The Racial Politicization of the Safety Net*

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Abstract

We show that the introduction of the Food Stamp program drove long-run political polarization across racial groups. Using voter roll data for the entire U.S. we show that individuals of voting age at the time of the program's rollout (1961–1975) diverge along racial lines in their likelihood of voting and registering as Republicans or Democrats up to a half-century later. Our design ensures that these findings are not driven by geographic or age-specific racial trends. Critically, these diverging political patterns on average benefited Republicans. We also explore mechanisms. First, we show that access to the safety net also had short-run effects consistent with racial politicization on voter turnout, voter registration, and changes in the ideological composition of Congress. Second, we explore the interaction between Food Stamps and contemporaneous events such as the Voting Rights Act of 1965, religious prevalence, and recessions.

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1. Introduction

The Food Stamp program covers 42 million people in the US today, including nearly one in four children.[1] Despite the importance of this major safety net policy, the downstream effects of the Food Stamp (FS) program as a racialized political phenomenon are yet to be thoroughly understood. The rollout of the FS program between 1961 and 1975 overlapped with key legislative achievements of the Civil Rights movement – the Civil Rights Act of 1964 and the Voting Rights Act of 1965 – and the decline of the Jim Crow system. Following these changes in the legal and political landscape, the introduction of the federal FS program was ripe for politicization, especially since controlling access to food assistance had been used as a strategy to constrain Black Americans' political participation.[2]

This paper quantifies the impact of the politicization of the FS program on individuals' political behavior over the long-run. Using individual-level microdata we explore the intersections between social support policies, race, partisan affiliation, and electoral outcomes. We provide novel evidence that the major expansion of the federal safety net represented by the FS program unfolded as a racialized phenomenon that impacted both party affiliation and voting patterns not only in the 1960s and 1970s, but also for decades afterward. We first describe the effects of the FS program on present day political affiliations and voting. Second, we examine the short run effects of the rollout on partisan political representation. Third, we explore the interactions between FS rollout effects and other major economic and political events of the period.

To examine the long run effects of the FS program, we use a comprehensive national dataset that captures the universe of U.S. voters as of 2020, as well as their voting history over the last two decades. We compare voting patterns between individuals who were already eligible to vote (age 18+) when their county first adopted a FS program to individuals who were younger when their county initially adopted FS. Papers examining long run or voting effects typically contend with the concern that age or cohort-specific differences drive results. The county-by-county rollout of the FS program allows us to use a rich set of fixed-effects in a stacked difference in differences design that ensures our estimates are not driven by age, geographic location, or shifting political attitudes

[1] USDA figures for total individuals covered by SNAP (May 2023). The number of children participating in SNAP or WIC is from the 2021 Survey of Income and Program Participation.

[2] For example: *"As the stream of voting applicants in Greenwood [Mississippi] increased [...], the economic screws were tightened on the Negro community [... The County] stopped distributing surplus food, cutting off 22,000 people – mostly Negro – who depended on it."* Howard Zinn (1964)

between 1960 and 2020. This combines difference in differences with an experience-based design. We first show that voting-age individuals at rollout – relative to those below voting age – are *more* likely to be registered as Republicans and *less* likely to be registered as Democrats in 2020, around a half-century later. Next, we show that this political divergence occurs along racial lines: White voters exposed to the FS rollout as adults are more likely to be registered to vote as Republicans in 2020, and less likely to be registered Democrats. By contrast, Black and Hispanic voters were much less likely to register as Republicans than Whites, registering instead as Democrats or Independents. In turn, Asian voters exposed to the FS rollout as adults are relatively more likely to register as Independents than White voters, and less likely to register as either Democrats or Republicans.

Electoral impact depends not only on voter registration, but also on the rate at which individuals choose to vote. We use data on individuals' voting history to examine how voting propensity changes in response to FS rollout. Overall, exposure to the rollout reduces voting propensity, with effects differing by party and race in ways consistent with increased *racial* political polarization. Exposure to the FS program increases the likelihood of White Republicans voting, as well as the likelihood of Black or Hispanic Democrats voting, with corresponding decreases in the voting rates of the opposing party for each racial group. When we focus on the subset of people who registered to vote during their impressionable years – who are likely more politically engaged than those who register at later ages – we find stronger effects. Thus, our results point to the introduction of the FS program as contributing to present-day patterns of racialized political polarization in voting.

We also show that shifts in partisan affiliation and turnout propensity differ by gender. FS rollout increases male Republican affiliation and voting by Republicans. By contrast, FS rollout appears to have pushed women away from the Democratic party and voting, and towards registering as Independents. Interestingly, we find that racial heterogeneity in response to the FS program appear to be quite similar between genders.

Next, we explore the *short run* effects of the FS program. Using voter registration data from 1960 to 1972 covering 11 southern states we show that the FS rollout led to an increase in Black voter registration rates, but no increase for Whites. Moreover, the Democrats' vote share in U.S. House elections rose immediately after the FS rollout, followed a few years later by an increase in the difference between Democratic and Republican vote shares, driven by areas with with either (i) a high black population, (ii) a low poverty share, or (iii) high household income. However, this increased Democratic vote share did not yield more House election victories on average, and instead was accompanied by reduced likelihoods of Democratic victories in areas with low Black populations

and areas of high poverty, suggesting political backlash effects. We also find that FS rollout also impacts the voting behavior of US House representatives. In areas with a high Black population share or high poverty – Democrat and Republican alike – the effect of FS implementation leads to more conservative voting by representatives, which is also consistent with a political backlash.

We then turn to how the effects of the FS program interacted with a major contemporaneous historical event: the changes to minority voting facilitated by the Voting Rights Act of 1965 (VRA), and the associated political polarization. To do this we compare counties covered by Section 5 of the VRA with adjacent non-covered counties (both within and across state borders), following Aneja and Avenancio-León (2022). The results for these counties indicate that the *interaction* of VRA coverage with exposure to the FS rollout contributed to a rightward shift in voter registrations overall, with Republican registrations rising and Democratic registrations falling, and reduced voting by Independents, with no relative effects by race. This is consistent with studies finding White backlash in response to the passing of the VRA (Bernini et al., 2023). However, our estimates of the effects of exposure to the FS rollout remain essentially unaffected, indicating that even in this subsample of VRA-covered counties, the VRA did not have a first order mediating effect on the long run response to the FS rollout. Instead, the racial politicization of food stamps appears to be a concurrent phenomenon.

Second, we examine how the long run political effects of FS differ by areas' exposure to recessions, which both raise the demand for the social safety net and may induce changes in the population's beliefs regarding social mobility and preferences for redistribution. We find that recessions appear to be an important mechanism through which the effects of the FS program transmit to long term political preferences and behavior. In particular, Whites in counties exposed to more local recession years since FS rollout shift away from the Republican party and towards the Democratic party in response to the FS program. The response of Blacks in these counties is similar, except that they shift even further towards Democratic party registration. By contrast, Hispanics in counties with greater incidence of recessions exhibit the opposite pattern: shifting towards the Republican and away from the Democratic party. Thus, local recessions appear to be associated with substantial and heterogeneous effects on the long run political consequences of the FS program rollout.

Third, we examine the role of churches. Churches play a pivotal role in voter mobilization in some states, and so may mediate the long run impact of the FS program. The empirical effects are unclear: churches may endorse government help for the poor, or they may reduce the locally-

perceived need for FS by providing an informal community-based safety net. We find that, while churches do serve to mobilize minority voters – increasing Democratic and reducing Republican rates of registration and voting – they do not drive the baseline response to FS rollout of minority voters. Instead, areas with higher church density display a differential response to FS, with voter response to the FS rollout shifting *rightwards*, with especially large effects on Hispanics.

Contributions. This paper furthers our understanding of the economics of race in the U.S. and lies at the intersection of economic history, political economy, and public finance. While the politicization of food stamps along racial lines has long been been studied by historians and legal scholars (e.g., Zinn, 1964, Edelman, 2004, Kornbluh, 2007, 2015), we provide, to the best of our knowledge, the first causal empirical estimates on the racial politicization of social welfare policies, as well as evidence on the mechanisms driving this process.

Second, by measuring the political changes generated by the FS program our paper contributes to the literature on the impact of the program beyond its direct economic effects. This adds to a series of papers documenting the positive effects of the FS program on contemporaneous and long run outcomes (e.g., Currie and Moretti, 2008, Hoynes and Schanzenbach, 2009, Almond et al., 2011, Hoynes et al., 2016).

Third, we contribute to the literature on how policies affect voting and political inequality. Existing research examines how legal barriers affect well-documented disparities in voting behavior (Fraga, 2018), including studies on voter identification laws (Hajnal et al., 2017), educational policies (Filer et al., 1991), race-based redistricting (Washington, 2012), and the Voting Rights Act (Schuit and Rogowski, 2017, Ang, 2019, Aneja and Avenancio-León, 2019, Aneja and Avenancio-León, 2022). Similar in spirit to this paper, Choi et al. (2024) shows that NAFTA led to job losses in exposed counties, driving voters away from the Democratic party, especially among those with protectionist views. This paper maps the political consequences of a different, welfare-based policy over both the short and the long run.

Finally, we contribute to the literature on the dynamics of race and voting during the civil rights era. Kuziemko and Washington (2018) shows that racial views were critical for Whites' exodus from the Democratic party in the South; we show that the FS program was a key contributor to long run racial polarization across the United States. Kogan (2021) shows effects on Democratic vote share and turnout in the period immediately following the FS rollout, but does not examine racial differences or any long run effects. Weaponization of food benefits to constrain Black Americans'

political participation preceded the FS program and the VRA (Zinn, 1964, Kornbluh, 2015). Consistent with the historical record, we show that the racial politicization effects of the FS program were more extreme in areas that were subject to the VRA, where economic gains were larger for minorities (Aneja and Avenancio-León, 2022) and where White backlash was more forceful (Bernini et al., 2023).

2. Data

Our main dataset is built around the county-level rollout of the Food Stamp program across the United States between 1961 and 1975, obtained from Hoynes et al. (2016). For long run outcomes we use voter roll data from L2, an established and non-partisan data vendor used by political campaigns and the academic literature (e.g., Allcott et al., 2020, Spenkuch et al., 2023, Engelberg et al., 2022, Dahl et al., 2023). The L2 data provides information on all registered voters in all U.S. states as of October 2020, including address, birthdate, and sex. Importantly, conditional on an individual appearing in the 2020 L2 vintage, the data also includes historical information on each individual, including voting and registration history. From this time-series, we compute a measure of voting propensity: the share of elections in which an individual voted (relative to the total number in which they were eligible to vote). The time-series aspect of L2 voting history also allows us to conduct several subsample and robustness analyses, including restricting our estimation to a subset of individuals who have been consistently registered in the same state since FSP rollout.

In addition, the voter roll data contains information on individuals' political partisanship. For 34 states (and DC), L2 assigns political affiliation using self-reported voter registration. For the remaining states, L2 infers party using a variety of data sources, including voter participation in primaries, demographics, exit polling, and commercial lifestyle data. L2 data is routinely used in the field by political campaigns and academic research has also tested the accuracy of the partisanship measures in voter files.[3] We also make use of L2's information on individuals' race. This data comes from voter registrations in some states, while for others it is inferred by L2.[4] We drop registered voters with missing year of birth, race or county information.

We also use historical data on voting at the county level from ICPSR and Dave Leip's Atlas of US

[3]Specifically, Bernstein et al. (2022) compares L2 partisanship data to state files; Brown and Enos (2021) compares L2 partisanship data to a survey, and Pew (2018) compares multiple commercial voter file data providers to microdata from Pew national surveys.

[4]Bernstein et al. (2022) compares L2's race data to HMDA mortgage applications; Pew (2018) compares race in commercial voter registration data to Pew national panel microdata. In all specifications using race with this data we drop individuals in the following categories: Islander, Native, mixed, other and unknown.

Presidential Elections. Historical voter registration at the county level for 11 southern states from 1960 to 1972 was obtained through the U.S. Commission on Civil Rights and the NAACP Voter Education Project. Additional data is joined to the registration data from Matthews and Prothro (1963) and was obtained from Jim Alt. County-level data on Black Elected officials from 1960 to 1975 was obtained by digitizing several editions of the National Roster of Black Elected Officials from the Joint Center for Political and Economic Studies (JCPES) and supplemented with data from Alt(1984). Finally, we obtain voting data for the U.S. Congress from the DW-NOMINATE project (https://voteview.com/) from 1962 through 1974.

2.1 Do county characteristics predict the timing of the Food Stamp program rollout?

Our empirical strategy exploits the pseudo-random timing of the Food Stamp (FS) program rollout across counties, following Hoynes and Schanzenbach (2009) and subsequent papers. In this section we examine whether the timing of FS rollout was a function of county characteristics related to our outcomes of interest, specifically political, racial and income variables potentially related to demand for the FS program among residents. To explore this, for each year we consider the set of counties that have not yet rolled out FS and regress an indicator for rollout in the following year on a pre-rollout county characteristic. Thus, if the timing of FS rollout is driven by, for example, whether the county is represented by a Democratic member of the U.S. House of Representatives, we would expect the latter to systematically predict rollout in these regressions.

Figure 3 reports the results of this exercise. The top panel plots unadjusted point estimates; the bottom panel divides these estimates by their sample averages to make the magnitudes easier to interpret. Both panels show that neither racial variables (county population share that is Black, or non-White) nor political variables (vote share for the Democratic party, whether the county was represented by a Democrat in the House, turnout in the preceding Presidential election) predict the timing of FS rollout at the yearly level. Moreover, the confidence intervals mostly rule out large economic magnitudes, especially for the political variables. Perhaps more surprisingly, the figure also shows that variables suggesting greater ex-ante local demand for the program (such as the share of residents using Public Assistance programs, mean family income and share in poverty) also do not predict the timing of FS rollout, although the confidence intervals for county share in public assistance are very large.

3. Experience DID Design & Long run effects

We examine the long run effects of the Food Stamps (FS) program by comparing lifetime voting patterns, observed as of 2020, for people exposed to the rollout as adults (18+) vs. same-age individuals who lived in a county that implemented FS before they were eligible to vote. This employs a difference in differences (DID) design to evaluate an experience effect. More specifically, the design exploits the staggered timing of FS rollout across counties, where the two differences are county and *birth year* rather than the more typical combination of geography and calendar year. Importantly, like any evaluation of experience effects, our design identifies the effect of FS from cross-cohort differences. For clarity, we outline our estimating equation below and then elaborate on exactly what variation identifies our estimates.

Our base specification is as follows:

$$Y_{ic} = \beta Food\ Stamps_{ic} + \alpha_c + \gamma_{i(b)} + \epsilon_{ic} \tag{1}$$

where i indexes the individual, c the county, and $\gamma_{i(b)}$ is a birth year fixed effect. $FoodStamps_{ic}$ is an indicator for whether the FS program rollout occurred in an individual's county when they were of voting age (18+). Thus, β estimates the conditional impact on the dependent variable of being exposed to the county-level implementation of the FS program as an adult, relative to being exposed at a younger age or growing up in a world where FS is a well-established part of the social contract. We refer to this as *adult exposure* or *treatment*. (In Section 3.2 we present results that relax this assumption of all-or-nothing treatment around age 18.)

Equation 1 is a two-way fixed effects DID estimator. A large literature has documented the potential for TWFE estimators to be biased in a staggered-DID setting (e.g., De Chaisemartin and d'Haultfoeuille, 2020, De Chaisemartin and D'Haultfoeuille, 2022, Goodman-Bacon, 2021, Callaway and Sant'Anna, 2021). For clarity of exposition, we proceeds as follows: we first clearly describe the difference-in-difference comparison that serves as the foundation for our empirical design, and thereafter we describe the empirical adjustment to address TWFE bias.

For some outcome, Y_{icb}, observed for individual i in county c, born in year b, $\hat{\beta}$ is given by a weighted average of the following expression across county pairs (C, C') and birth-year pairs (B, B'):

$$\Big(E[Y_{icb} \mid C, B] - E[Y_{icb} \mid C, B']\Big) - \Big(E[Y_{icb} \mid C', B] - E[Y_{icb} \mid C', B']\Big)$$

The critical identifying variation for the treatment effect is provided by comparisons of the following form:

$$\underbrace{\Big(E[Y_{icb} \mid FS(C) - B \geq 18] - E[Y_{icb} \mid FS(C) - B' < 18]\Big)}_{\text{differences due to treatment effect and age}} - \underbrace{\Big(E[Y_{icb} \mid FS(C') - B < 18] - E[Y_{icb} \mid FS(C') - B' < 18]\Big)}_{\text{differences due to age only}} \tag{2}$$

$FS(C)$ denotes the year that a given county first implemented Food Stamps, and thus $FS(C) - B \geq 18$ denotes a treated individual in county C (eligible to vote at the time of FS rollout). The first line represents a comparison within county between treated and untreated individuals. Any baseline effect of the county itself is differenced out, leaving the impact of FS rollout *and* differences driven by age. The second line represents a comparison between individuals of the same ages in different counties where rollout timing is such that neither group would be treated. Again, the baseline effect of this second county is differenced out. The net result is the average treatment effect (on the treated) for this particular pair of counties and individuals of two different ages.

Here is a concrete example. Clayton County, Georgia implemented FS in 1970, and Collier County, Florida did so in 1965. In 2020 we observe the lifetime voting pattern of two individuals from Clayton County: one 70 years old (and therefore born in 1950) and one 50 years old (born in 1970). In Clayton, the first individual is treated: they were 20 and eligible to vote when FS rolled out; and the second individual, unable yet to walk, much less vote, is untreated. These two individuals are compared to two people of the same ages in Collier County. The 70-year old in Collier was not eligible to vote when FS rolled out, and thus is untreated; so is the 50-year old who was born after FS implementation. Our estimator first compares voting outcomes between the individuals in Clayton. As per equation 1, voting patterns unique to residents of Clayton County fall out, and any differences left are driven by FS treatment and voting differences that arise between 50- and 70-year olds. That same comparison between the two untreated individuals in Collier results in only voting differences between 50- and 70-year olds. The net effect is FS treatment within this sample of four individuals.[5]

[5] $\hat{\beta}$ is identified directly from county- and year-pair combinations for which the data contains observations corresponding to each term in expression 2. Some year-pairs preclude the existence of treated individuals: no one born, for instance, in 1990 can be treated by definition. Such observations in the data affect our estimates only by helping to pin down the county fixed effect.

Now we return to the issue of TWFE bias. Full-sample estimates from the regression specification of equation 1 would use all possible comparisons across every combination of county pair (C, C') and birth-year pair (B, B'). However, in this setting we also face the problem of "bad comparisons" identified by the literature on TWFE bias. In the canonical description, bias arises from time-varying treatment heterogeneity or effects that intensify in time-since-treatment. In our setting, birth-cohort replaces time as one of the two DID margins. Therefore, if treatment is heterogeneous with respect to age at treatment then TWFE estimates may be biased. The problematic set of comparisons is easy to discern by reference to equation 2. As written, this equation focuses on one treated individual (left term, first line), and three untreated individuals. The problematic comparisons would be ones that flip all inequalities and focus on three treated individuals and one untreated individual. This use of treated individuals as controls for between-age differences in voting patterns is what we need to avoid in our setting.

The literature on TWFE bias has proposed a number of bias-robust estimators. For our long-run results, we adopt the "stacking" approach of Cengiz et al. (2019). The primary consideration behind this choice is the ability to estimate interacted treatment effects. Much of our focus is on racial heterogeneity with respect to FS treatment, which we will estimate by interacting race with the FS indicator. Of the current set of bias-robust estimators, the stacking approach extends most directly and transparently to interacted effects. We form a stacked data set as follows. A single stack is characterized by a FS-rollout year t. We take all voters from counties that roll out FS at t and pair these observations with *only untreated* voters from a set of control counties that do not implement FS until at least $t + 5$. We repeat this over all FS rollout years until 1970, so that our final dataset includes all possible stacks. The birth-cohort fixed effect in equation 1 becomes a birth-cohort-by-stack fixed effect.

It is worth highlighting a nuance of the fixed effects. Because our long-run regressions use lifetime outcomes observed in a single year, the birth-year fixed effect, $\gamma_{i(b)}$, is actually doing two things. First, perhaps self-evidently, this ensures that identifying variation comes only from people belonging to the same birth-cohort. Therefore, our estimates are not driven by comparing people who have been exposed to a different set of historical events throughout their lifetime. The second role arises specifically from the data structure. A single snapshot of outcomes in 2020 means that we don't observe a birth-cohort at multiple points in time, and in turn this means that a birth-year fixed effect also defines age. Therefore, in line with the logic of the prior illustrative example, $\gamma_{i(b)}$ also ensures that our estimates are not driven by comparing people of different ages.

The data structure has another important nuance. We observe individuals – along with their within-state voting history – conditional on being registered to vote in 2020. It is not possible to track individual identifiers across state lines.[6] Therefore, our empirical design implicitly assumes that county-of-residence in 2020 matches the county in which an individual was first exposed to the FS program. People who have moved away from the county they lived in during the FS rollout years (or as children) will thus be mis-classified, generating measurement error. Migration that is not correlated with treatment will lead to an attenuation bias.[7] As treatment results from the interaction of plausibly random timing in county-level FS program implementation and individuals' birth cohorts, it is hard to devise a mechanism that would systematically relate treatment and migration to generate correlated measurement error.

3.1 Results: Long-run effects of FS rollout on Voting Patterns

Table 1 Panel A examines the long run effects of the FS rollout on voters' registration as Republican, Democrat, or Independent. Individuals who were adults when FS were rolled out in their county are 1.0 percentage points (pp) *more* likely to be registered as Republicans, 1.1pp more likely to be registered as Independent, and 2.2pp *less* likely to be registered as Democrats in 2020, around 50 years after the program was implemented in the U.S. This means that the party most associated with the FS program experienced – in terms of political affiliation – a long run political backlash among those of voting age at the time of rollout.

Given the salience of race in the politics of the implementation period, our next step is to disaggregate these effects by racial groups.[8] Panel B of Table 1 adds race fixed effects and their interactions with the FS variable to equation 1, with whites as the omitted group. The long run effect of treatment on Whites, captured by the coefficient on FS, is that they become 2.1pp more likely to be registered as Republicans (relative to an unconditional outcome mean of 27 percent), and 1.9pp less likely to be registered as Democrats, with no change in the Independent share. In contrast to Whites, the long run effect of treatment on Black and Hispanic voters (relative to Whites) is a shift leftwards: they become less likely to register as Republicans by 11 and 6.0pp respectively. Instead, they are relatively more likely to register as Independents (8.4 and 3.6pp) and

[6] Our main specifications use the October 2020 vintage of the L2 data. We have access to earlier vintages of the L2 data extending back to 2014, however this still does not permit us to create a cross-state panel.

[7] This attenuation bias may be significant, as a Pew Research Survey on internal mobility found that only 57% of U.S. adults had always lived in the same state and 37% had never left their home towns (Pew Research, 2008).

[8] Black Americans were the largest racial minority in the US during the 1960s and 1970s; in the 1970 Census they made up around 11% of the population, with Hispanics making up less than 5% and Asians under 1%.

Democrats (2.7 and 2.4pp). Asians respond differently: they move away both from the Democratic party (a 5.0pp lower relative rate of registration), and the Republican party (1.3pp lower) and their rate of registration as Independents is correspondingly higher. These results show a clear racial difference in long run response. The party most associated with passing the FS program (the Democratic party) experienced a strong political backlash from White voters, but also realized increased and highly persistent support from non-White voters.

Whether the changes in partisan affiliation that we identify in voter registrations have electoral impact depends on the rate at which individuals in each group choose to vote. We use data on individuals' voting history to generate a variable called *Voted %*, capturing the share of elections that an individual has voted in since registering or since the mid-1990s, whichever is earliest; the mean share is 36 percent. Panel A of Table 2 shows that exposure to the FS rollout as an adult reduced overall voting likelihood by 1.1 pp (column 1). As before, these changes in voting likelihood differ meaningfully across parties. To explore this, we interact *Voted %* with an indicator for an individual's party of registration, generating an outcome variable that captures the likely voting impact of the FS rollout by party. For example, *Voted %*$\times$*Republican* measures the long run effects of treatment on voting likelihood by those registered as Republicans – a combined registration and voting effect which we refer to as *electoral impact*. Columns 2 to 4 show that registered Republicans are 1.9pp more likely to vote, treated Independents are 0.75pp more likely to vote, but Democrats are 3.8pp *less* likely to vote. Thus, the FS rollout appears to have benefited the Republican party in terms of long run electoral impact, at the cost of Democrats.[9]

Panel B of Table 2 unpacks this result by race. Column 1 shows that exposure to treatment leaves White voting propensity unchanged on average, while the relative rate for Black voters falls by nearly 1pp. In contrast, the Hispanic and Asian relative voting rate falls substantially, by 6.2 and 9.8pp. Moving to the partisan impact, treated Whites are 3.8pp more likely to vote if they are Republicans and 4.9pp less likely to vote if they are Democrats, suggesting that the overall positive electoral impact on Republicans' voting rate (and negative effect on that of Democrats') in Panel A was driven by White voters. Black individuals display larger responses with an opposite pattern: they are 12pp less likely to vote (relative to Whites) if they are registered Republicans, and equivalently 12pp more likely if they are Democrats. In contrast to the clear rightward and leftward shifts observed for White and Black votes respectively, Hispanic and Asian voters display a pattern

[9]This statement embeds the mild assumption that the marginal response to FS treatment does not make cross-party ballot casting disproportionately likely.

that is more consistent with political disaffection given the drops in overall voting likelihood in column 1. Specifically, the relative treatment effect for Hispanics is a large shift away from voting as registered Republicans (negative 9pp) with only a partially offsetting increase in voting for Democrats (2.7pp), such that the reduction in Hispanic voting in column 1 was made up of lost votes from registered Republicans. Similarly, for Asians the treatment effect is strongly negative for both Republicans and Democrats, with only a small corresponding increase in voting by registered Independents.

As the FS program rollout neared completion nationwide the overall political discourse about the program – which had initially focused on race alone (Valentino and Sears, 2005, Gilens, 1995, 1996) – underwent another evolution. In 1974, the Chicago Tribune ran an article about welfare fraud, describing Linda Taylor as a "Welfare Queen" living a lavish lifestyle through unwarranted exploitation of FS support. As scholars have documented, this rhetoric about the FS program – explicitly focused on women and implicitly referencing Black women – rapidly became a centerpiece of national politics (Hancock, 2004, Nadasen, 2007). Therefore, it is especially interesting to explore the intersectional dynamics of gender along with race in the long run effects of the FS program.

We find that female voters react differently to FS rollout. Comparing the top panels of Tables 1 and 3 we see that the full sample increase in Republican registrations is stronger for men. By contrast, the increase in Independent registrations is driven chiefly by women; and while both genders move away from Democratic registration, the effect is roughly twice as large for women (Table 3, columns 2 and 3). Further, column 4 shows that the full sample reduction in the voting rate (Table 2 panel A column 1) is driven entirely by women, who have a 2.1pp lower voted % than men in response to treatment. This male-female difference is also present by political parties. The overall increase in voting likelihood by registered Republicans and Independents is largely, but not solely, due to men (see Table 3, columns 5 and 7). The reduction in voting by registered Democrats is similar across genders. In short, FS rollout appears to have pushed women away from the Democratic party and voting, and towards registering as Independents. By contrast, treatment increases male Republican affiliation and voting by Republicans. Perhaps surprisingly in the light of these sex-based differences in response to treatment, when we disaggregate results by race in a female-only subsample in Panel B of 3 we find very similar results to those in the full sample (Tables 1 and 2), indicating that the intersection of race and gender does not drive any additional effects of FS exposure.

Because so much of the public rhetoric about the FS program invoked race, we explore we explore

whether long run effects of adult exposure to the FS rollout are different in areas with particularly concentrated Black populations. Panel A of Table 4 shows the results of intersecting our treatment indicator with the county-level share of Black individuals. Several patterns are evident. First, on the margin of party affiliation, the movement of White voters towards the Republican Party and away from the Democratic Party increases substantially with Black population: a 20pp shift in Black share induces as much additional registration for Republicans as the baseline treatment effect. A similar effect in the opposite direction holds for Black registration. As Black share increases, Black voters exposed to FS are much more likely to move towards the Democratic Party and away from the Independent and Republican parties (though for the latter, this effect is not statistically significant).

Looking at the margin of turnout, we see the same dynamic. Baseline impacts on both White and Black voters are similar to our core results, and magnitudes increase in the same direction with Black population share. Across both registration and turnout, these incremental effects of regional racial demographics are consistent both with stronger backlash by White voters in areas with more Black potential beneficiaries, as well as greater support for the party associated with FS by Black voters in areas where a larger share of Black residents makes it more likely for an individual to have some social connection with someone who has benefited from FS directly.

We also examine the long run effects of FS in the areas most likely to benefit directly: high poverty counties. Panel B of Table 4 interacts FS treatment with the share of families living under the poverty line. For White voters, poverty increases the likelihood of registration as a Republican (however without statistical significance) and significantly decreases likelihood of registering as a Democrat. For Black and Asian voters, regional poverty sharply increases likelihood of registering as a Democrat, and most of the marginal shift appears to be from Independent registration rather than from the Republican party. Hispanic voters evince a different pattern: Independent registration appears to increase while Republican registration decreases (along with Democratic registration to a lesser, and insignificant extent). For White voters, turnout propensity for Republicans sharply increases with regional poverty, and decreases for Democrats. For Black and Hispanic voters, the opposite. And regional poverty appears to be quite meaningful in increasing Asian turnout for Democrats, but less so for Republicans. These results again are consistent with the core dynamics that we document being magnified in regions where individuals are: (i) more likely to observe others receiving FS aid, (ii) have some basis for believing that larger numbers of individuals are receiving aid, or (iii) have deliberately been led to believe this by the shape and content of political rhetoric.

Table 5 explores the long run effects of treatment on individuals who registered to vote during their *impressionable years* – i.e., before the age of 25 – following a literature in political psychology which holds that individuals' values and attitudes are heavily influenced by the political and social environment experienced during this life stage (e.g., Mannheim, 1952). In addition, this subsample is likely to be more politically engaged, on average, than people who register later in life. Comparing the response of this group in Panel A to the overall sample (i.e., Panel A in Tables 1 and 2) reveals substantially larger effects for early registrants. While the patterns are the same, the estimated effect sizes for partisan registration are 5 times larger for Republicans, and nearly 3 times larger for Democrats.[10] The effect on turnout propensity by party is also larger, though by a smaller multiplier.

Disaggregating the response of individuals registering during their impressionable years along racial lines, Panel B shows that the overall pattern in Panel A is driven by White voters, in line with their population majority. However, Black voters in this sample show greater sensitivity to treatment in largely the same directions as in the full sample: they are 22pp less likely to register as Republicans (relative to Whites and vs. 11pp in the overall sample). The increased likelihood of Independent registration is also doubled. Additionally, in contrast to the main sample, treated Black individuals in this subsample are relatively more likely to register as Democrats by 8pp, vs. 2.7pp Table 1. In addition, congruent with early registrants being more politically engaged, all three non-White groups (but not Whites) generally show much larger treatment effects on turnout propensity vs. those for the full sample in Table 2. The net effect on turnout is somewhat smaller than in Table 1 and no longer points as uniformly towards political disengagement. As before, the net effect on turnout obscures strong directional shifts by party. These shifts are substantially larger in columns 5 and 6 of Table 5 for White, Black, and Hispanic voters alike. Asian voters are the exception: magnitudes are similar or a bit smaller. Also, in the full sample Asian voters registered as either Republicans or Democrats were less likely to vote, but in this impressionable years subsample, Asian Democrats are more likely to vote by 5.5pp.

To evaluate the robustness of these results we add a variety of interacted fixed effects to absorb possible confounders along multiple margins. Recall that the county and birth year fixed effects (FE) in our baseline specification absorb persistent differences associated with geography and age

[10]The general increase in magnitudes evident in Table 5 may also arise from reduced measurement error. Because L2 data is siloed within state, the fact of observing a registration history that extends back to age 25 (or younger) means that an individual has lived in the same state since that young age. Therefore, it is very likely that overall migration is lower in this subsample of individuals, meaning in turn that we are less likely to have classification errors with respect to county-of-residence at age 18.

cohorts. However, these differences may themselves vary within birth cohorts across counties (and vice versa), so as our first robustness test we replace county and birth year FE with county × birth year FE and report the results in Appendix Table IA1. Because our treatment is itself at the county × birth year level, this vector of new FE absorbs the treatment variable (*Food Stamps*), but still allows us to estimate the *FS* × *Race* coefficients, which capture the differential effects of treatment for each racial group relative to treated Whites. While specifications with interacted fixed effects absorb substantially more variation than the baseline, they reduce the scope for confounders to drive our main cross-racial findings. We find an extremely similar pattern of results despite the more demanding fixed effects we employ.

As a second robustness test, Appendix Table IA2 reports results from instead including a vector of birth year × race FE, which absorb differences across birth cohorts by race. These can be seen as race-specific "generation" effects, analogous to Boomers vs. Gen X, but with generations defined at the yearly level. We find similar directional results for both registration and turnout across races. Magnitudes, especially on registration by race, are meaningfully larger: treated Black individuals, for instance, are 35pp less likely to register as Republicans and 43pp more likely to register as Democrats. Finally, as a third robustness test, we replace the County and race FE with county × race FE in order to absorb county-specific differences by race. The results are reported in Appendix Table IA3: again, the direction of treatment effect is consistent with our core findings, and magnitudes increase somewhat relative to Table 1 and 2. In both these instances, the increased treatment effects that we find when allowing for race-specific controls by age or by region suggests that our core specification may downplay the effect of FS exposure by assuming that differences in lived experience by race in the US are separable from differences generated by location and age. That is: the robustness tests of Appendix Tables IA2 and IA3 suggest intersectional effects of race with both age-cohort and geography that are somewhat occluded by our core specification.

In summary, the long run effects of adult exposure to the FS rollout on partisanship and voting diverge along clear party and racial lines. The Republican party appears to be a net beneficiary of the FS program, both in terms of registrations and in the voting rate of its partisans. These Republican gains come at the cost of the Democratic party, and the overall patterns are driven by White voters moving towards the Republican party – a political backlash effect. In contrast, non-White voters largely move away from registering as Republicans, and from voting, consistent with political disengagement effects for them. Thus, a first order impact of the FS program is to drive racialized partisan polarization. We also find that women respond differently to treatment, in

that it appears to push them away from the Democratic party and voting, and towards registering as Independents. In contrast to this, treatment increases voting rates for men and pushes them towards Republican affiliation.

3.2 Experience DID with Fuzzy Treatment

All prior analyses assume a sharp delineation in treatment at FS onset around the age of 18. This is a natural distinction: we compare those who had the opportunity to register partisan affiliation and to cast ballots without the presence of any Food Stamps program in their region, with those who were initially enfranchised in a world where Food Stamps was an established feature of the (local) political landscape. As the prior results show, this treatment effect is large and extremely persistent.

In this section, we relax the assumption of a sharp treatment around age 18 in favor of a fuzzy-treatment framework that allows the data to determine the degree and extent of partial treatment around age 18. By way of a motivating example, consider the starkest comparison pair entailed by our core specification: a newly registered 18 year-old voter in the year his county implements a FS program, and a 17 year-old in that same county. Treatment effect in the DiD design is fundamentally identified by comparing lifetime voting patterns between these two individuals (together with the second difference involving two other individuals in another county). But the assumption of zero treatment for the 17 year-old may be too strong. Consider that 17 year-old a year later, when she registers to vote. A perfectly discontinuous treatment would imply that her political opinions – along with subsequent registration and voting patterns – take the existence of Food Stamps program as a given feature of the world, as if she had been politically oblivious until becoming eligible to vote. While such a sharp jump in political awareness upon attaining the age of majority is not wholly unreasonable – and indeed is more reasonable an assumption at 18 than at any other age – a less stylized model would allow individuals to become gradually more politically attuned and engaged as they approach the age of 18.

To accomplish this, we employ a joint-estimation framework that allows the data to determine the extent of treatment prior to age 18. We classify those of voting age at the time of rollout as fully treated, but let newer cohorts be partially treated as a function of the difference between their year of birth and the year of FS rollout in their county, $FS(c) - b(i)$, which we define as FS_{age} for notational simplicity (note that this is an individual-level variable). Treatment is then defined as follows:

$$FS_{ic} = \begin{cases} 1, & if \quad FS_{age} \geq 18 \\ \left(\frac{FS_{age}-(18-L)}{L}\right)^{\lambda}, & if \quad 18-L \leq FS_{age} < 18 \\ 0, & if \quad FS_{age} < 18-L \end{cases} \tag{3}$$

As before, treatment is 1 for those who have attained the age of majority. L is a parameter that governs how far down the age distribution FS impact extends. Anyone more than L years below the age of 18 is entirely untreated. For those in the range of $[18-L, 18)$, treatment takes a continuous value. λ, constrained to be non-negative, is a curvature parameter which characterizes the intensity of treatment for each year of partial treatment. As λ approaches zero, treatment approaches 1 for anyone in the range of $[18-L, 18)$; and as λ increases, treatment loads more heavily on those closer to $FS_{age} = 18$. The case of $\lambda = \infty$ corresponds to our baseline binary treatment specification of Equation (1).

We simultaneously estimate L and λ from the data. In particular, we choose L and λ to minimize the joint sum of squared residuals of individuals' political affiliation. That is, L and λ are chosen to minimize the joint sum of squared residuals of our main specification (used in Table 1), where registering as Republican, Democrat, or independent are the dependent variables. Note that L can take values greater than 18, which would allow the FS program to have had an impact on people who had not yet been born at rollout. We view this as capturing how political attitudes can be shaped by individuals' understanding of historical policy events occurring before their birth, such as the Civil Rights movement or the rollout of the FS program. The estimated L and λ are 38 and 0.56, respectively, which suggests that FS rollout does have a relatively long-tail of treatment affecting younger birth cohorts. This further suggests that the long-run treatment heterogeneities explored in Tables 3–5, as well as the intersectional mechanisms explored in Section 5, might be conservative, lower-bound estimates.

Table IA5 shows the results using this fuzzy-treatment framework. When compared against the estimates using a sharp treatment in Panel A to the overall sample (i.e., Panel A in Tables 1 and 2), we find that fuzzy treatment generates effects that are qualitatively similar to our previous set of estimates. In general, effects on party affiliation are larger, and racial heterogeneities in turnout propensity are slightly smaller. The treatment effect for White voters is larger than our estimates using sharp treatment, consistent with sharp treatment estimates conservatively capturing the effect of FS rollout. However, this difference is not large relative to other analyses – in particular, using

fuzzy treatment provides smaller estimates than our estimates for voters registering when young and likely more politically engaged (Table 5). Moreover, the relative effect on minorities is largely the same when estimated using either sharp or fuzzy treatment. Thus, this fuzzy-treatment framework yields generally similar estimates to the sharp-treatment framework, with sharp treatment providing conservative estimates for the effect of FS rollout on Whites' voting patterns, consistent with some degree of partial treatment for those under 18.

4. Short run effects

As we note at the beginning of Section 3, our empirical design captures an experience effect between two groups: we measure the *difference* in voting patterns between those exposed to FS as adults and those who come of age in a world where the FS program is in place. The nature of the DiD estimator is to identify the wedge between these two groups without taking a stand on which group shifts. In settings where time is one dimension of the DID, we naturally view the post-treatment group as the one that changes. However, in our setting, while it seems most natural to discuss adult voters being exposed to FS, it is equally accurate to consider the younger cohort as one that is subjected to a "treatment" of attaining majority in a world where FS is well-established.

To illustrate: one core finding is that adult exposure to FS induces a shift away from the Democratic party among voters on average. There are two alternative ways to interpret this (although a combination of the two is also possible). The first is as a movement by those 18+ away from Democratic registration and Democratic turnout. The second would be a shift away from the Democratic Party by those who are *not* exposed to FS as adults—those for whom FS is an established feature of the social landscape by the time they turn 18. By 2020, every individual falls into one of these two groups, and so for long-run outcomes the net impact of FS exposure cannot be apportioned between the two groups of voters.

However, we can exploit the period of active rollout between 1961 and 1975 to test directly for response to the most intuitive notion of treatment: shifts in behavior by the group of voting-age adults pursuant to rollout. This analysis is a standard staggered-rollout DID setting. We compare county-level outcomes between treated adults and untreated adults; the ability to observe outcomes at different times will allow us to absorb both national political shocks and persistent cross-county differences. Our core specification is:

$$y_{ct} = \alpha_c + \gamma_t + FS_{ct} + \epsilon_{ct} \tag{4}$$

where y_{ct} is an outcome of interest, c is a county fixed effect, t is a year fixed effect, and FS_{ct} takes a value of 1 once a county has adopted the FS program. While we focus on differences by race, two of our analyses parallel findings in Kogan (2021); we highlight these specifically below.

This DID design is also subject to the TWFE bias discussed in Section 3. Throughout this section we use a bias-robust estimator following Callaway and Sant'Anna (2021) (CS henceforth). Like the stacking estimator, this approach compares each cohort of treated counties (in which the FS program has been implemented) to all not-yet-treated counties, and compares these groups in each time period relative to the preceding period only. Thus, the CS estimator only requires the assumption of post-treatment parallel trends. Because all US counties are eventually covered by the FS program, using not-yet treated counties as the control group means that we cannot estimate treatment effects for the year in which the last cohort of counties were treated, or for any later periods, as no control counties remain.

We begin by examining whether the county-level Food Stamp (FS) program rollout affected voter registration. Table 7 reports CS difference in differences estimates of the effect of treatment on registration rates for Black and White individuals for 11 Southern states, using data from the NAACP Voter Education Project spanning 1960 through 1972. Column 1 shows that Black voter registration as a share of the county population rose by around 1 percentage point (pp), while White registration fell by 1pp. Columns 3 and 4 instead scale registration by the eligible voter population. The estimate for Black registration is essentially unchanged, but for whites the negative effect on registration rates falls to half the size and loses statistical significance. Thus, Table 7 indicates that the FS program rollout increased Black registration in southern states, but did not do so for Whites.

We next consider turnout in Presidential elections. Appendix Figure IA1 plots CS event study estimates for presidential turnout around FS implementation. Prior to the food stamp rollout there is essentially no difference between treated and control counties. Following rollout we see a growing decline in turnout, reaching around 7pp by the second election post-rollout, consistent with political disaffection in treated counties.[11,12]

Given that the Food Stamps policy was associated with the Democratic party (Kogan, 2021) we then explore the electoral consequences of the FS rollout. Some of this analysis is very similar

[11]This analysis parallels Table 5 of Kogan (2021), which finds increases in turnout rather than decreases. The difference arises from use of a bias-robust TWFE estimator. With a standard (and biased) TWFE regression, we find increases in turnout as well.

[12]Panel (b) of Appendix Figure IA1 reproduces the Figure excluding the latest adopters to test whether results are sensitive to the final control cohort.

to Figure 4 and Table 3 of Kogan (2021). In panel (a) of Figure 1, we plot event study estimates for the Democratic party's vote share in elections for U.S. Congress from 1948 through 1972. The Figure shows largely flat pre-trends, followed by a sharp rise of 5pp in the Democratic vote share in the first election following treatment. The effect appears persistent and stable in magnitude out to four elections post-treatment, the estimation limit. Panel (b) presents estimates for the *difference* between the Democratic and Republican vote shares in these congressional elections. Despite some fluctuation, overall pre-trends are largely flat. However, in the second election following FS rollout there is a sharp increase in the difference between Democratic and Republican vote shares to around 10pp, an advantage which persists in the two subsequent elections. Panel A of Table 8 estimates the average DID effect over the post period, also following Callaway and Sant'Anna (2021). Like Kogan (2021), we find that FS is associated with increased Democratic vote shares. Column 1 confirms the event study result, displaying an average increase in the Democratic minus Republican vote share of around 7pp, although this difference is slightly smaller when the elections are split into General and Midterm elections in columns 2 and 3. In light of the increased rate of Black voter registration in southern states documented in Table 7, columns 4 and 5 examine subsamples composed of the top and bottom quartiles of counties by share of Black population. While the estimate for the bottom quartile counties is similar to the baseline, the top quartile counties' display almost double the average effect. Despite this, the Democratic party does not appear to receive electoral benefits from the FS rollout in counties likely to benefit the most from the program – high poverty counties (columns 6 and 7) – perhaps because political participation tends to be lower among the poor (Schaub 2021), or potentially because the marginal voter in high-poverty counties is less likely to be moved towards the Republican party.

While the FS program appears to have increased Democrats' vote share relative to Republicans following implementation, this need not imply a larger Democratic Congressional delegation. For example, the vote share increase could be concentrated in already safe seats, or there could be partially offsetting changes in vote distribution across counties. Panel B of Table 8 confirms that treatment did not, in fact, affect the average likelihood of Democratic success. Column 2 shows a somewhat noisy negative average effect for general elections. High Black-population counties show a 7pp increase in Democratic win likelihood, which suggests that the increased Democratic votes were not all concentrated in already-safe seats. Appendix Figure IA2 corroborates this, displaying event study estimates that show a sustained increase Democratic win probability in counties with a high Black share. However, counties with a low Black share and high poverty counties show a 9

to 11pp lower Democratic win probability, offsetting the electoral advantage gained in high black share counties. This reduction in the likelihood of success in low-Black share counties, despite overall increases in Democratic vote share (Panel A) is consistent with political backlash from the majority block of White voters in these areas. Likewise, the sharp reduction in Democratic success within high-poverty areas (Column 6), in conjunction with no increase in Democratic vote share, would be consistent with poltical backlash in other, wealthier regions within a given congressional district.

Although the FS rollout did not lead to larger Democratic Congressional delegations on average, it could have had effects on representation at the local level. Indeed, given the strong effects of the FS rollout on Black registration and on voting in counties with a large Black population share, it is possible that the program contributed to electing more Black officials. In a final event study we examine this hypothesis. Figure 2 shows estimates for the share of Black elected officials between 1960 and 1975. We do see a gradual increase in the percentage of Black elected officials of about 0.7pp, starting three years after FS implementation.

4.1 Food Stamps and Congressional voting behavior

By changing registration and voting behavior, the development of a social safety net via the Food Stamp program could also have changed the types of candidates that were elected. In this section we explore whether the political ideology of U.S. House members, as measured by the DW-NOMINATE project (Poole and Rosenthal, 2000), changed following the rollout of the FS program. Table 9 investigates the impact of food stamps on the first DW-NOMINATE dimension, typically interpreted as a representative's position on a liberal to conservative axis. Column 1 shows that there is no average effect of treatment, while columns 2 and 3 show this is also true for subsamples of exclusively Democratic and Republican representatives, respectively. Columns 4 and 5 narrow the focus to counties in the top quartile by share of Black population, which we have shown move towards the Democratic party and show that both Democratic and Republican representatives shift rightwards following FS rollout. This rightwards move was despite the increased vote share and winning likelihood of Democrats in these counties, and may be a response to political backlash. Columns 6 and 7 perform the same analysis for high poverty counties and show the same pattern of more conservative voting following FS rollout from both Democratic and Republican representatives.

In total, this short-run analysis shows clear evidence of aggregate political shifts among voters

exposed to FS, compared to voters in counties which have not yet been exposed. This strongly reinforces the natural interpretation of our long-run findings: adult exposure to FS does indeed directly shift the behavior of those who personally experience this change in the national policy landscape. It remains possible that the dual group (those who grow up taking FS as a fixed point within the national safety net) also shifts its behavior, but there is convincing evidence that it is not *only* this younger group changing behavior.

5. Mechanisms

This section considers a variety of possible channels for the long run effect of the FS program by examining heterogeneous responses along several dimensions that plausibly mediate the effects of interest. We first explore how a major historical event occurring during the rollout – the Voting Rights Act of 1965 – interacted with the results we find. Next, we look at how the economic health of the county mediates the effects of Food Stamps. Finally, we consider how the presence of local churches is associated with the long run consequences of the FS program for political behavior.

5.1 The Voting Rights Act of 1965 and long run effects

The passage of the 1965 Voting Rights Act (VRA), which banned voting discrimination against racial minorities in the U.S., increased the size of the Black electorate almost overnight. It also improved the provision of public goods (Cascio and Washington, 2014) and increased labor income (Aneja and Avenancio-León, 2022) for minorities. But the VRA not only mobilized minority voters, it also increased the mobilization of White voters (Bernini et al., 2023). In other words, the passage of the VRA generated short-term political polarization that may have mediated the dynamics we document. In this subsection, we evaluate whether civil rights era legislation, and the VRA in particular, mediated the effects of FS rollout on long run political polarization, or if instead the racial politicization of food stamps is a concurrent phenomenon. This is a natural inquiry given that the weaponization of food security in response to Black political mobilization finds support in the historical record (Zinn, 1964).

To explore how increased political enfranchisement interacted with the long run effects of the FS program, we compare counties covered by Section 5 of the VRA with adjacent non-covered counties (both within and across state borders), following Aneja and Avenancio-León (2022).[13] To

[13] Covered counties include all counties in Alabama, Arizona, Arkansas, Georgia, Louisiana, Mississippi, Oklahoma, South Carolina, Tennessee, Texas, Virginia, West Virginia and select counties in North Carolina and Florida.

do so, we add an indicator for VRA Section 5 coverage (VRA in the tables), and interact it with race and FS indicators.

The results for this subsample of counties, reported in Table 10, indicate that the interaction of the FS program with VRA coverage ($VRA \times FS$) contributed to the shift in White registrations rightwards in response to treatment, with Republican registrations rising and Democratic registrations falling (columns 1 and 2). In terms of voting behavior, the VRA's interaction with FS contributed to reduced voting by White Independents, with some suggestive evidence of reduced voting by White Democrats (but no statistical significance). Thus, in this predominantly southern subsample, the VRA appears to shift Whites rightwards in their response to the FS program. For non-Whites we see relatively similar patterns to those for Whites for the joint effects of the VRA and FS rollout, as few of the $VRA \times FS \times Race$ coefficients are statistically different from zero. The main difference is a higher rate of registration as Independents for both Blacks and Hispanics (relative to Whites), along with greater voting by Black Independents. The positive effect for Blacks on Independent registration and voting (rather than this support flowing to Democrats) may reflect the Southern Democratic party's anti-civil rights position around the time of the FS rollout program.[14]

Taken together, the evidence in Table 10 suggests that, while the VRA had some effect on the long run political response to the FS program it did not have a first order mediating effect; instead, the racial politicization of food stamps is a concurrent phenomenon.

5.2 Local recessions and long run effects

There are many reasons to expect that recessions impact individuals' view of the FS program. There is a growing literature on how the experience of recession may induce persistent economic pessimism (e.g., Friedman and Schwartz, 1963, Cogley and Sargent, 2008, Malmendier and Nagel, 2011), which in turn may lead to support for a welfare state (e.g., Ravallion and Lokshin, 2000). Relatedly, recessions may change beliefs about the relative importance of luck vs. effort, inducing greater support for a safety net (Piketty, 1995). Recessions also increase zero-sum thinking, which is associated with greater support for redistribution towards society's poorest; moreover, this mindset may persist at the community level (Chinoy et al., 2023). A simpler mechanism may also be at work: areas with greater experience of recession have a greater share of FS recipients (or voters

[14] 20 of the 21 southern Democratic senators voted against the VRA; these senators were from the 11 states making up the Confederate States of America in the Civil War.

who know them) and this direct exposure to the program may increase support for it. It is also possible that recessions *reduce* support for the FS program. If the experience of receiving FS is stigmatizing, or if fraud is perceived to be widespread, areas with greater direct FS experience may have a less favorable view. Alternatively, aid to society's poorest may be seen by voters as a normal good, so areas with a history of recessions may see the level of FS provision as excessive relative to their perception of a tighter Government budget constraint.

We examine this issue by constructing a measure of county-level recessions using annual Bureau of Economic Analysis (BEA) data, defining recessions as years in which state per capita real personal income grew at less than -1.06% (the 10th percentile of personal income growth between 1929 and 2010). Our local recession measure is the percentage of years the state is in recession between each county's FS rollout year and 2020.

Table 11 replicates the specifications in Tables 1 and 2 and adds interactions with the economic vulnerability variable (the county fixed effect absorbs the main effect). The estimates on $FS \times LocalRecession_c$ and on its interactions with race indicators suggest that recessions are an important mechanism through which the effects of the FS program transmit to political preferences and behavior. For Whites, recessions appear to shift their response to FS, pushing them away from the Republican party, and to a lesser extent towards the Democratic party. Specifically, the more that a county has experienced recessions since FS rollout, the less FS is associated with their registering as Republicans (column 1): at the mean of the recession variable (4.77%) this reduces the main effect of FS by 2pp, while a county that has been in recession around 16% of years since rollout would fully offset the main effect. Further, the more a county has experienced recession, the greater the likelihood that all voters (except Hispanics) register as Democrats (column 2). In terms of voting behavior, White Republicans in counties more exposed to recessions are associated with a strong reduction in their voting rate (column 5), which is only partially offset by increased voting by White Democrats (column 6).

Blacks are even more likely to register as Democrats than Whites (and correspondingly less likely to register as Independents) in response to FS in counties with more extensive histories of recession. However, the net effect of local recessions and FS on Black *turnout* is around zero: for both Republicans and Democrats, the coefficient on the triple interaction ($FS \times LocalRecession \times Black$ is largely offset by the baseline effect of local recessions (i.e., the coefficient on $FS \times LocalRecession_c$).

In contrast to the response of Whites and Blacks, exposure to local recessions appears to shift the Hispanic registration response to FS *rightwards*, towards Republican registrations and away

from Democratic ones. However, this result does not extend to turnout, where the net effect is still to reduce voting by Hispanic Republicans and increase voting by Hispanic Democrats, albeit by less than for Whites. Finally, the response of Asians is estimated too imprecisely for clean interpretation, although the coefficients point to a pro-Republican effect of $FS \times LocalRecession_c$ for this ethnic group, relative to Whites.

Summarizing, local recessions are associated with substantial and heterogeneous effects on the long run political consequences of the FS program. Whites and Blacks in high recession areas are less likely to be Republican and and more likely to be Democrats in response to the FS treatment, while for Hispanics the shift is towards the Republican party. Examining turnout, white voters exhibit a much larger sensitivity to FS treatment with respect to local recessions.

5.3 Church density and long run effects

The presence of a network of church communities is a potential mediating factor for the long run effects of the FS program for several reasons. First, churches have long been a focal point for voter coordination and mobilization, including during the Civil Rights Movement.[15] Second, Christian theology promotes help for the poor, which may support political views in favor of public programs like FS.[16] Third, Churches may reduce the perceived need for a FS program if they already operate a community-based safety net.

We now explore the role of churches in mediating the long run effects of the FS program by interacting the a measure of Church density, measured as number of churches per 1,000 inhabitants (ICPSR, 1952), with the FS and race variables. Table 12 presents the estimates. The first thing of note is that the coefficients on *Church Density* $\times$ *Race* support the view that churches serve a voter mobilizing function, with high church density areas displaying far lower rates of Republican registration (and voting), and the opposite pattern for Democratic and Independent political behavior. In addition, for non-Whites, the baseline effects on registrations (columns 1 to 3) in the first four rows (i.e. the coefficients on the *FS* and *FS* $\times$ *Race* variables) are similar to those in Table 1, suggesting that church density modifies the effects of FS rather than drives them. However, this is not true for Whites, for whom the baseline effects are absent; instead, the coefficients on *FS* $\times$ *Church Density* suggest that the increased rate of Republican registrations generated by the FS program is associated with higher church density. More generally, the pattern of coefficients on

[15] For an overview, see Wald and Calhoun-Brown (2018).

[16] Christian social teaching (e.g., Vatican Council II, 1965) emphasizes the obligation to help the poor, e.g., Jesus: "For I was hungry and you gave me food, I was thirsty and you gave me drink" (Matthew 25:35, 1952).

FS × *Church Density* × *Race* is consistent with church density inducing a rightward shift in voter registrations in response to Food Stamps, with the strongest effects for Hispanics.

Church density has similar effects on voting behavior. As with registrations, the baseline effects for voting by registered Republicans and Democrats are present for each non-White group, but mostly absent for Whites. In turn, this suggests that the greater voting rate of White Republicans (and lower rates for White Democrats) in response to the FS program are associated with areas with high church density. Hispanic voting behavior responds even more strongly: Republicans and Independents are more likely than Whites to vote in response to treatment in areas with high church density. In fact, Blacks are the only Republican group for which the coefficient on *FS* × *Church Density* × *Race* is not positive.

Taken together, these results are not consistent with churches championing safety-net policies among their congregants. Instead, the evidence is more consistent with churches serving to push voters rightwards in response to the FS program rollout, perhaps by reducing the perceived need for state involvement in providing aid to society's poorest.

6. Conclusion

We study the Food Stamps program rollout – a pillar of the safety net in the U.S. – to understand the political impact of a major public policy. This paper shows that exposure to the FS rollout affected political engagement and increased political polarization over both the short and the long term. The Republican party was the net beneficiary at both horizons. The fact that the rollout happened over fifty years ago allows us to explore the persistence of these effects; our results indicate that major public policies can shape the political landscape for many decades after their launch.

The political framing of federal policies through a racial lens is reflected in the racialized response to the FS rollout that we document. Our exploration of mechanisms also shows that greater experience of recessions – and thus likely greater direct experience with FS – is associated with a more left-leaning response to the FS rollout. In turn, this suggests that the overall response to the policy, a rightward shift of the electorate, may have resulted from its political framing, rather than from voter experience of the policy in action.

More generally, this paper maps out the consequences for voter behavior of *politicizing* a major public policy. Our results indicate that managing a policy's political interpretation in order to mitigate backlash may be as important as the implementation of the policy itself. In addition, politicization has the potential to reduce a policy's effectiveness, for example by generating stigma

around recipients or rejection by partisans of the opposing party. We leave this and other potential consequences of politicization of public policy for future research.

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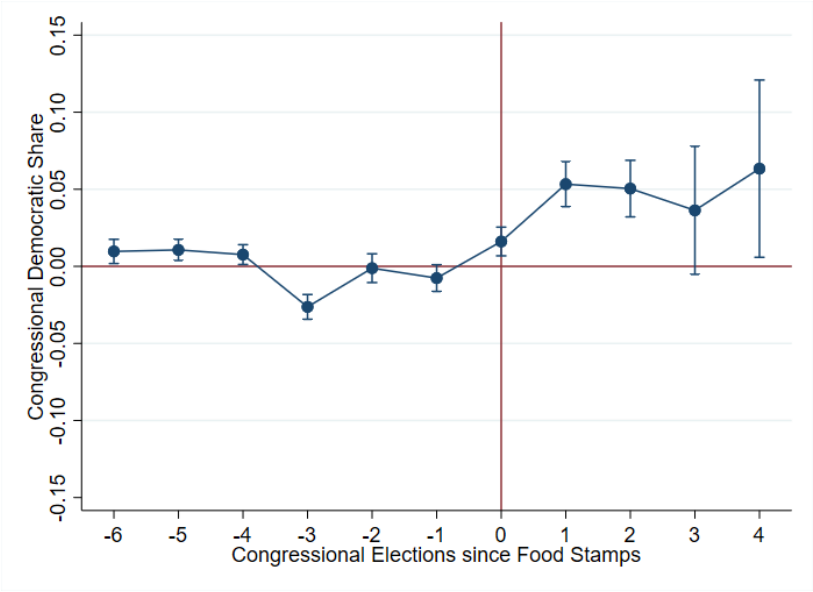
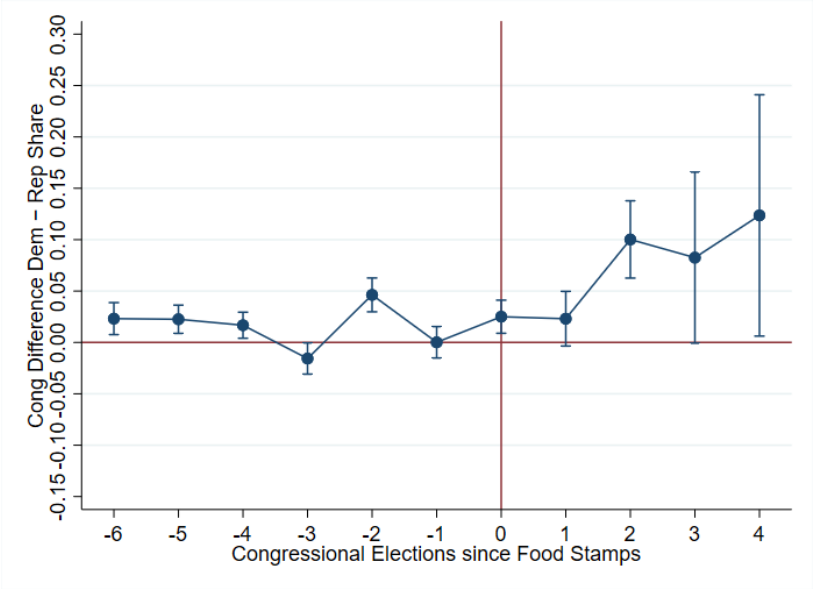
Figure 1: Event studies: elections



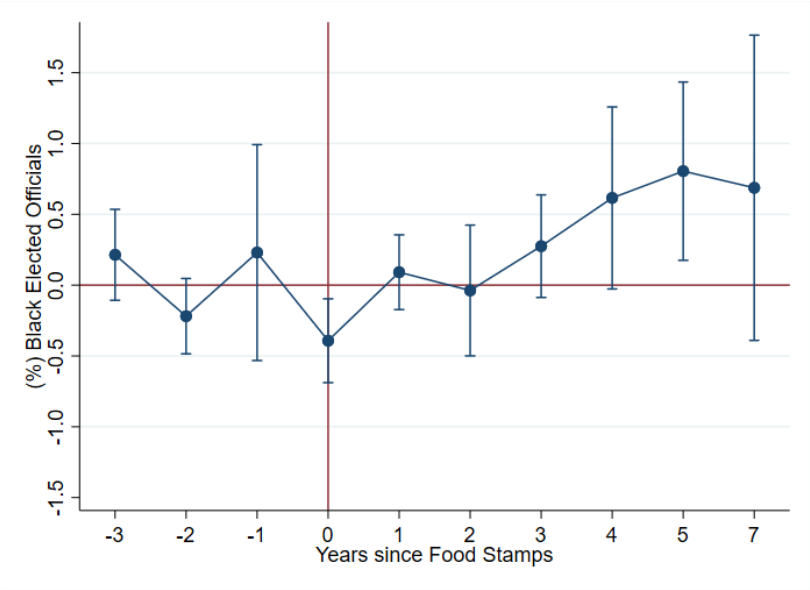
(a) Democratic vote share in elections to U.S. Congress



(b) Democratic vs. Republican vote difference in elections to U.S. Congress

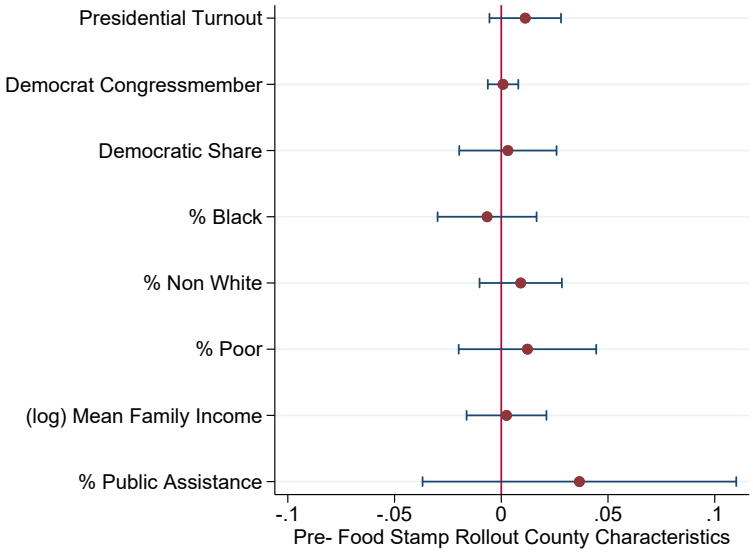
Note: This figure presents Callaway and Sant'Anna (2021) event study estimates of the effect of Food Stamp program roll-out on Democratic vote share in elections for U.S. Congress. The estimates use county×election level data from 1948 through 1972, sourced from ICPSR Electoral Data for Counties in the United States: Presidential and Congressional Races, 1840-1972, and Dave Leip's Atlas of US Presidential Elections. 95% confidence intervals are clustered by county.

Figure 2: Event study: Share of Black Elected Officials

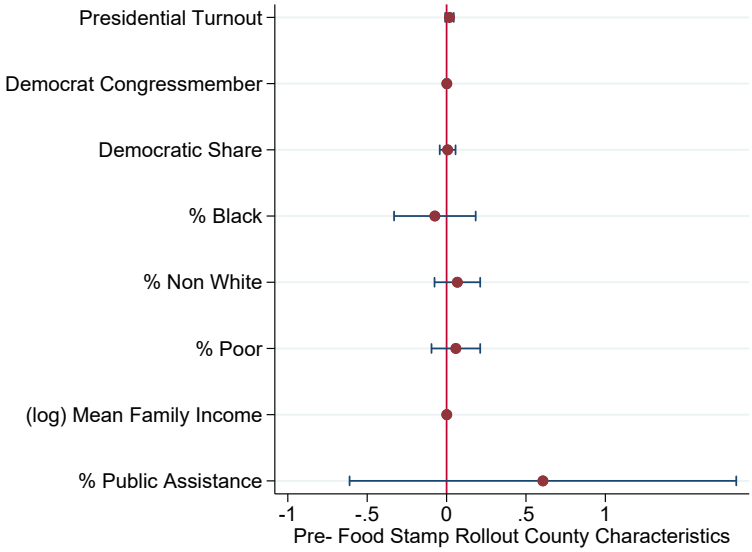


Note: This figure presents Callaway and Sant'Anna (2021) event study estimates of the effect of Food Stamp program roll-out on the share of Black elected officials (Mayors, Councillors, State and Federal Legislators, Governors). The data is at the county-year level for years 1960–1975, and is from the National Roster of Black Elected Officials, obtained through the Joint Center for Political and Economic Studies (JCPES) and supplemented with data from Alt (1984). 95% confidence intervals clustered by county.

Figure 3: Predicting rollout timing using pre-rollout county characteristics



(a) County characteristics in natural units



(b) County characteristics divided by sample average values

Note: This figure presents coefficients from regressions predicting Food Stamp rollout in a county for a given year based on a pre-determined characteristic (listed on vertical axis) and a year fixed effect. Each coefficient estimate is from a separate regression. County characteristics are measured in 1960, except for political variables which are measured as of the preceding election. Panel (a) does not change the units of the variables; panel (b) divides each variable by the sample mean. Some coefficients have such small confidence intervals that they are not visible in the Figure. 95% confidence intervals are clustered by county.

Table 1: Long run effects on voter registration

Panel A	(1) Republican	(2) Democratic	(3) Independent
Food Stamps	0.0102***	-0.0215***	0.0113***
	(0.0036)	(0.0042)	(0.0042)
N. obs.	353,311,262	353,311,262	353,311,262
N. clusters	6,483	6,483	6,483
County$_h$ FE	Y	Y	Y
Relative Age$_h$ FE	Y	Y	Y

Panel B	(1) Republican	(2) Democrat	(3) Independent
Food Stamps (FS)	0.0208***	-0.0191***	-0.0017
	(0.0042)	(0.0043)	(0.0044)
FS × Black	-0.1112***	0.0268***	0.0844***
	(0.0091)	(0.0082)	(0.0079)
FS × Hispanic	-0.0604***	0.0242***	0.0362***
	(0.0064)	(0.0090)	(0.0069)
FS × Asian	-0.0129*	-0.0494***	0.0623***
	(0.0076)	(0.0084)	(0.0059)
N. obs.	353,311,262	353,311,262	353,311,262
N. clusters	6,483	6,483	6,483
County$_h$ FE	Y	Y	Y
Relative Age$_h$ FE	Y	Y	Y
Race$_h$ FE	Y	Y	Y

Note: This table examines the effects of the Food Stamp program roll-out (*Food Stamps*) on voter registration as Republican, Democratic or Independent on the October 2020 voter rolls. *FS* is an indicator for whether the FS program rollout occurred in an individual's county when they were of voting age (18+). Panel A displays estimates of the coefficients in equation 1; Panel B adds race fixed effects and their interaction with the FS variable. White is the omitted ethnic group. Standard errors in parentheses are clustered by county. *** 1%, ** 5%, * 10% significance level.

Table 2: Long run electoral impact

Panel A	(1)	(2)	(3)	(4)
	Voted%	Voted%× Republican	Voted%× Democrat	Voted%× Independent
Food Stamps	-0.0113***	0.0190***	-0.0378***	0.0075***
	(0.0030)	(0.0035)	(0.0027)	(0.0019)
N. obs.	353,311,262	353,311,262	353,311,262	353,311,262
N. clusters	6,483	6,483	6,483	6,483
County$_h$ FE	Y	Y	Y	Y
Relative Age$_h$ FE	Y	Y	Y	Y

Panel B	(1)	(2)	(3)	(4)
	Voted%	Voted%× Republican	Voted%× Democrat	Voted%× Independent
Food Stamps (FS)	-0.0037	0.0375***	-0.0486***	0.0073***
	(0.0029)	(0.0039)	(0.0028)	(0.0021)
FS ×Black	-0.0094*	-0.1247***	0.1221***	-0.0068***
	(0.0053)	(0.0043)	(0.0072)	(0.0019)
FS ×Hispanic	-0.0619***	-0.0897***	0.0274***	0.0004
	(0.0040)	(0.0036)	(0.0046)	(0.0016)
FS ×Asian	-0.0980***	-0.0739***	-0.0318***	0.0077**
	(0.0066)	(0.0058)	(0.0041)	(0.0031)
N. obs.	353,311,262	353,311,262	353,311,262	353,311,262
N. clusters	6,483	6,483	6,483	6,483
County$_h$ FE	Y	Y	Y	Y
Relative Age$_h$ FE	Y	Y	Y	Y
Race$_h$ FE	Y	Y	Y	Y

Note: This table examines the effects of the Food Stamp program roll-out (*Food Stamps*) on individuals' voting behavior.*FS* is an indicator for whether the FS program rollout occurred in an individual's county when they were of voting age (18+). *Vote Percent* is the percentage of elections an individual has voted in since registration in a state. *Vote Pct.×Republican* interacts *Vote Percent* with an indicator for individuals registered as Republicans in 2020; the Democrat and Independent versions are similarly defined. White is the omitted ethnic group. Standard errors in parentheses are clustered by county. *** 1%, ** 5%, * 10% significance level.

Table 3: Long run effects
Women

Panel A	(1) Republican	(2) Democrat	(3) Independent	(4) Voted%	(5) Voted%× Republican	(6) Voted%× Democrat	(7) Voted%× Independent
FS × Female	-0.0031***	-0.0139***	0.0170***	-0.0209***	-0.0150***	-0.0005	-0.0053***
	(0.0011)	(0.0013)	(0.0014)	(0.0009)	(0.0008)	(0.0010)	(0.0006)
Food Stamps (FS)	0.0117***	-0.0138***	0.0021	-0.0001	0.0269***	-0.0373***	0.0103***
	(0.0037)	(0.0042)	(0.0044)	(0.0030)	(0.0037)	(0.0025)	(0.0020)
Female	-0.0444***	0.0826***	-0.0383***	0.0234***	-0.0146***	0.0424***	-0.0044***
	(0.0007)	(0.0009)	(0.0007)	(0.0005)	(0.0003)	(0.0005)	(0.0002)
N. obs.	353,311,262	353,311,262	353,311,262	353,311,262	353,311,262	353,311,262	353,311,262
N. clusters	6,483	6,483	6,483	6,483	6,483	6,483	6,483
County$_h$ FE	Y	Y	Y	Y	Y	Y	Y
Relative Age$_h$ FE	Y	Y	Y	Y	Y	Y	Y

Panel B: Women only	(1) Republican	(2) Democrat	(3) Independent	(4) Voted%	(5) Voted%× Republican	(6) Voted%× Democrat	(7) Voted%× Independent
Food Stamps (FS)	0.0233***	-0.0173***	-0.0060	-0.0030	0.0372***	-0.0466***	0.0065***
	(0.0040)	(0.0046)	(0.0045)	(0.0028)	(0.0037)	(0.0029)	(0.0020)
FS × Black	-0.1119***	0.0337***	0.0781***	-0.0082	-0.1183***	0.1163***	-0.0061***
	(0.0091)	(0.0085)	(0.0073)	(0.0053)	(0.0043)	(0.0074)	(0.0019)
FS × Hispanic	-0.0578***	0.0230**	0.0348***	-0.0622***	-0.0822***	0.0198***	0.0002
	(0.0062)	(0.0092)	(0.0068)	(0.0038)	(0.0035)	(0.0047)	(0.0016)
FS × Asian	-0.0075	-0.0578***	0.0653***	-0.0916***	-0.0626***	-0.0368***	0.0078**
	(0.0071)	(0.0084)	(0.0061)	(0.0073)	(0.0056)	(0.0043)	(0.0030)
N. obs.	184,454,312	184,454,312	184,454,312	184,454,312	184,454,312	184,454,312	184,454,312
N. clusters	6,478	6,478	6,478	6,478	6,478	6,478	6,478
County$_h$ FE	Y	Y	Y	Y	Y	Y	Y
Relative Age$_h$ FE	Y	Y	Y	Y	Y	Y	Y
Race$_h$ FE	Y	Y	Y	Y	Y	Y	Y

Note: Panel A adds a Female indicator and interaction with FS to the specifications in Tables 1 and 2 Panel A. Panel B replicates Tables 1 and 2 Panel B, restricting the sample to women only. *FS* is an indicator for whether the FS program rollout occurred in an individual's county when they were of voting age (18+). White is the omitted ethnic group. Standard errors in parentheses are clustered by county. *** 1%, ** 5%, * 10% significance level.

Table 4: Long run effects

High Black population & high poverty counties

Panel A: Black Counties	(1) Republican	(2) Democrat	(3) Independent	(4) Voted%	(5) Voted%× Republican	(6) Voted%× Democrat	(7) Voted%× Independent
FS × Black Popn.	0.0767**	-0.1368***	0.0601**	-0.0897***	0.0614**	-0.1230***	-0.0280***
	(0.0311)	(0.0293)	(0.0241)	(0.0187)	(0.0259)	(0.0173)	(0.0106)
FS × Black× Black Popn.	-0.0698	0.2025***	-0.1327***	0.0714***	-0.0718**	0.1275***	0.0157
	(0.0561)	(0.0591)	(0.0404)	(0.0245)	(0.0334)	(0.0372)	(0.0140)
FS × Hispanic× Black Popn.	-0.0147	0.2066**	-0.1919***	-0.2134***	-0.0676*	-0.1321***	-0.0137
	(0.0643)	(0.0941)	(0.0676)	(0.0331)	(0.0367)	(0.0354)	(0.0152)
FS × Asian× Black Popn.	-0.0396	0.0436	-0.0041	-0.1165*	-0.1479***	0.0296	0.0018
	(0.0924)	(0.1125)	(0.0687)	(0.0604)	(0.0536)	(0.0501)	(0.0333)
Food Stamps (FS)	0.0156***	-0.0082*	-0.0074	0.0033	0.0331***	-0.0391***	0.0093***
	(0.0049)	(0.0044)	(0.0050)	(0.0033)	(0.0045)	(0.0031)	(0.0027)
FS × Black	-0.1083***	0.0014	0.1070***	-0.0128*	-0.1182***	0.1116***	-0.0062**
	(0.0109)	(0.0126)	(0.0114)	(0.0066)	(0.0057)	(0.0085)	(0.0030)
FS × Hispanic	-0.0606***	0.0066	0.0541***	-0.0437***	-0.0844***	0.0385***	0.0022
	(0.0083)	(0.0120)	(0.0092)	(0.0033)	(0.0047)	(0.0051)	(0.0023)
FS × Asian	-0.0114	-0.0523***	0.0637***	-0.0882***	-0.0622***	-0.0339***	0.0079**
	(0.0130)	(0.0146)	(0.0068)	(0.0086)	(0.0089)	(0.0068)	(0.0039)
N. obs.	351,541,449	351,541,449	351,541,449	351,541,449	351,541,449	351,541,449	351,541,449
N. clusters	6,395	6,395	6,395	6,395	6,395	6,395	6,395
County$_h$ FE	Y	Y	Y	Y	Y	Y	Y
Relative Age$_h$ FE	Y	Y	Y	Y	Y	Y	Y
Race$_h$ FE	Y	Y	Y	Y	Y	Y	Y

Table 4 (cont.)

Panel B: High Poverty	(1) Republican	(2) Democrat	(3) Independent	(4) Voted %	(5) Voted %×Republican	(6) Voted %×Democrat	(7) Voted %×Independent
FS × Poverty	0.0277	-0.0745***	0.0468	0.0083	0.1613***	-0.1094***	-0.0436**
	(0.0354)	(0.0287)	(0.0334)	(0.0268)	(0.0317)	(0.0216)	(0.0172)
FS × Black × Poverty	0.0504	0.2285***	-0.2789***	0.0068	-0.1224***	0.1054**	0.0237
	(0.0601)	(0.0610)	(0.0579)	(0.0308)	(0.0389)	(0.0431)	(0.0205)
FS × Hispanic × Poverty	-0.1038**	-0.0523	0.1561***	0.0514**	-0.2207***	0.2647***	0.0074
	(0.0474)	(0.0648)	(0.0478)	(0.0246)	(0.0309)	(0.0305)	(0.0155)
FS × Asian × Poverty	0.0332	0.2371*	-0.2703***	0.1705	-0.0536	0.2251***	-0.0010
	(0.1084)	(0.1243)	(0.0744)	(0.1073)	(0.0838)	(0.0562)	(0.0330)
Food Stamps (FS)	0.0150*	-0.0068	-0.0082	-0.0048	0.0135*	-0.0323***	0.0139***
	(0.0079)	(0.0067)	(0.0077)	(0.0057)	(0.0071)	(0.0048)	(0.0043)
FS × Black	-0.1115***	-0.0133	0.1248***	-0.0117	-0.1043***	0.1036***	-0.0109**
	(0.0135)	(0.0136)	(0.0142)	(0.0082)	(0.0082)	(0.0105)	(0.0045)
FS × Hispanic	-0.0386***	0.0276*	0.0110	-0.0705***	-0.0552***	-0.0145*	-0.0008
	(0.0106)	(0.0149)	(0.0108)	(0.0060)	(0.0065)	(0.0074)	(0.0036)
FS × Asian	-0.0136	-0.0789***	0.0925***	-0.1176***	-0.0623***	-0.0618***	0.0065
	(0.0181)	(0.0195)	(0.0113)	(0.0154)	(0.0142)	(0.0081)	(0.0060)
N. obs.	353,311,262	353,311,262	353,311,262	353,311,262	353,311,262	353,311,262	353,311,262
N. clusters	6,483	6,483	6,483	6,483	6,483	6,483	6,483
County$_h$ FE	Y	Y	Y	Y	Y	Y	Y
Relative Age$_h$ FE	Y	Y	Y	Y	Y	Y	Y
Race$_h$ FE	Y	Y	Y	Y	Y	Y	Y

Note: Variables and data are the same as in Table 1, but Panel A is restricted to counties in the top 25% by Black population (> 10%), while Panel B is restricted to the top 25% of counties by the percent of families living under the poverty line (> 28%). *FS* is an indicator for whether the FS program rollout occurred in an individual's county when they were of voting age (18+). White is the omitted racial group. Standard errors in parentheses are clustered by county. *** 1%, ** 5%, * 10% significance level.

Table 5: Long run effects

Individuals registering to vote during impressionable years (before age 25)

Panel A	(1) Republican	(2) Democrat	(3) Independent	(4) Voted %	(5) Voted %×Republican	(6) Voted %×Democrat	(7) Voted %×Independent
Food Stamps (FS)	0.0553***	-0.0580***	0.0026	-0.0061*	0.0560***	-0.0648***	0.0026*
	(0.0076)	(0.0086)	(0.0086)	(0.0037)	(0.0060)	(0.0054)	(0.0016)
N. obs.	136,087,406	136,087,406	136,087,406	136,087,406	136,087,406	136,087,406	136,087,406
N. clusters	6,473	6,473	6,473	6,473	6,473	6,473	6,473
County$_h$ FE	Y	Y	Y	Y	Y	Y	Y
Relative Age$_h$ FE	Y	Y	Y	Y	Y	Y	Y
Race$_h$ FE	0.2370	0.4694	0.2936	0.2847	0.0914	0.1432	0.0501

Panel B	(1) Republican	(2) Democrat	(3) Independent	(4) Voted %	(5) Voted %×Republican	(6) Voted %×Democrat	(7) Voted %×Independent
Food Stamps (FS)	0.0789***	-0.0627***	-0.0162*	-0.0078**	0.0879***	-0.0973***	0.0016
	(0.0080)	(0.0087)	(0.0084)	(0.0036)	(0.0062)	(0.0051)	(0.0017)
FS × Black	-0.2281***	0.0799***	0.1483***	0.0265***	-0.2643***	0.2875***	0.0033
	(0.0110)	(0.0111)	(0.0110)	(0.0066)	(0.0068)	(0.0096)	(0.0021)
FS × Hispanic	-0.1141***	0.0040	0.1101***	-0.0398***	-0.1773***	0.1258***	0.0117***
	(0.0132)	(0.0191)	(0.0212)	(0.0101)	(0.0119)	(0.0105)	(0.0016)
FS × Asian	0.0405**	-0.0472***	0.0067	0.0381***	-0.0300**	0.0548***	0.0132***
	(0.0169)	(0.0137)	(0.0093)	(0.0076)	(0.0150)	(0.0177)	(0.0040)
N. obs.	136,087,406	136,087,406	136,087,406	136,087,406	136,087,406	136,087,406	136,087,406
N. clusters	6,473	6,473	6,473	6,473	6,473	6,473	6,473
County$_h$ FE	Y	Y	Y	Y	Y	Y	Y
Relative Age$_h$ FE	Y	Y	Y	Y	Y	Y	Y
Race$_h$ FE	Y	Y	Y	Y	Y	Y	Y

Note: This table replicates the specification in Tables 1 and 2 for individuals that registered to vote during their impressionable years (i.e., before the age of 25). *FS* is an indicator for whether the FS program rollout occurred in an individual's county when they were of voting age (18+). White is the omitted ethnic group. Standard errors in parentheses are clustered by county. *** 1%, ** 5%, * 10% significance level.

Table 6: Long run effects

Fuzzy treatment based on age at rollout

	(1) Republican	(2) Democrat	(3) Independent	(4) Voted%	(5) Voted%×Republican	(6) Voted%×Democrat	(7) Voted%×Independent
Food Stamps (FS)	0.0480***	-0.0695***	0.0214	-0.0156**	0.0173***	-0.0310***	-0.0019
	(0.0081)	(0.0190)	(0.0196)	(0.0061)	(0.0059)	(0.0050)	(0.0034)
FS × Black	-0.0976***	0.0134	0.0842***	-0.0120**	-0.1029***	0.0936***	-0.0028**
	(0.0080)	(0.0086)	(0.0083)	(0.0054)	(0.0031)	(0.0068)	(0.0013)
FS × Hispanic	-0.0463***	0.0102	0.0360***	-0.0646***	-0.0668***	-0.0025	0.0047***
	(0.0050)	(0.0079)	(0.0063)	(0.0043)	(0.0030)	(0.0045)	(0.0010)
FS × Asian	-0.0000	-0.0621***	0.0621***	-0.1005***	-0.0531***	-0.0590***	0.0116***
	(0.0073)	(0.0083)	(0.0059)	(0.0071)	(0.0056)	(0.0035)	(0.0028)
N. obs.	353,311,262	353,311,262	353,311,262	353,311,262	353,311,262	353,311,262	353,311,262
N. clusters	6,483	6,483	6,483	6,483	6,483	6,483	6,483
County$_h$	Y	Y	Y	Y	Y	Y	Y
Relative Age$_h$	Y	Y	Y	Y	Y	Y	Y
Race$_h$	Y	Y	Y	Y	Y	Y	Y

Note: FS is a *continuous* treatment indicator. White is the omitted ethnic group. Standard errors clustered by county. *** 1%, ** 5%, * 10% significance level.

Table 7: Short run effects
Voter registration rates by race 1960-1972

	(1) Black Reg/Popn.	(2) White Reg/Popn.	(3) Black Reg/Eligible	(4) White Reg/Eligible
Food Stamps	0.0101***	-0.0103**	0.0131***	-0.0044
	(0.0029)	(0.0052)	(0.0051)	(0.0126)
N. obs.	1,033	1,033	1,062	1,062
Year FE	Y	Y	Y	Y
County FE	Y	Y	Y	Y

Note: This table reports Callaway and Sant'Anna (2021) DID estimates of the effects of the Food Stamp program roll-out on voter registration data at the county level from 1960 through 1972. The data is from the U.S. Commission on Civil Rights and the NAACP Voter Education Project, with additional data from Matthews and Prothro (1963) obtained from Jim Alt. The data covers counties in the following states: Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas, and Virginia. Registrations are scaled by each county's population (Popn.) or number of eligible voters (Eligible). Standard errors in parentheses are clustered by county. *** 1%, ** 5%, * 10% significance level

Table 8: Short run effects
Congressional elections

Panel A: Democratic vs. Republican Vote Difference

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Baseline	General Elections	Midterms	% Black Pop.		Poverty Share	
				High	Low	High	Low
Food Stamps	0.0709***	0.0585***	0.0343**	0.1308***	0.0721***	0.0087	0.0858**
	(0.0186)	(0.0211)	(0.0141)	(0.0400)	(0.0192)	(0.0324)	(0.0334)
N. obs.	24,028	12,959	10,962	5,301	13,131	4,925	6,518
Year FE	Y	Y	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y	Y

Panel B: Likelihood of a Democratic Win

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Baseline	General Elections	Midterms	% Black Pop.		Poverty Share	
				High	Low	High	Low
Food Stamps	-0.0406	-0.0604*	-0.0120	0.0722***	-0.0891**	-0.1139**	-0.0234
	(0.0286)	(0.0315)	(0.0311)	(0.0252)	(0.0383)	(0.0528)	(0.0558)
N. obs.	24,028	12,959	10,962	5,301	13,131	4,925	6,518
Year FE	Y	Y	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y	Y

Note: This table reports Callaway and Sant'Anna (2021) DID estimates of the effects of the Food Stamp program roll-out on Congressional elections at the county level from 1948 through 1972. The outcome variable in Panel A is the difference in Democratic relative to Republican vote shares; the outcome for Panel B is the likelihood of a Democratic victory. *High % Black Pop., High Poverty Share* restricts the sample to counties in the top quartile of each characteristic. *Low* restricts the sample to counties in the bottom quartile. The data is from ICPSR Electoral Data: Presidential and Congressional 1840–1970, and Dave Leip's Election Atlas. The data covers counties in 49 states. Standard errors in parentheses are clustered by county. *** 1%, ** 5%, * 10% significance level

Table 9: Short run effects
Changes in congressional voting behavior

DW-NOMINATE Dimension 1

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Baseline	Democrats	Republicans	High Black Population Share		High Poverty Share	
				Democrats	Republicans	Democrats	Republicans
Food Stamps	-0.0007	0.0131	0.0020	0.0253***	0.0232*	0.0204**	0.0387**
	(0.0183)	(0.0084)	(0.0102)	(0.0089)	(0.0138)	(0.0094)	(0.0165)
N. obs.	19,949	10,635	7,789	4,453	482	3,951	676
Year FE	Y	Y	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y	Y

Note: This table reports Callaway and Sant'Anna (2021) DID estimates of the effect of the Food Stamp program rollout on the voting behavior in the U.S. House of Representatives from 1962 through 1974. The dependent variable here is the first dimension of the *DW-NOMINATE* vote-based measure, typically interpreted as a measure of political ideology ranging from negative 1 (liberal) to positive 1 (conservative). The *Baseline* column uses the whole sample; subsequent columns are for subsamples identified in the column header. *High Black Population, High Poverty* restricts the sample to counties in the top quartile of each characteristic. The data is from the DW-NOMINATE project (Poole and Rosenthal). Standard errors in parentheses are clustered by county. *** 1%, ** 5%, * 10% significance level.

Table 10: Long run effects
VRA border counties

	(1) Republican	(2) Democrat	(3) Independent	(4) Voted %	(5) Voted %×Republican	(6) Voted %×Democrat	(7) Voted %×Independent
VRA × FS	0.0360**	-0.0255*	-0.0105	-0.0246***	0.0087	-0.0163	-0.0170***
	(0.0146)	(0.0134)	(0.0120)	(0.0065)	(0.0134)	(0.0113)	(0.0048)
VRA × FS × Black	-0.0058	-0.0537	0.0595**	0.0050	-0.0047	-0.0150	0.0247***
	(0.0286)	(0.0353)	(0.0241)	(0.0092)	(0.0182)	(0.0198)	(0.0083)
VRA × FS × Hispanic	-0.0313	-0.0332	0.0645***	-0.0147	-0.0214*	0.0010	0.0057
	(0.0196)	(0.0218)	(0.0162)	(0.0129)	(0.0114)	(0.0110)	(0.0048)
VRA × FS × Asian	-0.0703***	0.0337	0.0366	0.0133	-0.0238**	0.0362*	0.0009
	(0.0158)	(0.0259)	(0.0256)	(0.0195)	(0.0096)	(0.0194)	(0.0057)
Food Stamps (FS)	-0.0096	-0.0205*	0.0300**	0.0156***	0.0375***	-0.0375***	0.0156***
	(0.0116)	(0.0118)	(0.0135)	(0.0058)	(0.0101)	(0.0088)	(0.0039)
FS × Black	-0.1035***	0.1348***	-0.0314*	0.0116*	-0.1172***	0.1595***	-0.0306***
	(0.0202)	(0.0276)	(0.0181)	(0.0065)	(0.0124)	(0.0139)	(0.0063)
FS × Hispanic	0.0107	-0.0220*	0.0112	-0.0709***	-0.0538***	-0.0162**	-0.0009
	(0.0129)	(0.0132)	(0.0114)	(0.0110)	(0.0073)	(0.0073)	(0.0043)
FS × Asian	0.0380***	-0.0878***	0.0498**	-0.1126***	-0.0592***	-0.0559***	0.0025
	(0.0118)	(0.0187)	(0.0209)	(0.0174)	(0.0069)	(0.0163)	(0.0054)
VRA × Black	-0.1039***	0.1415***	-0.0376*	0.0029	-0.0221***	0.0221**	0.0029
	(0.0182)	(0.0299)	(0.0201)	(0.0042)	(0.0079)	(0.0100)	(0.0052)
VRA × Hispanic	-0.0644***	0.1618***	-0.0974***	0.0140***	-0.0097	0.0251***	-0.0015
	(0.0169)	(0.0173)	(0.0160)	(0.0038)	(0.0067)	(0.0047)	(0.0024)
VRA × Asian	-0.0435***	0.2404***	-0.1969***	0.0228***	-0.0043	0.0487***	-0.0216***
	(0.0144)	(0.0207)	(0.0189)	(0.0037)	(0.0064)	(0.0089)	(0.0028)
N. obs.	30,326,671	30,326,671	30,326,671	30,326,671	30,326,671	30,326,671	30,326,671
N. clusters	599	599	599	599	599	599	599
County$_h$ FE	Y	Y	Y	Y	Y	Y	Y
Relative Age$_h$ FE	Y	Y	Y	Y	Y	Y	Y
Race$_h$ FE	Y	Y	Y	Y	Y	Y	Y

Note: This table compares counties covered by section 5 of the VRA of 1965 with adjacent non-covered counties (both within and across state borders), following Aneja and Avenancio-León (2022). Covered counties include all counties in Alabama, Arizona, Arkansas, Georgia, Louisiana, Mississippi, Oklahoma, South Carolina, Tennessee, Texas, Virginia, West Virginia and select counties in North Carolina and Florida. *VRA* is an indicator for VRA section 5 coverage. White is the omitted ethnic group. Standard errors in parentheses are clustered by county. *** 1%, ** 5%, * 10% significance level.

Table 11: Long run effects
Local recessions since FS rollout

	(1) Republican	(2) Democrat	(3) Independent	(4) Voted%	(5) Voted%×Republican	(6) Voted%×Democrat	(7) Voted%×Independent
Food Stamps (FS)	0.0670***	-0.0510***	-0.0160	0.0155**	0.0816***	-0.0613***	-0.0048
	(0.0108)	(0.0097)	(0.0099)	(0.0075)	(0.0075)	(0.0069)	(0.0035)
FS × Black	-0.1234***	-0.0112	0.1346***	-0.0247**	-0.1707***	0.1357***	0.0102**
	(0.0239)	(0.0236)	(0.0227)	(0.0105)	(0.0103)	(0.0127)	(0.0047)
FS × Hispanic	-0.1195***	0.0933***	0.0262	-0.0470***	-0.1143***	0.0459***	0.0214***
	(0.0240)	(0.0325)	(0.0209)	(0.0122)	(0.0119)	(0.0139)	(0.0045)
FS × Asian	-0.0561*	-0.0513*	0.1074***	-0.0891***	-0.1065***	-0.0160	0.0333***
	(0.0311)	(0.0301)	(0.0251)	(0.0209)	(0.0204)	(0.0139)	(0.0103)
FS × Local Recession$_c$	-0.4138***	0.2917***	0.1221	-0.1689***	-0.3927***	0.1149**	0.1089***
	(0.0876)	(0.0754)	(0.0778)	(0.0541)	(0.0642)	(0.0487)	(0.0361)
FS × Local Recession$_c$ × Black	-0.0127	0.5251**	-0.5124**	0.1397	0.3848***	-0.0875	-0.1575***
	(0.2213)	(0.2291)	(0.2457)	(0.0896)	(0.0953)	(0.1078)	(0.0519)
FS × Local Recession$_c$ × Hispanic	0.5934***	-0.7273**	0.1338	-0.1495	0.2307**	-0.1862	-0.1941***
	(0.2098)	(0.2982)	(0.2073)	(0.1022)	(0.1073)	(0.1171)	(0.0445)
FS× Local Recession$_c$ × Asian	0.3706	0.0354	-0.4060*	-0.1182	0.2921*	-0.1816	-0.2287***
	(0.2491)	(0.2511)	(0.2180)	(0.1706)	(0.1556)	(0.1130)	(0.0859)
N. obs.	353,311,262	353,311,262	353,311,262	353,311,262	353,311,262	353,311,262	353,311,262
N. clusters	6,483	6,483	6,483	6,483	6,483	6,483	6,483
County$_h$ FE	Y	Y	Y	Y	Y	Y	Y
Relative Age$_h$ FE	Y	Y	Y	Y	Y	Y	Y
Race$_h$ FE	Y	Y	Y	Y	Y	Y	Y

Note: Local Recession$_c$ is a county-level measure equal to the percentage of years the state is in recession in the period between a county's FS rollout year and 2020. Recessions are years in which real state per capita personal income (from the BEA) grew at less than -3.4%. Standard errors in parentheses are clustered by county. *** 1%, ** 5%, * 10% significance level.

Table 12: Long run effects
Church density

	(1) Republican	(2) Democrat	(3) Independent	(4) Voted%	(5) Voted%×Republican	(6) Voted%×Democrat	(7) Voted%×Independent
Food Stamps (FS)	-0.0004	-0.0171**	0.0175**	-0.0018	0.0066	-0.0291***	0.0207***
	(0.0077)	(0.0072)	(0.0070)	(0.0065)	(0.0076)	(0.0050)	(0.0043)
FS × Black	-0.0979***	0.0123	0.0856***	-0.0175	-0.0979***	0.0989***	-0.0185***
	(0.0156)	(0.0145)	(0.0141)	(0.0131)	(0.0093)	(0.0179)	(0.0044)
FS × Hispanic	-0.0693***	0.0487***	0.0207	-0.0763***	-0.0875***	0.0252***	-0.0140***
	(0.0114)	(0.0149)	(0.0128)	(0.0083)	(0.0074)	(0.0096)	(0.0038)
FS × Asian	-0.0151	-0.0544***	0.0695***	-0.1132***	-0.0669***	-0.0492***	0.0030
	(0.0107)	(0.0137)	(0.0111)	(0.0112)	(0.0085)	(0.0072)	(0.0059)
FS × Church Density	8.8883**	1.6288	-10.5171***	-1.3462	17.6535***	-10.9843***	-8.0153***
	(3.5540)	(2.7011)	(2.9221)	(2.7317)	(3.1709)	(2.0490)	(1.6615)
FS × Church Density × Black	14.7741*	-4.6567	-10.1174	5.0243	-8.6164*	7.8682	5.7726***
	(8.1258)	(7.7217)	(7.4432)	(6.6505)	(4.6243)	(9.2519)	(2.0110)
FS × Church Density × Hispanic	26.3844***	-31.2196***	4.8352	14.2904***	11.7259***	-6.0338	8.5983***
	(6.4922)	(8.9445)	(7.2814)	(5.4233)	(3.7629)	(5.3825)	(1.7945)
FS × Church Density × Asian	18.3226***	-4.0194	-14.3032*	14.9054*	7.9485	5.5440	1.4129
	(6.2665)	(7.2247)	(7.8465)	(8.7229)	(5.8285)	(4.1711)	(2.9413)
Church Density × Black	-110.1023***	72.7236***	37.3787***	7.6449***	-41.1066***	40.2218***	8.5298***
	(5.8332)	(7.7328)	(8.7945)	(1.5329)	(2.3245)	(2.0379)	(1.0437)
Church Density × Hispanic	-70.9875***	49.5060***	21.4816***	-6.6377***	-29.6788***	17.4307***	5.6103***
	(5.7733)	(11.0645)	(7.2033)	(1.4250)	(1.8947)	(2.0269)	(0.9701)
Church Density × Asian	-50.7315***	43.2764***	7.4551	2.7152	-27.3774***	30.4085***	-0.3160
	(5.6248)	(6.7297)	(6.1071)	(1.8587)	(2.0193)	(2.4408)	(1.0081)
N. obs.	349,991,253	349,991,253	349,991,253	349,991,253	349,991,253	349,991,253	349,991,253
N. clusters	6,424	6,424	6,424	6,424	6,424	6,424	6,424
County$_h$ FE	Y	Y	Y	Y	Y	Y	Y
Relative Age$_h$ FE	Y	Y	Y	Y	Y	Y	Y
Race$_h$ FE	Y	Y	Y	Y	Y	Y	Y

Note: Church Density is measured as the number of churches per 1,000 county inhabitants and is from the Survey of Churches and Church Membership by County as of 1952 (ICPSR, 1952). The mean of the variable is 1.2694 and its standard deviation is 0.8342. Standard errors in parentheses are clustered by county. *** 1%, ** 5%, * 10% significance level.

Appendix

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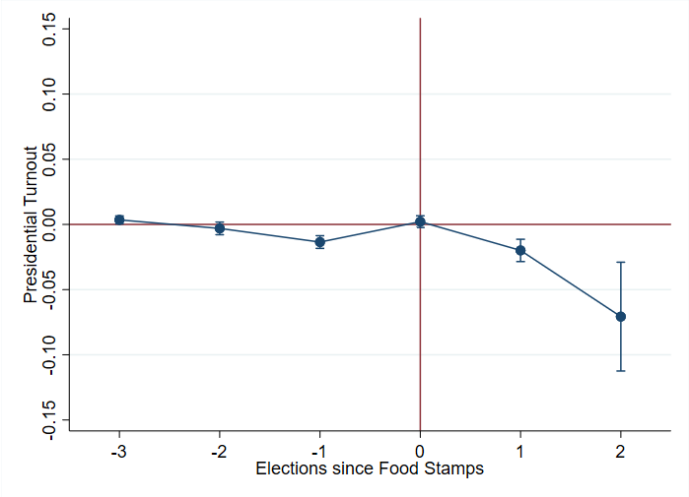
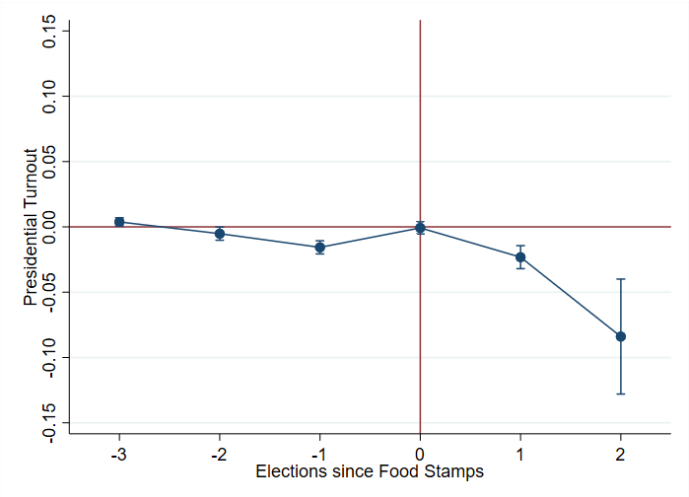
A. Appendix Figures

A.1 Event Studies

Figure IA1: Event study: Turnout



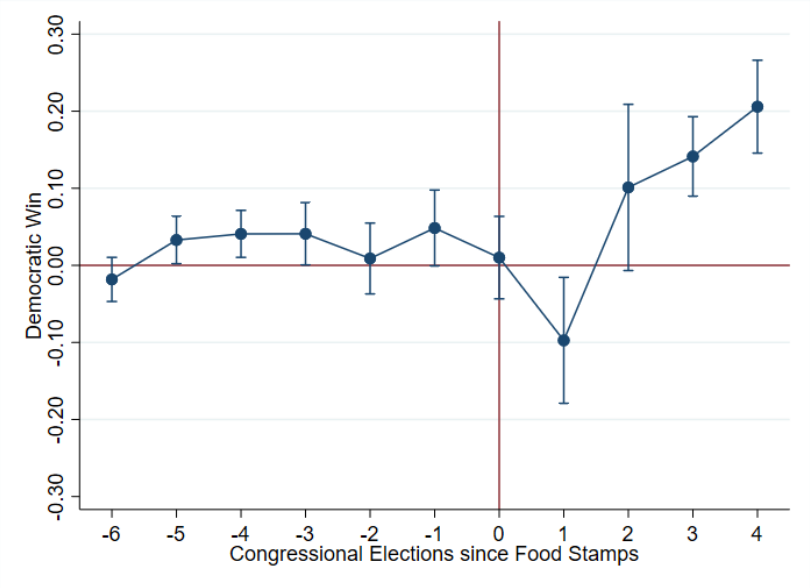
(a) Presidential Turnout



(b) Turnout excluding final rollout cohort

Note: Figure (a) presents Callaway and Sant'Anna (2021) event study estimates of the effect of Food Stamp program rollout on presidential turnout, measured as a share of registered voters. The estimates use data from 1948 to 1972 from ICPSR Electoral Data for Counties in the United States: Presidential and Congressional Races, 1840-1972, and Dave Leip's Atlas of US Presidential Elections. Figure (b) excludes the final cohort of counties that implemented the Food Stamp program – those in Indiana and Montana – as a robustness test. 95% confidence intervals clustered by county.

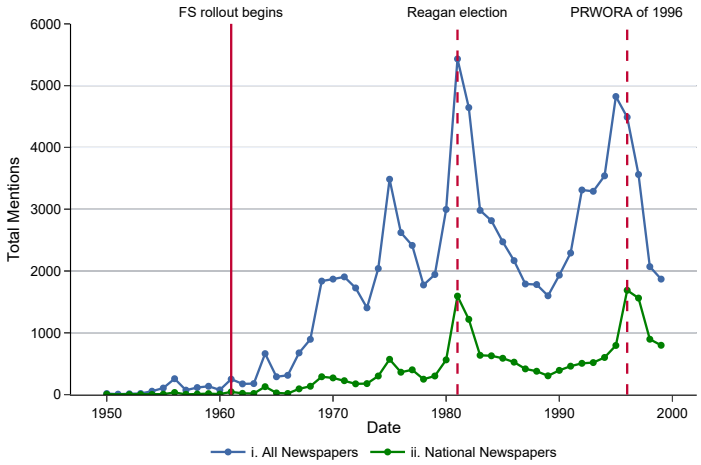
Figure IA2: Event study: Likelihood of Democratic Win in Counties with a High Black Population Share



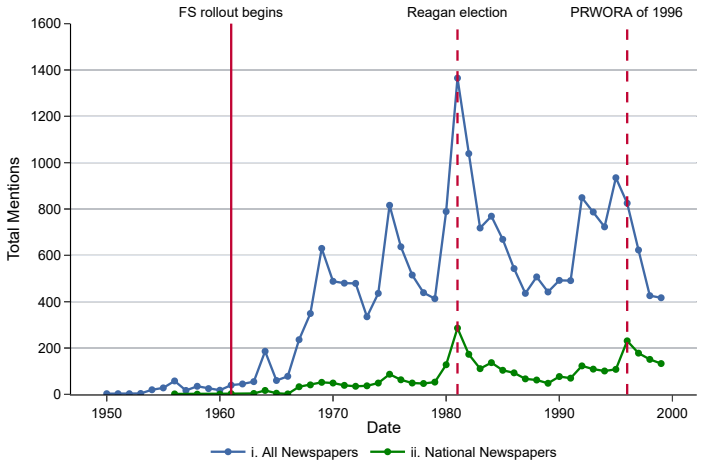
Note: This figure presents event study estimates of the effect of Food Stamp program roll-out on the probability of a Democratic party victory in counties with a high black share in Congressional elections. High black share counties have a Black population share above 10%, which equates to around 25% of counties (75th percentile is 11%). The data is at the county-election level for years 1940–1992 (see section 2 for data sources). The data source is ICPSR Electoral Data for Counties in the United States: Presidential and Congressional Races, 1840-1972, and Dave Leip's Atlas of US Presidential Elections. Coefficients are estimated following Callaway and Sant'Anna (2021), with 95% confidence intervals clustered by county.

A.2 Newspaper coverage of Food Stamps

Figure IA3: Newspaper coverage



(a) Newspaper articles mentioning "Food Stamps"



(b) Newspaper articles mentioning "Food Stamps" + Race identifiers

Note: These graphs display yearly counts of news articles mentioning Food Stamps between 1950 and 2000. (a) counts news articles containing the term "food stamp" within the article's body for both All (blue line) and National newspaper categories (green line). (b) adds a racial term (Black, Negro, or African American) to the search within the article's text. In both graphs, the first red line indicates the beginning of the Food Stamp program rollout in 1961; the second line the 1985 election of President Ronald Reagan; and the final line the implementation of the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA), also known as the Welfare Reform Act. "National" newspapers: Boston Globe, Chicago Tribune, Los Angeles Times, New York Times, Wall Street Journal, and Washington Post. "All" incorporates the National newspapers plus: San Francisco Chronicle, San Francisco Examiner, Chicago Defender, Newsday, New York Tribune, New York Herald, Philadelphia Inquirer, Philadelphia Tribune, Pittsburgh Post-Gazette, Pittsburgh Courier, Austin American-Statesman, and St. Louis Post Dispatch. All news data is from ProQuest TDM Studio.

B. Appendix Tables

B.1 Long run effects

Table IA1: Long run effects
Robustness #1: County × Relative age fixed effects

	(1) Republican	(2) Democrat	(3) Independent	(4) Voted%	(5) Voted%×Republican	(6) Voted%×Democrat	(7) Voted%×Independent
Food Stamps (FS)	-	-	-	-	-	-	-
	(.)	(.)	(.)	(.)	(.)	(.)	(.)
FS × Black	-0.1261***	0.0423***	0.0838***	0.0064***	-0.1260***	0.1347***	-0.0023
	(0.0118)	(0.0092)	(0.0087)	(0.0025)	(0.0068)	(0.0064)	(0.0021)
FS × Hispanic	-0.0493***	0.0336***	0.0157**	-0.0623***	-0.0804***	0.0219***	-0.0038**
	(0.0075)	(0.0098)	(0.0063)	(0.0034)	(0.0051)	(0.0046)	(0.0019)
FS × Asian	-0.0052	-0.0425***	0.0477***	-0.0937***	-0.0625***	-0.0355***	0.0044*
	(0.0073)	(0.0082)	(0.0054)	(0.0049)	(0.0046)	(0.0058)	(0.0026)
N. obs.	353,309,686	353,309,686	353,309,686	353,309,686	353,309,686	353,309,686	353,309,686
N. clusters	6,473	6,473	6,473	6,473	6,473	6,473	6,473
County$_h$ × Relative Age$_h$	Y	Y	Y	Y	Y	Y	Y
Race$_h$	Y	Y	Y	Y	Y	Y	Y

Note: This table replicates the specification in Tables 1 and 2 but replaces County and Relative age fixed effects (FE) with County×Relative age FE. Because the *Food Stamps* (*FS*) treatment is at the county×birth year level this vector of new fixed effects absorbs the *FS* variable, but still allows for the estimation of the *FS* × *Race* coefficients, which capture the differential effects of treatment for each racial group relative to treated Whites. *FS* is an indicator for whether the FS program rollout occurred in an individual's county when they were of voting age (18+). White is the omitted group. Standard errors clustered by county. *** 1%, ** 5%, * 10% significance level.

Table IA2: Long run effects

Robustness #2: Relative age × Race fixed effects

	(1) Republican	(2) Democrat	(3) Independent	(4) Voted%	(5) Voted%×Republican	(6) Voted%×Democrat	(7) Voted%×Independent
Food Stamps (FS)	0.0608***	-0.0732***	0.0124***	0.0075**	0.0543***	-0.0585***	0.0116***
	(0.0050)	(0.0056)	(0.0044)	(0.0031)	(0.0041)	(0.0029)	(0.0021)
FS × Black	-0.3468***	0.4253***	-0.0786***	-0.0369***	-0.2199***	0.2206***	-0.0376***
	(0.0127)	(0.0153)	(0.0058)	(0.0054)	(0.0068)	(0.0103)	(0.0024)
FS × Hispanic	-0.2050***	0.1863***	0.0187***	-0.1195***	-0.1506***	0.0453***	-0.0142***
	(0.0084)	(0.0100)	(0.0052)	(0.0040)	(0.0043)	(0.0058)	(0.0019)
FS × Asian	-0.1141***	-0.0439***	0.1580***	-0.1851***	-0.1306***	-0.0673***	0.0127***
	(0.0078)	(0.0100)	(0.0077)	(0.0061)	(0.0047)	(0.0047)	(0.0034)
N. obs.	353,311,262	353,311,262	353,311,262	353,311,262	353,311,262	353,311,262	353,311,262
N. clusters	6,483	6,483	6,483	6,483	6,483	6,483	6,483
County$_h$	Y	Y	Y	Y	Y	Y	Y
Relative Age$_h$ × Race$_h$	Y	Y	Y	Y	Y	Y	Y

Note: This table replicates the specification in Tables 1 and 2 but replaces Race and Relative age fixed effects (FE) with Relative Age×Race FE. *FS* is an indicator for whether the FS program rollout occurred in an individual's county when they were of voting age (18+). White is the omitted group. Standard errors clustered by county. *** 1%, ** 5%, * 10% significance level.

Table IA3: Long run effects
Robustness #3: County × Race fixed effects

	(1) Republican	(2) Democrat	(3) Independent	(4) Voted%	(5) Voted%×Republican	(6) Voted%×Democrat	(7) Voted%×Independent
Food Stamps (FS)	0.0594***	-0.0706***	0.0112**	0.0073**	0.0543***	-0.0583***	0.0114***
	(0.0049)	(0.0056)	(0.0045)	(0.0031)	(0.0040)	(0.0029)	(0.0021)
FS × Black	-0.3435***	0.4129***	-0.0693***	-0.0370***	-0.2232***	0.2220***	-0.0357***
	(0.0110)	(0.0128)	(0.0053)	(0.0049)	(0.0057)	(0.0089)	(0.0021)
FS × Hispanic	-0.2042***	0.1855***	0.0187***	-0.1194***	-0.1505***	0.0453***	-0.0142***
	(0.0086)	(0.0102)	(0.0051)	(0.0040)	(0.0043)	(0.0058)	(0.0019)
FS × Asian	-0.1134***	-0.0442***	0.1576***	-0.1851***	-0.1304***	-0.0673***	0.0126***
	(0.0078)	(0.0102)	(0.0077)	(0.0061)	(0.0047)	(0.0047)	(0.0033)
N. obs.	353,311,264	353,311,264	353,311,264	353,311,264	353,311,264	353,311,264	353,311,264
N. clusters	6,485	6,485	6,485	6,485	6,485	6,485	6,485
County$_h$ × Race$_h$	Y	Y	Y	Y	Y	Y	Y
Relative Age$_h$	Y	Y	Y	Y	Y	Y	Y

Note: This table replicates the specification in Tables 1 and 2 but replaces County and Race fixed effects (FE) with County×Race FE. *FS* is an indicator for whether the FS program rollout occurred in an individual's county when they were of voting age (18+). White is the omitted group. Standard errors clustered by county. *** 1%, ** 5%, * 10% significance level.

Table IA4: Long run effects

Robustness #4: County × Relative age × Race fixed effects

	(1) Republican	(2) Democrat	(3) Independent	(4) Voted%	(5) Voted%×Republican	(6) Voted%×Democrat	(7) Voted%×Independent
Food Stamps	-	-	-	-	-	-	-
	(.)	(.)	(.)	(.)	(.)	(.)	(.)
FS × Black	-0.3900***	0.4974***	-0.1074***	-0.0278***	-0.2331***	0.2445***	-0.0392***
	(0.0121)	(0.0142)	(0.0059)	(0.0032)	(0.0089)	(0.0086)	(0.0025)
FS × Hispanic	-0.2284***	0.2337***	-0.0053	-0.1339***	-0.1567***	0.0439***	-0.0211***
	(0.0103)	(0.0126)	(0.0057)	(0.0042)	(0.0066)	(0.0055)	(0.0021)
FS × Asian	-0.1298***	-0.0124	0.1421***	-0.1910***	-0.1303***	-0.0682***	0.0075**
	(0.0080)	(0.0097)	(0.0071)	(0.0057)	(0.0052)	(0.0074)	(0.0030)
N. obs.	353,309,686	353,309,686	353,309,686	353,309,686	353,309,686	353,309,686	353,309,686
N. clusters	6,473	6,473	6,473	6,473	6,473	6,473	6,473
County$_h$×Relative Age$_h$×Race$_h$	Y	Y	Y	Y	Y	Y	Y

Note: This table replicates the specification in Tables 1 and 2 but replaces County, Relative age, and Race fixed effects (FE) with County×Relative age×Race FE. *FS* is an indicator for whether the FS program rollout occurred in an individual's county when they were of voting age (18+). White is the omitted group. Standard errors clustered by county. *** 1%, ** 5%, * 10% significance level.

Table IA5: Long run effects
Fuzzy treatment based on age at rollout

	(1) Republican	(2) Democrat	(3) Independent	(4) Voted%	(5) Voted%×Republican	(6) Voted%×Democrat	(7) Voted%×Independent
Food Stamps (FS)	0.0480***	-0.0695***	0.0214	-0.0156**	0.0173***	-0.0310***	-0.0019
	(0.0081)	(0.0190)	(0.0196)	(0.0061)	(0.0059)	(0.0050)	(0.0034)
FS × Black	-0.0976***	0.0134	0.0842***	-0.0120**	-0.1029***	0.0936***	-0.0028**
	(0.0080)	(0.0086)	(0.0083)	(0.0054)	(0.0031)	(0.0068)	(0.0013)
FS × Hispanic	-0.0463***	0.0102	0.0360***	-0.0646***	-0.0668***	-0.0025	0.0047***
	(0.0050)	(0.0079)	(0.0063)	(0.0043)	(0.0030)	(0.0045)	(0.0010)
FS × Asian	-0.0000	-0.0621***	0.0621***	-0.1005***	-0.0531***	-0.0590***	0.0116***
	(0.0073)	(0.0083)	(0.0059)	(0.0071)	(0.0056)	(0.0035)	(0.0028)
N. obs.	353,311,262	353,311,262	353,311,262	353,311,262	353,311,262	353,311,262	353,311,262
N. clusters	6,483	6,483	6,483	6,483	6,483	6,483	6,483
County$_h$	Y	Y	Y	Y	Y	Y	Y
Relative Age$_h$	Y	Y	Y	Y	Y	Y	Y
Race$_h$	Y	Y	Y	Y	Y	Y	Y

Note: FS is a *continuous* treatment indicator. White is the omitted ethnic group. Standard errors clustered by county. *** 1%, ** 5%, * 10% significance level.

Table IA6: Alternate geographic fixed effects
Zipcode × *Race*

	(1) Republican	(2) Democrat	(3) Independent	(4) Voted%	(5) Voted%×Republican	(6) Voted%×Democrat	(7) Voted%×Independent
Food Stamps	0.0124***	-0.0117***	-0.0007	-0.0012	0.0363***	-0.0452***	0.0078***
	(0.0037)	(0.0033)	(0.0040)	(0.0027)	(0.0035)	(0.0026)	(0.0019)
FS × Black	-0.0698***	0.0028	0.0670***	-0.0171***	-0.1177***	0.1098***	-0.0092***
	(0.0029)	(0.0056)	(0.0061)	(0.0053)	(0.0026)	(0.0059)	(0.0014)
FS × Hispanic	-0.0346***	-0.0056	0.0402***	-0.0651***	-0.0828***	0.0179***	-0.0001
	(0.0034)	(0.0069)	(0.0068)	(0.0040)	(0.0029)	(0.0040)	(0.0012)
FS × Asian	0.0004	-0.0660***	0.0656***	-0.1000***	-0.0669***	-0.0463***	0.0132***
	(0.0062)	(0.0071)	(0.0049)	(0.0062)	(0.0054)	(0.0024)	(0.0027)
N. obs.	347,630,162	347,630,162	347,630,162	347,630,162	347,630,162	347,630,162	347,630,162
N. clusters	6,475	6,475	6,475	6,475	6,475	6,475	6,475
$\text{Zip}_h \times \text{Race}_h$	Y	Y	Y	Y	Y	Y	Y
Relative Age_h	Y	Y	Y	Y	Y	Y	Y

Note: Standard errors clustered by county. *** 1%, ** 5%, * 10% significance level.

Table IA7: Alternate geographic fixed effects
Census tract × Race

	(1) Republican	(2) Democrat	(3) Independent	(4) Voted%	(5) Voted%×Republican	(6) Voted%×Democrat	(7) Voted%×Independent
Food Stamps	0.0117***	-0.0114***	-0.0004	-0.0007	0.0359***	-0.0444***	0.0078***
	(0.0036)	(0.0032)	(0.0040)	(0.0026)	(0.0035)	(0.0026)	(0.0019)
FS × Black	-0.0672***	0.0015	0.0657***	-0.0177***	-0.1160***	0.1074***	-0.0092***
	(0.0029)	(0.0055)	(0.0060)	(0.0053)	(0.0026)	(0.0058)	(0.0014)
FS × Hispanic	-0.0334***	-0.0057	0.0391***	-0.0645***	-0.0820***	0.0177***	-0.0002
	(0.0034)	(0.0069)	(0.0067)	(0.0039)	(0.0028)	(0.0039)	(0.0011)
FS × Asian	0.0013	-0.0662***	0.0649***	-0.0991***	-0.0661***	-0.0462***	0.0132***
	(0.0062)	(0.0070)	(0.0049)	(0.0063)	(0.0055)	(0.0024)	(0.0027)
N. obs.	347,620,386	347,620,386	347,620,386	347,620,386	347,620,386	347,620,386	347,620,386
N. clusters	6,475	6,475	6,475	6,475	6,475	6,475	6,475
$\text{Tract}_h \times \text{Race}_h$	Y	Y	Y	Y	Y	Y	Y
Relative Age_h	Y	Y	Y	Y	Y	Y	Y

Note: Standard errors clustered by county. *** 1%, ** 5%, * 10% significance level.

Table IA8: Alternate geographic fixed effects
Census block × Race

	(1) Republican	(2) Democrat	(3) Independent	(4) Voted%	(5) Voted%×Republican	(6) Voted%×Democrat	(7) Voted%×Independent
Food Stamps	0.0455***	-0.0553***	0.0097**	0.0055*	0.0487***	-0.0534***	0.0102***
	(0.0042)	(0.0046)	(0.0042)	(0.0028)	(0.0038)	(0.0028)	(0.0020)
FS × Black	-0.2410***	0.2791***	-0.0382***	-0.0179***	-0.1791***	0.1865***	-0.0254***
	(0.0088)	(0.0123)	(0.0057)	(0.0052)	(0.0046)	(0.0083)	(0.0017)
FS × Hispanic	-0.1547***	0.1358***	0.0189***	-0.0993***	-0.1281***	0.0377***	-0.0089***
	(0.0062)	(0.0074)	(0.0045)	(0.0037)	(0.0035)	(0.0052)	(0.0014)
FS × Asian	-0.0949***	-0.0529***	0.1478***	-0.1752***	-0.1207***	-0.0678***	0.0132***
	(0.0085)	(0.0099)	(0.0070)	(0.0063)	(0.0052)	(0.0040)	(0.0031)
N. obs.	347,639,447	347,639,447	347,639,447	347,639,447	347,639,447	347,639,447	347,639,447
N. clusters	6,475	6,475	6,475	6,475	6,475	6,475	6,475
$\text{Block}_h \times \text{Race}_h$	Y	Y	Y	Y	Y	Y	Y
Relative Age_h	Y	Y	Y	Y	Y	Y	Y

Note: Standard errors clustered by county. *** 1%, ** 5%, * 10% significance level.