Dissecting Racial Politicization: Long-Run Evidence from the Food Stamp Program*

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Abstract

Many public policies – such as those on immigration, welfare, and policing – consistently attract partisan political attention, often with a racial dimension. How a public policy becomes politicized along racial lines is the focus of this paper. We develop a framework in which political parties gain electoral advantage by framing policy in political terms. This shows that an ex-ante group-neutral policy can generate political polarization across different voter groups (e.g., by race), that polarization is larger for cohorts learning about the policy at its onset, and that polarization persists over time. We apply this framework to study the politicization of the Food Stamp program. Using voter roll data for the entire U.S., we show empirically that the introduction of the program increased political polarization across racial groups, that this racial polarization was larger for voters that experienced the FS rollout at its onset, and that this polarization persists today, about a half-century later. More specifically, 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, with this divergence present but decreasing among younger cohorts. Our design ensures that these findings are not driven by geographic or age-specific racial trends. We also explore contemporaneous effects and additional contributing factors. First, we show that access to the safety net also had short-run effects on voters' beliefs and turnout, as well as on the ideological composition of Congress. Second, we explore the interaction between Food Stamps and contemporaneous events such as the Voting Rights Act and recessions.

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1. Introduction

Political polarization in America has been rising for decades. However, not all issues are equally polarizing: the safety net and racial attitudes are two of the topics that consistently generate the greatest disagreement between partisans (see Figure 1). The historical record suggests that viewing entrenched partisan disagreement as a sorting equilibrium arising spontaneously from personal preferences is overly simplistic. In particular, political parties have the ability and incentive to engender partisanship for political advantage.[1]

This paper provides multiple pieces of evidence that characterize how policy-based political polarization can arise, and shows that the voting impacts of politicized policies can persist for more than five decades. Our setting is the 20th century's major expansion of the Federal safety net: the original Food Stamps program, which was implemented between 1961 and 1975. The current incarnation of this program covers 42 million people in the U.S., including nearly one in four children.[2] The rollout of the program overlapped with key legislative achievements of the Civil Rights movement, such as the Civil Rights Act of 1964 and the Voting Rights Act of 1965, as well as the decline of the Jim Crow system. In this context of legal and political transformation, the introduction of the federal Food Stamp (FS) program was ripe for politicization along *racial* lines, especially since access to food assistance had historically been manipulated to suppress Black Americans' political participation.[3]

We provide, to the best of our knowledge, the first causal estimates on the racial politicization of the social safety net.[4] Our central empirical results use individual-level data on voting history to show that: (i) the introduction of the FS Program increased political polarization across racial groups, (ii) this racial polarization is most pronounced for adults who lived through the FS rollout itself, and (iii) this polarization persists and affects electoral outcomes up to a half-century later.

To provide structure for our empirical analyses, we develop a model linking voters' perceptions of a policy with how it is framed politically. As voters are rarely well-informed about a government

[1] A prominent example is the "Southern Strategy" which, in its most overtly political form, represented a deliberate attempt to frame federal policies through an explicitly racialized lens. Scholars have argued that this contributed to an exodus of Southern White voters away from the Democratic and towards the Republican Party (e.g, Valentino and Sears, 2005, Maxwell and Shields, 2019).

[2] 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.

[3] 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)

[4] 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).

policy when it is first rolled out (e.g., Fowler and Margolis, 2014, Achen and Bartels, 2016), voters in our model are initially uncertain about a policy's impact, with truth being revealed over time. This uncertainty gives parties the scope to affect perceptions through narrative framing—and greater scope when policies are new. The electorate is partitioned into voter blocks, which parties can target separately, and the policy does not favor any particular block. Our model shows that political parties can gain advantage by strategically allocating narrative efforts towards voting blocks *differently*, thereby amplifying political polarization.

Our empirical analysis supports several predictions of this model. We begin by focusing on long-run outcomes. We employ a comprehensive individual-level dataset covering the universe of U.S. voters as of 2020, as well as their voting history over the last two decades. We compare voting patterns for individuals who were adults (age 18+) when their county first adopted a FS Program to individuals who were younger when their county initially rolled out 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 the use of a rich set of fixed-effects in a stacked difference in differences design to ensure that our estimates are not driven by age, geography, or shifting political attitudes between 1960 and 2020. This combines difference in differences with an experience-based design.

We show that exposure to FS rollout affects the *long-run* partisan composition of electorate: adult individuals at rollout – relative to those below the age of majority – are *more* likely to be registered as Republicans and *less* likely to be registered as Democrats in 2020, around a half-century later. Motivated by the model predictions, we decompose these changes in the overall electorate into shifts by voter block - by race. *Racial* polarization is often an order of magnitude larger than electorate-wide effects. White voters exposed to the FS rollout as adults are substantially 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. We also show increases in racial polarization in voting turnout, conditional on party affiliation. 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 before the age of 25 – who are likely more politically engaged than those who register at later ages – we find stronger effects.

Next, we explore the extent to which *short run* effects of the FS Program match our model's

structure, as well as its predictions. A key model element is the link between narrative, policy framing, and political attitudes. To shed light on this connection we use survey data from the rollout period and the two decades after to show that FS treatment quickly generates change in voters' ideology and how they view parties. For example, White voters who live in counties with a FS Program report decreased approval of the Democratic party and Black voters report very large increases. The evidence also supports racialized re-balancing of partisan ties: Black Republicans exposed to FS rollout are more likely to indicate support for the Democratic party, and White Democrats are less likely to do so.

We also provide evidence linking these short-term shifts in views to changes in contemporaneous electoral outcomes. 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 the effect of FS implementation leads to more conservative voting by representatives – Democrat and Republican alike – which is also consistent with a political backlash.

Finally, we explore interactions and heterogeneity of FS impact along two margins. We first consider how 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 opposite of the general pattern. Black voters in these counties shift even further towards Democratic party registration. Thus, local recessions appear to be associated with substantial and heterogeneous effects on the long-run political consequences of the FS Program rollout.

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. Although historians and legal scholars have extensively studied the racial dimensions of the Food Stamps Program and how it interacted with U.S. politics (e.g., Zinn, 1964, Edelman, 2004, Kornbluh, 2007, 2015), we offer, to the best of our knowledge, the first causal empirical estimates of the racial politicization of social welfare policies, along with evidence on the mechanisms driving this process. 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 expenditure, health, education, and employment outcomes (e.g., Currie and Moretti, 2008, Hoynes and Schanzenbach, 2009, 2012, Almond et al., 2011, Hoynes et al., 2016).

Second, we contribute to the literature on how policies affect voting and political inequality. Existing research examines how formal and informal barriers affect well-documented disparities in voting behavior (Fraga, 2018, Jones et al., 2012), 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. The theoretical framework of this paper also contributes to the literature modeling learning in politically polarized environments (e.g., Alesina et al., 2020, Izzo et al., 2023, Angelucci et al., 2024).

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 stronger 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).

The paper is structured as follows. Section 2 provides some background on the FS Program. Section 3 lays out a model framework to organize and interpret our empirical results. Section 4 describes the data, while section 5 discusses the empirical strategy and the long-run effects of the FS Program. Section 6 shows how voter beliefs change, both during the FS rollout and in the two decades after. Section 7 documents short-run effects of the FS rollout on electoral outcomes. Finally, section 8 explores how the Voting Rights Act and recessions interact with the long-run effects of FS. Section 9 concludes.

2. Background: The Safety Net Politics of the 1960s

> *"Among the strands of American political development most mired in racial conflict is the growth of the welfare state."*
>
> –Lieberman, "Race and the Organization of Welfare Policy" in *Classifying by Race*

The Food Stamps Act, signed into law by Lyndon B. Johnson in 1964, codified and expanded a pilot program that had begun three years prior under the Kennedy administration. This pilot – and even more so the subsequent national expansion – represented a major pivot in America's approach to social supports: it helped adults buy food not because they had children or were unable to work, but simply because they were poor. Prior to the early 1960s, the Federal government's

major food assistance program was Aid to Dependent Children (ADC), which not only explicitly linked assistance to children, but was legally constrained to cases in which fathers were "deceased, absent, or unable to work" (Blank and Blum, 1997). Before the Food Stamps Act, most social insurance programs were specifically designed to avoid extending eligibility to able-bodied men.

These exclusions responded to concerns about free-riding by those who could provide for themselves. However, the historical record also suggests a degree of racially-based motivation (Gilens, 1995, 1996, Lieberman, 2001, 1995, Quadagno, 1996). Many states administered ADC in conjunction with "man in the house" laws which overlaid a moral lens to partition welfare recipients into children of widowed mothers (seen as most deserving), and children of divorced, separated, or never-married mothers (seen as less deserving and disproportionately African American; see for example Lefkovitz, 2011, Lieberman, 2001).[5]

The early-1960s brought a large-scale re-imagining and expansion of the federal safety net. The Food Stamps Act was a central pillar of Lyndon B. Johnson's War on Poverty and was part of the Civil Rights Era's broader push towards increasing civic and economic security for Black Americans. Importantly, the Food Stamps Act explicitly prohibited discrimination on the basis of race.

National rollout of the Food Stamps (FS) Program took place between 1961 and 1975. In the pilot phase, 43 counties adopted between 1961 and 1963. The Food Stamps Act, passed the following year, led to steady expansion over the next decade. In 1973, the Act was amended to require all counties to offer FS by 1975. Hoynes and Schanzenbach have documented that the timing of adoption for specific counties appears to be driven primarily by availability of federal funding rather than by characteristics of the county itself (Hoynes and Schanzenbach, 2009, Hoynes et al., 2016).[6]

During the period of national rollout, political opposition to safety net expansion was a regular feature of both state and national politics. This opposition was frequently coded in racialized ways (Valentino and Sears, 2005, Gilens, 1995, 1996, Lieberman, 2001, Quadagno, 1996).[7] Perhaps

[5] ADC itself was established as part of the Social Security Act of 1935, which exempted agricultural and domestic workers from coverage, largely at the request of Southern Congressmen mindful of the South's economic dependence on labor supplied to these industries, predominantly by Black workers (Lieberman, 2001, Quadagno, 1996).

[6] Our baseline specifications all include county fixed effects and we offer several robustness analysis that allow these fixed effects to be time-varying. We also show that county characteristics do not predict the timing of the Food Stamp rollout.

[7] In one example from 1961, the city manager of Newburgh, NY described signs being hung in Southern railroad stations advertising that anyone who moved to Newburgh could receive welfare assistance without having to work, and said: "We challenge the right of moral chiselers and loafers to squat on the relief rolls forever." There is no evidence that such signs existed (The Uncertain Hour, 2023, Lieberman, 2001). The reference to Southern bus stations invoked the racial population flows of the Great Migration – the city of Newburgh had seen its Black population triple during the 1950s – and the depiction of welfare recipients as lazy aligns with racial stereotypes of the 19th and 20th centuries

the most famous Welfare-related trope occurred in 1974, when the Chicago Tribune ran an article about welfare fraud describing Linda Taylor as a "Welfare Queen" living a lavish lifestyle through exploitation of FS support (Slate, 2019). 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 political narratives (Hancock, 2004, Nadasen, 2007).

3. Framework: Political Polarization with Slow Learning of Outcomes

This section provides a model of how policies can be politicized to provide structure for our empirical analyses. We use this model to increase transparency about the potential interplay between political incentives and narrative, and accordingly select model features to be consistent with the historical record (such as parties deploying narratives; e.g., Card et al., 2022) and the presence of frictions in voters' policy understanding (e.g., Achen and Bartels, 2016, Fowler and Margolis, 2014).

When voters hold sticky views about a policy and information about the policy diffuses slowly, the model shows that racial polarization in views about the policy can arise even when policies are ex-ante race-neutral. The key elements of the model are: (i) two political parties; (ii) an arbitrary partition of the electorate into blocks of voters (e.g., White voters, W, Black voters, B) of different sizes; and (iii) initial uncertainty about the impact of a policy that resolves over time.

At the highest level, polarization arises in this model because the slow revelation of policy impact gives political parties the scope to affect opinions by investing in narrative. However, such investments are not only costly but asymmetrically so: when a policy is already viewed positively by one party it is easier to marginally decrease approval than to marginally increase it. As a result, when a policy is rolled out, the party initially positioned in opposition will optimally allocate its resources to shift the viewpoint of the largest voter block. In turn, the other party's optimal choice will be to partially, *but not entirely*, offset this targeted narrative investment and, simultaneously, to increase investment in the groups the opposition is not prioritizing. This generates a partisan mismatch in investment for both blocks and leads to political polarization: decreasing net approval in the majority block (shifting the majority voters towards the opposition party), and increasing approval in the minority block (shifting these voters away from the opposition).

(Gilens, 1995).

3.1 Structure

The model features two types of risk-neutral agents: political parties ($P \in \{L, R\}$) and voter blocks (i). Voter blocks are predetermined, correspond to a share α_i of the electorate, and without loss of generality satisfy:

$$\alpha_1 \geq \alpha_2 \geq ...\alpha_k \text{ with } \sum_i \alpha_i = 1$$

We focus on the two voter block case, $i \in \{W, B\}$, with $\alpha_W > \alpha_B$ and $\alpha_W + \alpha_B = 1$.

There is a single policy which has true and time-invariant impact of y^*. This is voter block neutral (i.e., $y_i^* = y^* \ \forall i$) and imperfectly observed by voters. $y^* = 0$ denotes a politically neutral policy; define $y^* > 0$ as a policy impact favored by $P = L$.

There are two components to a voter's view regarding policy y. One component relates to the impact the policy has on the world, which is imperfectly perceived. The second component can be viewed as political narrative, or "spin" and is a function of investment by both parties in how a policy is framed in public discourse.

Voter beliefs: impact-based component. A central model feature is that voters are assumed to form their political beliefs about a policy's impact once, and this component of their view is never revised. This is consistent with limited-attention models: each voter carefully evaluates whatever they can discern about a given policy one time, and subsequently does not revisit their conclusion.

We refer to this initial belief formation as the point in the life-cycle at which a voter becomes politically aware.[8] However, because policy outcomes are imperfectly observed by voters, the accuracy of information available to an individual when they become politically aware depends on the time since policy implementation. Information on (true) policy impact diffuses throughout the electorate recursively:

$$\begin{aligned} y_i^N &= (1-\phi)y_i^* + \phi y_i^{N-1} \\ y_i^0 &= 0, \end{aligned} \tag{1}$$

where y_i^N denotes the view held by the voter block initially forming impressions N years after

[8] Following a common assumption in the literature, our benchmark empirical analysis allows this belief formation to coincide with the age of majority for individuals who were not adults when a policy is first implemented. As a result we use the term "new voter" to refer to those forming initial policy beliefs. In an extension, we relax this assumption and allow belief formation to occur flexibly prior to age 18.

policy rollout ($N \geq 0$). Thus, the model operates at the voter block (i) and *cohort* (N) level. $|\phi| < 1$ parametrizes the speed at which accurate information about the policy disseminates: $\phi = 0$ means that new voters are able to perfectly perceive the true policy impact at the moment of belief formation, regardless of policy tenure. When $N = 0$ there is no access to information about policy impact, and thus voter beliefs are based entirely on political framing, which is detailed in the following subsection.

Voter beliefs: framing-based component. Parties can make costly investments in order to affect the perception of policies. Costs may be non-pecuniary: while perceptions are affected by advertising or other efforts requiring financial expenditures, they are also affected by the frequency with which politicians reiterate a message, the extent to which certain framing is represented in party-wide marketing and positioning, and the amount of time or attention any issue receives during a legislative cycle. In the real world, party choices can affect each of these margins.

At any point in time, each political party has a total resource budget B^P that they can spend on political framing for a given policy. Expenditure is tied to voter blocks, so that for each party $P \in \{L, R\}$ there is a budget B^P such that:

$$B^P(t) = \sum_i \beta_i^P(t)$$

For the two-block case with equal budgets, $B(t) = \beta_W^P(t) + \beta_B^P(t)$.

At some point in time, t, voter views are a convex combination of that voter's initial beliefs (defined by their cohort) and current party choices of framing:

$$y_{it}(cohort_N) := y_{it}^N = (1 - \phi^N)y_i^* + \phi^N(\beta_{it}^L - \beta_{it}^R). \tag{2}$$

The first term is cohort-specific and static in time, and comes from forward iteration of equation (1). This term represents partial grasp of true policy impact, with cohort perceptions asymptotically converging towards true policy impact. The second term means that for finite policy tenure ($N < \infty$), all new voters complement their impact-based perception of the policy by incorporating the political narrative of the day. If investment is equal between parties, then policy perception is pushed towards zero – the point of political neutrality. Two outcomes are possible when parties invest differently: a partisan policy becomes even more appealing to the party that initially favors it

(when there is relatively more investment by the sponsoring party), or a policy seems less attractive than it otherwise would (when there is relatively more investment from the opposition party).

When a policy is new, existing voters – those already politically aware – have no concrete evidence upon which to base their beliefs about the actual effect of the policy. As a consequence, impressions are shaped *entirely* by political framing of the policy. As time goes by, cohort perceptions for new voters entering the electorate are increasingly dominated by accurate perceptions of the true policy impact, with any fixed level of (net) partisan "spin" correspondingly decreasing in importance. Prior work uses similar learning structures to study inflation expectations (Malmendier and Nagel 2016, Orphanides and Williams 2004).

Parties can target different narratives to different voting blocks, but they cannot customize messages to target specific cohorts within voting blocks. Thus, party choices will be made optimally, taking into account the average views over all cohorts within a voter block, $N \in \{0, 1, 2, 3...\}$:[9]

$$y_{it} := \frac{1}{T} \int_{N=0}^{T} y_{it}^{N} \; dN.$$

Partisan Competition and Equilibrium. The probability that voter block i supports the ideological position of party L is determined by the policy perception of voter block i and an idiosyncratic shock $\xi \sim U[\frac{-1}{2\psi}, \frac{1}{2\psi}]$ such that:

$$P_i^L(t) := Pr(y_{it} + \xi > 0). \tag{3}$$

Each party allocates their framing investments to maximize the expected share of voters, less an electoral cost or backlash proportional to the penetration of their ideological narrative. For the general case with an arbitrary number of voting blocks, parties solve:

$$\max_{\beta_1, ...\beta_k} \sum_i \Big(\alpha_i \times P_i^P - c(\beta_i^P \times \alpha_i \times P_i^P) \Big)$$

where c is a weakly convex function. For two voter blocks and $c(x) = \gamma x$, with $\gamma > 0$, each party maximizes:

[9] For expositional clarity, the paper discussion assumes that individuals who form impressions when the policy launches ($N = 0$) are still alive within the total electorate, but no model conclusions require this. Our proofs in the appendix all rely on the more general case allowing for an arbitrary earliest-exposed cohort of $\underline{N} > 0$.

$$\max_{\beta_W^P} \quad \alpha_W P_W^P + \alpha_B P_B^P - \gamma\Big(\beta_W^P \alpha_W P_W^P + (B - \beta_W^P)\alpha_B P_B^P\Big). \tag{4}$$

We use subgame perfect equilibrium as the solution concept.

3.2 Analysis and Predictions

Our objective is to explore the partisan support across different voter blocks (i) in response to political parties' (P) efforts to sway views of policy y. The model is designed to explore these outcomes at time $t = T$, T years after policy rollout (which occurs at $t = 0$). We call the difference in response across different blocks of voters, *cross-block voter polarization*, and when voter blocks align with racial groups, *racial voter polarization*. We refer to the joint response in racialized political investments and racial voter polarization as the *racial politicization* process.

Proposition 1 (Racialized Political Investment). *For an existing policy y with a race-neutral policy with true outcome $y^* > 0$ and information dissemination parameter ϕ, the equilibrium political investment by race will differ between parties. That is, party optimization will inject racial politicization into a race-neutral policy. Moreover, the difference in political investment by voter block increases with policy tenure, decreases in the share of Black voters, and is pinned down by:*

$$\beta_B^L(T) - \beta_B^R(T) = \beta_W^R(T) - \beta_W^L(T) = y^*(\alpha_W - \alpha_B)\frac{2\left(\frac{1}{\chi(T)} - 1\right)}{3} \tag{5}$$

where $\chi(T) := \chi(T;\phi) = \frac{1-\phi^T}{-\ln\phi} \times \frac{1}{T}$ is a scalar between 0 and 1 that depends on the time-horizon, T, of the model.[10]

Proof: See Appendix Section B. □

Proposition 1's key insight is that in the presence of incomplete information for voters, even when policies are race-neutral, parties will attempt to racialize them. This arises from diminishing returns to party investment in narrative and from the size difference in voting blocks. Because the true policy impact is favored by one party's voters, there is cost asymmetry to shifting views: it is

[10]The first equality in equation (5) holds because parties allocate their full budgets; the model does not permit any intertemporal 'saving' with respect to framing resources.

less costly for the opposing party to marginally reduce enthusiasm than for the favoring party to marginally increase enthusiasm. The payoffs to shifting views are also mediated by voter block size. For the case in which the White block is larger than the Black block, Proposition 1 shows that for a policy initially favored by the Left, without loss of generality, the Right will choose to allocate relatively more of its budget to narrative affecting the larger block (W). Because of asymmetric costs, it will be optimal for the Left to marginally increase investment in the smaller voting block (B), rather than attempt to fully offset the Right's investment in the larger voting block. This generates racial polarization.

There are two key additional predictions. First, racialized investment in narrative should *increase* as policy tenure increases. While potentially counterintuitive, this is because new voter cohorts have more accurate information about the policy and hence larger investment is required to achieve any shift in voter views. This has the effect of enhancing the previous dynamic that generates racial difference in investment by party. Second, because optimal choices are mediated by block-size, Proposition 1 also shows that racialized efforts decrease as the share of the minority block approaches the size of the majority block.

Beyond parties' effort to induce racial politicization into policies, we are interested in evaluating the dynamics of voter polarization across racial lines. We define racial polarization both within-cohort, and across the entire electorate:

Definition 1 (Within-cohort Racial Voter Polarization). For each cohort N, within cohort racial voter polarization is given by:

$$\sigma^N(T) := \Big(P_B^L(N,T) - P_B^R(N,T)\Big) + \Big(P_W^R(N,T) - P_W^L(N,T)\Big).$$

$P_i^P(N,T)$ is cohort-block favorability, defined as in equation (3). Definition 1 says that within-cohort polarization is the difference in cohort-level support summed over each block.

Definition 2 (Average Racial Voter Polarization). Racial voter polarization in the electorate as a whole is averaged across all cohorts, and it is given by:

$$\sigma(T) := \Big(P_B^L(T) - P_B^R(T)\Big) + \Big(P_W^R(T) - P_W^L(T)\Big).$$

Proposition 2 (Racial Voter Polarization). *Equilibrium voter political polarization is given by:*

1. ***Within-cohort racial voter polarization*:**

$$\sigma^N(T) = \frac{8}{3}\psi(\alpha_W - \alpha_B)y^*\phi^N(\frac{1}{\chi(T)} - 1). \tag{6}$$

2. ***Average racial voter polarization*:**

$$\sigma(T) = \frac{8}{3}\psi(\alpha_W - \alpha_B)y^*(1 - \chi(T)). \tag{7}$$

Proof: See Appendix Section B. □

Propositions 1 and 2 generate the following corollaries which provide the basis for our benchmark empirical tests:

Corollary 1 (Comparative Statics). *Consider $y^* > 0$, $\alpha_W > \alpha_B$, and $0 < \phi < 1$. The following comparative statics for racial voter polarization hold in equilibrium:*

1. ***Positive Polarization.*** *Both within-cohort and average racial voter polarization are positive. That is, $\sigma^N(T) > 0$ and $\sigma(T) > 0$.*

2. ***Cross-cohort Polarization.*** *Cohorts with shorter policy tenure (i.e. smaller N) are more polarized than cohorts with longer policy tenure. That is, $\sigma^1(T) > \sigma^2(T)$. This implies that within-cohort racial voter polarization is greatest for those who are already politically aware at the time of policy rollout ($N = 0$).*

3. ***Persistent Polarization.*** *Average polarization does not dissipate by policy tenure increases alone. In particular, $\sigma(T)$ increases as $T \to \infty$, with larger increases arising from cohorts with shorter policy tenures.*

Proof: By inspection of Proposition 2. □

Corollary 1 offers several testable predictions. First, exposure to politicized policies should result in long-lasting racial polarization. Second, this polarization will be greatest between voters who form impressions when a policy is relatively newer, and lower within younger cohorts that become politically aware when the policy has longer tenure. Third, as policy tenure increases, polarization persists as parties increase their investment in narrative in order to impact the less-sensitive younger cohorts.[11]

There exists a literature modeling learning in politically polarized environments (e.g., Alesina et al., 2020, Angelucci et al., 2024). This model is closest in spirit to the insightful general model of ideological competition in Izzo et al. (2023), which is based on a different notion of polarization. In that model, voters are fully informed about outcomes but are uncertain about how policies affect outcomes. Parties develop competing narratives explaining the relationship between a policy and an outcome, and voters adopt the one that best matches their observations. By contrast, in our model, polarization is induced by parties investing in a narrative that does not seek to explain outcomes. Instead, voter knowledge of outcomes percolates slowly, allowing parties to invest in narratives that substitute for this missing knowledge. Another key difference is that our model focuses on polarization across voter blocks, rather than on differences across narratives; we view voter blocks as fundamental to understanding race and politics in the US.

These model approaches are different and potentially apply to different contexts. Where voters are unable to observe or understand the outcomes of policies, they may still be subject to ideological efforts by parties, in line with our model. This might be the case of voters evaluating a *specific policy*. In contrast, where voters are fully informed about outcomes but uncertain about the relationship with policy, Izzo et al. (2023) shows that parties may exploit the uncertainty, which might be the case when voters are evaluating a *specific outcome*. In the context of this paper, the process through which voters absorb new information is key to understanding political attitudes towards specific policies.

4. Data

4.1 FS Rollout and Voting Microdata

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

[11] Of course, the model is for a world with a single policy. In reality, parties shift narratives to new policies over time.

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., 2024, 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 data we compute a measure of voting propensity: the share of elections in which an individual voted (relative to the 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 FS 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.[12] 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.[13] We drop registered voters with missing year of birth, race or county information.

4.2 Other data sources

We use historical data on voting at the county level from ICPSR and Dave Leip's Atlas of US 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 (1995). Finally, we obtain voting data for the U.S. Congress from the DW-NOMINATE

[12] 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.

[13] 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.

project from 1962 through 1974.

To examine changes in partisanship during the rollout we use survey microdata from forty-three nationally representative Gallup Organization surveys conducted between 1958 and 1978. We also explore how political attitudes change over the fifteen years following the rollout (1975–1990) using survey microdata from 31 nationally representative surveys conducted by the Roper Organization. Both sets of survey data are available from the Roper Center (https://ropercenter.cornell.edu/).

4.3 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 views of 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 2 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.

5. Long-Run Effects

In this section, we develop an experience-based empirical analysis showing that exposure to FS rollout causes changes in voting patterns more than five decades in the future. We find empirical evidence that is consistent with several key predictions of the model developed in Section 3.

5.1 Experience DID Design

We begin our empirical analysis by testing the central predictions of our model: that exposure to the FS Program generates long-lasting racial political polarization, and that this affects individuals of voting age at rollout more than it does younger individuals. We compare historical voting patterns, observed as of 2020, for people exposed to FS rollout as adults (18+) vs. same-age individuals who lived in a county that implemented FS before they became eligible to vote, employing a difference in differences (DID) design to evaluate an experience effect. We identify the 'treatment' as being of voting age and living through a major safety net regime-shift: for the first time, food assistance is made widely available to adults on the basis of poverty alone, without any trigger requirements associated with children or physical disability. Untreated individuals are those who attain majority in a world where the FS Program is already a feature of the civic landscape.

This empirical design implies that the moment of 'impression formation' outlined in our model happens discontinuously at age 18. A DID design requires us to take a position on when in life individuals begin to form impressions of salient social policy. We view age 18 as the most natural single choice: the legal age of majority on multiple social dimensions – including, for much of our sample, the ability to vote.[14] However, as the FS Program impacts many families with children, attitudes may be formed before age 18. We begin with the simpler approach of a sharp discontinuity around the age of majority. If some individuals internalize the policy landscape prior to age 18, our sharp-discontinuity estimates will represent the effect arising from something less than one full "unit" of policy change, attenuating the estimates towards zero. In Section 5.4, we employ a specification that relaxes this assumption of all-or-nothing treatment around age 18. We find support for some degree of partial treatment below the age of majority, but overall find similar qualitative results. This suggests that both designs are similarly effective at measuring the degree of long-run polarization actually induced by the politicization of FS.

[14] Most of the United States changed the voting age from age 21 to 18 in 1971, pursuant to the 26th Amendment. Supporting our belief that age 18 is a natural inflection point, the historical record shows that this choice arose from a strong belief that 18 year-olds were both aware of and affected by national policy – most saliently with respect to the military draft.

Our 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.

We begin with the following specification:

$$Y_{ic} = \beta FS_{ic} + \alpha_c + \gamma_{b(i)} + \epsilon_{ic}, \tag{8}$$

where i indexes the individual, c their county, and b their birth year, so that the specification includes fixed effects for county (α_c) and birth year ($\gamma_{b(i)}$).

FS_{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*. (As noted, in Section 5.4, we present results that relax this assumption of all-or-nothing treatment around age 18.)

Equation 8 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 first 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:

$$\underbrace{\Big(E[Y_{icb} \mid FS(C) - B \geq 18] - E[Y_{icb} \mid FS(C) - B' < 18]\Big)}_{\text{differences due to treatment effect and age}} - \underbrace{\Big(E[Y_{icb} \mid FS(C') - B < 18] - E[Y_{icb} \mid FS(C') - B' < 18]\Big)}_{\text{differences due to age only}} \tag{9}$$

$FS(C)$ 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 (i.e., a person 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 due to age. The second line represents a comparison between individuals of the same ages in a different county 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 8, 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.[15]

Now we return to the issue of TWFE bias. Full-sample estimates from the regression specification of equation 8 would use all possible comparisons across every combination of county pair

[15] $\hat{\beta}$ is identified directly from county- and year-pair combinations for which the data contains observations corresponding to each term in expression 9. 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.

(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 treatment heterogeneity or effects that change with time-since-treatment. In our setting, birth year replaces time as one of the two DID margins, so if treatment is heterogeneous with respect to age at treatment then TWFE estimates may be biased.[16]

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), which allows us to estimate interacted treatment effects. We focus on racial heterogeneity with respect to FS treatment, which we estimate by interacting race with the FS indicator, so that our base TWFE specification becomes:

$$\begin{aligned} Y_{ic} =& \beta_1 FS_{ic} + \beta_2(FS_{ic} \times Black_i) + \beta_3(FS_{ic} \times Hispanic_i) + \beta_4(FS_{ic} \times Asian_i) + \lambda_{r(i)} \\ &+ \alpha_c + \gamma_{b(i)} + \epsilon_{ic}, \end{aligned} \tag{10}$$

where i indexes the individual, c county, b birth year and r race (Black, Hispanic, or Asian, with White as the omitted category) and $\lambda_{r(i)}$ denotes race indicators. This specification includes fixed effects for county (α_c) and birth year ($\gamma_{b(i)}$). The coefficients on FS_{ic} interacted with race indicators (β_2, β_3, and β_4) estimate the differential effect of rollout at voting age by race.

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.[17] We interact all fixed effects (birth year, county, race) in equation 10 with stack fixed effects to produce the specification we use for our estimates.

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 ($\gamma_{i(b)}$) does two things. First, it ensures that identifying variation comes only from people belonging to the same birth year, so our estimates are not driven by comparing people who have been exposed to a different set of

[16] Equation 9 focuses on one treated individual (left term, first line), and three untreated individuals. The problematic comparisons here 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.

[17] We use a bandwidth of five birth years such that the oldest individuals are 23 (with no constraint on the youngest); this implies that our final stack is for rollout in 1970.

historical events over their lifetimes. Second, because all our data is from 2020, we do not observe a birth year cohort at multiple points in time, which means that a birth year fixed effect *also* defines age. Therefore, $\gamma_{b(i)}$ 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. 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.[18] 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.

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

The core predictions of our model are encapsulated in Corollary 1.1 and 1.2, which describe polarization in terms of racial differences in support for policies. We use two empirical proxies for the model objects P_W^P and P_B^P: party affiliation and propensity to vote (turnout) conditional on party. We estimate treatment effects by race – i.e., within voter block. An increase in propensity for Black voters to register (or vote) as Democrats, therefore, would correspond to the model quantity $(P_B^L - P_B^R)$.

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.

[18] 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). Section 5.2.2 restricts attention to a subsample where this measurement error will be smaller, and finds larger estimates, as one would expect.

Given the salience of race in the politics of the implementation period, our next step is to disaggregate these effects by racial groups.[19] Panel B of Table 1 adds race fixed effects and their interactions with the FS variable to equation 8, 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 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.

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

[19] 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%.

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 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). 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.

5.2.1 Heterogeneity by Gender and Regional Demographics

Given the explicit emphasis on gender invoked by nationally prominent "Welfare Queen" narratives in the 1970s, it is 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.

We also 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 IA1 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 IA1 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.

5.2.2 Stronger Effects for More Politically Engaged Individuals

Table 4 explores the long-run effects of treatment on individuals who registered to vote early in life – i.e., before the age of 25. 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.[20] The effect on turnout propensity by party is also larger, though by a smaller multiplier.

Disaggregating the response of individuals registering before the age of 25 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 4 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 younger-registering subsample, Asian Democrats are more likely to vote by 5.5pp.

[20] The general increase in magnitudes evident in Table 4 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 likely that overall migration is lower in this subsample, meaning in turn that we are less likely to have classification errors with respect to county-of-residence at age 18.

5.2.3 Robustness: More Granular Fixed Effect Specifications

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 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 IA2. 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 IA3 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. 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 IA4: again, the direction of treatment effect is consistent with our core findings, and magnitudes increase somewhat relative to Table 1 and 2.[21] In both these instances, the increased treatment effects that we find when allowing for race-specific controls by age or by region suggest that our core specification may downplay the effect of FS exposure by assuming that differences in lived experience by race are separable from differences generated by location and age. That is: the robustness tests of Appendix Tables IA3 and IA4 suggest intersectional effects of race with both age-cohort and geography that are somewhat occluded by our core specification.

Finally, in Appendix Table IA6 we interact all three fixed effect vectors used in our baseline specification: County, Birth Year and Race. As was the case for Table IA2, these absorb the

[21] In Appendix Table IA5 we further replace county × race FE with census block × race FE; estimates are similar, but slightly smaller than those in Table IA4.

treatment variable (*Food Stamps*), but we are still able to estimate $FS \times Race$ coefficients. The pattern of results is very similar to those in our baseline specification, with Black voters moving leftwards, but estimated magnitudes are substantially higher.

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 overall patterns are that White voters exposed to the rollout move towards the Republican party (a political backlash effect), while 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 somewhat differently to treatment, in that it appears to push them away from the Democratic party and voting, and towards registering as Independents.

5.3 Event-study DID by age bins

The model in Section 3 predicts that a group-neutral policy will generate cross-cohort variation in the extent of political polarization. Specifically, the model predicts that polarization by race will be larger for cohorts learning about the policy at its onset, relative to cohorts who become old enough to vote in a world where the policy is already well-established. To test this, we split the FS 'treatment' into five-year birth cohort bins in the following event-study version of our main specification:

$$\begin{aligned} Y_{ic} = &\sum_{j=1}^{n} \beta_j Cohort_{icj(i)} + \sum_{j=1}^{n} \lambda_j (Cohort_{icj(i)} \times Black_i) \\ &+ \alpha_c + \gamma_{b(i)} + \lambda Black_i + \epsilon_{ic} \end{aligned} \tag{11}$$

where, as before, i indexes individuals and c counties. $Cohort_{icj(i)}$ is an indicator for individuals in birth cohort bin j that were of voting age (18+) when the FS Program rollout occurred in their county. We run the stacked version of this specification, interacting all fixed effects with a stack indicator. Whites and Blacks constitute the largest racial groups at rollout by a large margin, so for ease of exposition we drop other races and plot the coefficients for $Cohort_{icj(i)}$ and $Cohort_{icj(i)} \times Black_i$ in Figure 3. The baseline category is the cohort furthest to the right in the figure, which corresponds to the youngest cohort.

Consistent with the predictions of the model, older cohorts display greater political polarization, both within-race and across race. Figure 3 shows political registration on the vertical axis, with positive values corresponding to a Republican lean and negative values to a Democratic lean. The figure shows that treated White voters (the upper set of plotted coefficients) born in earlier cohorts (points further to the left) are more likely to have shifted towards the political right relative to the baseline cohort, and this effect decreases to zero as cohorts become younger. The second coefficient from the left corresponds to the age bin that was just eligible to vote (around age 18) in the rollout year; the estimates for this cohort and the older cohorts to the left are of similar magnitudes, indicating limited treatment heterogeneity across age categories for fully treated age bins.

However, the figure also shows that cohorts that were not adults at rollout – and which we have considered untreated until now – also appear to have received some treatment. The four estimates to the right of leftmost vertical line indicate that White voters who were children at rollout experience treatment effects of similar magnitudes – albeit slightly less precisely estimated – to those who were adults at rollout.

Estimates for the differential effect of treatment on Black voters are plotted in the the bottom set of coefficients in Figure 3. Older cohorts shift markedly towards the Democratic party in response to treatment, while the estimates for younger cohorts converge towards zero, in accordance with the model's prediction. As was the case for White voters, cohorts who were children at rollout – and even some cohorts who were not yet born – show evidence of treatment, suggesting some inter-generational transmission of the experience of older cohorts. This supports estimating a fuzzy treatment model relaxing the assumption that only those of voting age can experience the FS treatment. We now turn to such a model.

5.4 Experience DID with Fuzzy Treatment

Thus far we have assumed a sharp delineation in treatment at FS onset around the age of 18. This is a natural breakpoint, comparing individuals 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 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 of partial treatment around age 18. As 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 compared to a 17 year-old in that same county. Treatment effect in the DID design is identified by comparing lifetime voting patterns between these two individuals (together with the second difference between 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 entirely 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 likely at 18 than at other ages – 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 younger 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 write as FS_{age} for notational simplicity (note that this is an individual-level variable). Treatment is then defined as follows:

$$FS_{ic} = \begin{cases} 1, & \text{if} \quad FS_{age} \geq 18 \\ \left(\frac{FS_{age}-(18-L)}{L}\right)^{\lambda}, & \text{if} \quad 18 - L \leq FS_{age} < 18 \\ 0, & \text{if} \quad FS_{age} < 18 - L \end{cases} \tag{12}$$

As before, treatment is 1 for those who have attained voting age. 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 8.

We simultaneously estimate L, λ, and $\{\hat{\beta}\}$ from the data. In particular, we choose L and λ to

minimize the joint sum of squared residuals of individuals' choice of political affiliation conditional on their race and exposure to FS. That is, L and λ are chosen to minimize the joint sum of squared residuals of our main specification (equation 10, used in Table 1B), 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, consistent with the event study dynamics in Figure 3.

Table 5 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, while for minorities, the relative effect is largely the same when estimated using either sharp or fuzzy treatment. Overall, this fuzzy-treatment framework yields qualitatively similar estimates to the sharp-treatment framework.

6. Survey Evidence on voter beliefs

In our model, long-lasting polarization of the type we document in Section 5 arises from political narratives that change people's perception of the policy (and the party associated with it). In this section, we show direct evidence that FS rollout changed political beliefs and partisan views. We analyze repeated nationally representative surveys, and show racial polarization in political beliefs both at the time of rollout and in the subsequent two decades. This links the the mechanism outlined in our model and the empirical analysis of long-run voting patterns, illustrating that exposure to policy rollout changes political attitudes.

6.1 Changes to Voters' Political Beliefs over the FS Rollout window

In this subsection, we focus on changes in political beliefs – as captured by expressed support for the Republican and Democratic parties – that are contemporaneous with rollout of the FS Program. To do this, we use microdata from Gallup surveys spanning the rollout period to construct

dependent variables that capture respondents' support for the Democratic party: either in Congress (Panel A of Table 6) or in the Presidency (Panel B). In this analysis, the FS variable is the share of the state population living in counties where program rollout has occurred in each year, because county is not available in these surveys. We run a standard DID analysis, where the differences are time (i.e., before vs. after the year of the rollout) and FS implementation. This specification includes state, year, and respondent age (birth year) fixed effects.

Column 1 of Table 6 Panel A indicates that as FS coverage increases, average support for the Congressional Democratic party falls: moving from zero to full FS coverage is associated with a fall of around 21 percentage points in the support for congressional Democrats. This average effect is driven by the White voters that make up the majority of the electorate. In contrast, the beliefs of Black voters show an opposing effect. Specifically, Black support for congressional Democrats is 80 percentage points higher in states with FS coverage relative to those without coverage, all else equal. Columns 2 through 4 report results for subsamples of self-identified Democrats, Republicans and political independents and display a consistent pattern of Black voters shifting towards the Democrats as FS coverage rises. This is true for Black Democrats, but more so for independents.

Panel B examines how support for Democratic presidents, or lack of support for Republican presidents depending on the survey year, changes as the FS Program is implemented. This outcome variable is likely a noisier measure of political beliefs because Democratic presidents are not always in office and because presidents are assessed on a variety of dimensions, including their personal charisma. Nonetheless, the patterns are similar to those in Panel A, especially for the headline result: a shift of Black voters towards the Democratic party as the FS Program is rolled out.

6.2 Changes to Voter Beliefs after the FS Rollout window

In this section, we test whether the FS rollout affects the beliefs of survey respondents over the fifteen years following the end of the rollout. We use 31 nationally representative surveys collected by the Roper organization between 1975 and 1990, which ask respondents to describe their political outlook as conservative, moderate, or liberal. We employ a stacked difference-in-differences (DID) design mirroring the fuzzy treatment approach used for the long-run results in Table 5.[22] In this stacked DID, the first difference is based on the treatment status of respondents, while the second

[22] A fuzzy treatment framework allows the data to determine the degree of treatment received by individuals who were not adults at rollout. Fuzzy treatment is more appropriate for the 1975–1990 period because, as Figure 3 shows, all the cohorts that were of survey-eligible age at the time appear to be partially treated (on the left half of the Figure).

difference is by their geography. However, unlike the specification in Table 5, which uses data from October 2020, the table also includes a version with year fixed effects to absorb variation across survey years.

The results of this analysis are reported in Table 7. While there is no average effect beyond a suggestive but small and statistically insignificant increase in conservative views, there is a large effect for Black respondents. FS exposure for Black voters drives a strong shift away from a conservative political outlook (a drop of around 40 percentage points), and towards moderate views (an increase of around 30 percentage points). These results are consistent with the long run results, suggesting that changes to voters' political beliefs constitute a channel through which the FS Program generated long run political polarization along racial lines.

7. Shifts in Partisan Views: Short and Medium-Term Impact

7.1 Food Stamps and Elections

Consistent with the impact on political beliefs in the rollout period, we find that FS rollout also affects short-run voting outcomes. We exploit the period of active rollout between 1961 and 1975 to test directly for impact on voting-age adults. Because this design looks at outcomes from the 1960s and 1970s, we have access to a control group voters untreated by the FS Program in a way that is impossible when considering outcomes in 2020, fifty years after the program attained national coverage. Exploiting this sharp variation during the rollout years allows us to provide an additional evidence on linkages between policy implementation, belief changes, and ensuring political polarization. While we focus on differences by race, two of our analyses parallel findings in Kogan (2021) which also looks at short-run impacts of the Food Stamps Program; we highlight these two specifically below.

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 base specification is:

$$y_{ct} = FS_{ct} + \alpha_c + \gamma_t + + \epsilon_{ct} \tag{13}$$

where FS_{ct} takes a value of 1 once a county has adopted the FS Program, and α_c and γ_t are county and year fixed effects, respectively. This DID design is also subject to the TWFE bias discussed in Section 5 so, in this section, we use a bias-robust estimator following Callaway and Sant'Anna

(2021) (CS henceforth).[23]

We begin by examining whether the county-level Food Stamp (FS) program rollout affected voter registration. Table 8 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 8 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 the difference between treated and control counties is small and stable. Around rollout the difference falls to zero, and is followed by a growing decline in turnout, reaching around 7pp by the second election post-rollout, consistent with political disaffection in treated counties.[24]

Given that the Food Stamps policy was associated with the Democratic party, we then explore the electoral consequences of the FS rollout. Some of this analysis is very similar to Figure 4 and Table 3 of Kogan (2021). In panel (a) of Figure 4, 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, in the pre-period the estimates are economically small. However, in the second election following FS rollout there is a sharp increase in the difference between Democratic and Republican vote

[23]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 to the period preceding treatment 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.

[24]This analysis parallels Table 5 of Kogan (2021), which reports increases in turnout rather than decreases. The difference arises from our use of a bias-robust TWFE estimator. With a standard (biased) TWFE regression, we also find increases in turnout. Panel (b) of Appendix Figure IA1 reproduces the Figure excluding the latest adopters to show that results are not sensitive to the final control cohort.

shares to around 10pp, an advantage which persists in the two subsequent elections.

Panel A of Table 9 reports the average DID estimate over the post period. 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. In light of the increased rate of Black voter registration in southern states documented in Table 8, columns 2 and 3 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 (column 4) – perhaps because political participation tends to be lower among the poor (Schaub, 2021), or because the marginal voter in high-poverty counties may be 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 9 confirms that treatment did not, in fact, affect the average likelihood of Democratic electoral victory. This average masks underlying heterogeneity: High Black-population counties show a 7pp increase in Democratic win likelihood. Appendix Figure IA2 corroborates this, displaying event study estimates that show a sustained increase Democratic win probability in counties with a high Black share starting from the second Congressional election following rollout. In contrast, 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 victory 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 4), in conjunction with no increase in Democratic vote share, would be consistent with political 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. Figure 5 shows event-

study estimates for the share of Black elected officials between 1960 and 1975. We see a gradual increase in the percentage of Black elected officials of about 0.7pp, starting three years after FS implementation.

7.2 Food Stamps and Congressional Voting Behavior

By changing registration and voting behavior, expansion of the 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 10 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 inferred from their roll-call votes. 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 reinforces the natural interpretation of our long-run findings: adult exposure to FS does indeed directly shift the behavior of those who experience this change in the national policy landscape.

8. How Other Events Affect Polarization Arising From Food Stamps

This section considers historically salient factors capable of amplifying or mitigating the long-run polarization associated with the FS program. We first explore the interaction with contemporaneous changes in electoral policy, specifically how a major historical event that occured during the rollout – the Voting Rights Act of 1965 – affected polarization. Second, we explore whether the economic

health of the county mediates long-run politicization. Finally, we consider how the presence of local churches impacts the long run consequences of the FS program for political behavior.

8.1 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).[25] To 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 11, 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 Black and Hispanic

[25] 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.

voters (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.[26]

Taken together, the evidence in Table 11 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.

8.2 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); the economic cycle also affects support for redistribution (Brunner et al., 2011). Recessions may 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 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.

[26] 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.

Table 12 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.

8.3 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.[27] Second, Christian theology promotes help for the poor, which may support political views in favor of public programs like FS.[28] Third, Churches may reduce the perceived need for a FS Program if they already operate a community-based safety net (e.g., see Scheve and Stasavage, 2006).

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 IA7 presents the estimates. The first thing of note is that the coefficients on *Church Density* $\times$ *Race* support the view that churches served to mobilize minority voters in this period, with high church density areas displaying far higher rates of Democratic registration (and voting), and the opposite pattern for Republicans. 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 *FS* $\times$ *Church Density* $\times$ *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* $\times$ *Church Density* $\times$ *Race* is not positive.

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

[28] 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).

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.

9. Conclusion

We study racial politicization induced by the original addition of one of the major pillars of the U.S. social safety net: the Food Stamps Program. This paper shows that exposure to the FS rollout affected political engagement, increased polarization along racial lines, and affected voting outcomes. The fact that the FS rollout happened over fifty years ago allows us to explore not only the short term effects, but also their persistence; 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 process and consequences for voter behavior of politicizing major public policies. We trace the process of politicization of the policy both theoretically and empirically. Theoretically, we model the politicization process and show that, when knowledge about a policy's impact develops slowly, policies that are *prima facie* group-neutral policy can generate political polarization across different voter groups, that polarization is larger for cohorts learning about the policy when recently implemented, and, crucially, that political polarization persists over time. We then show empirically how each implication of this process played out in the context of the Food Stamp Program rollout.

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 impact a policy's design, effectiveness, and long-term viability. For example, policy implementation may be impaired in areas where a negative narrative has taken hold, leading to outcomes that reinforce the hostile narrative. Additionally, by distorting voters' perception of policy outcomes, politicization weakens their ability to hold elected representatives accountable for

their performance – a critical component of some theories of democratic accountability (Key, 1966, Fiorina, 1981). We leave this and other potential consequences of politicization of public policy for future research.

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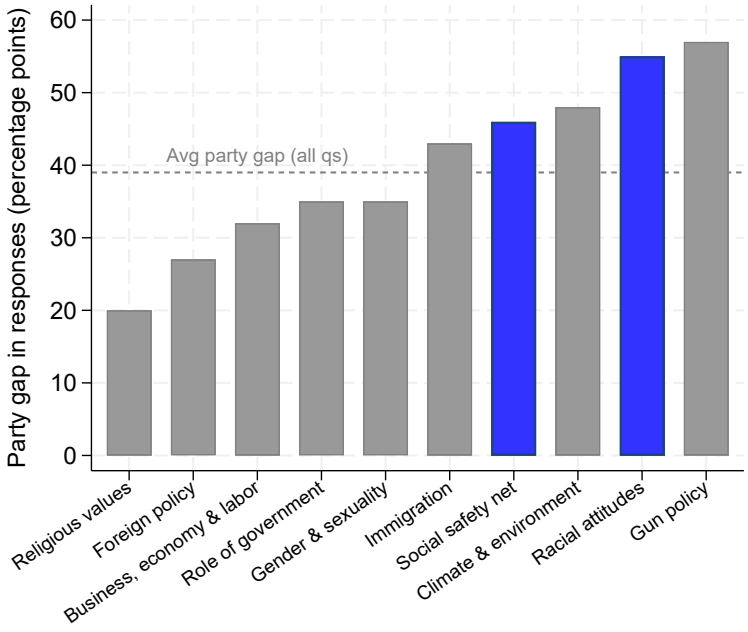
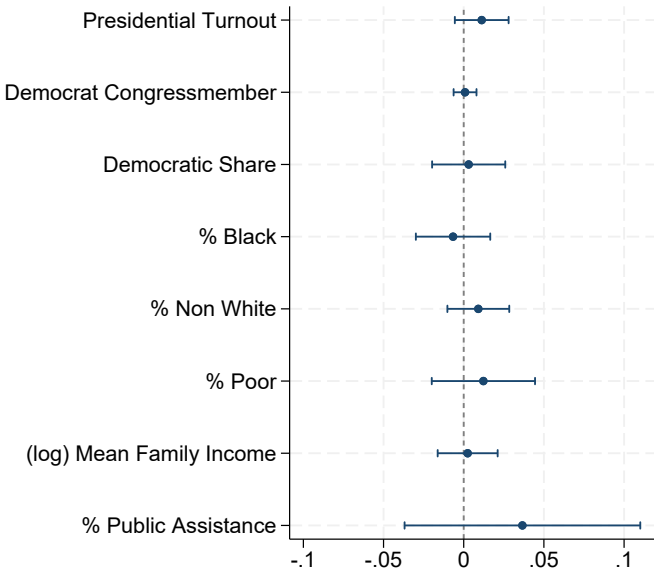
Figure 1: Political polarization by topic

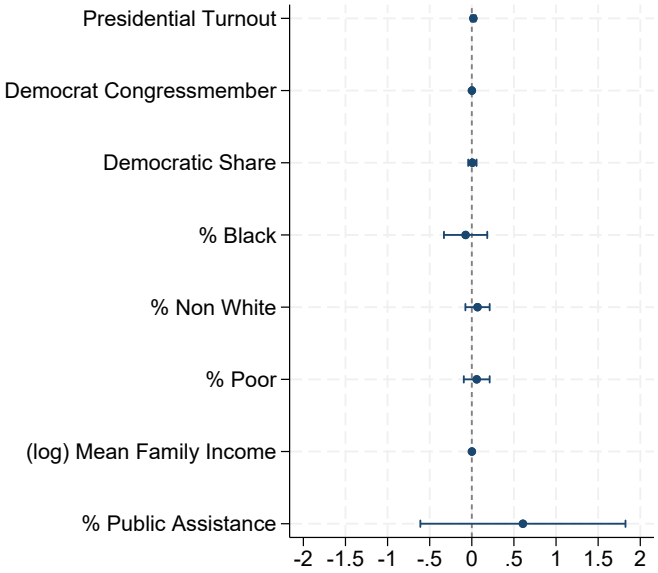


Note: This figure presents survey evidence from Pew Research Center's American Trends Panel, conducted September 3–15, 2019 (Pew Research, 2019). Each column represents the average differences between Republican and Democratic respondents to all questions on that topic.

Figure 2: Predicting rollout timing using pre-rollout county characteristics



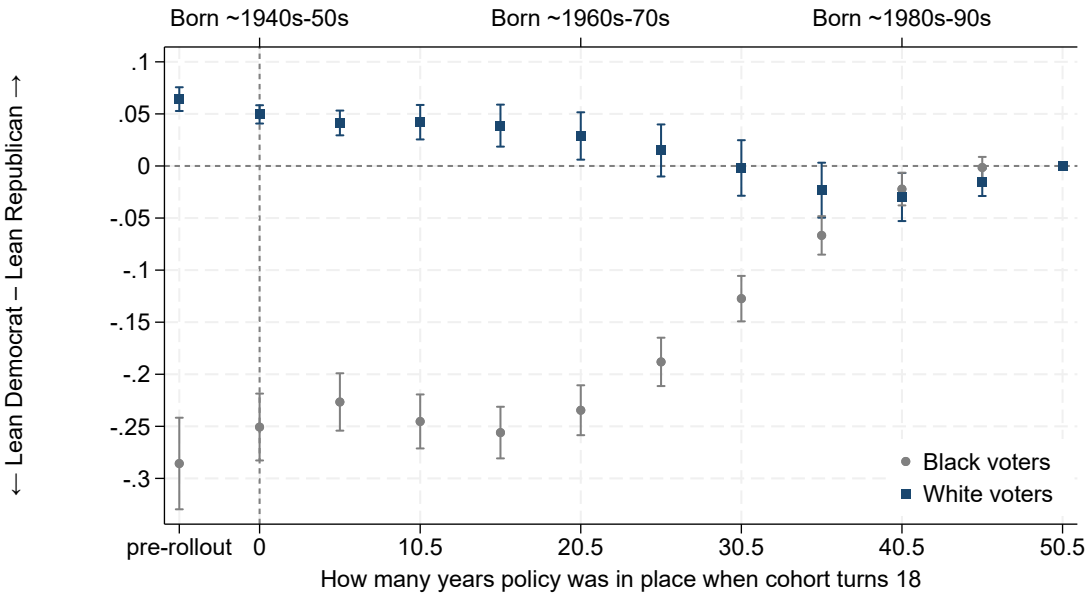
(a) County characteristics in natural units



(b) County characteristics as a fraction of sample average

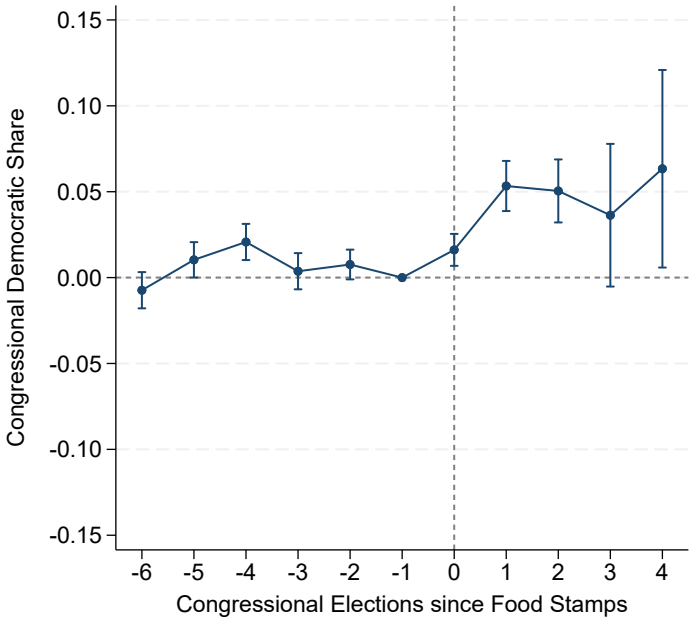
Note: This figure presents coefficients from regressions predicting Food Stamp rollout in a county for a given year based on a pre-determined characteristic (listed on vertical axis) and a year fixed effect. Each coefficient estimate is from a separate regression. County characteristics are measured 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 value. Some coefficients have such small confidence intervals that they are not visible in the Figure. 95% confidence intervals are clustered by county.

Figure 3: Long-run effects on voter registration: Event study by birth cohort bins

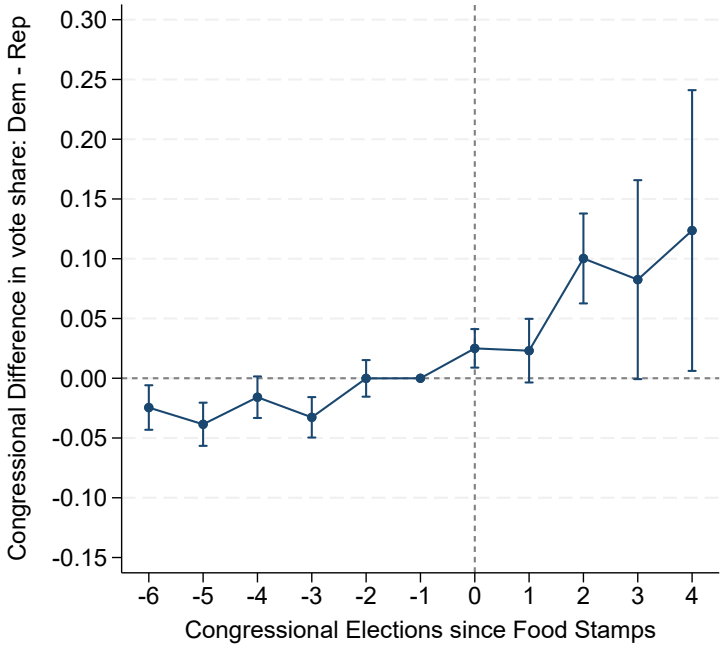


Note: This figure plots the event-study coefficients from a stacked version of equation 10. Each coefficient corresponds to the estimated treatment effect for a five year birth cohort bin (cohort). All estimates are relative to the youngest cohort for each race (at far right). Cohorts on the left of the Figure are older than those on the right. The top horizontal axis indicates a cohort's age at policy rollout; negative values indicate cohorts that were not yet born at the time of rollout. The vertical line corresponds to the cohort aged 18 when the program was rolled out; cohorts to the left were older. The bottom horizontal axis shows how long the policy had been in place when a cohort is first eligible to vote: policy tenure. The third coefficient from the left corresponds to the cohort which was around 18 years old at rollout (i.e., at 0 on the lower horizontal axis and at 18 on the upper axis). The top series of coefficients corresponds to White voters and the bottom series to Black voters. The dependent variable (vertical axis) is voter registration projected onto a -1 to +1 scale; positive values indicate Republican leaning, and vice versa. 95% confidence intervals are clustered by county.

Figure 4: Event studies: elections



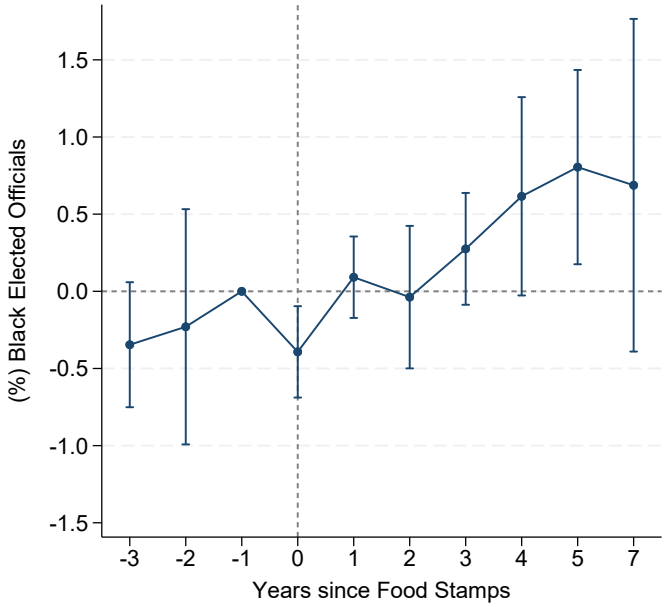
(a) Democratic vote share in elections to U.S. Congress



(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 5: 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 (1995). 95% confidence intervals 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 FE	Y	Y	Y
Birth year 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 FE	Y	Y	Y
Birth year FE	Y	Y	Y
Race 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 the stacked version of equation 8 as described in Section 5; Panel B adds race fixed effects and their interaction with the FS variable. White is the omitted racial/ethnic group. FE denotes fixed effects. 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 FE	Y	Y	Y	Y
Birth year 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 FE	Y	Y	Y	Y
Birth year FE	Y	Y	Y	Y
Race 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 racial/ethnic group. The specification corresponds to the stacked version of equation 8 described in Section 5. FE denotes fixed effects. 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 FE	Y	Y	Y	Y	Y	Y	Y
Birth year 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 FE	Y	Y	Y	Y	Y	Y	Y
Birth year FE	Y	Y	Y	Y	Y	Y	Y
Race 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 racial/ethnic group. Standard errors in parentheses are clustered by county. *** 1%, ** 5%, * 10% significance level.

Table 4: Long-run effects

Individuals registering to vote 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 FE	Y	Y	Y	Y	Y	Y	Y
Birth year FE	Y	Y	Y	Y	Y	Y	Y

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 FE	Y	Y	Y	Y	Y	Y	Y
Birth year FE	Y	Y	Y	Y	Y	Y	Y
Race 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 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 racial/ethnic group. Standard errors in parentheses are clustered by county. *** 1%, ** 5%, * 10% significance level.

Table 5: 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 FE	Y	Y	Y	Y	Y	Y	Y
Birth year FE	Y	Y	Y	Y	Y	Y	Y
Race FE	Y	Y	Y	Y	Y	Y	Y

Note: This specification incorporates a fuzzy-treatment framework as described in Section 5.4: *FS* now denotes a *continuous* treatment variable. White is the omitted racial/ethnic group. Standard errors clustered by county. *** 1%, ** 5%, * 10% significance level.

Table 6: Survey evidence: voter support for political parties (1958–1978)

Panel A: Support for Democratic party in Congress

	(1) All Affiliations	(2) Democrats	(3) Republicans	(4) Independents
Food Stamps (FS)	-0.206**	-0.083**	-0.042	-0.003
	(0.088)	(0.032)	(0.068)	(0.175)
FS × Black	0.801***	0.133***	0.748	0.950***
	(0.139)	(0.036)	(0.539)	(0.182)
Black	0.304***	0.045***	0.123**	0.239**
	(0.079)	(0.014)	(0.057)	(0.096)
N. obs.	34,046	17,677	10,447	5,920
N. clusters	50	50	50	50
Year FE	Y	Y	Y	Y
State FE	Y	Y	Y	Y
Birth Year FE	Y	Y	Y	Y

Panel B: Approval of Democratic president

	(1) All Affiliations	(2) Democrats	(3) Republicans	(4) Independents
Food Stamps (FS)	-0.065	-0.005	-0.122	-0.058
	(0.069)	(0.076)	(0.080)	(0.092)
FS × Black	0.253***	0.125	0.379**	0.156
	(0.066)	(0.077)	(0.153)	(0.095)
Black	0.346***	0.194***	0.455***	0.383***
	(0.045)	(0.056)	(0.065)	(0.066)
N. obs.	59,193	27,307	16,141	15,737
N. clusters	50	50	50	50
Year FE	Y	Y	Y	Y
State FE	Y	Y	Y	Y
Birth Year FE	Y	Y	Y	Y

Note: This table reports DID estimates of the effect of FS implementation on survey measures of support for the Democratic party. The outcome variable in panel A is an indicator for responding *"the Democratic party"* when asked *"If the elections for congress were being held today, which party would you like to see win in this congressional district–the Democratic party or the Republican party?"* If 'undecided or refused,' they were asked: *"As of today, do you lean more to the Republican party or more to the Democratic party?"* The outcome variable in Panel B is an indicator for approval in response to the question *"Do you approve or disapprove of the way 'last-name-of-president' is handling his job as president?"* We multiply responses by negative 1 when the president is a Republican to ensure consistent interpretation across administrations. The data is at the survey respondent × year level. The FS variable is the share of the state population living in counties where program rollout has occurred in each year because county is not available in these surveys. Columns 2, 3 and 4 report estimates from subsamples of individuals identifying as Democrats, Republicans or Independents when asked: *"In politics as of today, do you consider yourself a Republican, Democrat, or Independent?"* Racial groups other than White and Black are not consistently recorded in the surveys, so the omitted racial category includes all non-Black respondents. The survey microdata is from nationally representative surveys conducted by the Gallup Organization between 1958 and 1978 (17 for Panel A and 26 for Panel B) and is provided by the Roper Center (https://ropercenter.cornell.edu/). Standard errors in parentheses are clustered by state. *** 1%, ** 5%, * 10% significance level.

Table 7: Survey evidence: voter ideology (1975–1990)

	(1) Liberal	(2) Moderate	(3) Conservative	(4) Liberal	(5) Moderate	(6) Conservative
Food Stamps (FS)	-0.026	-0.055	0.081	-0.045	-0.004	0.049
	(0.077)	(0.072)	(0.068)	(0.041)	(0.060)	(0.066)
FS × Black	0.118	0.306***	-0.424***	0.106	0.290***	-0.396***
	(0.078)	(0.101)	(0.089)	(0.077)	(0.105)	(0.090)
Black	0.042	-0.369***	0.327***	0.050	-0.354***	0.305***
	(0.069)	(0.094)	(0.079)	(0.068)	(0.098)	(0.079)
N. obs.	37,581	37,581	37,581	37,581	37,581	37,581
N. clusters	224	224	224	224	224	224
County FE	Y	Y	Y	Y	Y	Y
Birth-Year FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	N	N	N

Note: This table reports stacked DID estimates of the effect of FS implementation on voters' ideological position on a conservative-liberal spectrum. The outcome variables in this table are named at the top of each column and are indicators based on responses to the following survey question: *"Now, thinking politically and socially, how would you describe your general outlook–as being very conservative, moderately conservative, middle-of-the-road, moderately liberal, or very liberal?"* We group responses of *"very"* and *"moderately." FS* is a continuous FS exposure measure, as in Table 5. Survey year fixed effects (FEs) are included in columns 1–3. County is not available in these surveys, so we combine state and city-size to form unique pseudo-counties, which we refer to as counties. Racial groups other than White and Black are not consistently recorded in the surveys, so the omitted racial category includes all non-Black respondents. The survey microdata is from 31 nationally representative surveys conducted by the Roper Organization (once in 1975 then twice yearly from 1976 through 1990) and is available at https://ropercenter.cornell.edu/. Standard errors in parentheses are clustered by county. *** 1%, ** 5%, * 10% significance level.

Table 8: 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	0.010***	-0.010**	0.013***	-0.004
	(0.003)	(0.005)	(0.005)	(0.012)
N. obs.	1,388	1,388	1,062	1,062
N. clusters	443	443	443	443
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 9: Short-run effects
Congressional elections

Panel A: Democratic vs. Republican Vote Difference					
	(1)	(2)	(3)	(4)	(5)
	Baseline	% Black Pop.		Poverty Share	
		High	Low	High	Low
Food Stamps	0.074***	0.133***	0.070***	0.009	0.081**
	(0.018)	(0.040)	(0.019)	(0.033)	(0.033)
N. obs.	24,103	5,379	13,164	5,012	6,491
N. clusters	1,801	585	1,017	552	557
Year FE	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y

Panel B: Likelihood of a Democratic Win					
	(1)	(2)	(3)	(4)	(5)
	Baseline	% Black Pop.		Poverty Share	
		High	Low	High	Low
Food Stamps	-0.038	0.071***	-0.091**	-0.114**	-0.028
	(0.029)	(0.027)	(0.038)	(0.055)	(0.056)
N. obs.	24,103	5,379	13,164	5,012	6,491
N. clusters	1,801	585	1,017	552	557
Year FE	Y	Y	Y	Y	Y
County FE	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 10: 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	-0.002	0.013	0.002	0.025***	0.023	0.020**	0.039**
	(0.019)	(0.009)	(0.009)	(0.009)	(0.014)	(0.009)	(0.017)
N. obs.	19,855	10,583	7,760	4,420	459	3,951	674
N. clusters	1,781	1,635	1,423	566	317	540	315
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. Standard errors in parentheses are clustered by county. *** 1%, ** 5%, * 10% significance level.

Table 11: 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 FE	Y	Y	Y	Y	Y	Y	Y
Birth year FE	Y	Y	Y	Y	Y	Y	Y
Race 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 racial/ethnic group. Standard errors in parentheses are clustered by county. *** 1%, ** 5%, * 10% significance level.

Table 12: 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
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)
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)
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 FE	Y	Y	Y	Y	Y	Y	Y
Birth year FE	Y	Y	Y	Y	Y	Y	Y
Race 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.

Appendix

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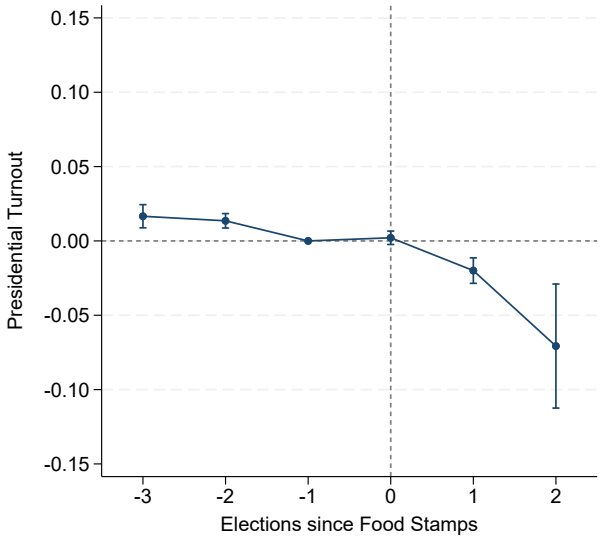
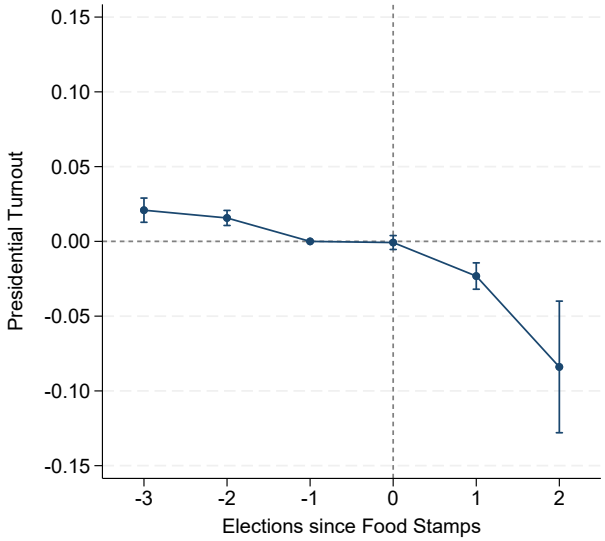
A. Appendix Figures and Tables

A.1 Event Studies

Figure IA1: Event study: Turnout



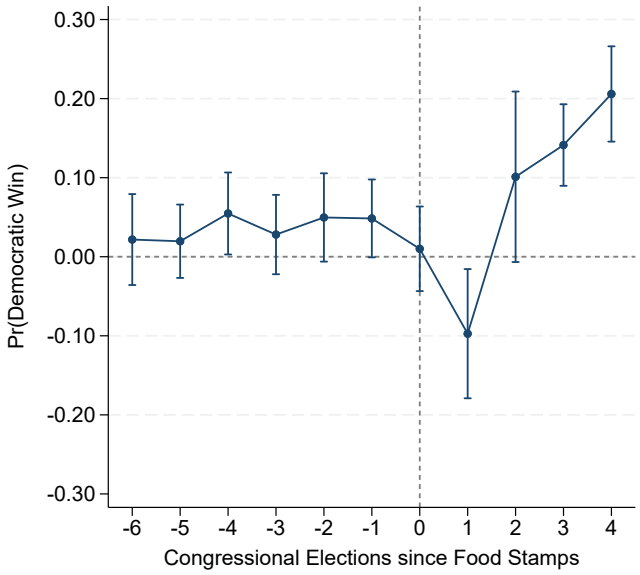
(a) Presidential Turnout



(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.

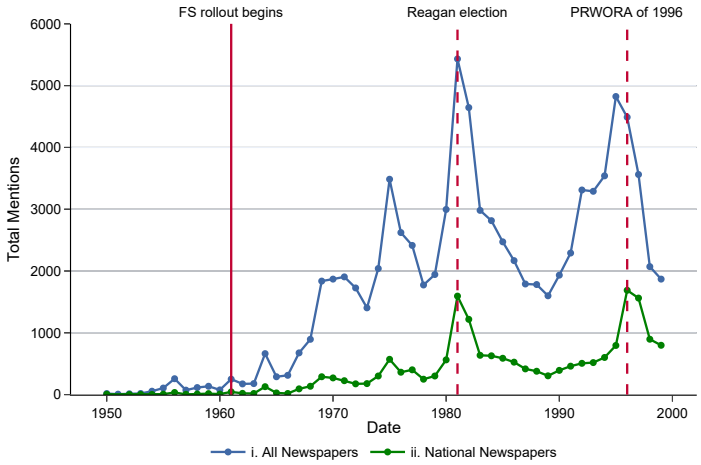
Figure IA2: Event study: Likelihood of Democratic Win in Counties with a High Black Population Share



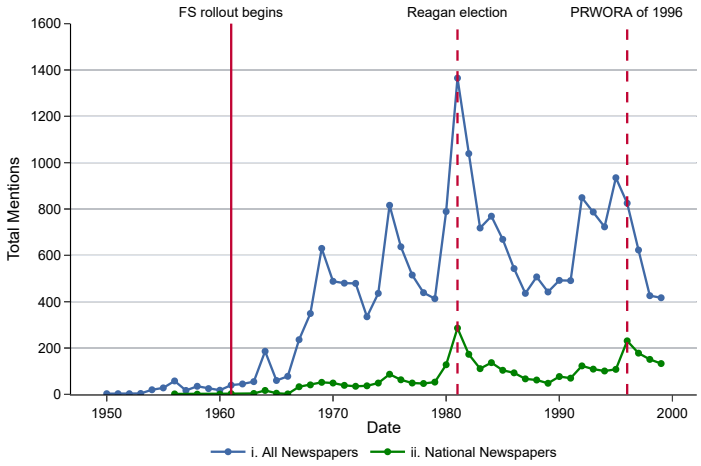
Note: This figure presents event study estimates of the effect of Food Stamp program roll-out on the probability of a Democratic party victory (in Congressional elections) in counties with a high black share. High black share counties have a Black population share above 10%, which equates to around 25% of counties (75th percentile is 11%). The data is at the county-election level for years 1940–1992 (see section 4 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.

A.3 Long run effects: Heterogeneity

Table IA1: Long-run effects: Heterogeneity
High Black population & high poverty counties

Panel A: High Black popn.	(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 FE	Y	Y	Y	Y	Y	Y	Y
Birth year FE	Y	Y	Y	Y	Y	Y	Y
Race FE	Y	Y	Y	Y	Y	Y	Y

Table IA1 (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 FE	Y	Y	Y	Y	Y	Y	Y
Birth year FE	Y	Y	Y	Y	Y	Y	Y
Race FE	Y	Y	Y	Y	Y	Y	Y

Note: The specification 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/ethnic group. Standard errors in parentheses are clustered by county. *** 1%, ** 5%, * 10% significance level.

A.4 Long run effects: Robustness to alternative fixed effects

Table IA2: Long-run effects
Robustness #1: County × Birth Year 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×Birth year FE	Y	Y	Y	Y	Y	Y	Y
Race FE	Y	Y	Y	Y	Y	Y	Y

Note: This table replicates the specification in Tables 1 and 2 but replaces County and Birth year fixed effects (FE) with County×Birth year 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 IA3: Long-run effects

Robustness #2: Birth Year × 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 FE	Y	Y	Y	Y	Y	Y	Y
Birth year×Race FE	Y	Y	Y	Y	Y	Y	Y

Note: This table replicates the specification in Tables 1 and 2 but replaces Race and Birth year fixed effects (FE) with Birth year×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 #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×Race FE	Y	Y	Y	Y	Y	Y	Y
Birth year FE	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 IA5: Long-run effects

Robustness #4: Census Block × Race fixed effects

	(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
Block×Race FE	Y	Y	Y	Y	Y	Y	Y
Birth year FE	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 Census Block×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 IA6: Long-run effects

Robustness #5: County × Birth Year × 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×Birth year×Race	Y	Y	Y	Y	Y	Y	Y

Note: This table replicates the specification in Tables 1 and 2 but replaces County, Birth year, and Race fixed effects (FE) with County×Birth year×Race 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.

A.5 Long run effects: Interactions

Table IA7: Long-run effects
Church density

	(1) Republican	(2) Democrat	(3) Independent	(4) Voted%	(5) Voted%×Republican	(6) Voted%×Democrat	(7) Voted%×Independent
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)
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)
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 FE	Y	Y	Y	Y	Y	Y	Y
Birth year FE	Y	Y	Y	Y	Y	Y	Y
Race 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. *FS* now denotes a *continuous* treatment variable. White is the omitted racial/ethnic group. Standard errors in parentheses are clustered by county. *** 1%, ** 5%, * 10% significance level.

B. Model Proofs

This appendix section provides the proofs for the results presented in Section 3. In all the derivations below, we have taken steps to simplify notation. In particular, we have (i) defined $\beta_i^P(t)$ in terms of $\beta_W^P(t)$ using equation 15; and (ii) dropped time inputs from $\beta_i^P(t)$, such that $\beta_P = \beta_W^P(t)$ and $B - \beta_P = \beta_B^P(t)$.

The following lemma quantifies the probability of each party winning support from each voter-block in each cohort N, given their political investments β_P:

Lemma A1 (Cohort voting probabilities). For a voter of cohort N, the probabilities of voting for each party by voter-block are given by:

1. $P_W^L(N) = \frac{1}{2} + \psi[(1-\phi^N)y^* + \phi^N(\beta_L - \beta_R)]$
2. $P_B^L(N) = \frac{1}{2} + \psi[(1-\phi^N)y^* + \phi^N((B-\beta_L) - (B-\beta_R))] = \frac{1}{2} + \psi[(1-\phi^N)y^* + \phi^N(\beta_R - \beta_L)]$

   $= P_W^L(N) + 2\psi\phi^N(\beta_R - \beta_L)$
3. $P_W^R(N) = 1 - P_W^L(N) = \frac{1}{2} - \psi[(1-\phi^N)y^* + \phi^N(\beta_L - \beta_R)]$
4. $P_B^R(N) = 1 - P_B^L(N) = \frac{1}{2} - \psi[(1-\phi^N)y^* + \phi^N(\beta_R - \beta_L)]$.

Proof: Stems directly from the voter learning process and the expected value of uniform distribution. □

The following lemma quantifies the probability of each party winning support from each voter-block for all cohorts $N \in \{\underline{t}, \underline{t}+T\}$, given their political investments β_P. Note that cohorts may not have lived through the rollout, i.e. $\underline{t} > 0$. In the text, we present results where some cohorts have lived through the rollout setting $\underline{t} = 0$.

Lemma A2 (Representative-voter voting probabilities). *For all cohorts $N \in \{\underline{t}, \underline{t}+T\}$, $T > 0$, the representative voter in each voter-block has probabilities of voting for each party that are given by:*

1. $P_W^L(T) = \frac{1}{2} + \psi[(1-\chi(T)\phi^{\underline{t}})y^* + \chi(T)\phi^{\underline{t}}(\beta_L - \beta_R)]$

2. $P_B^L(T) = \frac{1}{2} + \psi[(1 - \chi(T)\phi^{\underline{t}})y^* + \chi(T)\phi^{\underline{t}}((B - \beta_L) - (B - \beta_R))] = \frac{1}{2} + \psi[(1 - \chi(T)\phi^{\underline{t}})y^* + \chi(T)\phi^{\underline{t}}(\beta_R - \beta_L)]$

   $= P_W^L(T) + 2\psi\chi(T)\phi^{\underline{t}}(\beta_R - \beta_L)$

3. $P_W^R(T) = 1 - P_W^L(T) = \frac{1}{2} - \psi[(1 - \chi(T)\phi^{\underline{t}})y^* + \chi(T)\phi^{\underline{t}}(\beta_L - \beta_R)]$

4. $P_B^R(T) = 1 - P_B^L(T) = \frac{1}{2} - \psi[(1 - \chi(T)\phi^{\underline{t}})y^* + \chi(T)\phi^{\underline{t}}(\beta_R - \beta_L)]$.

where $\chi(T) = \frac{1-\phi^T}{-\ln\phi} \times \frac{1}{T}$ *has the following attributes:*

i. $0 < \chi(T) < 1$

ii. $\lim_{T\to\infty} \chi(T) = 0$

iii. $\chi(T)$ *is increasing in* ϕ. *Furthermore,* $\lim_{\phi\to 0} \chi(T) = 0$ *and* $\lim_{\phi\to 1} \chi(T) = 1$.

Proof: The average support for party P by voting block i is given by:

$$\frac{\int_{\underline{t}}^{\underline{t}+T} P_i^P(N)dN}{\int_{\underline{t}}^{\underline{t}+T} t\ dN}$$

Plugging the cohort voting probabilities from Lemma A1 and setting $\chi(T) = \frac{1-\phi^T}{-\ln\phi} \times \frac{1}{T}$ yields the probabilities given in 1–4. □

The following proposition characterizes the key results we present in the text:

Proposition A1 (Equilibrium). *Define* $\phi^{t^*} := \chi(T)\phi^{\underline{t}}$. *A (unique) equilibrium exists where ideological investment is characterized by:*

$$\beta_L^* - \beta_R^* = \frac{2}{3}(\alpha_W - \alpha_B)\big[y^* - \frac{y^*}{\phi^{t^*}}\big] \tag{14}$$

$$\beta_L^* + \beta_R^* = 2B\alpha_B + (\alpha_W - \alpha_B)\Big(\frac{2}{\gamma} - \frac{1}{\psi\phi^{t^*}}\Big). \tag{15}$$

Proof: Assume each party has the same budget, that is $B = B^L = B^R$. For notational ease, set $\beta_W^P = \beta_P$ and, thus, $\beta_B^P = B - \beta_P$. Then, the parties' maximization program takes the form:

$$\max_{\beta_L} \quad P_W^L \alpha_W + P_B^L \alpha_B - \gamma\big(\beta_L P_W^L \alpha_W + (B - \beta_L) P_B^L \alpha_B\big)$$

$$\max_{\beta_R} \quad P_W^R \alpha_W + P_B^R \alpha_B - \gamma\big(\beta_R P_W^R \alpha_W + (B - \beta_R) P_B^R \alpha_B\big)$$

β_L^* solves the first-order condition for party L:

$$\psi\phi^{t^*}(\alpha_W - \alpha_B) - \gamma\Big(P_W^L \alpha_W - P_B^L \alpha_B + \beta_L^* \frac{\partial P_W^L}{\partial \beta_L^*}\alpha_W + (B - \beta_L^*)\frac{\partial P_B^L}{\partial \beta_L}\alpha_B\Big) = 0$$

Writing in terms of P_W^L:

$$\implies P_W^L(\alpha_W - \alpha_B) + 2\psi\phi^{t^*}(\beta_L - \beta_R)\alpha_B + (\alpha_W + \alpha_B)\psi\phi^{t^*}\beta_L - \psi\phi^{t^*} B\alpha_B = \frac{\psi}{\gamma}\phi^{t^*}(\alpha_W - \alpha_B)$$

Plugging the definition of P_W^L and $\alpha_W + \alpha_B = 1$ and rearranging yields:

$$\implies \frac{\alpha_W - \alpha_B}{2} + (\alpha_W - \alpha_B)\psi(1 - \phi^{t^*})y^* + (\alpha_W + \alpha_2 B)\psi\phi^{t^*}(\beta_L - \beta_R) + \psi\phi^{t^*}\beta_L - \psi\phi^{t^*} B\alpha_B = \frac{\psi}{\gamma}\phi^{t^*}(\alpha_W - \alpha_B)$$

$$\implies 2\beta_L - \beta_R = B\alpha_B + (\alpha_W - \alpha_B)\big[y^* - \frac{y^*}{\phi^{t^*}}\big] + (\alpha_W - \alpha_B)\Big(\frac{1}{\gamma} - \frac{1}{2\psi\phi^{t^*}}\Big). \tag{16}$$

Similarly, solving the FOC for party R:

$$\implies 2\beta_R - \beta_L = B\alpha_B + (\alpha_B - \alpha_W)\big[y^* - \frac{y^*}{\phi^{t^*}}\big] + (\alpha_W - \alpha_B)\Big(\frac{1}{\gamma} - \frac{1}{2\psi\phi^{t^*}}\Big). \tag{17}$$

Using Equations (16) and (17), we obtain the relationships:

$$\beta_L^* - \beta_R^* = \frac{2}{3}(\alpha_W - \alpha_B)\big[y^* - \frac{y^*}{\phi^{t^*}}\big]$$
$$\beta_L^* + \beta_R^* = 2B\alpha_B + (\alpha_W - \alpha_B)\Big(\frac{2}{\gamma} - \frac{1}{\psi\phi^{t^*}}\Big).$$

□

Proof of Proposition 1:

Proposition 1: Stems directly from Equation (14) and setting $\underline{t} = 0$.

□

Proof of Proposition 2:

Proposition 2: Follows directly from Definitions 1 and 2, and Proposition A1.

□