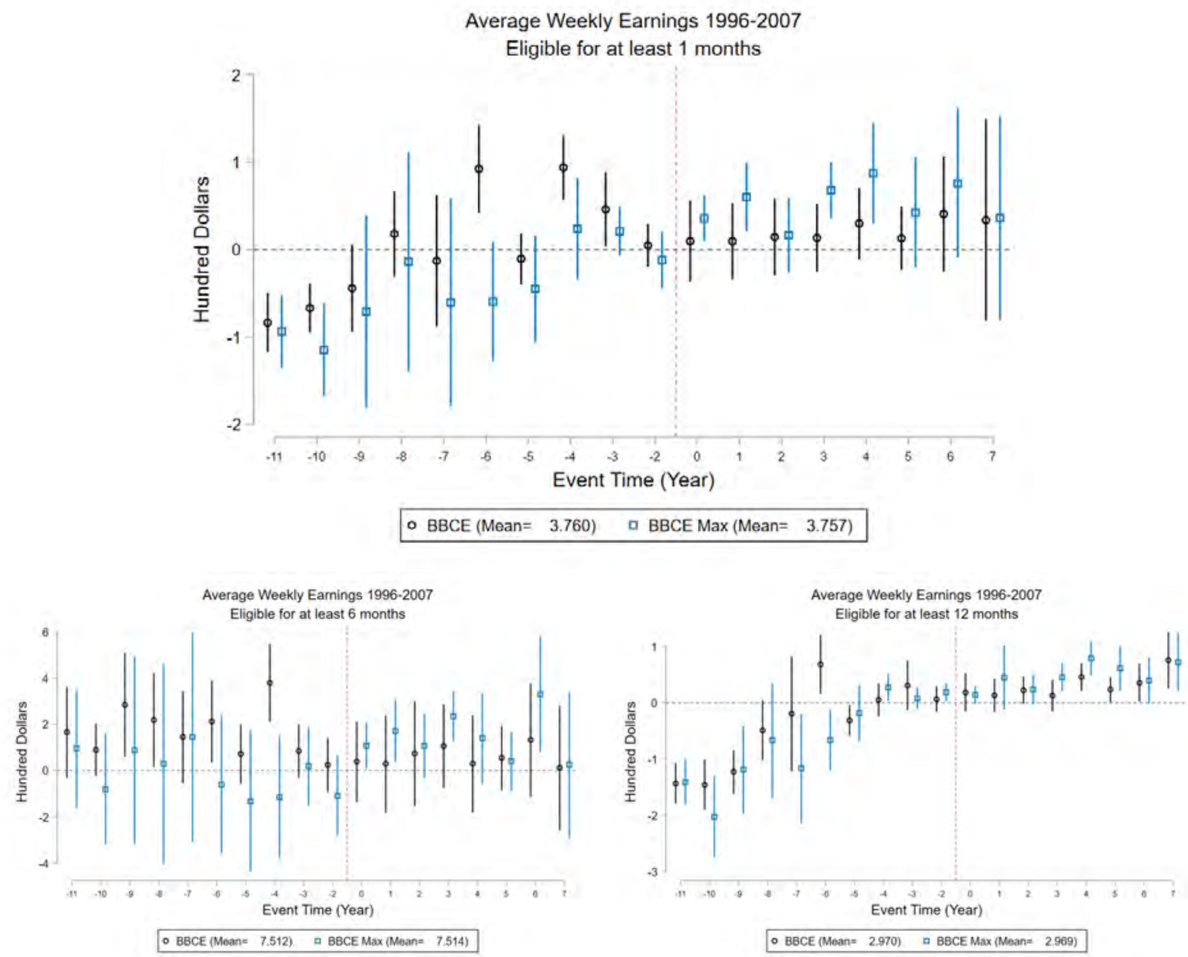
Eight out of thirteen treated states (about 85% of the sample) are included in the balanced panel of BBCE. Five out of seven treated states (about 88% if the sample) are included in the balanced panel of BBCE Max.

Figure A.4.12: Take-Up Among Always Eligible: Unconditional Estimates



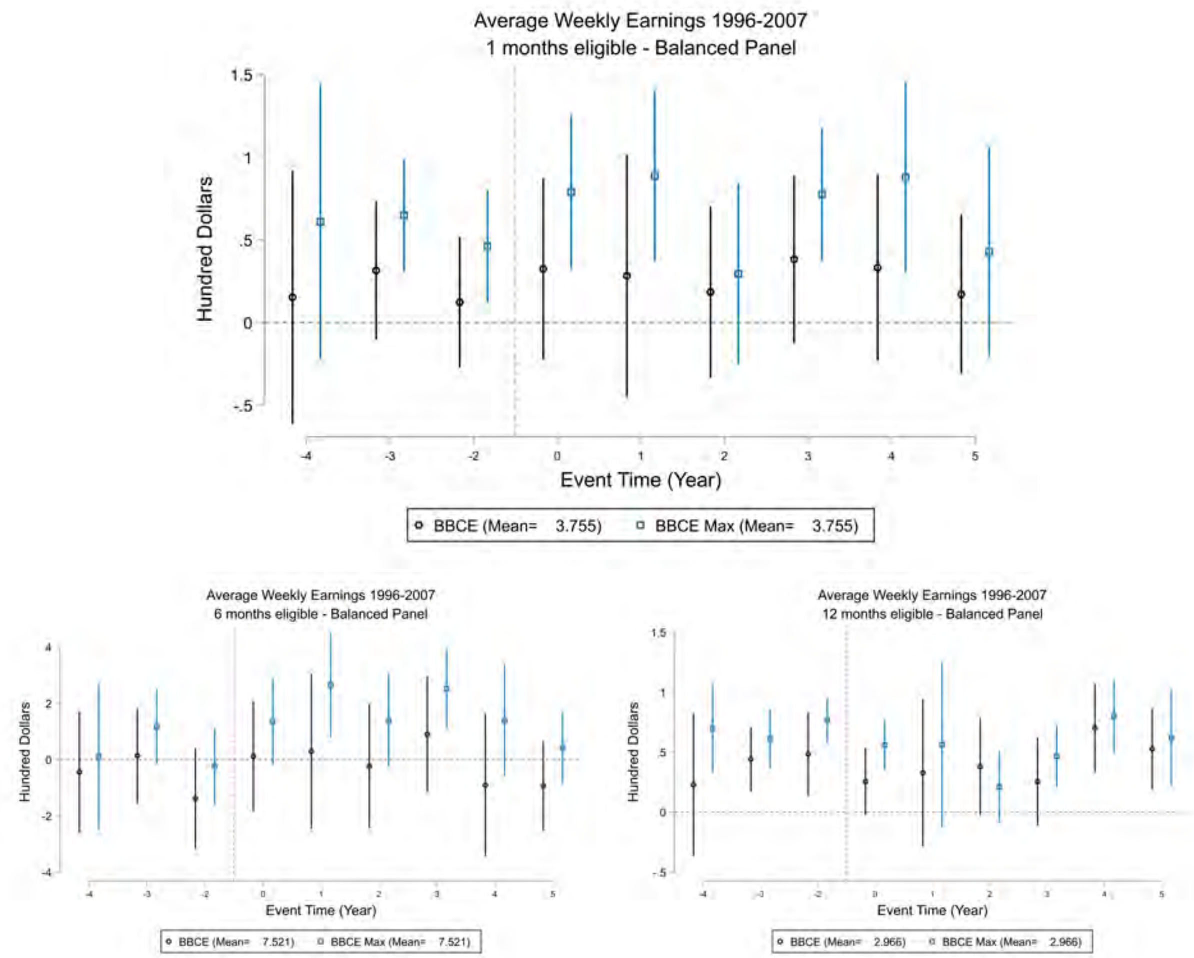
Sun & Abraham (2021) Event Study Estimates. Weighted by household sampling weights. Clustered standard errors at the state level. No covariates included except for the state and year fixed effects.

Figure A.4.13: Average Weekly Earnings Among Always Eligible: All Event Time Estimates



Sun & Abraham (2021) Event Study Estimates. Weighted by household sampling weights. Clustered standard errors at the state level. All convariates as well as state and year fixed effects included.

Figure A.4.14: Average Weekly Earnings Among Always Eligible: Balanced Panel



Sun & Abraham (2021) Event Study Estimates. Weighted by household sampling weights. Clustered standard errors at the state level. All convariates as well as state and year fixed effects included.

Eight out of thirteen treated states (about 85% of the sample) are included in the balanced panel of BBCE. Five out of seven treated states (about 88% if the sample) are included in the balanced panel of BBCE Max.