The Electoral Effects of Social Policy: Expanding Old-Age Assistance, 1932–1940

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Under what conditions do means-tested programs increase beneficiaries' political participation? Recent scholarship has begun to shed light on this question through a series of causal studies of Medicaid expansion. This article builds on those analyses by exploring an additional case, the US expansion of Old-Age Assistance (OAA) programs between 1932 and 1940. It provides new evidence that means-tested programs can mobilize their beneficiaries and also sheds light on how these effects emerge. Exploiting state-by-state variation in expansion, I find that increases in OAA generosity increased turnout in elderly counties but increases in the OAA coverage rate did not. These findings show that resource effects are crucial to generating positive feedback and can do so even in the case of a highly stigmatizing, means-tested program. I further find that by mobilizing elderly Republican recipients, OAA cost FDR votes in Republican-leaning counties, suggesting that even positive participatory effects may undermine social programs' entrenchment.

The notion that policies reshape the electorate has long roots in political science (Pierson 1993; Schattschneider 1935). More recently, scholars have shown that the emergence of positive feedback effects from new policies is contingent (Oberlander and Weaver 2015; Patashnik and Zelizer 2013). Among the central findings of this research is that universal programs generally create engaged voter-citizens, while only certain means-tested programs do the same (Campbell 2003; Soss 1999). Scholars attribute this variation among means-tested programs to the generosity of their benefits, whether beneficiaries understand they are receiving help from the government, and the lessons those programs teach recipients about their political efficacy (Bruch, Ferree, and Soss 2010; Campbell 2007; Mettler 2018; Michener 2018; Soss 1999).

Existing research, however, has found it difficult to disentangle the effect of these program design elements from preexisting differences within beneficiary populations that also shape their political participation (Mead 2001). While scholars have begun to address this challenge using quasi-experimental evidence, that evidence thus far is limited to the case of Medicaid expansion under the Affordable Care Act (ACA; Baicker and Finkelstein 2019; Clinton and Sances 2018). Moreover, evidence of Medicaid increasing political participation raises further questions about how means-tested programs might generate these effects.

This article builds on causal studies of means-tested programs' effects on political participation in three ways. First, it evaluates another social program to determine whether Medicaid's participatory effects are reproducible in other contexts. Second, it offers suggestive evidence as to how those effects emerge through different components of program expansion. And third, it extends the policy feedback literature that focuses on political participation and asks, if new policies increase turnout and change the composition of the electorate, can they also affect partisan vote share?

To address these questions, I examine the electoral effects of Old-Age Assistance (OAA) expansion as part of the Social Security Act of 1935. OAA was a means-tested public assistance program that provided cash payments to destitute elderly applicants. Following the existing literature, expectations about its effects on political participation are unclear: it provided meaningful and visible benefits to recipients, which may have increased participation, but also subjected those recipients to extensive and stigmatizing scrutiny by caseworkers, which may have decreased participation.

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This article evaluates those possibilities. Because OAA was funded by a federal-state matching program beginning in 1936, states varied the extent to which they expanded their programs. This variation allows me to use a difference-in-differences design that compares differences in electoral outcomes over time, among counties in high- and low-expansion states and among counties with a high and low proportion of beneficiaries—in this case, the elderly. This identification strategy isolates the effects of OAA over electoral outcomes and limits the possibility of selection bias (Clinton and Sances 2018).

Moreover, OAA provides an ideal opportunity to isolate the mechanisms by which a means-tested program might affect political participation. This is because states varied not just in whether they implemented the program, as in the case of Medicaid expansion, but in how they chose to expand the program. This allows me to evaluate the possibility that a certain kind of expansion (increasing the generosity of payments) increased voter turnout, while another kind (increasing the percentage of people covered by the program) did not.

Consistent with my expectations, I find that an average increase in OAA payments per beneficiary increased turnout in presidential elections by 0.93 percentage points in elderly counties from 1932 to 1940, while increasing the coverage rate of elderly residents did not have any effect on turnout. This indicates that the resource mechanism was crucial to OAA's participatory effects, but it also offers evidence that OAA did not induce large negative interpretive effects. Taken together, results from both treatment measures suggest that the generosity of means-tested programs, even more so than their interpretive lessons, is crucial to shaping the trajectory of feedback effects. Even under conditions that contemporaries considered highly stigmatizing (Mettler 1998), OAA expansion did not have negative participatory effects via either treatment measure. I also find that OAA expansion affected not just voter turnout but partisan electoral outcomes: OAA cost Franklin Delano Roosevelt (FDR) votes in elderly Republican-leaning counties.

These findings offer two insights into how policy feedback effects operate within means-tested programs. First, they indicate that the evidence from Medicaid expansion was not limited to just that policy, and they more broadly suggest that generous means-tested programs have the potential to mobilize beneficiaries. Second, this article shows how social programs can affect aggregate electoral outcomes by mobilizing beneficiaries and changing the composition of the electorate. In this case, expansion affected partisan vote share by mobilizing a voting bloc—the elderly—that was more prone to voting Republican (Andersen 1979). As such, policy feedback scholars may need to consider vote share in addition to turnout when it comes assessing whether the effects of other social programs lead to entrenchment (Oberlander and Weaver 2015).

POLICY FEEDBACK AND ELECTORAL OUTCOMES

The core tenant of the policy feedback literature is that, under certain conditions, social programs have the power to affect both political participation and public opinion (Campbell 2012). In elaborating these conditions, scholars focus on how both *resource* and *interpretive* mechanisms shape the trajectory of potential feedback (Pierson 1993). First, social programs increase recipients' resources and in doing so may enhance their capacity for political engagement. This logic follows from the association between voters' level of resources and their political participation (Brady, Verba, and Schlozman 1995; Leighley and Nagler 2014). Although the causal impact of economic resources over individuals' likelihood of voting is unclear, there is evidence that social policies have induced recipients to participate by increasing their resources (Campbell 2003; Mettler 2002).

Policies also have interpretive effects, as they provide cues to information-constrained citizens about how to understand their own interests (Pierson 1993). While resource effects depend solely on the size of benefits, interpretive effects vary on the basis of program design and administration. Welfare programs are "sites of adult political learning" (Soss 1999) that teach recipients about both the state and their own political efficacy.

The remainder of this section will elaborate how resource and interpretive mechanisms shape the participatory effects of means-tested programs. I then consider how social programs might also affect partisan electoral outcomes. Scholars have provided suggestive evidence that policies—through both mobilization and demobilization—can affect partisan vote share in important ways (Manza and Uggen 2004; Mettler 2018). This section will evaluate the mechanisms by which this might occur and set the stage for an empirical test in the case of OAA.

Means testing and political participation

For reasons of both resource and interpretive effects, means-tested programs, unlike universal programs such as Social Security, have often produced null or even negative effects on political participation (Mettler and Stonecash 2008; Michener 2018; Soss 2000; Verba, Schlozman, and Brady 1995). This outcome, however, is not preordained: social programs such as the Earned Income Tax Credit (EITC) and Medicaid have been shown to increase voter turnout, and others like Head Start and public housing have had null effects (Baicker and Finkelstein 2019; Bruch et al. 2010; Clinton and Sances 2018; Shanks-Booth and Mettler 2019).

What determines whether means-tested programs will generate positive feedback effects? Existing evidence points to three factors that operate through either the resource or the interpretive mechanisms. The first is the program's generosity. Does a program provide sufficient benefits to affect political participation? In the case of means-tested programs such as Temporary Assistance for Needy Families (TANF), the answer is often no. TANF provides a fraction of the benefit of Social Security and does not carry many recipients over the poverty line (Campbell 2007). This is not true of all means-tested programs, however, as the EITC provides the working poor large sums of cash annually (Halpern-Meekin et al. 2015), while other programs like Medicaid and housing vouchers provide essential goods to poor families.

Two other factors operate through the program's interpretive effects. The first of these is visibility (Mettler 2011). For recipients to learn about the government through their participation in a social program, they have to first recognize that they are receiving help from the government and attribute credit accordingly. Given the fact that an increasingly large portion of social spending is channeled through "hidden" or "submerged" tax expenditures (Howard 1993; Mettler 2011), this recognition is not guaranteed. In fact, as Mettler (2018) shows, recipients of tax expenditures are far less likely than recipients of visible policies to recognize that those programs help them.

The final factor shaping interpretive effects has to do with how recipients are treated when applying for assistance. To what extent are they subjected to scrutiny of their private lives and made to feel stigmatized for their need? The answer lies in America's history of offering welfare on the grounds of moral distinctions between the deserving and undeserving poor (Gordon 1994; Katz 1989), a demarcation that is continually reproduced as the government offers different tiers of social provision to different "kinds" of citizens (Brown 1999; Mettler 1998). The deserving participate in universal, contributory programs like Social Security, while the undeserving—often single mothers construed as Black—receive stigmatizing, means-tested public assistance such as TANF (Gilens 2000).

As a result, those deemed undeserving may be further stigmatized through their participation in means-tested social programs. But as with visibility, the degree of stigmatization varies across programs. While means-tested programs are often linked to a social work/caseworker model that requires individuals to undergo extensive scrutiny of their personal lives, they can alternatively be provided through less stigmatizing means, such as group-based eligibility requirements that offer benefits to everyone below a certain income threshold (Cates 1983; Halpern-Meekin et al. 2015; Mettler 1998).

These elements of program administration profoundly alter participants' experiences and what they learn about themselves and their government as a result. Bruch et al. (2010) argue that Head Start, which operates on a model of maximum participation by the parents, teaches recipients how to participate in the democratic process, while programs like TANF draw on a paternalistic model that reduces participants' feelings of political efficacy. This is because they are subject to what Michener (2018) refers to as the "capriciousness" of the bureaucracy and state policy. This stands in contrast to participants' experiences receiving EITC benefits without having to undergo caseworker scrutiny, which Halpern-Meekin et al. (2015) describe as "citizenship affirming."

Partisan electoral effects

If means-tested programs increase political participation, they might also affect partisan vote share. This could happen through two pathways. If a new social program mobilizes a beneficiary population that is unevenly distributed across the parties, its expansion will change partisan vote share accordingly. A program might alternatively change the way beneficiaries vote, encouraging them to reward the party that provided them with the benefit.

Extant literature suggests that the second possibility is unlikely, as it requires voters to overcome a substantial cognitive burden. First, they would have to develop favorable attitudes toward the program, then correctly attribute the program to the responsible party, and finally weight this information heavily in their vote choice. While program beneficiaries do tend to approve of new programs more than nonbeneficiaries (McCabe 2016; Mettler and Stonecash 2008), attribution and vote choice are subject to multiple obstacles, particularly partisan biases (Bartels 2002; Tilley and Hobolt 2011). This makes it more difficult for beneficiaries to correctly attribute credit and blame for new policies, particularly when the change in their status is somewhat ambiguous (McCabe 2016). Even if recipients do overcome this barrier, any evaluation of personal gain is unlikely to emerge as a key driver of vote choice, in part—again—because of partisanship.[1]

But a lack of change at the individual level does not mean that social programs will not affect partisan vote share. Conditional on mobilizing or demobilizing beneficiaries, means-tested programs will affect partisan electoral outcomes if their

1. While voters have been shown to overcome attribution challenges (see, e.g., Healy and Malhotra 2009), there is little evidence that voters make a self-interested calculation in choosing the candidate most favorable to their own material condition (e.g., Kinder and Kiewiet 1981). In fact, scholars generally regard social policy expansion as an unlikely mechanism for changing individual-level voting behavior (e.g., Galvin and Thurston 2017).

target population is generally more affiliated with one political party than the other. Scholars have suggested that exactly these effects have emerged in other cases, arguing that disparities in turnout linked to social programs and carceral policies have led to such outcomes as Tea Party control of Congress in 2010 (Mettler 2018) and George Bush's ultimate victory in Florida in 2000 (Manza and Uggen 2004).

Specifying the causal effects of means-tested programs

While a growing body of literature has provided evidence of how means-tested programs' generosity, visibility, and degree of stigmatization shape the emergence of participatory effects and ultimately partisan electoral effects, it still stumbles on two roadblocks: determining whether those relationships are causal and specifying how those such causal effects emerge. First, investigations of means-tested programs often struggle to attribute observed outcomes to "what the program does" rather than to the program's target population itself (Mead 2001, 676). In other words, they suffer from selection bias because means-tested beneficiaries have, by definition, fewer resources than nonbeneficiaries and are likely to vary in other ways that are relevant for political participation (Bruch et al. 2010). As a result, in many analyses, the causal relationship between program status and lower political participation is unclear (Baicker and Finkelstein 2019). To address these concerns, scholars have begun using quasi-experimental opportunities to isolate the causal effects of new social policies, specifically in the context of Medicaid expansion under the ACA (Baicker and Finkelstein 2019; Clinton and Sances 2018).

The current study builds on these analyses, using the case of OAA to indicate whether Medicaid's mobilizing effects may be generalized to other means-tested policies. It also goes one step further by providing suggestive evidence as to how those effects emerge, working to disentangle the resource from the interpretive mechanisms. Because means-tested programs may produce negative interpretive effects, depending on their degree of visibility and stigmatization (Bruch et al. 2010; Soss 2000), and null or positive resource effects, depending on their generosity (Patashnik and Zelizer 2013; Verba et al. 1995), null and positive findings have proved particularly difficult to interpret.

Medicaid expansion is the perfect example: Does it mobilize recipients (Baicker and Finkelstein 2019; Clinton and Sances 2018) because positive resource effects overcome negative interpretive effects? Or, as it is generally considered somewhat less stigmatizing than other means-tested programs (Cook and Barrett 1992; Grogan and Patashnik 2003), is it possible that it induces no negative interpretive effects? Michener's (2018) work suggests a third possibility, which is that resource effects are only able to overcome negative interpretive effects in states with expansionary policies.

OAA offers an opportunity to explore both mechanisms, as states used their resources to expand the program in two ways: by increasing the generosity of payments to recipients or by covering more recipients. While neither method of expansion isolates the resource and interpretive mechanisms entirely, exploring their effects side by side will offer further insight into how each mechanism contributes to the observed effects of means-tested programs.

OLD-AGE ASSISTANCE: GENEROSITY, VISIBILITY, AND STIGMA ACROSS THE STATES

OAA was a social assistance program included as a provision of the Social Security Act of 1935. The federal law built on preexisting OAA legislation in many states, the earliest of which was passed in 1923 in Montana. Before passage of the federal OAA legislation, 28 states had laws on the books that provided assistance to the elderly, but in only 10 states was coverage statewide, and in three the law was not actually carried out because the states did not have funds (Bateman and Newman 1941). In most states before 1935, a large share of the fiscal burden lay on towns and counties (Fetter 2017), which were often unable to provide any assistance. In some cases, counties simply divided their pool of resources across all applicants rather than provide subsistence benefits to the most needy (Parker 1936). The lack of federal funds, long residency restrictions, and governments' reluctance to take on new pensioners meant that only a tiny fraction of the elderly were actually receiving public assistance before 1936 (CES 1935).

In these early years of the Great Depression, as states' ability to support their own OAA programs was flagging, pressure for increased federal intervention was growing: Townsend clubs (Amenta, Caren, and Olasky 2005) and labor unions (Quadagno and Meyer 1989) organized in the mid-1930s to advocate for federal legislation to assist the elderly. With the passage of the Social Security Act, they achieved some degree of compromise from the federal government, which began its first payments on behalf of the program on February 11, 1936 (Altmeyer 1941).

OAA funding under Title I of the Social Security Act was provided through a federal-state matching program, easing the burden on both municipal and state governments. Until 1939, the federal government funded half of OAA payments, up to a cap of $30 per month per person; in 1940, the cap was raised to $40 per month (SSA 1941). The remainder was funded by local and state governments according to laws laid out in the states' OAA provisions (Fetter 2017). This meant a rapid expansion in the generosity and coverage of existing

programs after 1936, as well as expansion to new states that had previously been unable or unwilling to provide any benefits. Total payments for OAA programs skyrocketed, as figure A1 (available online) shows. Although Georgia, Virginia, Tennessee, North Carolina, South Carolina, and Kansas did not implement programs until 1937 or 1938, by 1938 every state was operating an OAA plan approved by the Social Security Board (SSB; Bateman and Newman 1941).

Generosity

OAA remained the predominant form of old-age protection in the United States until Old-Age Insurance (OAI) payments eclipsed it in the 1950s (Amenta et al.2005). Between 1932 and 1940, OAA was an overwhelmingly larger financial resource for seniors than OAI. In fact, OAI did not make any payments until the end of the period, in January 1940. Once OAI payments began, benefits from both programs were comparable: the average OAI payment was about $22.71 in December 1940, while the average OAA payment was $20.24 (Marquard 1943). But OAI's reach remained narrow in comparison with OAA. In December 1942, OAA payments reached 2.2 million elderly recipients, while OAI reached only 260,000 (Marquard 1943). As there were over 9 million Americans 65 and over in 1940, this means that less than 3% were receiving OAI payments, while about 24% were receiving OAA payments.

Not only was OAA's reach comparatively extensive, it also provided a material change in recipients' finances that was sufficient to induce positive resource effects. The poverty line for a couple in 1940 was $884.38 (Smolensky, Danziger, and Gottschalk 1987).[2] That same year, the federal government's matching cap was $40, which also set the effective maximum payment for the majority of OAA recipients. If both people in a couple received the maximum payment, they would have earned $960 annually, taking them from destitute to above the poverty line. There is also evidence that this expansion had a meaningful impact on elderly beneficiaries' lives: it reduced late-in-life work (Fetter and Lockwood 2018) and mortality (Balan-Cohen 2007). OAA was thus generous enough to keep the elderly alive without forcing them to keep working.

Visibility

OAA was not just generous, it was also visible. Much like TANF, housing vouchers, or Medicaid, it required individuals to apply for benefits from a local government office, submit to an application review, and then receive a government benefit directly.

Stigmatization

But despite its generosity and visibility, OAA was a program that by design subjected recipients to deeply stigmatizing conditions. Policy makers explicitly built OAI around a contributory, wage-related principle to avoid the stigma and social control of Elizabethan poor laws (Cates 1983). It was also implemented on a national level, above the objection of a number of US senators and New Dealers (Mettler 1998). OAA was the opposite in both cases: each state had to submit its plans to the Social Security Administration for approval but had wide bandwidth in writing its own legislation (Fetter and Lockwood 2018). Moreover, OAA eligibility was determined on the basis of means testing rather than attachment to covered employment (Murray and Pancoast 1945). As a result, states' programs varied on the basis of their eligibility requirements, fiscal capacity to make payments, and administration.

When the federal government began matching payments to state OAA programs in 1936, the SSB provided a formula that caseworkers used to determine each applicant's benefit amount: benefit = requirement − resources. For the next four years, the SSB worked to ensure that states' plans subjected applicants to the maximum degree of scrutiny to determine their exact resources: they not only ruled out offering the same benefits to everyone below an income threshold, they also insisted that applicants submit to home visits and detailed investigations of their own and their relatives' resources (Cates 1983).

As a result, OAA continued to carry the "stigma of charity," as one contemporary described it (Mettler 1998). In many ways, the experience of OAA applicants with their caseworkers mirrors the paternalistic authority relations described under TANF today: beneficiaries moved within hierarchical structures characterized by direction and supervision, learning they had little autonomy or voice (Bruch et al. 2010; Soss 2000).

State-by-state variation

What did those state laws governing OAA administration look like? Most included residency requirements and income and asset limits, but some states also imposed restrictions on family members' assets and the "moral qualifications" of recipients (Lansdale et al. 1939). The result of these policies was not just to subject the beneficiaries to humiliating experiences but also a wide variation in the generosity and coverage of OAA programs across states. In Idaho, for example, maximum payments were $25 per month, the income threshold was set at $300 a year, and the residency requirement was 15 years. California, in contrast, provided for a maximum payment of $35 per month with an asset threshold of $500 plus

2. To calculate this figure in 1940 dollars I adjusted Smolensky et al.'s figure, which is in 1980 dollars, by the Bureau of Labor Statistic's Consumer Price Index estimates.

a realty threshold of $3,000 and a residency requirement of only five out of the past nine years. Iowa fell somewhere in between, with lower maximum payments like Idaho's and looser residency and asset restrictions like California's.

Figure 1 shows how these laws led to variation in each state's OAA policies in 1936. Figure 1*A* maps differences in coverage (the portion of elderly people that received any OAA payments in each state in 1936), and figure 1*B* maps the generosity of the states' plans (the average annual payment offered to recipients in each state in 1936). As the maps indicate, the uneven expansion of OAA offers an excellent opportunity for exploiting state-by-state variation to estimate the electoral effects of the policy. Moreover, because this unevenness was created through a patchwork of state laws rather than a choice to expand/not expand—as in the case of Medicaid under the ACA—OAA presents a case in which we might garner some evidence as to the separate roles of resource and interpretive effects in shaping the electorate.

Expectations

Given that OAA provided sufficient financial resources to lift otherwise indigent people out of poverty, I expect that its expansion increased turnout through the resource mechanism. But OAA beneficiaries were also likely experiencing negative interpretive effects. Because OAA was a highly visible program, there is some possibility that it increased recipients' sense that the government was helping them, but even so, visibility would likely be insufficient to influence their turnout (Mettler 2018). More importantly, much like in paternalistic welfare arrangements today, OAA likely taught

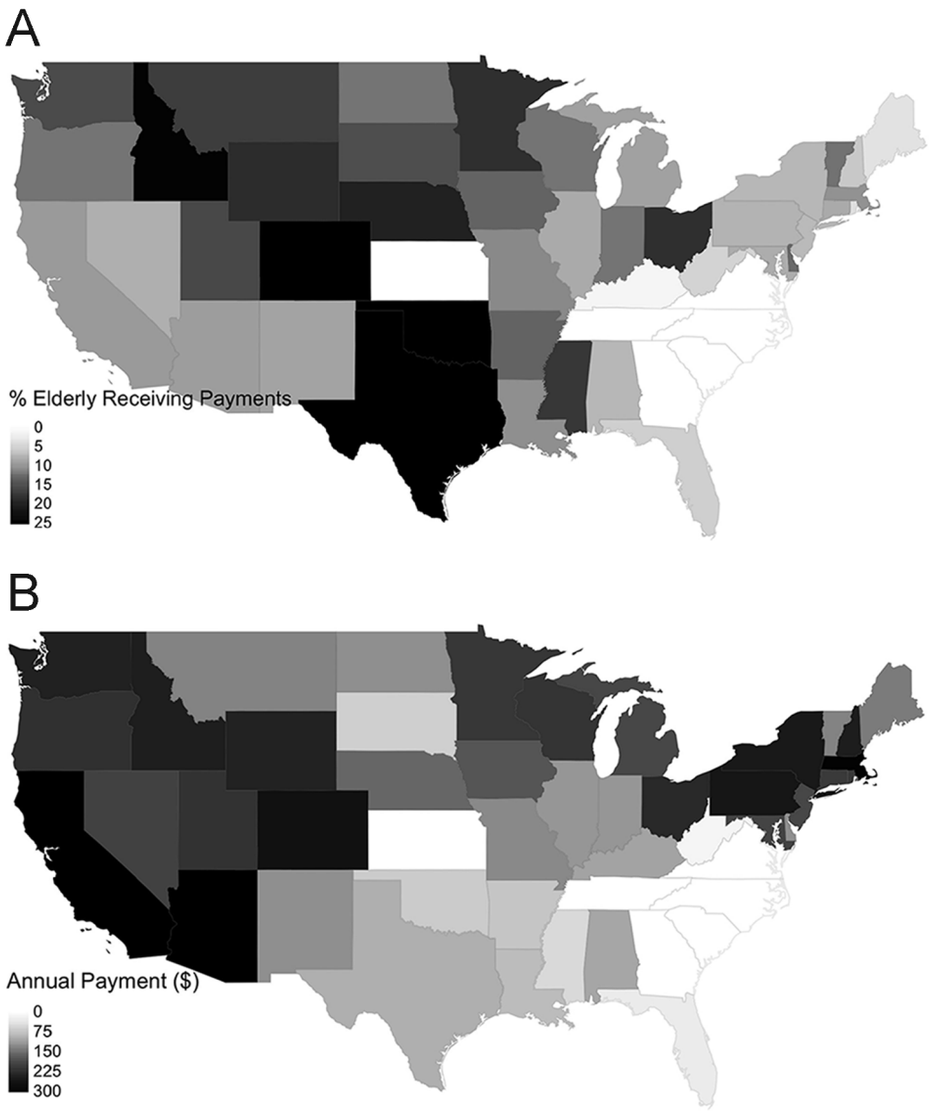

Figure 1. State-by-state variation in OAA coverage and expansion, 1936. *A*, Percentage of elderly receiving OAA payments. *B*, Average annual payment per recipient ($).

recipients that their voices did not matter and could have depressed their political engagement (Bruch et al. 2010; Soss 1999).

The combination of these expectations would render null or positive results from expansion difficult to interpret, as described in the case of Medicaid above. To begin to address this challenge, this article analyzes two measures of expansion: increased generosity of payments to recipients and increased coverage of the elderly. While neither measure alone isolates resource and interpretive effects, as both capture recipients' contact with capricious caseworkers and their receipt of government resources, taken together they will provide further insight into how the resource and interpretive mechanisms operate separately to shape political participation.

First, I expect that there will be an increase in voter turnout in states that provide more generous OAA benefits relative to states that provide less generous benefits. I further expect that these effects will be concentrated in counties with a greater share of elderly residents, those who are benefiting from the policy. Where the treatment is an expansion in OAA recipiency, my expectations are ambiguous. Relative to the generosity measure, the coverage measure should capture a larger role for interpretative rather than resource effects. As a result, increasing OAA coverage could have no effect on voter turnout in elderly counties, decrease turnout in elderly counties, or increase turnout in those counties—depending on the size of resource and interpretive effects.

The results from both treatment measures considered together will provide the clearest insight into how the resource and interpretive mechanisms are operating. If only generosity increases turnout and coverage does not, this provides support for the crucial role of the resource mechanism in shaping political participation. If coverage decreases turnout while generosity increases turnout, this would suggest that OAA expansion induced negative interpretive effects that were only counterbalanced in states with the most generous policies. However, if both generosity and coverage increase turnout, this would provide suggestive evidence that even programs offering relatively limited resources might overcome negative interpretive effects.

Finally, I expect that both mobilization and demobilization will affect partisan vote share. If elderly voters are mobilized by OAA expansion, I expect that this may have had negative electoral consequences for FDR, as older voters who grew up during a time of Republican dominance were among the least Democratic group during the New Deal era (Andersen 1979).

DATA AND METHOD

The primary analyses use county-level data on voter turnout and presidential election results from 1932 to 1940 to assess the effects of OAA expansion over two outcome variables: voter turnout in presidential elections and Democratic Party share of the total votes cast for president. These data come from Nardulli (1994).[3]

I consider two continuous treatment measures to capture variation in the resource and interpretive mechanisms. The first treatment is each state's average annual OAA payment per recipient in each year, which captures the generosity of its program as determined by its maximum monthly payment cap. The second is a measure of coverage—the portion of the population 65 and over that was actually receiving OAA benefits in a given year, as determined by their residency requirements and income and asset limitations. With limited resources, states were often faced with trade-offs between extending coverage to more people or increasing their maximum payments to existing recipients (Parker 1936). The case of Iowa exemplifies these trade-offs, as the state statute allows for easier access to the program but a lower monthly payment relative to California and Idaho. As described above, choosing one strategy or the other likely had different effects on turnout through the resource and interpretive mechanisms.

To create these measures, I collected data on states' annual payments from Social Security Administration reports and publications from the Bureau of Labor Statistics' *Monthly Labor Review*. For the years 1932–35 I use a Bureau of Labor Statistics report (Parker 1936). For the years 1936–39 and 1940, I use two Social Security Administration bulletins (Bateman and Newman 1941; Perkins 1951).

To evaluate the above hypotheses, I use a difference-in-difference model to compare voter turnout and Democratic Party vote share in counties in states with less generous benefits to those in states with more generous benefits. Following Fetter and Lockwood (2018), I limit the data to include only counties along a state border. This reduces the variance in the sample, allowing for greater precision in the estimates.[4] My expectations pertain not just to within-state, over-time variation but to variation in the treatment effect based on differences at the county level in the number of people who

3. Nardulli draws on data from the Inter-university Consortium for Political and Social Research historic archive but accounts for missing data and data discrepancies, such as places where turnout is estimated at above 100%.

4. The identifying assumption of the research design does not depend on limiting the sample to counties on state borders. Fixed effects absorb all time-invariant characteristics of the counties. This choice only serves to decrease the variance of the sample and increase the precision of the estimates, as counties on either side of a state border are more similar to one another. This is a useful procedure in this case because the state-level treatment does not provide much variation. Even in interaction with a county-level variable, it renders relatively imprecise results, such that I cannot precisely estimate null effects when they arise.

will benefit from the policy. To evaluate these possibilities, I include interaction effects in the difference-in-difference model, as follows:

$$\text{Outcome}_{it} = \beta_1 \text{OAA}_{st} + \beta_2 \text{OAA}_{st} : \text{HighElderly}_i + \alpha_i + \gamma_t + \rho_b + \mathbf{X}_{it} + \varepsilon_{it},$$

where Outcome_{it} is either the turnout or the vote share of the Democratic candidate in county i in year t; α_i represents county fixed effects, which control for observed and unobserved time-invariant differences across counties; γ_t represents year fixed effects, which account for any shocks particular to certain election years; ρ_b represents state-border fixed effects (these are necessary because the data repeat observations for counties that fall on more than one state border); and ε_{it} represents the idiosyncratic error term, which is clustered at the state level.

The treatment is OAA_{st}, which is either the OAA payment per recipient in state s in year t or the percentage of elderly residents receiving any OAA benefit in state s in year t. Because the treatment measures are continuous and many states had OAA policies in place before federal intervention in 1936, there is no clear pre- and posttreatment period in this analysis. Thus, I am differencing over the level of OAA generosity or coverage at the time of the preceding election, regardless of whether that is zero. The effect of a state's expansion of OAA benefits over turnout or the democratic vote share in each election in young counties is represented by β_1.

This model allows the treatment effect to vary according to whether the county had an above-average portion of the population that was 65 years and older, HighElderly_i, as they were the beneficiary population. Following Clinton and Sances (2018), counties were coded 1 (elderly) if their average elderly population was above the median for census years 1930 and 1940 and 0 (young) otherwise.[5] Because HighElderly_i is a time-invariant characteristic of each county, it is subsumed in the county fixed effect and cannot be estimated separately. The triple-differences (DDD) estimate of interest is represented by β_2 (Angrist and Pischke 2009, 241–43). This means that I am comparing the outcome variables as OAA is extended over time, not just between counties that have more and less generous OAA programs but also between counties with low and high levels of eligible residents.

I also include several time-varying controls at the county level from US Census data, represented by vector $\mathbf{X}_{it}$. These covariates include the unemployment rate, the percentage of the population that is Black, population density, and the percentage living on farms.[6] Table A1 (table A1–A6 are available online) shows the descriptive statistics for the treatments and outcome variables.

As Clinton and Sances (2018) note, the interaction term relaxes the strict parallel trends assumption required for the difference-in-differences estimate of β_2 to be unbiased. To interpret β_2 as an unbiased estimate of a causal effect, I assume that the difference in the over-time change in the outcome variable between high- and low-expansion states would have been the same for high- and low-elderly counties, absent the treatment. Even if changes in demographic composition between high- and low-expansion states were independently affecting turnout or partisan voting during the 1930s, those changes will be differenced out if they evenly affect high- and low-elderly counties.

Although the modified parallel trends assumption of the DDD does not require parallel trends between high- and low-expansion states, there is a historical reason to believe that this was the case. Of the 12 states that passed OAA laws in 1930 and before, six were passed under Republican governors, and six were passed under Democratic governors. Even as the New Deal began to clearly align the Democratic Party with the issue of welfare provision, there is little partisan trend in the enactment of OAA provisions.[7] In general, it is unlikely that the governors signed OAA bills into law with the partisanship or voter turnout of their state in mind. OAA laws were designed primarily based on state budgetary constraints and concerns about the expansion of public assistance. In other words, they were not endogenous to the political outcomes of interest here. In the analyses that follow, I provide evidence that the parallel trends assumption holds in this case.

In addition to these data, which allow me to isolate the causal effect of OAA expansion, I provide visual and descriptive analyses of Gallup poll data from 1940 on voter

5. I first calculated each county's average elderly population based on the 1930 and 1940 censuses. Then I took the median of that figure and coded counties as "elderly" if they were above the median and "young" if they were below the median.

6. Given that the census is collected only decennially, I have used linear interpolation to estimate the demographic measures for election years between censuses. Where linear interpolation was necessary, I followed the US Census Bureau's (2012) recommended method of estimation, which uses a weighted average of the two decennial years to estimate the population in the intervening period.

7. This becomes clear when we examine the pre–New Deal partisanship of the states that were most generous and provided the greatest OAA coverage. As fig. 1 shows, California, Arizona, Massachusetts, Colorado, and Pennsylvania provided the largest annual payments per OAA recipient in 1936. Among this group, Pennsylvania was one of five states that voted for Hoover even in 1932, and Massachusetts was one of two states outside the South that voted for Al Smith in 1928. The other three voted for the wining president in each case. The top five states in terms of coverage in 1936 were Oklahoma, Colorado, Texas, Idaho, and Nebraska. Before the New Deal, Oklahoma and Texas were historically more Democratic states, while the other three tended Republican.

turnout and vote choice. Beginning in 1938, Gallup polls regularly included OAA recipiency status as an indicator of respondents' class. I compare 1936 turnout and vote choice recall to 1940 prospective vote choice and turnout recall among three groups of respondents: those 65 years and older (elderly) and not receiving OAA, those elderly receiving OAA, and those between 25 and 65 who were old enough to vote in the 1936 election but not eligible for OAA. For all survey data, I use the most appropriate survey weights available from those created by Berinsky and Schickler (2011) for estimating the opinion of the effective national electorate.[8] These data provide valuable insight into the voting behavior of individual OAA recipients during the period of OAA expansion but are subject to the same selection biases as other policy feedback studies using correlational analyses.

FINDINGS

Turnout

Figure 2 summarizes the responses to three Gallup poll surveys conducted in the months following the 1940 presidential election. In each survey, respondents were asked if they remembered for certain whether they voted in the 1936 presidential election and in the most recent election. The graphs compare the recalled voter turnout in both years among respondents between 25 and 65 years old, those 65 and older who are not receiving OAA, and those elderly who are receiving OAA.[9] Across all three surveys in 1936, OAA recipients reported lower turnout than did elderly nonrecipients and younger voters, suggesting that elderly people who were eligible for OAA had fewer resources and were less likely to vote than elderly nonbeneficiaries. But by 1940, every survey indicates that OAA recipients closed the turnout gap with elderly nonrecipients, indicating that OAA expansion between 1936 and 1940 may have reduced resource deficits and increased turnout among indigent elderly citizens.

Tables 1 and 2 formally evaluate this possibility. They show the effects of OAA expansion on voter turnout in presidential elections from 1932 to 1940. In table 1, the treatment is the state's average annual OAA payment per recipient; in table 2, the treatment is the coverage rate. All models include county and year fixed effects as well as state-border fixed effects.

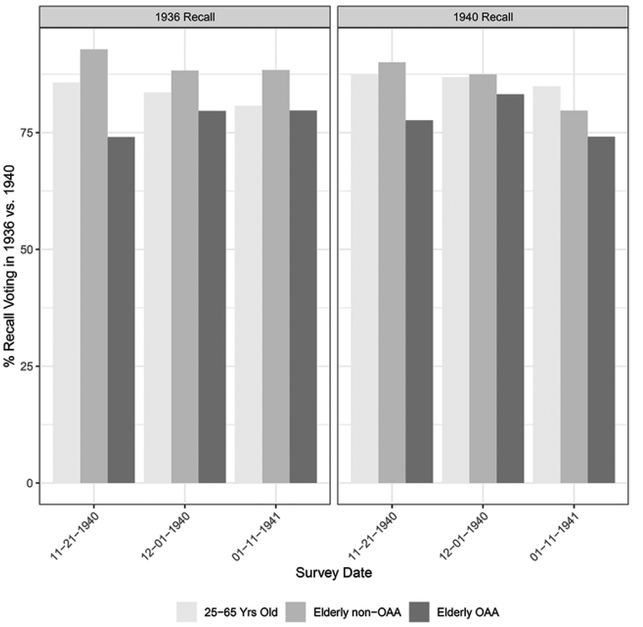

Figure 2. Gallup poll data of voter turnout, 1936 versus 1940. On November 21 and December 1, 1940, respondents were asked about their 1940 vote choice: "Did you vote in the Presidential election on November 5? If 'yes': Did you vote for Willkie, Roosevelt, or Thomas?" On November 11, 1941, respondents were asked: "Do you remember what time of the day you voted in the Presidential election November 5—morning or afternoon?" On all surveys, respondents were asked about their 1936 vote choice: "Do you remember FOR CERTAIN whether or not you voted/were able to vote in the 1936 Presidential election? If 'yes': Did you vote for Lemke, Roosevelt, Thomas, or Landon?" Color version available as an online enhancement.

In both tables, model 1 includes only the treatment and interaction term as well as the fixed effects. For each treatment measure, the estimate for the interaction term is in the expected direction—positive—but not statistically significant. Model 2 shows a robustness check that includes state-specific linear time trends, relaxing the parallel trends assumption. In both tables, the estimates for the treatment and interaction terms change slightly from model 2 but remain just at the 95% confidence interval of those estimates. This provides evidence that the parallel trends assumption holds true, as relaxing it does little to substantively affect the estimates.

Model 3 introduces time-varying covariates, which increase the precision of the estimates in both tables. The interaction term in table 1 is now statistically significant and positive, confirming expectations: increasing the generosity of OAA payments increased voter turnout in elderly counties.[10] Finally, model 4 in tables 1 and 2 includes a South-by-year

8. The only exception is the January 1, 1941, survey, for which the weights for estimating the opinion of the national electorate were not available. Following Berinsky and Schickler (2011), I use the next most appropriate weights, which were designed for estimating the national opinion when it is correlated with race.

9. Although voter turnout data in surveys are substantially overreported, this would only affect the between-group comparisons if voters in different groups overreport to different degrees. It would only affect the within-group comparisons if voters within each group overreport their two most recent votes to different degrees.

10. Tables A2–A6 show the full results for all analyses presented here, including the coefficients and confidence intervals for the covariates.

Table 1. Effect of OAA Expansion on Voter Turnout—Generosity

	Turnout			
	(1)	(2)	(3)	(4)
OAA per recipient	.003	−.003	−.003	−.003
	(−.015, .021)	(−.031, .025)	(−.021, .015)	(−.022, .015)
OAA per recipient: high elderly	.009	−.001	.011*	.011**
	(−.0001, .018)	(−.007, .006)	(.003, .020)	(.003, .018)
R^2	.975	.981	.977	.977
Country, border, and year FEs	Yes	Yes	Yes	Yes
State time trends	No	Yes	No	No
Covariates	No	No	Yes	Yes
South-by-year FE	No	No	No	Yes

Note. The 95% confidence intervals shown in parentheses are calculated using standard errors that are clustered at the state level. $N = 3{,}936$. OAA = Old-Age Assistance; FE = fixed effect.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

fixed effect to control for electoral shocks specific to region and year.[11] Because of the large differences in politics between the former Confederacy and the rest of the country, this is my preferred model. Model 4 again increases the precision of the estimates, as shown in the shrinking confidence intervals, but does not substantively change the results.

From tables 1 and 2 we can safely conclude that OAA expansion did have positive participatory effects in counties with an above-average share of beneficiaries. However, these effects operated primarily through increases in the generosity of payments, rather than through increases in coverage for the elderly population. As noted above, the generosity measure captures variation in states' impact on beneficiaries' resources, while the coverage measure captures variation in the portion of elderly residents who were learning about government through OAA recipiency. While neither measure isolates resource or interpretive effects of OAA expansion, the findings from tables 1 and 2 taken together point to resource effects as the crucial mechanism in mobilizing voters: using resources to provide citizens with more money increased turnout more than using those resources to simply offer some benefit to more people. But, while increasing coverage did not increase turnout, it also did not cause it to decline like other means-tested programs might have. This is despite contemporaries' understandings of the program as highly stigmatizing (Cates 1983; Mettler 1998).

Moreover, the magnitude of the effects shown in table 1 were not insubstantial: increasing OAA payments in a state from $0 per person per annum to the maximum during this period, $454.40, would have increased turnout in elderly counties by 5 percentage points. The average increase in payments per recipient between presidential elections during this time was, however, much less than $400—it was $84.42 per person per annum. Given the estimates from model 4 in table 1, the effect of an average increase in OAA payments per recipient was to increase turnout in elderly counties by 0.93 percentage points (95% confidence interval [CI] 0.27 to 1.59). Thus, OAA expansion via this mechanism did have small but meaningful mobilizing effects within elderly counties.

In "young" counties, there is no evidence that OAA expansion had any kind of participatory effect. Given the estimates from model 4 in table 1, we can reject any negative effect from an average increase in OAA payments larger than 1.74 percentage points and any positive effect larger than 1.24 percentage points with 95% confidence. The estimates from model 4 in table 2 provide similar results for increasing coverage by an average amount.

The participatory effects of OAA were thus limited to elderly counties that received larger increases in payments per person, indicating that resource effects were essential in increasing turnout in those counties. Although the aggregate results cannot conclusively determine that it was elderly OAA recipients whose turnout increased during this time, the Gallup poll data suggest that changes in their behavior

11. The South here refers to the 11 former Confederate states.

Table 2. Effect of OAA Expansion on Voter Turnout—Coverage

	Turnout			
	(1)	(2)	(3)	(4)
Coverage rate	−.062	−.104	−.083	−.086
	(−.138, .014)	(−.289, .080)	(−.169, .003)	(−.173, .001)
Coverage rate: high elderly	.056	−.022	.052	.036
	(−.21, .133)	(−.065, .021)	(−.018, .121)	(−.015, .086)
R^2	.975	.982	.977	.977
Country, border, and year FEs	Yes	Yes	Yes	Yes
State time trends	No	Yes	No	No
Covariates	No	No	Yes	Yes
South-by-year FE	No	No	No	Yes

Note. The 95% confidence intervals shown in parentheses are calculated using standard errors that are clustered at the state level. $N = 3{,}936$. OAA = Old-Age Assistance; FE = fixed effect.

were at least part of the aggregate change. It is also possible, however, that some of the increased turnout was due to a spillover resource effect on recipients' caretakers and adult children.

Partisan vote share

OAA expansion through increasingly generous payments thus mobilized elderly counties by providing elderly residents with more resources. How did this affect partisan vote share? As described above, the most likely pathway by which OAA might have affected partisan vote share would be by changing the composition of the voting population in a way that benefited one party more than the other. This is conditional on OAA mobilizing recipients, which I have now shown to be the case. I also suggested that it is less likely that individual recipients would have changed their vote choice after receiving OAA.

To assess this possibility, figure 3 examines individual-level voting behavior as reported in a series of Gallup polls conducted between August and November 1940. In each survey, respondents were asked whether they could recall their vote choice in the 1936 presidential election and for whom they planned to vote in the 1940 election. Figure 3 depicts the percentage of people who reported voting for FDR in 1936 (*left*) and planning to vote for FDR in 1940 (*right*) among respondents between 25 and 65, elderly not receiving OAA, and elderly receiving OAA. OAA recipients were overwhelmingly the most likely to vote for FDR of the three groups in both elections, followed by those less than 65. Elderly non-OAA recipients were the least likely to vote for FDR in almost every survey for both elections. These patterns, as those with turnout, are likely attributable to differences among the three groups that are correlated with vote choice and do not tell us much about within-group changes as a result of OAA expansion.

There is also little evidence that OAA recipients moved more toward FDR between 1936 and 1940 than did other groups. While voters 25–65 expressed less inclination to vote for FDR in 1940 than in 1936 across all surveys, elderly nonrecipients indicated some erosion of support in four of the seven surveys, and OAA recipients indicated some erosion of support in three of the surveys. Although OAA recipients were the only group to indicate increasing support for FDR in some of the surveys, there is no clear trend, as in the case of turnout, that the partisan gap between elderly OAA recipients and nonrecipients changed between 1936 and 1940.

What figure 3 clearly shows is that elderly non-OAA recipients were the least Democratic of the three groups. This is also consistent with an analysis of cohort-based political differences during the New Deal era, which suggests that older voters who grew up during a time of Republican dominance tended to stay that way (Andersen 1979). Thus, if any of those Republican-leaning non-OAA recipients received OAA benefits under expansion, it could have increased turnout among a relatively Republican group.

Tables 3 and 4 evaluate this possibility, showing the effect of OAA expansion over Democratic presidential vote share. They repeat the modeling strategies used in tables 1 and 2 above, with table 3 using payment generosity as a measure of expansion and table 4 using coverage as a measure of expansion. Model 1 includes only the main and interaction effects of the treatment as well as fixed effects. In this model we can see that neither treatment measure offers much evidence of a partisan effect in young counties; however, in elderly counties, OAA expansion decreases Democratic presidential vote share,

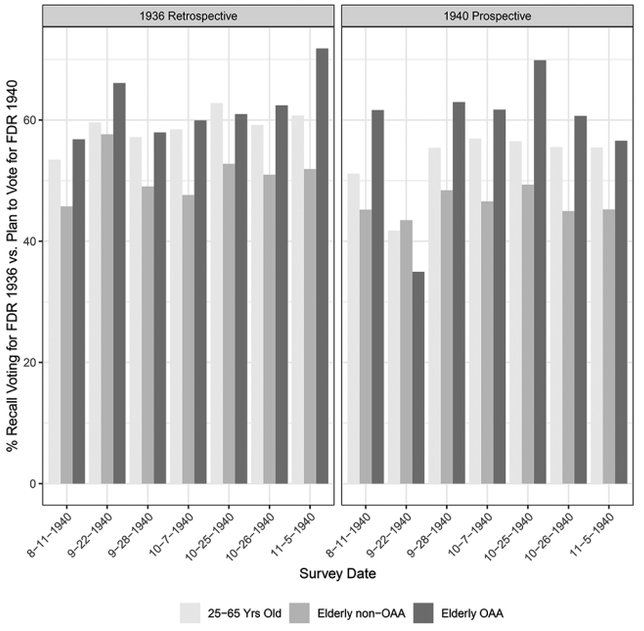

Figure 3. Gallup poll data of retrospective 1936 versus prospective 1940 vote. Respondents were asked about their 1936 retrospective vote choice: "Do you remember for certain whether or not you voted in the 1936 Presidential election?" They were then asked, on August 11 and September 22 and 28, 1940: "If 'yes', did you vote for Lemke, Roosevelt, Thomas, or Landon?" On October 7, 25, and 26 and November 5, 1940: "If yes, did you vote for Landon, Thomas, Roosevelt, or Lemke?" Respondents were asked about their 1940 prospective vote choice, on August 11 and September 22, 1940: "As it looks now, do you intend to vote for Roosevelt or for Willkie this fall?" On September 28, October 25 and 26, and November 5, 1940: "If the Presidential election were held today, would you vote for Willkie or Roosevelt?" On October 7, 1940: "If the Presidential election were held today, would you vote for Roosevelt, Willkie, or would you just not vote?" Color version available as an online enhancement.

regardless of the treatment measure. Model 2 includes state-linear time trends as a robustness check for the parallel trends assumption. The estimates for the interaction effect from model 1 are little changed when the parallel trends assumption is relaxed through the inclusion of these trends. In both tables 3 and 4, the main effect of OAA expansion becomes positive and statistically significant.

These results are robust to the inclusion of time-varying covariates in model 3, which do not change the substantive interpretation of the interaction term. In model 4 I introduce the South-by-year fixed effect. The interaction term remains negative and statistically significant in both tables 3 and 4. This evidence indicates that OAA expansion did affect partisan electoral vote share, as expanding the coverage rate and increasing the generosity of payments cost FDR votes in older counties. The likeliest culprit for OAA's negative effect on Democratic voting is prior partisanship. This raises the question: Was the negative effect of OAA expansion concentrated in Republican counties?

To assess the likelihood of this explanation, I divide counties into three groups based on their democratic presidential vote share in the 1928 election. Those in the top third of democratic voting registered 64% for Al Smith in 1928, on average, and are labeled "Democratic"; those in the bottom third voted 28% Democratic in 1928 and are labeled "Republican"; and those in the middle third voted 41% Democratic in 1928 and are labeled "Purple." I then repeated the analyses from model 4 in tables 3 and 4 within each separate set of counties. If partisanship explains the negative effect of OAA expansion, I would expect to see the effect concentrated in Republican counties and perhaps Purple counties, which were actually relatively Republican in 1928. Table 5 shows the results of this analysis. All models include the main and interaction effects for the treatment, covariates, and county, border, year, and South-by-year fixed effects. Models 1–3 estimate the effect of increasing OAA generosity over democratic presidential vote share in Democratic, Purple, and Republican counties, respectively. Models 4–6 replicate this analysis with coverage rate as the treatment measure.

Consistent with expectations, there is no evidence that expanding OAA by either mechanism affected partisan vote share in Democratic counties. Both, however, have negative effects on Democratic party vote share in Purple and Republican counties. Given the analyses presented in table 5, an average increase in the coverage rate of 11.4% decreased FDR's vote share in elderly Republican counties by 1.88 percentage points (95% CI −2.97 to −0.79). An average increase in payment generosity of $84.42 decreased his vote share in those counties by 1.10 percentage points (95% CI −2.09 to −0.10). These findings are partially in line with my expectations. OAA expansion by both mechanisms decreased Democratic Party vote share in Republican-leaning counties but not in Democratic counties. However, as only generosity increased turnout, OAA's effect over vote share was due to more than just mobilization of beneficiaries. I explore possible explanations for this in the next section.

DISCUSSION

This article evaluates the American electorate's response to one of the earliest systematic extensions of social welfare in US history—Old-Age Assistance. OAA offers the opportunity to examine the electoral consequences of expanding a means-tested program by different mechanisms. As such, this article offers causal evidence as to the size of OAA's electoral effects and suggestive evidence as to how the program achieved those effects.

The empirical analyses support my theoretical expectations: a generous means-tested program increased voter

Table 3. Effect of OAA Expansion on Democratic Presidential Vote Share—Generosity

	Turnout			
	(1)	(2)	(3)	(4)
OAA per recipient	−.003	.023*	.001	−.002
	(−.024, .018)	(.005, .040)	(−.018, .020)	(−.019, .015)
OAA per recipient: high elderly	−.019**	−.020***	−.023***	−.013*
	(−.033, −.006)	(−.030, −.011)	(−.035, −.010)	(−.025, −.002)
R^2	.938	.962	.941	.945
Country, border, and year FEs	Yes	Yes	Yes	Yes
State time trends	No	Yes	No	No
Covariates	No	No	Yes	Yes
South-by-year FE	No	No	No	Yes

Note. The 95% confidence intervals shown in parentheses are calculated using standard errors that are clustered at the state level. $N = 3{,}939$. OAA = Old-Age Assistance; FE = fixed effect.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

turnout. Moreover, even using the coverage measure, OAA did not generate negative effects on participation. Taken together, the evidence suggests that OAA created channels for positive resource effects without engendering so much stigma that it negated those financial gains. This result is somewhat surprising given policy makers' intent to make OAA as demeaning as possible for its beneficiaries (Cates 1983). Even with these conditions, states that poured sufficient resources into elderly communities were able to increase their political participation.

In expanding OAA, states also changed the composition of the electorate. As a result of increasing payment generosity and mobilizing elderly counties, FDR lost votes in elderly Republican-leaning counties in highly generous states. Expanding coverage

Table 4. Effect of OAA Expansion on Democratic Presidential Vote Share—Coverage

	Turnout			
	(1)	(2)	(3)	(4)
Coverage rate	−.077	.170**	−.036	−.075
	(−.237, .083)	(.057, .283)	(−.195, .124)	(−.209, .060)
Coverage rate: high elderly	−.191***	−.113***	−.204***	−.119*
	(−.294, −.089)	(−.173, −.053)	(−.306, −.102)	(−.216, −.023)
R^2	.939	.962	.942	.946
Country, border, and year FEs	Yes	Yes	Yes	Yes
State time trends	No	Yes	No	No
Covariates	No	No	Yes	Yes
South-by-year FE	No	No	No	Yes

Note. The 95% confidence intervals shown in parentheses are calculated using standard errors that are clustered at the state level. $N = 3{,}939$. FE = fixed effect.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

Table 5. Effect of OAA Expansion on Democratic Presidential Vote Share by Partisanship

	Payment Generosity			Coverage		
	Democratic (1)	Purple (2)	Republican (3)	Democratic (4)	Purple (5)	Republican (6)
OAA per recipient	−.022	.008	.002			
	(−.053, .009)	(−.012, .028)	(−.020, .024)			
OAA per recipient: high elderly	−.005	−.023**	−.013*			
	(−.035, .024)	(−.037, −.008)	(−.024, −.002)			
Coverage rate				−.071	−.037	−.061
				(−.192, .050)	(−.205, .130)	(−.217, .095)
Coverage rate: high elderly				.019	−.160*	−.165**
				(−.173, .212)	(−.281, −.040)	(−.260, −.069)
N	1,200	1,371	1,368	1,200	1,371	1,368
R^2	.937	.875	.879	.935	.875	.882
Country, border, and year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
South-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes

Note. The 95% confidence intervals shown in parentheses are calculated using standard errors that are clustered at the state level. OAA = Old-Age Assistance.

* $p < .05$.

** $p < .01$.

*** $p < .001$.

also cost FDR votes in elderly Republican-leaning counties in high-coverage states. As these were places where turnout did not increase, this cannot be the explanation for FDR's losses. Instead, it may be that Republican counties received more targeted anti-FDR and anti–New Deal messaging than other places and that that messaging had a greater effect in places where OAA programs were the most prevalent—high-elderly counties in high-coverage states.

These findings add to our understanding of how means-tested programs shape the mass public. First, they highlight the crucial role of generosity in shaping the possible feedback effects of means-tested programs. Alongside new evidence of Medicaid's mobilizing effects (Baicker and Finkelstein 2019; Clinton and Sances 2018), this article suggests that means-tested programs have real potential to induce political participation by providing financial resources, even if they stigmatize their beneficiaries. In so doing, these findings add a cautionary note to literature arguing that states have significant leeway within the federalist system to shape policies' feedback effects: while this is certainly true, particularly within the South, the best way to increase participation through social policies might be by simply increasing federal funding for them (Mettler 1998; Michener 2018).

Finally, this article suggests the importance of examining partisan vote share as a possible effect of social policy expansion. Some theoretical and empirical efforts in this vein have already pointed to places where public policy may have determined crucial elections (Manza and Uggen 2004; Mettler 1998), ultimately undermining the future efficacy of those policies. By focusing on this outcome scholars may come nearer to closing the policy feedback loop.

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