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

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Abstract

We explore long-run racial polarization induced by the original Food Stamps Program: a major policy choice to expand the social safety net in the United States in the 1960s and 1970s. Combining the county-level rollout with an experience-exposure DiD design, we use voter microdata for 175M Americans as of 2020 to compare lifetime voting patterns between those who experienced this expansion of the safety net as adults and those who came of age in a world where Food Stamps was an established feature of the redistributive landscape. Exposure to Food Stamps rollout generates racial political polarization that persists fifty years later: White voters who experienced rollout are more likely to register and vote as Republicans, and treated Black voters are more likely to register and vote as Democrats. These effects are not driven by age, historical experiences, life cycle factors, or changing racial attitudes over the 20th century. We link these findings to a model of partisan competition which illustrates why parties may seek advantage by deliberately politicizing voter perceptions of public policies. Using congressional speeches, survey data, and voting outcomes from the rollout period, we show that short-run evidence is consistent with polarization unfolding as suggested by our model. Our results suggest that today's politicization of public policies can be highly consequential tomorrow: in this setting, we find electoral impacts persisting for at least a half-century.

JEL Classification: D72, J15, P46

Keywords: Political Polarization, Racial Polarization, Safety Net, Electoral Politics

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1. Introduction

A central role of government in market economies is to implement redistributive policies in accordance with some social consensus. For many developed economies, the majority of this agreed-upon redistribution comes in the form of a social safety net, and these flows are typically large, ranging from 10 to 30% of GDP.[1] The wide variation in redistributive spending, even across wealthy countries, gave rise to a literature seeking to explain the differing social consensus embodied in ex-post policies (e.g., Alesina et al., 1999, 2001). Several influential papers posited that racially and ethnically heterogeneous societies face additional headwinds to redistribution due to tensions arising from in-group versus out-group dynamics (see Stichnoth and Van der Straeten, 2013, Alesina and Giuliano, 2011, for surveys). Related work modeled how political actors might seek to deliberately exacerbate these tensions for strategic advantage (Glaeser et al., 2005, Glaeser, 2005).

This paper provides a well-identified empirical exploration of the interplay between redistributive choices, partisan alignment, and racial polarization in the context of one of the 20th century's most consequential expansions of the U.S. safety net. We empirically document long-lasting shifts in political alignment along racial lines caused by this change in redistributive policy. Our setting is the original Food Stamps Program (FS), the first national commitment to providing ongoing nutrition assistance to adults purely on the basis of poverty, without requiring the presence of children or physical disability. For the past fifty years, this program has been a major plank of the U.S. safety net, with the current incarnation covering 42 million people, including nearly one in four children.[2]

The FS program rolled out county-by-county from 1961 to 1975 (Hoynes and Schanzenbach, 2009). Our approach combines staggered-rollout DiD with an experience-exposure component. Our benchmark analysis compares political affiliations and voting between individuals who lived through the FS expansion as adults (18+ at rollout) and those who attain the age of majority in a world where FS is already an established feature of the safety net. We generalize this approach with a semiparametric design allowing for partial treatment at rollout for children – a "fuzzy-treatment experience-DiD" – and show that results are quali-

[1] OECD, 2019 and 2022, https://web-archive.oecd.org/temp/2024-06-24/63248-expenditure.htm

[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.

tatively and quantitatively similar. Consistent with this, we also use an event-study to show that the polarizing effects of FS rollout are largest for the peer-group alive at rollout, with cohort-level partisan impact gradually falling across subsequent generations.

Our most consequential empirical finding demonstrates that individual-level racial political polarization arising from expansion of the FS safety net is long-lasting. Using a comprehensive micro-level dataset covering the universe of voters as of 2020, we show that FS exposure increases polarization by amplifying the alignment between racial blocks and political parties. We show that this political alignment is observable five decades later because of the lasting imprint of the policy rollout on the political identities and behavior of the initially exposed cohorts. Specifically, White voters exposed to the FS rollout as adults are substantially more likely to be registered as Republicans in 2020, and less likely to be registered Democrats, relative to younger voters. Black and Hispanic voters exposed to the FS rollout show a corresponding leftward shift: they are much less likely to register as Republicans than Whites, registering instead as Democrats or Independents. We show that this increased polarization impacts electoral outcomes through a turnout channel: White individuals exposed to rollout are more likely to cast a vote as registered Republicans, while treated Black individuals are more likely to cast a vote as registered Democrats.[3] The gradual geographic expansion of the FS Program allows the use of a rich set of fixed-effects to ensure that our estimates are not driven by age, historical events, geography, or shifting political attitudes between 1960 and 2020.

To provide context and structure for our primary long-run empirical analysis, we construct a theoretical framework that illustrates a process of strategic polarization. Parties are able to seek advantage by deliberately inducing group-level polarization around a policy. The two core features of the model are: (i) voters who form sticky and only partially accurate beliefs about the policy landscape upon attaining the age of majority, and (ii) parties that compete over public opinion by allocating a resource budget to fill the gap in views created by partial information. This framework can be interpreted in multiple ways; we discuss two interpretations.

[3] Of course, these are partial-equilibrium estimates on adults exposed to the FS safety net rollout; a general-equilibrium interpretation is not appropriate, as the policy landscape over the last 50 years would have evolved differently in the absence of Food Stamps.

One interpretation centers the role of *political narratives*, with party investment in the model corresponding to narrative framing of a government policy. Voters are initially uncertain about a policy's impact. 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. Our model shows that political parties can gain advantage by strategically allocating narrative efforts towards voting blocks unequally, thereby amplifying political polarization. The historical record is rife with evidence suggesting that political parties have the ability and incentive to engender partisanship for political advantage.[4] As multiple scholars have documented, these dynamics were evident in the FS setting as well: as FS neared national rollout, the explicitly racialized specter of "Welfare Queen" abuses of the FS program became central to political rhetoric during the 1976 presidential campaign.

A second interpretation of the model interprets *partisan investment* as resource-steering or agenda-catering. This is consistent with the fundamental framework of multiple political economy models. Viewed through this lens, our model suggests that the safety net expansion of FS is a shock that strengthens the partisan alignment between voters that approve of FS and the party championing the policy. Parties subsequently allocate resources to further the (unspecified) agendas of their constituents. The additional enthusiasm that results from successfully passing a policy shifts incentives going forward: it becomes relatively more attractive for the Republican party to invest in peeling off for the opposition marginal FS supporters in the larger (White) voter block, and, because of underinvestment by the Republican party, relatively more attractive for the Democratic party to partially reallocate investing towards the smaller (Black) voter block. Through this interpretation, the successful passing of a policy like FS creates a self-reinforcing feedback loop through which the partisan alignment of voter blocks with parties strengthens over time.

Linking the theoretical framework to the empirical analysis, we offer several pieces of evidence that point to polarization unfolding as suggested by the model. Aligning with a narrative-based interpretation, we use digitized records of Congressional speeches to show

[4] 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).

that elected officials increase rhetorical engagement with Food Stamps after their county launches the program. This coincides with widening racial divides in perceptions of political parties: survey data from the rollout period shows that White voters who live in counties with a FS Program report decreased approval of the Democratic party and Black voters report large increases. This analysis also supports racialized polarization in existing partisan alignment: Black Republicans exposed to FS rollout are more likely to indicate support for the Democratic party, while White Democrats are less likely to do so. Finally, we also show that FS rollout leads to shifts in contemporaneous electoral outcomes: (i) Black voter registration rates increase; (ii) Democratic vote share in U.S. House elections rises; and (iii) the share of Black elected officials increases.

The predictions from our theoretical framework and the voting patterns we document are consistent with widely-documented stylized facts about polarization in the United States: the safety net and racial attitudes are two of the topics that consistently generate the greatest disagreement between Democrats and Republicans (see Figure 1). The historical record suggests that viewing entrenched partisan disagreement as a sorting equilibrium arising spontaneously from personal preferences is overly simplistic. Our framework shows that when political parties have the ability and incentive to engender partisanship for political advantage, entrenched group-level polarization can arise endogenously. While we apply this framework to consider redistributive policy and racial polarization, there are no shortage of highly-politicized issues in the current political landscape. In recent years, these have included healthcare policy (Obamacare), vaccines, and immigration enforcement. Our empirical results suggest that *today's* partisan framing of these issues may resonate in electoral outcomes not just in the next election or the one after that, but for decades to come.

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, Bailey et al., 2024).

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 via persistent effects on the cohorts exposed to the rollout. 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 background on the FS Program.

Section 3 develops a model framework to organize and interpret our empirical results. Section 4 describes the data. Section 5 provides short-term evidence supporting the mechanisms implied by our model framework, while section 6 examines the long-run effects of the FS Program. Section 7 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 early 1960s brought a large-scale re-imagining and expansion of the federal safety net. 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 policy represented a major pivot in America's approach to social supports: it helped adults buy food 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 explicitly linked assistance to children and 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]

[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).

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, 2012, 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 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). Appendix Figure IA1, Panel (a) plots the number of articles mentioning FS in national and regional newspapers over time, while Panel (b) plots articles that additionally include a term identifying the Black population. Newspaper mentions of FS, both with and without a racial identifier, spike around the 1976 and 1980 presidential elections (as well as in 1995–6), indicating the enduring presence of FS narratives in political campaigns.

3. Framework: Political Polarization with Slow Learning of Outcomes

This section provides a model in which parties find it advantageous to deliberately politicize policies and voter perceptions. We use this model to structure the connections between polit-

[6]Our specifications all include county fixed effects (or their interactions). 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 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 (Gilens, 1995).

ical incentives and partisan framing and, accordingly, select model features to be consistent with the historical record (such as parties deploying narratives; e.g., Card et al., 2022) and with frictions in voters' understanding of policy (e.g., Achen and Bartels, 2016, Fowler and Margolis, 2014).

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. When voters hold sticky views about a policy and information about the policy diffuses slowly, the model illustrates why parties may choose to deliberately introduce group (racial) polarization into the policy landscape.

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 allocating resources to 'fill' the perceptual gap. That is, we model a world where perfect understanding of policy proposals would fully determine how voters view each party, but – because understanding is partial – parties can invest effort to affect perceptions on the margin. However, such investments are asymmetrically costly: when a policy is already viewed positively by one voter block 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 investment and, simultaneously, to invest in the block 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).

The model's concept of investment is abstract. Parties have many ways to affect public opinion, and the historical record points to several natural interpretations. One possible form of investment in affecting public opinion operates through *narrative.* This could encompass deliberate (and potentially misleading) "spin" about policy impact and outcomes disseminated through political speeches, advertising, public relations campaigns, or party platform documents. We provide some empirical results consistent with such a channel. Investment could also take the form of catering to an agenda preferred by one block or another, in which case investment is less about manipulating narrative, and more about partisan alignment with a given block on dimensions related to the specific policy being modeled. As an example, the U.S. partisan realignment in the 1960s, and the subsequent perception of the Democratic party as the party (relatively) more aligned with the interests of racial minorities, is an outcome consistent with the model's investment being the allocation of political capital.

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 imperfectly observed by voters and the impact does not vary across voter blocks (i.e., $y_i^* = y^* \; \forall i$).[8] $y^* = 0$ denotes a politically neutral policy. One party favors and sponsors the 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 filling the gap created by incomplete perception of policy impact.

Voter beliefs: policy-impact component. A central model feature is that voters 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 never revisits their conclusion.

[8] This is a sufficient but not a necessary condition; model conclusions hold for historically-consistent U.S. voter block population shares (α_i) even when y_B^* is multiples greater than y_W^*. For a more general formulation, see Appendix A, Proposition 2'.

We refer to this initial belief formation as the point in the life-cycle at which a voter becomes politically aware.[9] 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 (y^*) diffuses throughout the electorate recursively:

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

where y^N denotes the view held by the voter block initially forming impressions N years after policy rollout ($N \geq 0$). Because the policy impact does not vary across voter blocks, this component varies only at the *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.[10] 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 voter perceptions. 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. Expenditure is tied to voter blocks, so that for each party

[9] 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.

[10] Initial belief formation is without loss of generality. Appendix A.2 shows that imperfect belief updating is observationally equivalent to this baseline formulation of the model with a rescaled learning parameter, so all effects are attenuated but not qualitatively altered.

$P \in \{L, R\}$ 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 linear combination of two elements: (i) the cohort-specific perception of the true policy's impact, and (ii) the perceptual gap left by any cohort's partial understanding of the policy, which is filled as a function of party investment choices:

$$\begin{aligned} \text{Partisan View} := v_{it}^N &= y^N + \iota_{it}^N \\ &= (1 - \phi^N)y^* + \phi^N(\beta_{it}^L - \beta_{it}^R). \end{aligned} \tag{2}$$

The first term in Equation 2 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 captures the notion that for finite policy tenure ($N < \infty$), all new voters complement their impact-based perception of the policy with partisan impressions that depend on party investment. If parties invest identically, then cohort views depend only on the policy itself. If parties invest differently, two outcomes are possible: either voters are even more enthusiastic about the party that sponsors the policy (when there is relatively more investment by the sponsoring party), or voter enthusiasm is damped by 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. 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 framing to different voting blocks, but they cannot customize messages to target specific cohorts within voting blocks. Thus, party choices are based on the average views over all cohorts within a voter block, $N \in \{0, 1, 2, 3...\}$:[11]

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

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

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

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:

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

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

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

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 $t = T$, i.e., T years after policy rollout at $t = 0$. We call the difference in support 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 a process of *racial politicization*.

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

Proposition 1 (Racialized Political Investment). *For an existing policy y 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:*

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

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.[12]

Proof: See Appendix Section A. □

Proposition 1's key insight is that in the presence of incomplete information for voters, parties can gain advantage by deliberately racializing them. This arises from diminishing returns to party investment 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 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 party L, without loss of generality, party R 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 L to marginally increase investment in the smaller voting block (B), rather than attempt to fully offset R's investment in the larger voting block. This generates racial polarization.

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:

[12] 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.

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 (Electorate-Wide 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. ***Electorate-wide racial voter polarization*:**

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

Proof: See Appendix Section A. □

Propositions 1 and 2 generate the following corollaries which provide the basis for our 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 the cohort that is already politically aware at the time of policy rollout ($N = 0$).

3. ***Persistent Polarization.*** *Average polarization across the electorate does not dissipate as 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 the following 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 newer, and lower within younger cohorts that become politically aware when the policy's tenure is longer. Third, as policy tenure increases, polarization persists as parties increase their investment in narrative in order to impact the less-sensitive younger cohorts.

There is a related 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 major 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 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, birth date, and sex. Importantly, conditional on an individual appearing in the 2020 L2 vintage, the data also includes historical information on each individual, including voting history and registration date. From this data we compute a measure of voting propensity: the share of even year general and primary elections that each individual voted in since 2011 (relative to the number in which they were eligible to vote).[13]

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.[14] 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 from multiple sources.[15] We drop registered voters with

[13] Many states do not have statewide elections in odd years. Even year general elections occur in November and are for federal offices (e.g., congressional and presidential) and many state offices. Even year primaries nominate candidates for the November general.

[14] 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.

[15] Pew (2018) compares race in commercial voter registration data to Pew national panel microdata and Bernstein et al. (2022) compares L2's race data to HMDA mortgage applications. We also run our baseline long-run specification using only states that collect race on voter registration forms, and report estimates in

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 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. The 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 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.

Appendix Table IA1. Results are similar, supporting the accuracy of L2's race variable in our setting. In all specifications using L2 race we drop individuals in the following categories: Islander, Native, mixed, other and unknown.

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 (e.g., county population share that is Black, or non-White) nor political variables (e.g., 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. First Stage Evidence

In this section, before turning to our central analysis of long-run polarization, we offer several pieces of short-term evidence that describe an unfolding of polarization in ways that align with the mechanisms implied by our theoretical framework. Because this section explores outcomes from the 1960s and 1970s, we have access to a control group of voters untreated by the FS Program in a way that is impossible when considering outcomes in 2020, fifty years after the program attained full national coverage.

We first provide evidence that elected officials spend more time discussing Food Stamps after their county launches the program, which in the language of the model maps over to an 'investment' of resources in deliberate rhetorical framing and engagement. In our model, these investments are intended to impact voter views of the policy and the party associated with it; we therefore also use survey evidence to show directly that FS rollout did generate racial polarization in political beliefs at the time of rollout. Finally, although data on voting outcomes from the 1970s and 1980s is much less detailed than the voting microdata we use in our main analyses, we also show that FS rollout affects voting outcomes in the short-run as well.

5.1 Evidence of Rhetorical Engagement

In practice, parties can invest in many ways to shape voter views of a policy landscape. In this section, we provide evidence consistent with one potential form of party investment: narrative engagement. Rhetoric is a key tool that politicians use to affect voter impressions; and, in this setting in particular, the historical record is, in fact, rife with examples of elected officials strategically deploying narratives around the FS program (as discussed in Section 2). An investment in narrative framing would map to model quantities of β^L, and β^R.[16]

Empirically quantifying narrative engagement is challenging, and more so in a historical setting. We focus on politician speech in a highly salient and consequential forum: speeches in the House of Representatives. Every word of members' floor speeches is recorded, allowing us to examine whether members talk more about the FS program following its rollout in their county, relative to elected officials from counties that have not yet implemented FS.

To do this, we begin with all speeches from the Congressional Record (via Proquest) from 1950 to 1997 and identify days in which the the term 'Food Stamp' (or its variants) appear in the record.[17] For this subset of days, we parse each individual speech and identify the speaker, along with their party and district. We then generate an indicator capturing whether each member spoke at all on each day and another indicator for whether their speech mentioned 'Food Stamps.' We then map these district-level indicators to counties using the population-based historical congressional district-to-county crosswalks in Ferrara et al. (2024). Finally, we aggregate the data to the party-county-year level.

Our analysis exploits the rollout of the FS program described in Section 4.3 to identify the causal effect of the FS program on congressional speech. We use a standard staggered-rollout DiD design at the county-level, comparing outcomes between treated and untreated counties. The ability to observe outcomes at different times allows us to absorb both national political shocks and persistent cross-county differences. The naive two-way fixed effects (TWFE)

[16] The model features investment by voter block, in other words distinguishing between β^L_W and β^L_B. Given the challenges of reconstructing an empirical measure of narrative from the mid-1900s, we cannot apportion investment by voter block in this sub-analysis.

[17] The Congressional Record captures all floor speeches and debate, but excludes committee proceedings. Before 1985 the data consists of pdf scans of bound paper volumes, so OCR may miss some mentions of FS, likely at random.

specification would be:

$$FS\ mention_{ct} = FS_{ct} + \alpha_c + \gamma_t + \epsilon_{ct}, \tag{8}$$

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. To address now-standard concerns about bias in staggered DiD designs (e.g., De Chaisemartin and d'Haultfoeuille, 2020, De Chaisemartin and D'Haultfoeuille, 2022, Goodman-Bacon, 2021, Callaway and Sant'Anna, 2021), we use a bias-robust estimator following Callaway and Sant'Anna (2021) (CS henceforth). The outcome is a county-year count of the number of days on which a representative talks about 'Food Stamps' on the floor of the House.

Figure 3 plots CS estimates of the effect of FS rollout on representatives' yearly likelihood of talking about FS in a congressional speech, separately for Democratic (Panel a) and Republican (Panel b) representatives. Both panels display flat pre-trends, followed by a sustained rise in FS mentions over the decade following FS rollout. As all counties are eventually treated, the precision of the estimates declines as event-time progresses, because the number of available untreated counties in the control group falls towards zero.

To provide context for the magnitudes of these estimates, which are at the county-by-year level for each party, consider that the average number of days per county-year on which a member of the House makes a speech about *any* topic on the days in our sample is 2.1. Thus, if the FS program rollout led to an additional 0.1 days per county-year on which FS were mentioned (the approximate magnitude of the highest estimates in the Figures), this would correspond to an approximately 5% increase in the share of rhetorical engagement allocated to Food Stamps.[18] Given the number of potential topics with which Federal lawmakers can engage, this represents a consequential increase. Thus, Figure 3 shows evidence of representatives from regions exposed to FS rollout investing scare resources – speaking time on the House floor – to shape congressional narratives and agendas.

[18] To place the average of 2.1 days per county-year on which a House member speaks on any topic in our sample in context, this corresponds to around 15 speaking days per year per member (2.1 $\times$ 3,000 counties $\approx$ 6,300 total speaking days; dividing this by the 435 representatives $\approx$ 15).

5.2 Evidence of Impact on Partisan Attitudes

In this subsection, we focus on changes in political beliefs, as captured by expressed support for the Republican and Democratic parties in surveys spanning the rollout period. In our theoretical framework, these attitudes map to the partisan views captured by v^B and v^W.

We obtain responses from nationally representative surveys conducted by the Gallup Organization between 1958 and 1978, thus including responses from both treated and untreated counties. The survey solicits opinions about each party's congressional delegation and about the current President. We code partisan views symmetrically (support for Democrats corresponds to lack of support for Republicans, and vice-versa) and construct an outcome variable where a positive value denotes respondents' support for the Democratic party. We then regress this measure of support against a RHS variable capturing FS exposure – the share of the state population living in counties where program rollout has occurred in each year. We aggregate exposure to FS to the state level because county identifiers are not available. We estimate:

$$y_{ict} = \beta FS_{state\ share(ct)} + \alpha_c + \gamma_t + age_{i(t)} + \epsilon_{ict}. \tag{9}$$

This specification is effectively a DiD analysis with a continuous variable, 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.[19]

Table 1, Panels B and C show the results. Column 1 of Table 1, Panel B 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 (pp) 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 80pp higher in states with full 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

[19] Continuous-variable DiD designs are also potentially exposed to the bias in binary-treated staggered DiD. Unfortunately, there is not yet a standard bias-robust estimator for continuous treatment. All other findings in the paper use a bias-robust approach.

Democrats as FS coverage rises. Additionally, the top estimate in column 2 is of particular interest: this shows that FS implementation leads to reduced support for the party sponsoring the program. This is exactly the dynamic suggested by our theoretical framework – in the language of the model, this movement away from the Democratic party by White voters would be the result of the party opposing the policy choosing to invest in reducing enthusiasm among the larger voter block.

Panel C of Table 1 examines how FS implementation affects presidential support – support for Democratic presidents, or lack of support for Republican presidents depending on the survey year. This outcome variable is likely a noisier measure of political beliefs because presidents are assessed on a variety of dimensions including personal charisma. In addition, the salience of U.S. involvement in Vietnam in this period may have limited the scope for other issues to affect views of the President. Nonetheless, we find similar patterns to those in Panel B, especially for a pro-Democratic party shift by Black voters. Point estimates in the top row are also consistent with a shift away from the Democratic party by White Republicans and Independents, but confidence intervals are quite wide.

5.3 Evidence of Short-Run Political Impacts

In our theoretical framework, polarization arises as parties choose investments that will maximize the chance of voter support: P^L (and implicitly its complement, P^R). So far, we have shown evidence that FS rollout affects the arguments of this voter support function: increased investment in rhetorical engagement and shifts in partisan views. We conclude this section by showing that FS rollout also affects voting outcomes in the short run.

We exploit the period of active rollout between 1961 and 1975 to test directly for impact on electoral outcomes, with a focus on polarization between racial blocks. As in Section 5.1, this analysis is a standard staggered-rollout DiD design at the county-level, with the same naive TWFE specification as equation 8. As before, we implement our design using the approach of Callaway and Sant'Anna (2021) (CS).

We begin by examining whether the county-level Food Stamp (FS) program rollout affected voter registration. Panel A of Table 1 reports CS 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 eligible population rose by around 1pp as a result of FS rollout, while White registration remained unchanged (column 2).

Given that the FS policy was associated with the Democratic party, we then explore the electoral consequences of the FS rollout.[20] 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. 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 shares to around 10pp, an advantage which persists in the two subsequent elections.

Panel A of Table 2 reports the average CS 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 1, 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 (e.g., 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,

[20] Some of this analysis is similar to Figure 4 and Table 3 of Kogan (2021).

or there could be partially offsetting changes in vote distribution across counties. This is especially true if the opposition has focused their investments on competitive seats. Panel B of Table 2 confirms that treatment did not, in fact, affect the overall average likelihood of Democratic electoral victory (column 1). However, this masks underlying heterogeneity: high-Black population counties show a 7pp increase in Democratic win likelihood (column 2).[21]

In contrast, Panel B of Table 2, suggests that counties with a low Black share (column 3) or high poverty share (column 4) have 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 (Table 2, Panel A) would be consistent with Republican investment in the White voter block being successfully targeted to closely contested districts. Likewise, the sharp reduction in Democratic success within high-poverty areas (Column 4) is consistent with successful Republican investment against the food stamp program.

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 may have 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.

In summary, this section exploits sharp variation in treatment during the rollout years to provide evidence of political shifts that arise quickly after a county adopts Food Stamps. Elected officials from treated counties are more likely to invoke FS on the floor of the House of Representatives; residents of states with a larger share of treated counties do change partisan views, with Black residents moving towards the Democratic party and White residents moving towards the Republican party; and finally, changes in partisan views also appear to be manifest in electoral outcomes, including registration and partisan vote share. We now

[21] 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.

turn our attention to the long-run impact of FS exposure on lifetime voting patterns.

6. Central Empirical Analysis: Long-Run Polarization

We test the core prediction of our theoretical framework: long-lasting racial political polarization generated by exposure to the FS rollout. We use a cross-sectional snapshot of individual voting history as of 2020 for 175 million registered voters. By 2020, however, the FS program (along with its more modern incarnations) had been part of the policy landscape in all counties for at least 45 years. As a result, there is no control group of individuals who have never been exposed to this expansion of the safety net. Therefore, we begin this section by developing an application of the general DiD framework that will identify long-run treatment effects in this setting.

Our goal is to compare historical voting patterns, observed as of 2020, for people exposed to FS rollout as adults (18+) versus same-age individuals who lived in a county that implemented FS before they became eligible to vote. We use a DiD design to evaluate an experience effect, where the 'treatment' is being of voting age and living through a major regime-shift in the national safety net. 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.[22] However, because the FS program impacts many families with children, attitudes may be formed before age 18. We begin with an approach estimating a sharp discontinuity in treatment 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 estimates towards zero.

In Section 6.2.3, 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, suggesting that both specifications are similarly effective at measuring the degree of long-run polarization actually induced by

[22] We view age 18, the legal age of majority, as the natural single choice. Most of the United States changed the voting age from age 21 to 18 in 1971, pursuant to the 26th Amendment. The historical record shows that this change arose from the widespread belief that 18 year-olds were aware of national policy – most saliently with respect to the military draft.

treatment. The difference would be in the timing of how one unit of that treatment lands across the electorate: immediately at policy rollout only for those 18+, or in addition, unfolding slowly across some of the younger cohorts. Our results in Section 6.2.3 offer some support for the latter, but the focus of this paper is on existence of treatment effects rather than on exact timing.

6.1 Empirical Strategy: Experience DiD Design

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. Like any evaluation of experience effects, our design identifies the effect of FS from cross-cohort differences. We begin with the following naive specification:

$$Y_{ic} = \beta FS_{ic} + \alpha_c + \gamma_{b(i)} + \epsilon_{ic}, \tag{10}$$

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 outcome 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.*

Equation 10 is a TWFE estimator, which may be biased in a staggered-rollout DiD setting such as ours, as noted in Section 5.1. For clarity of exposition, we first describe the basic difference-in-difference comparison underlying 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 identifying variation for the treatment effect is provided by comparisons of the following form:

$$\underbrace{\Big(E[Y_{icb} \mid FS(C) - B \geq 18] - E[Y_{icb} \mid FS(C) - B' < 18]\Big)}_{\text{differences due to treatment effect and age}} - \underbrace{\Big(E[Y_{icb} \mid FS(C') - B < 18] - E[Y_{icb} \mid FS(C') - B' < 18]\Big)}_{\text{differences due to age only}} \tag{11}$$

$FS(C)$ denotes the year that a given county first implemented FS, and thus $FS(C) - B \geq 18$ denotes a treated individual in county C (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.

Now we return to the issue of TWFE bias. The regression specification of Equation 10 would use all possible comparisons across every combination of county pair (C, C') and birth year pair (B, B'). However, in this setting, we also face the "bad comparison" problem that leads to TWFE bias. We adopt the "stacking" approach of Cengiz et al. (2019) rather than Callaway and Sant'Anna (2021): this allows us to estimate the interacted treatment effects that are crucial for our focus on racial heterogeneity with respect to FS treatment. Our base TWFE specification becomes:

$$\begin{aligned} Y_{ic} = &\beta_1 FS_{ic} + \beta_2(FS_{ic} \times Black_i) + \beta_3(FS_{ic} \times Hispanic_i) + \beta_4(FS_{ic} \times Asian_i) \\ &+ \lambda_{r(i)} + \alpha_c + \gamma_{b(i)} + \epsilon_{ic}, \end{aligned} \tag{12}$$

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.

We form a stacked data set, with each stack 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.[23] We interact all fixed effects (birth year, county, race) in Equation 12 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 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 at different stages of their life-cycles.

6.2 Core Results: Long-run Voting Patterns

The central predictions of our model are encapsulated in Corollary 1.1 and 1.2, which describe persistent racial polarization in partisan support. Using voter microdata from L2, we examine two outcomes that are empirical proxies for the model objects P_W^P and P_B^P: (i) party affiliation and (ii) electoral impact: the propensity to vote as a registered voter of a given party. An increase in propensity for Black voters to register (or vote) as Democrats, therefore, would correspond to the model quantity $(P_B^L - P_B^R)$.

Recall that the empirical strategy uses two differences to estimate the long-run effects of FS rollout: (i) comparing adults when FS was rolled out in their county to those who were younger at the time, and (ii) comparing individuals in counties that rolled FS out earlier to those in counties that rolled out later. Thus, the treated group is individuals who were adults when FS rolled out in their county, relative to younger individuals. As our data is from 2020, the treated group is composed of currently older individuals.

[23] 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.

Partisan Affiliation. In Panel A of Table 3, columns 1 to 3 examine the long-run effects of the FS rollout on treated voters' registration as Republican, Democrat, or Independent for the electorate as a whole (i.e., before turning to polarization between racial groups). Treated individuals are 1.0 percentage point (pp) *more* likely to be registered as Republicans (relative to the untreated), 1.1pp more likely to be registered independents, and 2.2pp *less* likely to be registered as Democrats in 2020. 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, relative to younger voters.

These estimates, and those that follow, should be understood as a partial equilibrium quantification of a counterfactual without the FS program. In general equilibrium – the counterfactual that we would observe in the real world – the absence of FS would not be the FS equilibrium minus our estimates (e.g., treated voters shifted 1pp less towards the Republican party relative to untreated voters). This is because parties endogenously react to the polarization opportunities provided by policies, as illustrated by our model. Thus, in the absence of FS, parties would subsequently have politicized other policies, with uncertain net effects.[24]

Panel B explores racial polarization, estimating the specification of Equation 12, with Whites as the omitted group. Many of our tables have a parallel structure: the FS indicator captures the treatment effect for White voters, and interaction terms like $FS \times Black$ capture the treatment effect for Black voters *relative to White voters*.[25]

Columns 1 to 3 of Panel B show that FS exposure leads treated White voters to be 2.1pp more likely to be registered as Republicans (the outcome mean is 27 percent), and 1.9pp less likely to be registered as Democrats, with no change in the independent share. In contrast to White voters, the long-run effect of treatment on Black and Hispanic voters 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

[24] An analogy: an estimated treatment effect of marriage (partial equilibrium) assumes the counterfactual of staying single. However, in reality (the general equilibrium counterfactual), the given individual might have found a different spouse.

[25] Black Americans were the largest racial minority in the U.S. 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%.

lower relative rate of registration), and the Republican party (1.3pp lower), and their rate of registration as independents is correspondingly higher.

Electoral Impact. The extent to which changes in partisan affiliation have electoral impact depends also on the rate at which individuals in each group choose to vote. Our second main outcome captures the partisan impact of exposure to FS rollout in voting outcomes by taking into account registration along with turnout. We use data on individuals' voting history to generate a variable denoted "*Voted %*," capturing the share of even year general or primary elections that an individual has voted in since 2011 (or since registering, for younger voters). In isolation, this variable captures turnout propensity conditional on registration. We are most interested in partisan electoral impact, which we explore by interacting *Voted %* with an indicator for an individual's party of registration. This variable reflects the propensity of a registered voter to cast a vote – a combined registration and voting effect which we refer to as *electoral impact*.

In Panel A of Table 3, column 4 shows that exposure to the FS rollout as an adult reduces treated individuals' voting propensity by 1.1pp (the sample mean is 36pp). Columns 5 to 7 show that electoral impact varies meaningfully by party. FS exposure leads to a 1.9pp increase in the relative likelihood of voting as a Republican and a 3.8pp decline in the likelihood of voting as a Democrat.

As before, Panel B unpacks these aggregate shifts result by race. Column 4 shows that exposure to treatment leaves White voting propensity unchanged on average, while the relative rate for Black voters falls by nearly 1pp. In contrast, treated Hispanic and Asian relative voting rates fall by 6.2 and 9.8pp. Turning to electoral impact, treated Whites are 3.8pp more likely to vote as a registered Republican and 4.9pp less likely to vote as a Democrat. Black individuals display larger responses with an opposite pattern: they are 12pp less likely to vote (relative to Whites) as Republicans and 12pp more likely to vote as Democrats. In contrast to the clear rightward and leftward shifts observed for White and Black votes, respectively, Hispanic and Asian voters display a pattern more consistent with political disaffection, given the drops in overall voting likelihood in column 1. Specifically, the relative treatment effect for Hispanics is a 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 negative for both Republicans and Democrats, with only a small corresponding increase in voting as independents.

Appendix Table IA1 reproduces Table 3 using only states that ask voters their race on voter registration forms in the 2010s (Alabama, Florida, Georgia, Louisiana, North Carolina, South Carolina, Pennsylvania, Tennessee). Results are similar for this subset of states, supporting the accuracy of L2's race variable.

Overall, Table 3 shows that the FS rollout in the 1960s and 1970s increased the alignment of racial blocks with political parties as measured in 2020, both in terms of voter registration and electoral impact. As we will show, and consistent with our model, this enduring effect of the FS program is driven by the cohorts exposed to the program's rollout – older voters in 2020. Further in line with model predictions, we see voters from the larger block (White voters) shifting towards the party opposing the policy (Republicans) and smaller-block voters shifting towards the championing party (Democrats).

6.2.1 Mobility and Measurement Error

We observe individuals conditional on being registered to vote in 2020 and it is not possible to consistently track individual identifiers across state lines. Therefore, our empirical design assumes that county-of-residence in 2020 matches the county in which an individual was first exposed to the FS program.

How might measurement error generated by migration affect our estimates? Migration that is uncorrelated with treatment should generate attenuation bias, and our empirical design places mechanical limits on the scope for correlated measurement error. Specifically, movers born substantially before the FS rollout period will be correctly classified as treated, whether their true county of residence during the FS rollout is observed or not. Analogously, younger individuals born during or after the rollout period will always be correctly classified as untreated. This means that migration can only generate misclassification for those born in a 15-year window between 1943 and 1958.

What patterns of systematic movement within this group would bias estimates upwards, rather than attenuate them? Black people moving to areas where FS was already implemented would not be sufficient; this is absorbed by the race fixed effect. Similarly, any tendency of people in their early-twenties to be more likely to move than those in their early-thirties is absorbed by birth-year fixed effects. The most plausible form of correlated measurement error would arise if more liberally-inclined people (relative to others of the same race) moved to areas with active FS programs. For example, if Black people who are more likely to support the Democratic party are systematically more likely to move to areas with FS, while Black people who are more likely to align with the Republican party are less likely, then our estimated treatment effect for Black individuals would be too high.[26]

There are two reasons to believe that measurement error generates attenuation rather than an upward bias. First, examining the same setting, Bailey et al. (2024) use restricted longitudinal PSID data to explore migration that is endogenous to FS availability. Specifically, they examine whether children in counties without FS are more likely to move to counties with FS before age 5. They do not find this: "We do not find evidence consistent with endogenous migration – if anything, Food Stamps exposure in one's county of birth is slightly positively correlated with the likelihood of moving to a county with Food Stamps during childhood" (footnote 36). While the analysis is for children, their migration is likely highly correlated with that of their parents – and children under 5 are likely to have parents that fall into our critical window of adults between 18 and 33. Bailey et al. (2024) also find that there are no discontinuous jumps in migration rates between children under age 5 and over age 5 among children in disadvantaged families. Their evidence suggests that migration endogenous to FS availability is not a major source of upward bias in our estimates. In fact, Bailey et al. (2024) also write that endogenous mobility may be attenuating their estimates, potentially explaining the smaller estimates they find for movers relative to stayers.[27]

Second, in the following analyses we replicate our analysis in two sub-samples where measurement error will be much smaller. Comparing these estimates with the baseline findings offers an indirect signal on the effect of measurement error. In both analyses, the evidence is consistent with attenuation rather than bias: larger estimates in the subsamples with lower measurement error.

[26]The same conclusion would apply to more-conservative people systematically moving from counties that launch FS to counties that have not yet done so. However, this is probably a less plausible story given the eventual national footprint of the program.

[27]Bailey et al. (2024) do find that children born in regions with a FS program are more likely to move *later in life* – however, in our setting this type of mobility is not a concern, as the treatment classification would remain unchanged.

Individuals who register to vote before age 25. Table 4 explores the long-run effects of treatment on individuals who registered to vote between the ages of 18 and 25, meaning that the data *is* sufficient to pin these individuals down in space at a time much closer to FS rollout. Because we observe a registration when the individual was 25 years old (or younger), this means that they have lived in the same state since that age (as L2 data is siloed within states). Therefore, overall migration is likely lower in this subsample, meaning in turn that classification errors with respect to county-of-residence at age 18 will also be reduced. However, this group of individuals is also likely to be more politically engaged, on average, than people who register later in life. This points towards increased sensitivity to shifts in the policy landscape, and potentially larger treatment effects.

Comparing the response of this group in Panel A to the overall sample (i.e., Panel A in Table 3) reveals 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 over twice as large for Democrats. The aggregate electoral impact by party is also larger, though by a smaller multiplier.

Disaggregating partisan affiliation along racial lines, columns 1 and 2 in Panel B show an 8pp shift towards the Republican party for treated White voters and a 6pp shift away from the Democratic party. Treated Black voters in this sample also 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 also rises.

Turning to electoral impact in columns 5–7, we see both larger partisan shifts for all treated voters (Panel A) and also generally larger shifts by racial group in Panel B – especially for White, Black, and Hispanic voters.[28] Summarizing, while we cannot disentangle increased policy salience from the mechanical impact of reduced measurement error, the larger magnitudes in Table 4 align with the prediction that would arise from either channel.

[28]Uncorrelated measurement error is also likely lower in this sample when we consider Hispanic and Asian voters. As a result of immigration flows over the past 40 years, a much larger share of the U.S. population is Hispanic and Asian in 2020 than in the 1970s. In our baseline analysis, without the ability to know which individuals in L2 have immigrated more recently, we will necessarily misclassify some Hispanic and Asian voters as exposed to FS rollout when they were, in fact, not yet living in the U.S. With the restriction to early-life registrants, identifying variation comes from those who *were* in the U.S. within at least 7 years of FS rollout. The general pattern of larger estimates again suggests reduced attenuation bias.

Individuals who buy homes at earlier ages. We also replicate our main analysis while successively conditioning on individuals who bought their homes (as of 2020) at earlier and earlier ages. This is an alternate tactic for extracting sub-samples that pin individuals down in space earlier in their life. We merge the L2 data to CoreLogic's Deeds records – a near-comprehensive dataset of properties assembled from county administrative records. We match on owners' first name, last name, and longitude and latitude of an address, since these variables appear in both the L2 voter and CoreLogic databases. Because L2 contains age, and the Deeds records contain date of sale, we are able to analyze voters who have purchased their *current home* at specific age cutoffs: before 25, or 30, or 35, etc.

Table IA2 shows the results across a range of age cutoffs. Each column considers the electoral impact measure: $Voted\ \% \times Rep$ in Panel A, and $Voted\ \% \times Dem$ in Panel B – the analog, respectively, to columns 5 and 6 in Table 3. By construction, measurement error will be smaller as the age cutoff is lower. Identifying variation in column 1, for instance, comes from people old enough to have been exposed to FS rollout, who also have been in their current home since age 25 or earlier. Like the analysis of those who register to vote at an early age, this significantly reduces the scope for measurement error. As the age cutoff rises, the window of time during which mobility could lead to misclassified treatment increases. Column 9 shows the estimated electoral impact in this subsample without any age restriction. This column, by comparison to the baseline results in Table 3, columns 5 and 6, captures the difference induced by selection into the L2–CoreLogic matched sample. Overall estimates are smaller in this subsample. Comparing columns 1–8 with column 9 speaks to the role of measurement error: estimates are much larger in subsamples with a lower age cutoff, and magnitudes decline near-monotonically as the scope for measurement error increases. Some selection and composition effect is also possible: the further left columns are identified from people who are meaningfully less mobile than the average American. However, the overall pattern in Table IA2 is also consistent with the main impact of measurement error being attenuation.

6.2.2 Event-study DiD by age bins

Our theoretical framework also predicts that a 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 rollout, relative to cohorts who become old enough to vote in a world where the policy is already well-established. Thus, the model predicts the treatment effect will be persistent because the cohorts exposed to the rollout are still alive in 2020. To test this, we split the FS 'treatment' into five-year birth cohort bins in an event-study version of our main specification:

$$\begin{aligned} Y_{ic} = &\sum_{j=1}^{n} \beta_j Cohort_{icj(i)} + \sum_{j=1}^{n} \lambda_j (Cohort_{icj(i)} \times Black_i) \\ &+ \alpha_c + \gamma_{b(i)} + \lambda Black_i + \epsilon_{ic} \end{aligned} \tag{13}$$

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 6.

This figure shows political registration on the vertical axis, with positive values corresponding to a Republican lean and negative values to a Democratic lean. The reference cohort is furthest to the right in the figure, corresponding to the latest-born cohort. Consistent with the predictions of the model, older cohorts display greater political polarization.

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. Estimates for the differential effect of treatment on Black voters are plotted in the bottom set of coefficients in Figure 6. Black voters in older cohorts show stronger alignment towards the Democratic party as a consequence of treatment, and, for younger cohorts, this effect converges towards zero, as the model predicts.

However, the figure also shows that cohorts that were not adults at rollout – which we have considered untreated until now – also appear to have received some treatment. For both White and Black voters, the estimates immediately to the right of the vertical line indicate that voters who were children at rollout seem to exhibit treatment effects of similar magnitudes to those who were adults. This pattern begins to decline once a policy has been in place for about 20 years, suggesting some inter-generational transmission of the experience of older cohorts. This supports considering 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.

6.2.3 Experience DiD with Fuzzy Treatment

Thus far, we have assumed a sharp delineation in treatment at FS onset around the age of 18. In this section, we relax the assumption of a sharp treatment around age 18 in favor of a fuzzy-treatment framework.

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:

$$FS_{ic} = \begin{cases} 1, & \textit{if} \quad FS_{age} \geq 18 \\ \left(\frac{FS_{age}-(18-L)}{L}\right)^{\lambda}, & \textit{if} \quad 18-L \leq FS_{age} < 18 \\ 0, & \textit{if} \quad FS_{age} < 18-L \end{cases} \tag{14}$$

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 10.

We simultaneously estimate L, λ, and $\{\hat{\beta}\}$ to minimize the joint sum of squared residuals of our main specification (Equation 12, used in Table 3, Panel B), where registering as Republican, Democrat, or independent are the outcomes. 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 6.

Table 5 shows the results using this fuzzy-treatment framework. When compared against the estimates using a sharp treatment (i.e., Panel B of Table 3), we find that fuzzy treatment generates effects that are qualitatively similar to our previous set of estimates. For White voters, registration effects appear slightly larger than our estimates using sharp treatment, while electoral impact is slightly smaller. For minority voters, 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. The two designs are not inherently at odds. In each, the estimated coefficient captures the effect of one unit of treatment. The distinction concerns the timing of how that unit of treatment is delivered to cohorts within the electorate. The sharp design implies that all voter cohorts of 18+ receive the full unit of treatment when a policy launches, and that cohorts below the age of majority receive no treatment. A fuzzy design implies that younger cohorts receive some partial treatment, implying in turn that a major change in policy may 'echo' forward in time for a decade or two before voters take that policy feature entirely as given. But in either case, the same prediction for long-run polarization holds: older voters are affected by the policy shift and associated party investment in framing, and (eventually) younger voters are unaffected. Our parametric estimation of the fuzzy-treatment design – along with the event study results in Section 6.2.2 both support a more gradual decline towards zero treatment. Nonetheless, this paper's focus is on documenting the existence of long-run polarization, rather than the timing of treatment delivery.

6.3 Heterogeneities and Robustness

In our Internet Appendix, we explore several dimensions of heterogeneity and robustness. Section B.1 looks at polarization by gender. Perhaps the most famous example of strategic narrative framing with respect to Food Stamps is the "Welfare Queen" trope, which associated welfare abuses with Black Women in particular. As a result, it is natural to look for intersectional effects. While overall patterns do differ by gender, we do not find that racial polarization induced by FS appears to be stronger for Black women than for Black men.

Section B.2 explores heterogeneity by regional demographics. We find that polarization appears to be larger in counties with higher Black population share – with larger shifts by both White and Black treated individuals towards the Republican and Democratic party, respectively. We also find that polarization effects are larger in higher-poverty counties.

Section B.3 explores how FS rollout may have been amplified or dampened by local exposure to other salient regional or historical factors. As the Voting Rights Act (VRA) coincides with the rollout period and is known to have impacted voting patterns by race, Section B.3.1 compares counties covered by Section 5 of the VRA with adjacent non-covered counties. In this subsample, we see a slight increase in White voters registering as Republicans, but otherwise patterns are broadly similar to the full sample, indicating that the effects of FS and the VRA are independent.

Section B.3.2 explores whether counties which are more highly exposed to recessionary environments show differing effects. Here, we continue to find larger racial polarization, but we also see a level shift by treated White and Black voters alike towards the Democratic party. This is consistent with voters in regions that are more subject to negative shocks placing higher value on the safety net, and aligning accordingly with the party that championed the expansion.

Section B.3.3 asks whether FS polarization appears to be mediated by the strength of church communities. Churches have historically served an dual role in this setting: non-governmental providers of a safety net; and a focal point for political organizing. We find that church density does appear to increase FS-induced polarization, with the largest effect coming from treated White voters moving rightward.

Finally, Section B.4 repeats our core long-run specification with various combinations of more-demanding fixed effect specifications, permitting race-specific trends over time, and/or race-specific trends by neighborhood. Our results are robust to all of these extensions.

7. 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 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 when major public policies are politicized, the downstream effects can shape the political landscape for many decades via their enduring effects on the cohorts experiencing the rollout.

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 policy both theoretically and empirically. Theoretically, we model the politicization process and show that, when knowledge about a policy's impact develops slowly, even policies that are group-neutral can generate political polarization across different voter groups, that polarization is larger for cohorts learning about the policy when recently implemented, and that political polarization persists over time. We then show empirically how this process played out in the context of FS rollout, in both short-term responses and in long-run electoral outcomes.

Our results indicate that managing a policy's political interpretation in order to mitigate backlash may be as politically 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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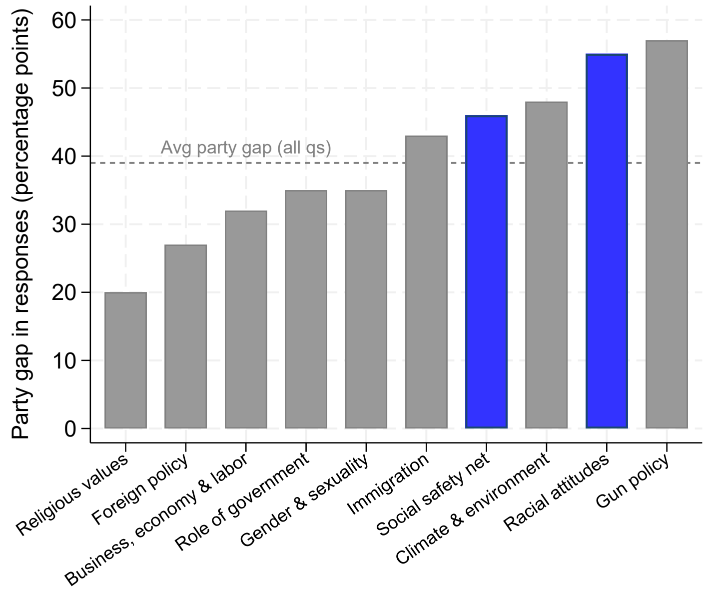
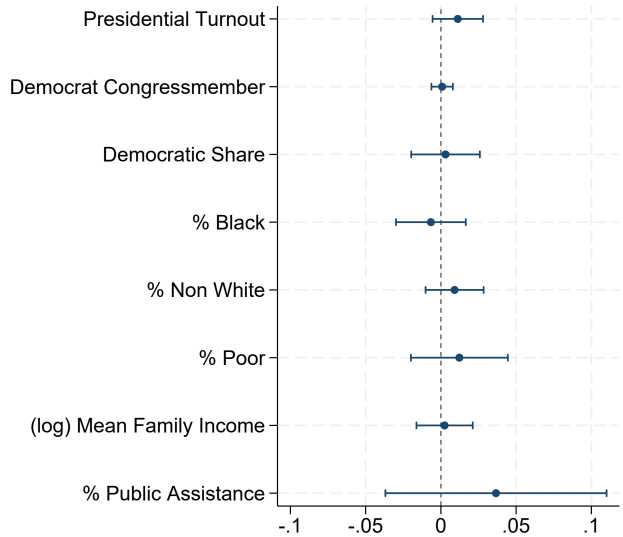
Figure 1: Political polarization by topic

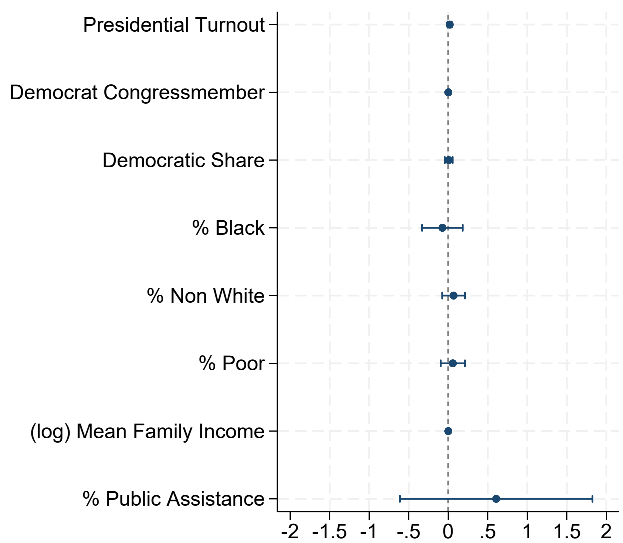


This figure presents survey evidence from Pew Research Center's American Trends Panel, conducted September 3–15, 2019 (Pew Research, 2019). Each column represents the average differences between Republican and Democratic respondents to all questions on that topic.

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



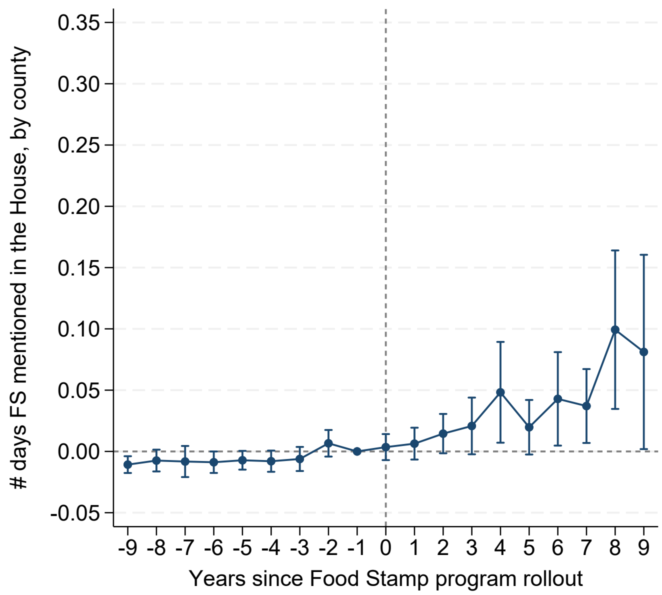
(a) County characteristics in natural units



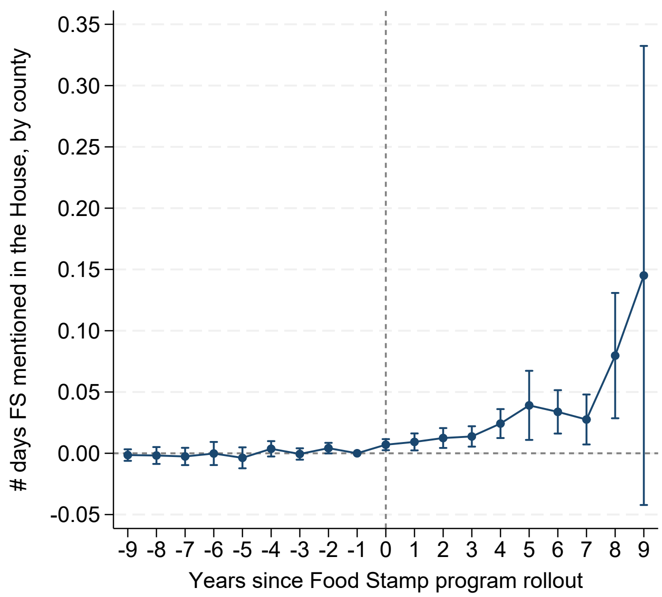
(b) County characteristics as a fraction of sample average

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: Additional mentions of Food Stamps by treated counties' House Representative



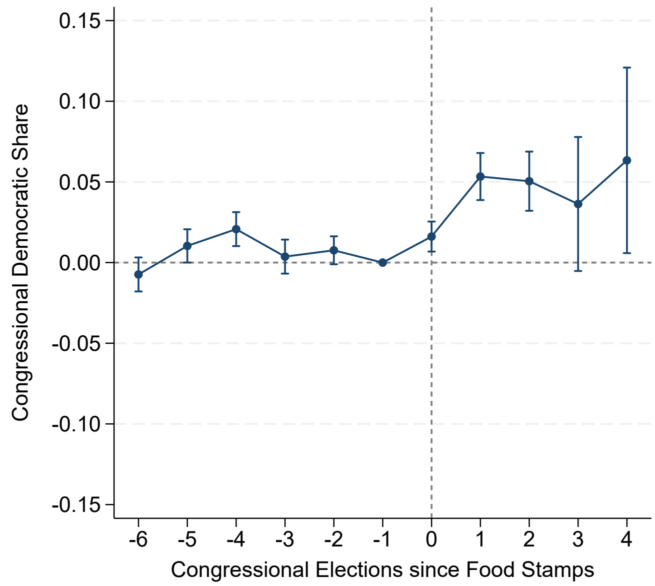
(a) FS mentioned by Democratic representatives



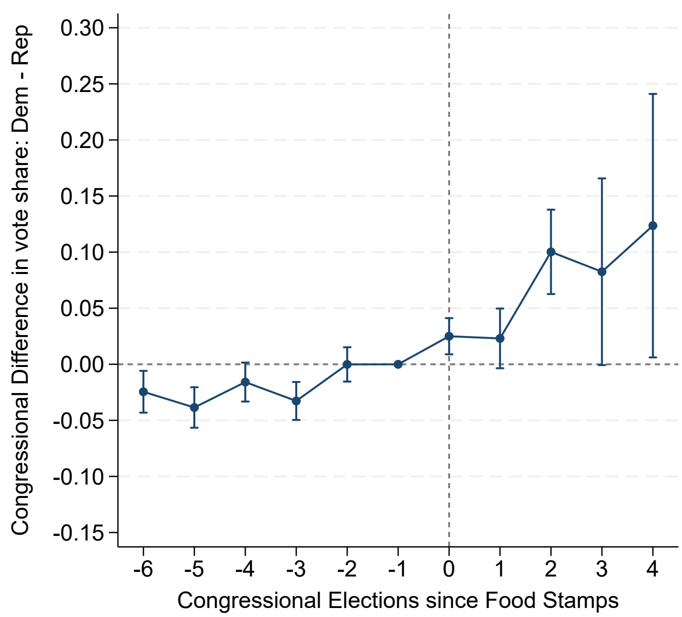
(b) FS mentioned by Republican representatives

This figure plots event study estimates of the effect of the Food Stamp (FS) program roll-out on the number of days that FS are mentioned in the House of Representatives, by county-party-time. Specifically, the outcome is an indicator for days on which a Representative mentions FS in a Congressional speech, aggregated to the county-year level. The year before rollout is the omitted baseline. Callaway and Sant'Anna (2021) estimates, with 95% confidence intervals clustered by county.

Figure 4: Event studies: elections



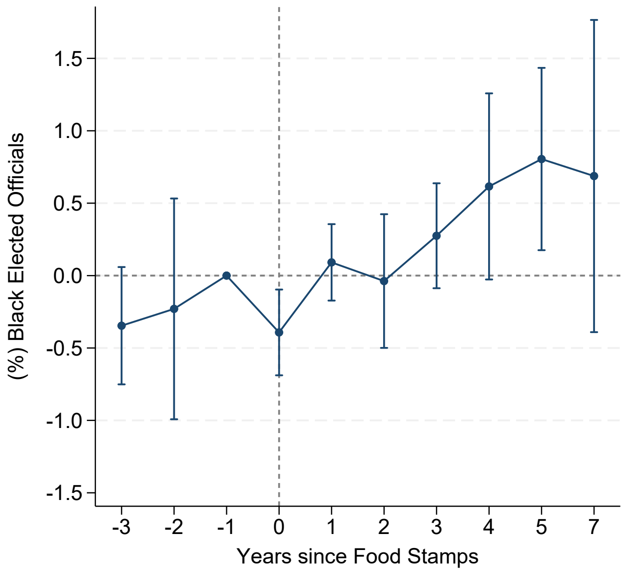
(a) Democratic vote share in elections to U.S. Congress



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

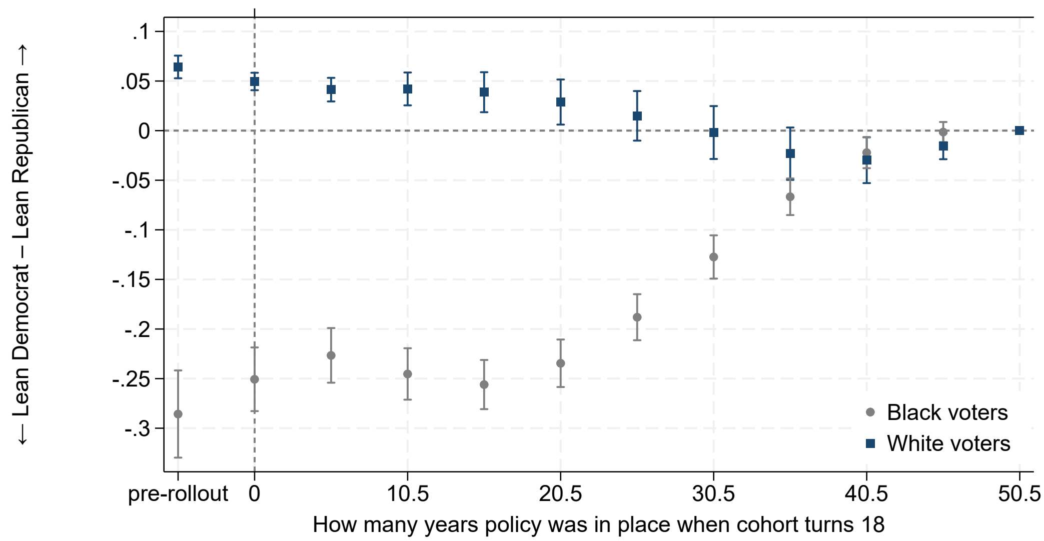
This figure plots event study estimates of the effect of Food Stamp program roll-out on Democratic vote share in elections for U.S. Congress. The estimates (in percentage points) use county×election level data from 1948 through 1972, sourced from ICPSR Electoral Data for Counties in the United States: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

Figure 5: Event study: Share of Black elected officials



This figure plots 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). Callaway and Sant'Anna (2021) estimates, with 95% confidence intervals clustered by county.

Figure 6: Long-run effects on voter registration: Event study by birth cohort bins



This figure plots the event-study coefficients from a stacked version of equation 12. 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 vertical line corresponds to the cohort aged 18 when the program was rolled out; cohorts to the left were older. The 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 horizontal 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.

Table 1: Short-run effects

Panel A: Voter registration rates by race 1960–1972

	(1) Black Reg/Popn.	(2) White Reg/Popn.
Food Stamps	0.013***	−0.004
	(0.005)	(0.012)
N. obs.	1,062	1,062
N. clusters (county)	443	443
Year FE	Y	Y
County FE	Y	Y

Panel B: Support for Democratic party in Congress

	(1) All Affiliations	(2) Democrats	(3) Republicans	(4) Independents
$FS_{state\,share}$	−0.206**	−0.083**	−0.042	−0.003
	(0.088)	(0.032)	(0.068)	(0.175)
$FS_{state\,share}\times$ 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 (state)	50	50	50	50
Year FE	Y	Y	Y	Y
State FE	Y	Y	Y	Y
Birth Year FE	Y	Y	Y	Y

Panel C: Approval of Democratic president

	(1) All Affiliations	(2) Democrats	(3) Republicans	(4) Independents
$FS_{state\,share}$	−0.065	−0.005	−0.122	−0.058
	(0.069)	(0.076)	(0.080)	(0.092)
$FS_{state\,share}\times$ 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 (state)	50	50	50	50
Year FE	Y	Y	Y	Y
State FE	Y	Y	Y	Y
Birth Year FE	Y	Y	Y	Y

Panel A reports Callaway and Sant'Anna (2021) DID estimates of the effects of the Food Stamp program roll-out on voter registration at the county level from 1960 through 1972. Registration numbers are scaled by each county's number of eligible voters. 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 Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas, and Virginia. Panels B and C report DID estimates of the effect of FS implementation on survey responses. The outcome in panel B 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 in Panel C 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 survey 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 recorded in these surveys. Columns 2, 3 and 4 report estimates from subsamples of individuals identifying as Democrats, Republicans or Independents. 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 B and 26 for Panel C) and is provided by the Roper Center (https://ropercenter.cornell.edu/). Standard errors in parentheses; *** 1%, ** 5%, * 10% significance level.

Table 2: 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

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 3: Long-run effects on voter registration and electoral impact

Panel A	(1) Republican	(2) Democrat	(3) Independent	(4) Voted%	(5) Voted%× Republican	(6) Voted%× Democrat	(7) Voted%× Independent
Food Stamps	0.0102***	-0.0215***	0.0113***	-0.0113***	0.0190***	-0.0378***	0.0075***
	(0.0036)	(0.0042)	(0.0042)	(0.0030)	(0.0035)	(0.0027)	(0.0019)
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	(1) Republican	(2) Democrat	(3) Independent	(4) Voted%	(5) Voted%× Republican	(6) Voted%× Democrat	(7) Voted%× Independent
Food Stamps (FS)	0.0208***	-0.0191***	-0.0017	-0.0037	0.0375***	-0.0486***	0.0073***
	(0.0042)	(0.0043)	(0.0044)	(0.0029)	(0.0039)	(0.0028)	(0.0021)
FS × Black	-0.1112***	0.0268***	0.0844***	-0.0094*	-0.1247***	0.1221***	-0.0068***
	(0.0091)	(0.0082)	(0.0079)	(0.0053)	(0.0043)	(0.0072)	(0.0019)
FS × Hispanic	-0.0604***	0.0242***	0.0362***	-0.0619***	-0.0897***	0.0274***	0.0004
	(0.0064)	(0.0090)	(0.0069)	(0.0040)	(0.0036)	(0.0046)	(0.0016)
FS × Asian	-0.0129*	-0.0494***	0.0623***	-0.0980***	-0.0739***	-0.0318***	0.0077**
	(0.0076)	(0.0084)	(0.0059)	(0.0066)	(0.0058)	(0.0041)	(0.0031)
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

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 state voter rolls and 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+). *Voted%* is the share of even year elections an individual voted in. *Vote%× Republican* interacts *Vote%* 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. Panel A displays estimates of the coefficients in the stacked version of equation 10 as described in Section 6; Panel B adds race fixed effects and their interaction with the FS variable. FE denotes fixed effects. 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

This table replicates the specification in Table 3 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
$FS_{cont.}$	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)
$FS_{cont.}$ × 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)
$FS_{cont.}$ × 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)
$FS_{cont.}$ × 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

This specification incorporates a fuzzy-treatment framework as described in Section 6.2.3: $FS_{cont.}$ denotes a *continuous* treatment variable. White is the omitted racial/ethnic group. Standard errors clustered by county. *** 1%, ** 5%, * 10% significance level.

Appendix

Table of Contents

A. Model Proofs and Extensions

A.1 Baseline 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 16; 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:*

$$\beta_L^* - \beta_R^* = \frac{2}{3}(\alpha_W - \alpha_B)\big[y^* - \frac{y^*}{\phi^{t^*}}\big] \tag{15}$$

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

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:

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

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

Similarly, solving the FOC for party R:

$$\implies 2\beta_R - \beta_L = B\alpha_B + (\alpha_B - \alpha_W)\big[y^* - \frac{y^*}{\phi^{t^*}}\big] + (\alpha_W - \alpha_B)\Big(\frac{1}{\gamma} - \frac{1}{2\psi\phi^{t^*}}\Big). \tag{18}$$

Using Equations (17) and (18), 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 (15) and setting $\underline{t} = 0$.

□

Proof of Proposition 2:

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

□

A.2 Imperfect Updating of Beliefs

The following result shows that updating beliefs is formally equivalent to our baseline model with an attenuated belief stickiness.

Proposition A2 (Cohort beliefs with imperfect updating). *Let $\phi \in (0,1)$ denote the persistence parameter in the learning process and let $\omega \in [0,1]$ denote the degree of belief stickiness. Fix an evaluation date $t+T$, and consider a cohort that became politically aware at date $N \leq t+T$.*

For each voter block $i \in \{W, B\}$, define

$$s_W := \beta_L - \beta_R \quad \text{and} \quad s_B := \beta_R - \beta_L.$$

Then cohort beliefs at date $t+T$ are a convex combination of the belief formed at the time of political awareness and the belief implied by current information, with weight (ω) capturing belief stickiness. A lower (ω) attenuates cohort-specific imprinting by shifting beliefs toward the contemporaneous information environment. More precisely, the belief of a voter in block i from cohort N at date $t+T$ is given by

$$\begin{aligned} v_i(N, t+T) &= \omega\big[(1-\phi^N)y^* + \phi^N s_i\big] + (1-\omega)\big[(1-\phi^{t+T})y^* + \phi^{t+T} s_i\big] \\ &= \Big[\omega(1-\phi^N) + (1-\omega)(1-\phi^{t+T})\Big]y^* + \Big[\omega\phi^N + (1-\omega)\phi^{t+T}\Big]s_i \\ &= (1-\tilde{\phi}_{N,t+T})y^* + \tilde{\phi}_{N,t+T}s_i, \end{aligned}$$

where the ***effective*** *information dissemination is*

$$\tilde{\phi}_{N,t+T} := \omega\phi^N + (1-\omega)\phi^{t+T}.$$

Proof: By construction, the belief of cohort N at date $t+T$ is a convex combination of its initial belief at the time of political awareness,

$$v_i^{\text{init}}(N) = (1-\phi^N)y^* + \phi^N s_i,$$

and the belief a newly aware cohort would hold at date $t+T$,

$$v_i^{\text{curr}}(t+T) = (1-\phi^{t+T})y^* + \phi^{t+T} s_i,$$

with weights ω and $1-\omega$, respectively. Substituting these definitions yields

$$v_i(N, t+T) = \omega v_i^{\text{init}}(N) + (1-\omega)v_i^{\text{curr}}(t+T),$$

which equals

$$\Big[\omega(1-\phi^N) + (1-\omega)(1-\phi^{t+T})\Big]y^* + \Big[\omega\phi^N + (1-\omega)\phi^{t+T}\Big]s_i.$$

Defining $\tilde{\phi}_{N,t+T} := \omega\phi^N + (1-\omega)\phi^{t+T}$ yields the stated expression. $\square$

Proposition A2 states that imperfect updating is formally equivalent to our baseline model. When

$\omega = 1$ (fully sticky beliefs), then $\tilde{\phi}_{N,t+T} = \phi^N$ and $v_i(N, t+T)$ collapses to the baseline model's cohort belief. At the other extreme, when $\omega = 0$ (fully flexible beliefs), $\tilde{\phi}_{N,t+T} = \phi^{t+T}$ and all cohorts share the same belief as a newly aware cohort at date $t + T$.

A.3 Extension: Heterogeneous Policy Impacts by Voter Block.

We now extend the model to allow the true policy impact to differ across voter blocks. In particular, let

$$y_i^* = \begin{cases} y_W^* & \text{if } i = W, \\ y_B^* & \text{if } i = B. \end{cases}$$

All other primitives of the model remain unchanged.

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

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

Proof: Follows directly from the voter learning process and the expected value of the uniform distribution, replacing y^* with y_i^* for each voter block. □

Lemma A2′ (Representative-voter voting probabilities with heterogeneous policy impacts). *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 given by:*

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

where $\chi(T) = \frac{1-\phi^T}{-\ln\phi} \times \frac{1}{T}$ *satisfies the properties listed in Lemma A2.*

Proof: Averaging the cohort voting probabilities in Lemma A1′ over $N \in \{\underline{t}, \underline{t}+T\}$ and defining $\phi^{t^*} := \chi(T)\phi^{\underline{t}}$ yields the stated expressions. □

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

$$\beta_L^* - \beta_R^* = \frac{2}{3}\big(\alpha_W y_W^* - \alpha_B y_B^*\big)\Big[1 - \frac{1}{\phi^{t^*}}\Big], \tag{19}$$

$$\beta_L^* + \beta_R^* = 2B\alpha_B + (\alpha_W - \alpha_B)\Big(\frac{2}{\gamma} - \frac{1}{\psi\phi^{t^*}}\Big). \tag{20}$$

Proof: Assume $B^L = B^R = B$ and define $\beta_W^P = \beta_P$, $\beta_B^P = B - \beta_P$. Parties maximize the same objective functions as in Proposition A1, with voting probabilities given by Lemma A2′.

The first-order conditions for parties L and R remain linear in (β_L, β_R), as $\partial P_W^L/\partial\beta_L = \psi\phi^{t^*}$ and $\partial P_B^L/\partial\beta_L = -\psi\phi^{t^*}$ are unchanged. Solving the resulting system yields Equations (19) and (20).

When $y_W^* = y_B^* = y^*$, the expressions collapse to those in Proposition A1. □

Proposition 2′ (Racial Voter Polarization with heterogeneous policy impacts). *Assume that the true policy impact differs across voter blocks, so that* $y_i^* = y_W^*$ *for voter block* W *and* $y_i^* = y_B^*$ *for voter block* B. *Equilibrium voter political polarization is given by:*

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

$$\sigma_N(T) = 2\psi(1-\phi^N)\big(y_B^* - y_W^*\big) + \frac{8}{3}\psi\,\phi^N\big(\alpha_W y_W^* - \alpha_B y_B^*\big)\Big(\frac{1}{\chi(T)} - 1\Big). \tag{21}$$

2. **Electorate-wide racial voter polarization:**

$$\sigma(T) = (1-\chi(T))\left[2\psi\big(y_B^* - y_W^*\big) + \frac{8}{3}\psi\big(\alpha_W y_W^* - \alpha_B y_B^*\big)\right]. \tag{22}$$

Proof: We follow the same steps as in the proof of Proposition 2 (see Proposition 2 and Definitions 1–2).

Step 1: Within-cohort polarization. By Definition 1,

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

Using complementarity $P_i^R(N,T) = 1 - P_i^L(N,T)$, we obtain

$$\sigma_N(T) = 2P_B^L(N,T) - 1 + 1 - 2P_W^L(N,T) = 2\big(P_B^L(N,T) - P_W^L(N,T)\big).$$

Under heterogeneous impacts, Lemma A1′ implies:

$$P_W^L(N,T) = \frac{1}{2} + \psi\big[(1-\phi^N)y_W^* + \phi^N(\beta_L^* - \beta_R^*)\big],$$
$$P_B^L(N,T) = \frac{1}{2} + \psi\big[(1-\phi^N)y_B^* + \phi^N(\beta_R^* - \beta_L^*)\big].$$

Therefore,

$$\begin{aligned} P_B^L(N,T) - P_W^L(N,T) &= \psi\Big[(1-\phi^N)(y_B^* - y_W^*) + \phi^N\big((\beta_R^* - \beta_L^*) - (\beta_L^* - \beta_R^*)\big)\Big] \\ &= \psi\Big[(1-\phi^N)(y_B^* - y_W^*) - 2\phi^N(\beta_L^* - \beta_R^*)\Big]. \end{aligned}$$

Multiplying by 2 yields

$$\sigma_N(T) = 2\psi(1-\phi^N)(y_B^* - y_W^*) - 4\psi\phi^N(\beta_L^* - \beta_R^*). \tag{23}$$

Finally, substituting the heterogeneous-impact equilibrium difference from Proposition A1′,

$$\beta_L^* - \beta_R^* = -\frac{2}{3}\big(\alpha_W y_W^* - \alpha_B y_B^*\big)\Big(\frac{1}{\chi(T)} - 1\Big),$$

into (23) yields Equation (21).

Step 2: Electorate-wide polarization. By Definition 2 and the same complementarity argument,

$$\sigma(T) = 2\big(P_B^L(T) - P_W^L(T)\big).$$

Using Lemma A2′ (heterogeneous impacts) and writing $\phi^{t^*} = \chi(T)$ for the case where the earliest cohort has $N = 0$,

$$P_W^L(T) = \frac{1}{2} + \psi\big[(1-\chi(T))y_W^* + \chi(T)(\beta_L^* - \beta_R^*)\big],$$
$$P_B^L(T) = \frac{1}{2} + \psi\big[(1-\chi(T))y_B^* + \chi(T)(\beta_R^* - \beta_L^*)\big].$$

Hence,

$$\begin{aligned}\sigma(T) &= 2\psi\Big[(1-\chi(T))(y_B^*-y_W^*) - 2\chi(T)(\beta_L^*-\beta_R^*)\Big] \\ &= 2\psi(1-\chi(T))(y_B^*-y_W^*) - 4\psi\chi(T)(\beta_L^*-\beta_R^*).\end{aligned}$$

Substituting the same equilibrium difference $\beta_L^*-\beta_R^*$ and simplifying yields Equation (22). $\square$

B. Heterogeneities and Robustness of Long-Run Effects

B.1 Heterogeneity by Gender

Given the explicit emphasis on gender invoked by nationally prominent "Welfare Queen" narratives in the 1970s, it is important 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 3 and IA3, 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 IA3, columns 2 and 3). Further, column 4 shows that the full sample reduction in the voting rate (Table 3 panel A column 4) 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 as a registered Republican or an independents is largely, but not solely, due to men (Table IA3, columns 5 and 7). The reduction in voting as a Democrats is similar across genders. In short, FS rollout appears to have pushed women away from the Democratic party, and towards registering as independents. By contrast, treatment increases male Republican affiliation and Republican electoral impact. However, when we disaggregate results by race in a female-only subsample in Panel B of IA3 we find very similar results to those in the full sample (Table 3), indicating that the intersection of race and gender does not drive any additional effects of FS exposure.

B.2 Heterogeneity by County Demographics

We also explore whether long-run effects of adult exposure to the FS rollout are different in areas with higher Black populations. Panel A of Appendix Table IA4 shows the results of intersecting our treatment indicator with the county-level share of Black individuals. Several patterns emerge. 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 Democratic and away from independent registration.

Looking at the margin of electoral impact, 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 electoral impact, these incremental effects of regional racial demographics are consistent with stronger backlash by White voters in areas with more Black potential beneficiaries.[1]

We also examine the long-run effects of FS in the areas most likely to benefit directly: high poverty counties. Panel B of Appendix Table IA4 interacts FS treatment with the share of families living under the poverty line. For White voters, treatment together with local 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, local 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 display a different pattern: Independent registration appears to increase while Republican registration decreases (along with Democratic registration to a lesser, and insignificant extent). For electoral impact, the shift of White voters toward the Republican party sharply increases with regional poverty, and decreases for Democrats, while for Black and Hispanic voters, the opposite occurs. And regional poverty appears to be quite meaningful in increasing Asian alignment with 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 or know people receiving FS aid, or (ii) have deliberately been led to believe this by political rhetoric.

B.3 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 occurred during the rollout – the Voting Rights Act of 1965 – affected polarization. Second, we explore whether the economic health of the county mediates impacts. Finally, we consider how the presence of local churches affects the magnitude of treatment response (Glaeser et al. 2005).

B.3.1 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, 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

[1] Greater support from Black voters for the Democratic party may be because living in areas with a larger share of Black residents makes it more likely for an individual to have a social connection with someone who has benefited from FS directly.

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.[2]

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).[3] 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 IA5, indicate that the interaction of the FS Program with VRA coverage contributed to the shift in White registrations rightwards in response to treatment, with Republican registrations rising and Democratic registrations falling (columns 1 and 2). For electoral impact, the VRA's interaction with FS contributed to reduced White voting as independents, with some suggestive evidence of reduced White voting as Democrats (but no statistical significance). Thus, in this predominantly southern subsample, the VRA appears increase the rightward shift of White people exposed to FS rollout. For non-Whites, we see relatively similar patterns 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 voters (relative to Whites), along with greater Black voting as independents.[4]

Taken together, the evidence in Table IA5 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.

B.3.2 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

[2] The weaponization of food security in response to Black political mobilization finds support in the historical record (Zinn, 1964).

[3] 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.

[4] This Black shift towards 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. 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.

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 or the recipients undeserving, 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 the 10th percentile of personal income growth between 1929 and 2010, -1.06%. Our local recession measure is the percentage of years the state is in recession between each county's FS rollout year and 2020.

Table IA6 replicates the specifications in Table 3 and adds interactions with the economic vulnerability variable (the county FE 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. 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 electoral impact, White voting as Republicans in counties more exposed to recessions is lower (column 5), which is only partially offset by increased White voting as 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 electoral impact, where the net effect is still to reduce voting by Hispanics as Republicans and increase voting by Hispanics as Democrats, albeit by less than for Whites.

Summarizing, local recessions are associated with substantial and heterogeneous effects on the long run political consequences of the FS Program. Blacks *and* Whites in high recession areas are less likely to register as Republicans and more likely to register as Democrats in response to the FS treatment, while for Hispanics the shift is towards the Republican party. Examining electoral impact, White voters exhibit a greater sensitivity to FS treatment with respect to local recessions.

B.3.3 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 (see, e.g., Wald and Calhoun-Brown 2018). Second, Christian theology promotes help for the poor, which may support political views in favor of public programs like FS. 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). Fourth, religion can serve as a source of political strategic extremism (Glaeser et al. 2005).

We explore the role of churches in mediating the long run effects of the FS Program by interacting a measure of Church density, measured as number of churches per 1,000 inhabitants (ICPSR, 1952), with the FS and race variables. Appendix Table IA7 presents the estimates. The first thing of note is that the coefficients on *Church Density* × *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* × *Race* variables) are similar to those in Table 3, 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* × *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* × *Church Density* × *Race* is consistent with church density inducing a rightward shift in voter registrations in response to Food Stamps, with the strongest effects for Hispanics.

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

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

B.4 Robustness: Fixed Effect Specifications

To evaluate the robustness of our long-run 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 IA8. Because our treatment is itself at the county × birth year level, this vector of new FE absorbs the main FS variable, 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.

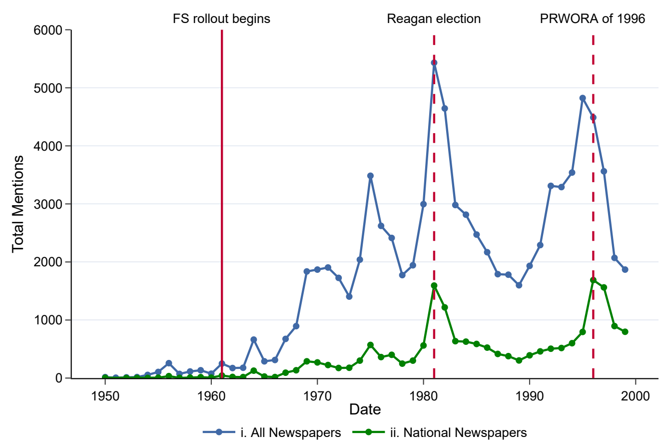
As a second robustness test, Appendix Table IA9 reports results from instead including a vector of birth year × race FE, which absorb differences across birth cohorts by race. These can be seen as race-specific "generation" effects, with generations defined very granularly at the yearly level. We find 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 IA10: again, the pattern of treatment effects is consistent with our core findings, and magnitudes increase somewhat.[5]

Finally, in Appendix Table IA12 we interact by pairs the three FE vectors used in our baseline specification to generate County × Birth Year, Birth Year × Race, and County × Race FEs. As was

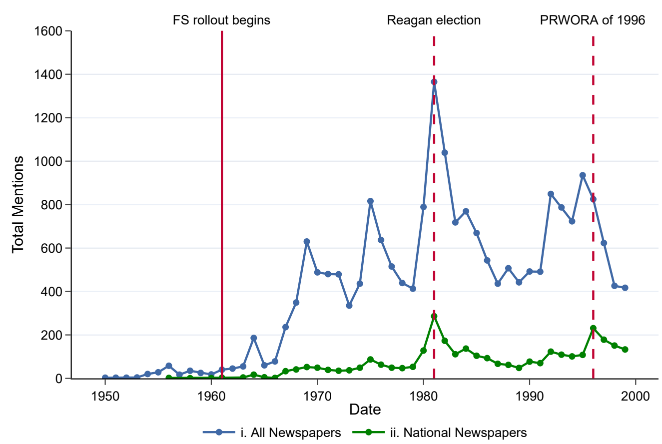
[5] In Appendix Table IA11 we further replace County × Race with *Census Block* × Race FEs. Estimates are similar but slightly smaller than those in Table IA10.

the case for Table IA8, these absorb the FS variable, 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.

Figure IA1: Newspaper coverage of Food Stamps



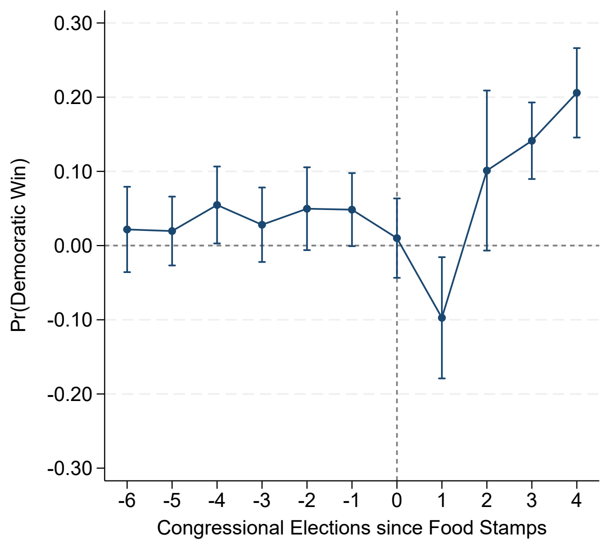
(a) Newspaper articles mentioning "Food Stamps"



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

These graphs display yearly counts of news articles mentioning Food Stamps between 1950 and 2000. Panel (a) counts news articles containing the term "food stamp" within the article body for both *All* (blue, top line) and *National* newspaper categories (green, bottom line). panel B (b) adds a racial term (Black, Negro, or African American) to the search within the article text. In both graphs, the first vertical 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.

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



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.

C.3 Long run effects: States collecting race directly

Table IA1: Long-run effects

Restricted to states that ask voters their race at voter registration

	(1) Republican	(2) Democrat	(3) Independent	(4) Voted%	(5) Voted%× Republican	(6) Voted%× Democrat	(7) Voted%× Independent
Food Stamps	0.0241***	-0.0221***	-0.0019	-0.0068**	0.0384***	-0.0528***	0.0077***
	(0.0083)	(0.0079)	(0.0084)	(0.0027)	(0.0060)	(0.0046)	(0.0018)
FS × Black	-0.0836***	0.0465***	0.0371***	0.0079**	-0.1357***	0.1562***	-0.0125***
	(0.0127)	(0.0142)	(0.0089)	(0.0032)	(0.0065)	(0.0062)	(0.0021)
FS × Hispanic	0.0051	-0.0047	-0.0005	-0.0697***	-0.0672***	-0.0059	0.0034**
	(0.0073)	(0.0097)	(0.0068)	(0.0043)	(0.0058)	(0.0057)	(0.0014)
FS × Asian	0.0201*	-0.0818***	0.0617***	-0.0968***	-0.0709***	-0.0352***	0.0094***
	(0.0112)	(0.0106)	(0.0128)	(0.0092)	(0.0072)	(0.0048)	(0.0020)
N. obs.	81,781,439	81,781,439	81,781,439	81,781,439	81,781,439	81,781,439	81,781,439
N. Clusters	1,466	1,466	1,466	1,466	1,466	1,466	1,466
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

This table reports estimates from the specification Table 3, Panel B, using only states that ask voters their race on voter registration forms in the 2010s (Alabama, Florida, Georgia, Louisiana, North Carolina, South Carolina, Pennsylvania, Tennessee). *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.

C.4 Long run effects: Reduced geographic measurement error

Table IA2: Long-run effects:
Estimates by age at which current home purchased

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Age at Purchase of Current Home	Under 25	Under 30	Under 35	Under 40	Under 45	Under 50	Under 55	Under 60	All
Panel A: Voted % × Republican									
Food Stamps	0.0179***	0.0148*	0.0132**	0.0143**	0.0160***	0.0167***	0.0175***	0.0174***	0.0124***
	(0.0036)	(0.0081)	(0.0066)	(0.0056)	(0.0047)	(0.0042)	(0.0040)	(0.0037)	(0.0032)
FS × Black	-0.1477***	-0.1199***	-0.0943***	-0.0793***	-0.0747***	-0.0726***	-0.0723***	-0.0720***	-0.0650***
	(0.0122)	(0.0113)	(0.0094)	(0.0085)	(0.0080)	(0.0076)	(0.0074)	(0.0072)	(0.0066)
Panel B: Voted % × Democrat									
Food Stamps	-0.0398***	-0.0328***	-0.0270***	-0.0222***	-0.0201***	-0.0195***	-0.0196***	-0.0199***	-0.0226***
	(0.0033)	(0.0043)	(0.0034)	(0.0032)	(0.0029)	(0.0027)	(0.0026)	(0.0025)	(0.0024)
FS × Black	0.1584***	0.1197***	0.0805***	0.0546***	0.0484***	0.0458***	0.0440***	0.0446***	0.0445***
	(0.0113)	(0.0109)	(0.0114)	(0.0107)	(0.0101)	(0.0097)	(0.0092)	(0.0090)	(0.0078)
N. obs.	2,142,195	7,776,081	13,969,623	18,692,401	21,767,830	23,675,526	24,910,785	25,710,233	26,702,168
N. Clusters	5,646	5,821	5,899	5,959	5,984	6,008	6,022	6,037	6,068
County FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Birth year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Race FE	Y	Y	Y	Y	Y	Y	Y	Y	Y

This table replicates the baseline specification in Table 3, columns 5 and 6, for subsamples of voters that purchased their current home at different ages. We our main dataset with CoreLogic's Deeds records – a nearly comprehensive dataset of properties obtained from county administrative records. Using Deeds records' date of sale, we are able to analyze voters who have purchased their current home at specific age cutoffs. *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.

C.5 Long run effects: Heterogeneity

Table IA3: Long-run effects: Heterogeneity
Women

Panel A	(1) Republican	(2) Democrat	(3) Independent	(4) Voted%	(5) Voted%× Republican	(6) Voted%× Democrat	(7) Voted%× Independent
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)
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)**
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

This table adds a Female indicator and interaction with FS to the specification in Table 3 Panel A. Panel B restricts 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 IA4: 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 IA4 (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

The specification and data are the same as in Table 3, Panel B, 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.

C.6 Long run effects: Other Events

Table IA5: Long-run effects: Other Events
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

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 IA6: Long-run effects: Other Events

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

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

Table IA7: Long-run effects: Other Events

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

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.

C.7 Long run effects: Robustness

Table IA8: 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

This table replicates the specification in Table 3, 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 IA9: 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

This table replicates the baseline specification in Table 3, 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 IA10: 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

This table replicates the baseline specification in Table 3, 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 IA11: 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

This table replicates the baseline specification in Table 3, 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 IA12: Long-run effects

Robustness #5: (County × Birth Year) & (Birth Year × Race) & (County × 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	Y	Y	Y	Y	Y	Y	Y
Birth year×Race	Y	Y	Y	Y	Y	Y	Y
County×Race	Y	Y	Y	Y	Y	Y	Y

This table replicates the baseline specification in Table 3, but replaces County, Birth year, and Race fixed effects (FE) with three interacted FEs: County×Birth year, Birth year×Race, and County×Race FE. Because the *Food Stamps* (*FS*) treatment is at the County×Birth Year level, these new interacted fixed effects absorb the *FS* variable, but still allow 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.