

The Means Matter, Too: Policy and Economic Retrospective Voting in American National Elections*

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Abstract

The theory of retrospective voting predicts that voters are responsive to policy *outcomes*, but not the policies, programs, and provisions that may produce those outcomes. I reevaluate this claim. Using a difference-in-differences design, I show that counties that received more New Deal economic relief money became more Democratic than counties that received less money *regardless of changes in local macroeconomic conditions*. Additionally, I find that voters were particularly responsive to direct financial assistance programs relative to economic stimulus spending, as well as suggestive evidence that the media facilitated these policy-based voting responses. Finally, while the effects of economic change are transient, the effects of New Deal spending on Democratic support in presidential elections have persisted through to today. My results suggest that voters reward and punish incumbents not just for policy ends, but for policy, too. The results have implications for democratic representation, and for politicians in times of national crisis.

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[Voters] realize that it is not necessary to know how to *make* a television set in order to buy one intelligently.

Schattschneider (1960, 137)

Elections offer citizens a mechanism for democratic accountability. In a standard accountability framework, voters reward or punish elected leaders based on their performance while in office (e.g., Ferejohn 1986; Key 1966). Along these lines, there is ample empirical evidence that voters punish politicians for poor economic conditions (Kramer 1971; Nadeau and Lewis-Beck 2001; Healy and Lenz 2017), local war casualties (Grose and Oppenheimer 2007; Karol and Miguel 2007), high crime rates (Arnold and Carnes 2012), and poor school performance (Berry and Howell 2007; Kogan, Lavertu, and Peskowitz 2016). These findings support the idea that voters tend to think retrospectively, using observable indicators of performance to evaluate whether an incumbent should stay in office or not.¹

Undergirding the theory of retrospective voting is the expectation that voters are responsive to policy, but *not* the policies, programs, and provisions that may produce those outcomes. Policies are defined in this paper as any action that elected leaders take to influence broader conditions (e.g., the economy), while policy outcomes are the conditions themselves. Fiorina (1978), for instance, asserts that “the citizen looks at *results* rather than the policies and events which produce them” (430). That is, what is believed to drive retrospective voting behavior is not the particulars of the new crime bill, economic stimulus package or job program (policies), but whether those policies reduced crime or improved macroeconomic conditions (policy outcomes). In short, voters are said to care about policy *ends*. In empirical terms, then, models of retrospective voting predict no independent effect of policy on voting once policy outcomes are accounted for.

¹Another model of voting behavior sees voters as selecting candidates whose policy preferences best match their own. There is empirical evidence in favor of this model, too, as issue positions appear to affect vote choice to a degree even approaching that of party identification (e.g., Ansolabehere, Rodden, and Snyder 2008). Note that these models tend to be *prospective* models of voting, as voters choose candidates based on future expected benefits or policies that they wish to see put in place. In this paper, I take up the task of testing whether and how *past* policy actions factor into retrospective evaluations.

Why have scholars concluded that voters appear to prioritize policy outcomes over the policies themselves? In practice, it may be because performance metrics represent an easily attained heuristic for voters, and perhaps even a shortcut for drawing inferences about often arcane and complex policies (Downs 1957). In other words, retrospective voting as theorized may reflect a true understanding of voter behavior. Yet, a read of the literature finds few direct tests of how policies affect, or do not affect, voting behavior or election outcomes net of the effect of broader conditions or outcomes.² In fact, the few published papers to date suggest run in contrast to the standard retrospective model, as voters appear to punish governors for state tax increases *regardless of macroeconomic conditions* (Kone and Winters 1993; Niemi, Stanley, and Vogel 1995). Other work finds that policy agreement with members of Congress shapes voter decisions much more so than their subjective evaluations of the economy and war (Jones 2011).

Despite empirical challenges of estimating policy effects in national elections, we need more direct evidence before drawing firm conclusions about the mechanisms behind retrospective voting. This paper attempts to do that. I look for policy effects on election outcomes after the Great Depression, the largest and most significant economic downturn in history. Specifically, I study the effects of the three largest New Deal-era programs designed to promote economic relief and recovery: the Works Progress Administration (WPA), the Home Owners' Loan Corporation (HOLC), and the Public Works Administration (PWA). Combined, these three programs spent \$12 billion between 1933 and 1939, or about 13% of nominal GDP in 1939.³ These programs put more than 9 million people to work, refinanced over 1 million mortgages on the brink of foreclosure , built or repaired more than 650,000

²There is, however, a large body of work on the effects of policy on political participation. Past work finds positive turnout effects of Social Security (Campbell 2002), the G.I. Bill (Mettler 2002), welfare (Soss 1999), FEMA disaster aid (Chen 2011), and Medicaid (Clinton and Sances 2018; Michener 2018). The argument here is that government benefits increases voter resources and in turn the capacity to participate in politics. Recipients are thought to participate primarily to protect their stake in the program and ensure the continuation of benefits.

³As a point of comparison, President Obama's 2009 economic stimulus package represented about 5% of nominal GDP in 2009. President Trump's first COVID-19 stimulus package was just over 10% of nominal GDP in 2019.

miles of road, and were responsible for about 70% of new schools and one third of new hospitals built between 1933 and 1939.

I link county-level data on WPA, HOLC, and PWA expenditures between 1933 to 1939 to county-level presidential election returns both before and after the Depression. I also merge these data with two county-level measures of economic conditions — retail sales and manufacturing output — collected in 1929, 1933, 1935, 1937, and 1939. Combined, these data allow me to test the unique effects of both county-level variation in New Deal allocations as well as changes in macroeconomic conditions at the county-level. Put in another terms, these data allow me to test whether receiving greater benefits from these three New Deal programs had a positive effect on support for the Democratic Party *independent* of the effect of economic change on voter support. Using a generalized difference-in-differences design leveraging within-county changes in voting patterns before and after the New Deal, I report four main findings:

- New Deal spending increased county support for the Democratic Party *even after adjusting for the positive effects of post-Depression economic growth* on support for the Democratic Party.
- WPA and HOLC spending appear to drive these positive effects. Unlike the PWA, both provided direct government assistance in the form of jobs and refinanced home mortgages. These findings appear consistent with other work on the electoral returns to direct benefits (Healy and Malhotra 2009), and with theoretical work on importance of visibility and traceability in policy feedbacks (Arnold 1990; Mettler 2011).
- The effects were larger in counties greater radio access, as well as where more media coverage was given to the three New Deal programs. This offers suggestive evidence that the media can facilitate policy-driven retrospective voting through both presence and content.
- The positive effects of New Deal spending on Democratic voting persist in every election

from 1936 up to and including the 2016 election. In contrast, the positive effects of 1930s economic growth on Democratic support disappears after 1948.

The results of my study suggest that policy *means* can matter in elections just as much as the ends of policy. Voters do not just assess whether times are good or bad, but also evaluate incumbents on the basis of the particular actions taken to produce those good times. These findings have broad implications for representation. In particular, if voters are responsive to policies, it means that politicians may be constrained by voter preferences in the policies that they can pursue. As a consequence, leaders may prioritize delegate forms of representation that emphasize implementing the preferences of the public (Canes-Wrone, Herron, and Shotts 2001; Canes-Wrone and Shotts 2007; Fox and Shotts 2009). Additionally, these results speak to how politicians communicate with their constituents. We know that politicians often credit claim for their accomplishments (Grimmer, Messing, and Westwood 2012; Mayhew 1974), and my results offer additional evidence of the potential electoral benefits to credit-claiming. Finally, these results have contemporary practical implications. As the world faces an economic crisis amid the COVID-19 pandemic, leaders have faced the challenge of determining how best to aid those in economic distress. While the Great Depression and the New Deal were undoubtedly unique and historic moments in history, my study provides some evidence that whether and how leaders respond in times of crisis matters to voters. And, it may matter for many decades to come.

1 Policy and Policy Outcomes in Accountability

Decades of empirical work has confirmed voters' use of performance heuristics in the ballot box, particularly as it relates to the economy. Kramer (1971) estimated that a 1% increase (decrease) in real income corresponded to a 0.5% increase (decrease) in support for the incumbent party nationally. Similar if not larger effects are obtained when using subjective voter evaluations of the national economy (e.g., Kinder and Kiewiet 1979, 1981; Nadeau

and Lewis-Beck 2001). More recent work has considered changes in the local economy (Ansolabehere, Meredith, and Snowberg 2014; Healy and Lenz 2017; Park and Reeves 2020) as well as economic voting in non-presidential elections (de Benedictis-Kessner and Warshaw 2020). Again, the economy appears to be a consistent predictor of election outcomes.

To be sure, there are normative questions about exactly how voters use economic conditions in their vote choice calculus. The most significant of these concerns is whether voters' prioritization of short-term, potentially-random, election-year economic change is an effective way to hold incumbents accountable (Achen and Bartels 2016; Healy and Lenz 2014; Huber, Hill, and Lenz 2012). Voters may also have different perceptions of the national economy depending on their own partisan predispositions (Bartels 2002; Gerber and Huber 2010). Still, the link between the economy and voting remains strong, even as voters' biases and limitations creep into their usage of economic performance metrics.

Voters also punish elected school board members for poor school performance (Berry and Howell 2007; Kogan, Lavertu, and Peskowitz 2016), city officials when crime rates are high (Arnold and Carnes 2012), and the president and members of Congress as local war casualties increase (Grose and Oppenheimer 2007; Karol and Miguel 2007). Ongoing research extends this framework to voters' responsiveness to COVID-19, and finds early evidence that county- and state-level COVID-19 deaths predict approval and support for Trump and other Republican officeholders (Warshaw, Vavreck, and Baxter-King 2020). Notably, the magnitude of these effects — about a 0.15% decrease in support for Trump for a doubling in COVID-19 fatalities — are on par with effects of the county economy on voting found in recent work (de Benedictis-Kessner and Warshaw 2020).

What's clear from this line of research is that voters are attuned to policy outcomes, and tend to vote using broad, easily observed performance metrics that indicate whether times are good or bad. Why does theory suggest that policy itself has no influence on vote choice? The most common explanation is that voters face significant information costs, and that using observable performance metrics offer a simple and readily available substitute for

policy awareness and understanding (Downs 1957). Woon (2012) finds empirical support for this argument in a laboratory setting, showing that retrospective decision rules are simple heuristics that help voters overcome cognitive limitations in evaluating politicians and their performance.

Normatively, Kiewiet and Rivers (1984) argue that evaluating politicians using outcomes makes sense for voters. Can we reasonably expect the average voter to have the kind of policy knowledge needed to assess the potential benefits (and pitfalls) of particular economic policies? As Kiewiet and Rivers (1984) write, “both parties claim to have the secret to macroeconomic success and if economists cannot agree on who is right, why should voters try?” (386). In short, using policy outcomes as a voting decision rule appears to be one way for voters to overcome a lack of information, and evaluate the skill and competence of incumbent politicians while still potentially tapping in to *some* dimension of policy actions. To be clear, there is some work suggesting that economic voting may be policy-oriented for some voters. The argument is that voters concerned about unemployment should support the Democratic candidate, while voters worried about inflation will back the Republican (Kiewiet 1981; Wright 2012). Voters do so because the Democratic officeholders tend to prioritize low unemployment as an indicator of economic success, while Republicans tend to focus on keeping inflation low. Such an argument, however, is notably distinct from whether particular policies or programs enacted influence the vote. That is, these arguments do not claim that voters are reacting to particular policy actions taken by those seeking reelection. Rather, voters are responding to broad perceptions of party priorities developed over many decades (Egan 2013).

To date, there remains relatively little *direct* evidence that policy has no effect on voting patterns net of the effect of policy outcomes on voting. In their study of Iraq War deaths and support for Bush, Karol and Miguel (2007) offer some indirect evidence in favor of traditional retrospective voting models. They find no statistical or substantive effect of local National Guard or Reservist call-ups on voting, but significant effects of local war fatalities.

As the theory would predict, voters only punished Bush for poor outcomes, and not the policy actions or decisions (more call-ups) that may have contributed to those outcomes.

Nevertheless, there is some work suggestive of real policy effects independent of outcome effects. Kone and Winters (1993) and Niemi, Stanley, and Vogel (1995) both find evidence that, even after adjusting for macroeconomic conditions, voters punished incumbent governors for raising state taxes. Jones (2011) compares the effects of perceived voter-incumbent policy congruence to the effects of broader evaluations of peace and prosperity on vote choice in Senate races, and concludes that voters appear to hold incumbents accountable for positions much more so than their perceptions of outcomes. Finally, Margalit (2011) finds that while voters punish incumbents for trade-related job losses, providing job loss benefits in response to those losses — such as job retraining opportunities — attenuates the negative effect of job loss on incumbent support. These results in particular are suggestive that policy actions are part of performance-based, retrospective evaluations. Specifically, whether policymakers take action to alleviate economic stress appears to matter to voters, too.

The truth, though, is that we do not really have enough empirical evidence in either direction. In part, this is because estimating the effects of policy is made challenging by reality. Take Healy and Lenz (2017)'s study of county-level wage and employment growth and voting in presidential elections. There are any number of policies that may have influenced county-level economic conditions. At the federal level, interest rates, trade policy, tax policy, and investments in education and public infrastructure each come to mind. The problem is that while we can measure variation in economic conditions at the county-level and over-time, we cannot measure variation in many of the federal policies that may have affected county-level outcomes at the county-level because many federal policies are uniform across space. That is, every county is subject to the same federal tax and trade policies. As a result, we are limited in our ability to estimate the effects of policy in presidential elections, which have been the primary focus of decades of research on retrospective voting.

Analysts are left with two options. First, researchers can take to the state-level. Here,

we can often observe both geographic variation in policy cross-sectionally — that is, some states have a state income tax while others do not — as well as over-time, where states can adopt new taxes from year to year (Kone and Winters 1993; Niemi, Stanley, and Vogel 1995). Second, researchers can study variation in the distribution of federal policies across states, counties, or regions, or variation in the timing of when policies are implemented across geographic space. Margalit (2011)'s study of job loss benefits is one example of this class of approaches, leveraging different levels of a policy across space. My paper takes a similar approach, using differences in the distribution of New Deal money to estimate the effects of policy.

2 The New Deal

The seismic economic collapse of the early 1930s is well-documented: industrial production declined by 47% between 1929 and 1933. Similarly, GDP declined by 30%. At its maximum, unemployment reached more than 20%.⁴ In response, newly-elected FDR and the Democrats enacted a “New Deal,” a series of policies, programs, and agencies designed to provide relief, recovery, and reform. New Deal policies dramatically and permanently expanded the size, scope, and power of the federal government. Indeed, some of these programs and agencies — such as the Social Security Administration — remain cornerstones of U.S. public policy today.

Though some of these programs focused on structural reforms (e.g., the FDIC), most of these programs were focused on alleviating economic stress and stimulating the economy in the short-run. The three largest of these programs were the Works Progress Administration (WPA), the Home Owners’ Loan Corporation (HOLC), and the Public Works Administration (PWA). The goal of the WPA was to provide jobs to those without work. Most WPA projects were small-scale, “make work” construction projects where those without work would be taken off the “dole,” hired to work on the project, and paid through the federal

⁴These figures are from <https://www.britannica.com/story/causes-of-the-great-depression>.

government. WPA workers also cooked school lunches, sewed clothes and other garments for the poor, and taught the illiterate how to read and write. In the end, the WPA employed more than 8 unique million workers, meaning that over its period of operation, nearly 25% of U.S. families relied on WPA jobs and wages (Federal Works Agency 1947).

The HOLC addressed another consequence of the Depression: many home owners were on the brink of foreclosure because they could no longer afford their mortgage. The HOLC replaced mortgages that were in or near default with new mortgagors that homeowners could afford. The HOLC bought old mortgages from banks, and issued new loans to homeowners that extended over longer time periods and at lower interest rates. Between 1933 and 1935, the HOLC made over 1 million new loans, and became the lender for one of every five residential mortgages in the country (Home Loan Bank Board 1952). The HOLC, like the WPA, provided direct government aid and assistance to people facing economic stress.

The PWA's main goal was to stimulate economic production. The PWA operated in the same way that most economic stimulus packages and spending on public infrastructure does today. In practice, the PWA contracted with local private sector construction firms who executed large-scale, public works projects. The PWA was established in 1933 with a \$3.3 billion authorization, and in the end, \$6 billion was spent on PWA projects between 1933 and 1939,. PWA projects included 11,428 road projects and 7,488 educational buildings, and by March 1939, PWA workers had constructed 70% of all school buildings constructed and and 35% of hospitals and health facilities constructed since 1933 (Public Works Administration 1939).

Taken together, these three programs reflect a clear policy response to the Great Depression. FDR and the federal government sought to influence policy outcomes — the state of the economy — via mix of direct government assistance policies and broader economic stimulus and private sector investment. Research in economics has concluded that relief and public works spending (e.g., the WPA and the PWA) increased retail sales (Fishback, Horace, and Kantor 2005) and net migration into counties receiving more funds (Fishback,

Horace, and Kantor 2006). These programs also lowered death rates (Fishback, Haines, and Kantor 2007), crime rates (Fishback, Johnson, and Kantor 2010), as well as infant mortality in the South (Fishback, Haines, and Kantor 2001). Two studies also show that the HOLC had its intended effects, increasing home ownership and preventing greater declines in home values between 1930 and 1940 (Courtemanche and Snowden 2011; Fishback et al. 2011).

The political consequences of New Deal policies and programs, however, are less clear. Indeed, retrospective voting models would predict that these policies and programs would have no unique influence on voting behavior. And in fact, Achen and Bartels (2016) advance just this argument in their recent treatise on democratic accountability. They argue that the New Deal realignment toward the Democratic Party was not a ringing endorsement of FDR’s economic policies. Rather, it was nonideological, and instead reflected standard economic voting as voters whose states experienced greater economic growth backed the incumbent (Democratic) party — and did so for many decades to come. Others, however, see the realignment as both a reflection of policy outcomes *and* of mass policy opinion toward New Deal policies. Using Gallup survey data from the 1930s through the 1950s, Caughey, Dougal, and Schickler (2020) show that increases in policy liberalism in the mass public realigned partisan identities toward the Democratic Party. In their interpretation, greater support for an expansive government explain voting in the 1930s and the subsequent partisan reconfiguring.

Below I bring to bear data on the actual *distribution* of spending in these three New Deal programs. Though all three were national programs active across the country at the same time, states and counties differed in how much money they received from each program. This feature of these programs allow me to estimate the effect of variation in these federal policies — in essence, the effect of differences in levels of support, help, and attention from FDR — on voting. Moreover, I can estimate the direct effects of variation in these programs *independent* of the effect of changes in policy outcomes — such as macroeconomic conditions — on voting.

3 Data

3.1 New Deal Spending

County-level data on New Deal expenditures were published in a 1940 report of the U.S. Office of Government Reports, and digitized by Fishback, Kantor, and Wallis (2003). These data give the total amount of money spent in each county by program aggregated across the years 1933 to 1939. Year-by-year data are not available. The data do not include expenditure information about every New Deal program — data on Civilian Conservation Corps (CCC) expenditures, for instance, were not reported — but does include data on many of the most prominent programs. Figure 1 gives an example of a full county report.⁵

Figure 1: Example of a County Expenditure Report

COUNTY REPORT OF FEDERAL EXPENDITURES			
			(1)
APPLING County, GEORGIA			
March 4, 1933 through June 30, 1939			
<u>Current Programs</u>	<u>LOANS</u>	<u>Number</u>	<u>Amount</u>
Federal Loan Agency			
1. Reconstruction Finance Corp. (From Feb. 2, 1932)		1.	\$ 102,796
2. Disaster Loan Corporation		2.	---
Federal Works Agency			
3. Public Works Admin. -- Non-Federal projects		3	20,147 3/
4. U.S. Housing Authority - Loan Contracts Signed		4.	---
Department of Agriculture			
5. Farm Credit Admin. -- Land Bank Commissioner	168	5.	173,640 2/
6. Farm Credit Admin. -- Emergency Crop and Feed	2,773	6.	233,404 2/
7. Farm Security Admin. -- Rural Rehabilitation		7.	141,450
8. Farm Security Admin. -- Farm Tenant Purchase	4	8.	22,632
9. Rural Electrification Admn. - (Total project	1	9.	105,375
Completed Programs			
10. Farm Credit Admin. - 1934-1935 Drought Relief		10.	---
11. Home Owners' Loan Corporation - 1933-1936	29	11.	32,026
	<u>Total Repayable</u>		\$ 831,470
<u>Current Programs</u>	<u>EXPENDITURES</u>		
Federal Works Agency			
1. Public Works Admin. -- Non-Federal projects		3	1. \$ 18,593
2. Public Works Admin. -- Federal projects		2.	---
3. Public Roads Admin. -- Completed projects		3.	200,180
4. Public Buildings Admin. - Federal buildings		4.	42,173
5. Work Projects Administration		5.	94,004
6. Other Projects under Works Program		6.	---
Federal Security Agency			
7. Social Security Board			
Old Age Assistance	\$ 7,622	54	
Aid to Dependent Children	5,283	78	
Aid to the Blind	563	5	13,468
Department of Agriculture			
8. A.A.A. Conservation Programs			
1936	\$ 47,898		
1937	29,483		
9. Farm Security Admin. - Rural Rehab.		8.	77,381
Completed Programs			
10. Federal Emergency Relief Administration		10.	104,786
11. Civil Works Administration		11.	39,399
12. A.A.A. Rental and Benefit Payments		12.	188,947
13. U.S.H.A. - Housing (Former PWA Housing only)		13.	---
	<u>Total Non-Repayable</u>		\$ 784,320
	<u>GRAND TOTAL REPAYABLE AND NON-REPAYABLE</u>		\$ 1,615,790

⁵In 1939, the Works Progress Administration was renamed the Work Projects Administration.

I focus on the WPA, HOLC, and PWA. Each was designed to provide short-term economic relief through a mix of direct assistance (WPA and HOLC) and private sector stimulus (PWA). Combined, these three programs make up about 45% of spending across the 15 New Deal programs included in this report. Throughout the paper, I use spending per capita, taking each county's expenditure level and dividing by its population in 1930. Per capita measures of spending ensures that we can compare allocations and the effects of spending across counties of different population sizes. In addition, I adjust for inflation and use spending in 1944 dollars throughout.

Table 1 reports summary statistics for spending. On average, counties received about \$85 per person in New Deal spending. Still, there is tremendous variation across counties and programs. The standard deviation of spending is \$136 per person. In addition, spending is clearly right-skewed, as the average county received much more than the median county. In other words, some counties received much more than the average county. There is also variation across programs, as programs like the WPA and PWA both awarded more per person average but also prioritized some counties over others much more than the HOLC. These descriptive statistics suggest significant variation in who benefitted from the New Deal, and my analyses below exploit this county-level variation to estimate the effects of policy on voting.

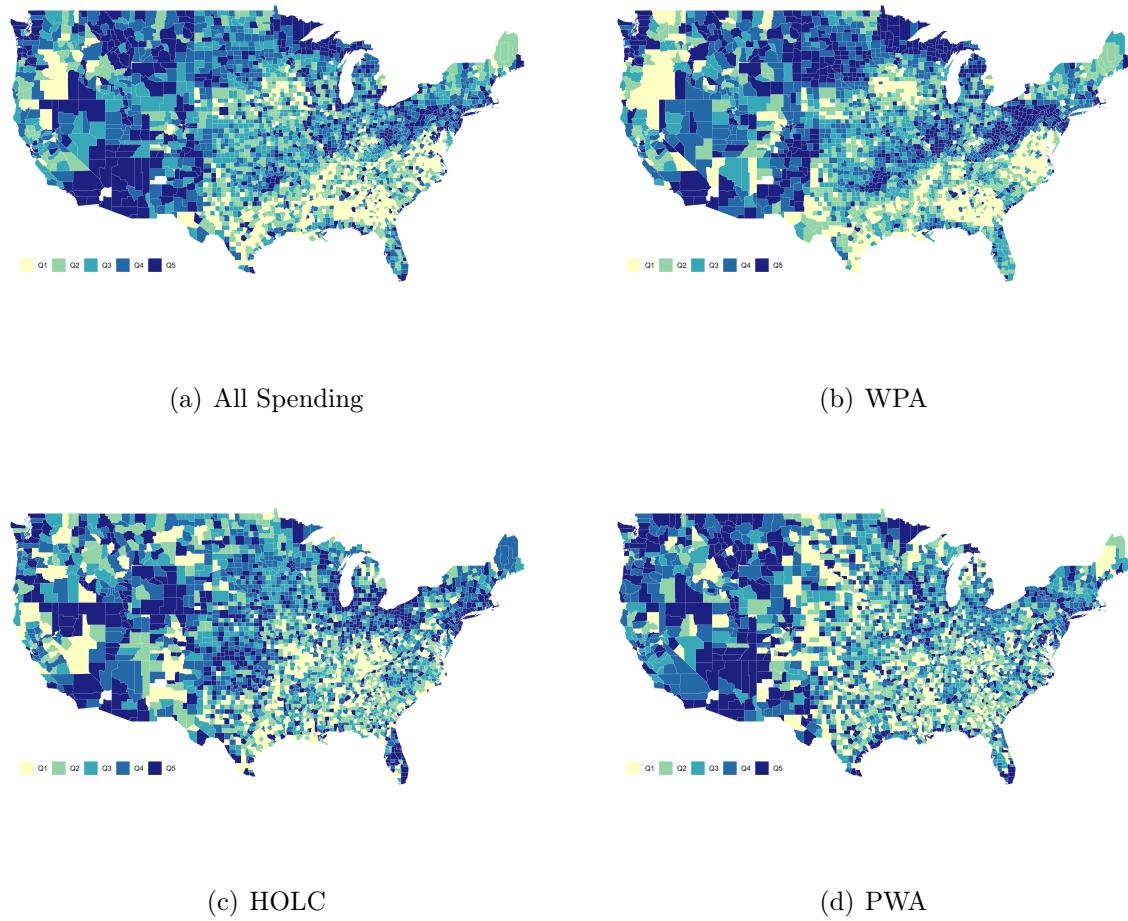
Table 1: Summary Statistics for New Deal Spending

	Median	Mean	SD
All Spending	\$60.61	\$84.50	\$136.44
WPA	\$34.02	\$44.80	\$43.44
HOLC	\$7.58	\$12.54	\$15.62
PWA	\$9.90	\$27.16	\$124.41

Figure 2 places each county into a spending quintile. As is clear, counties in the West, upper Midwest, and through parts of the Mid-Atlantic tended to receive the most money per capita. These patterns are consistent across programs, though regional differences in

allocations were noticeably sharper for the PWA. Here, most of the high-spending counties are completely concentrated in the West.

Figure 2: **Geographic Distribution of New Deal Spending**



What county characteristics predict the distribution of New Deal spending? One obvious possibility is that resources were targeted to political supporters (Berry, Burden, and Howell 2010; Cox and McCubbins 1986; Dynes and Huber 2015; Kriner and Reeves 2015), or to swing voters (Bickers and Stein 1996; Dixit and Londregan 1996). Figure 2 appears to suggest that New Deal programs were designed to expand the Democratic coalition, as the South — a sure-bet for the Democrats — received the least amount of money in total and across programs. Along

these lines, Wright (1974) shows that swing states receive more money than did non-swing states. Table 2 regresses county spending on the county % Democrat in the 1932 election, as well as three county characteristics — % Black, % urban, and % unemployed — as of 1930.⁶ In one specification, I also include state fixed effects. I find that urban counties, counties with fewer Blacks, and counties with higher unemployment received more New Deal money. However, within-states, there is no difference in New Deal allocations between more pro-FDR and anti-FDR counties. The positive effect of unemployment makes particular sense, given the nature of these programs. Indeed, the effect of unemployment is also substantively large, as each 1 percentage point increase in unemployment in 1930 nets an additional 5% in New Deal funds between 1933 and 1939. I address the importance of these factors as potential confounders in estimating the effect of spending on voting patterns below.

⁶Here, and throughout the paper, I take the natural logarithm of spending per capita to account for the significant right-skew in the data across counties.

Table 2: Predictors of New Deal Spending Distribution

	<i>DV: log(Spending)</i>	
	(1)	(2)
% Black	-0.01*** (0.001)	-0.01*** (0.001)
% Urban	0.01*** (0.001)	0.01*** (0.001)
% Unemployed	0.06*** (0.01)	0.05*** (0.01)
% Democrat, 1932	-0.004*** (0.001)	-0.002 (0.001)
Intercept	4.09*** (0.06)	3.61*** (0.12)
State FEs	No	Yes
Observations	3,061	3,061
R ²	0.33	0.46

Notes:

*p<0.05; **p<0.01; ***p<0.001

3.2 Economic Outcomes

County-level measures of economic conditions in the 1930s are hard to come by. Common contemporary measures of the economy such as income and GDP are not available at the county-level during this time period. Those that are available — such as unemployment — are not available at regular intervals throughout the 1930s. Rather, we only know county-level unemployment in 1930 and 1940 as recorded in the decennial Census.

As alternatives, I draw on measures of county-level retail sales and manufacturing output. Retail sales data were collected in 1929, 1933, 1935, and 1939 through the Census of Business, and digitized by Fishback, Horace, and Kantor (2005). These data report the total value of sales from retail establishments in the county for the year, giving a sense for county

consumption before, during, and after the Depression. Businesses included in this measure range from grocery stores to motor vehicle dealers. Manufacturing output was collected in 1929, 1933, 1935, 1937, and 1939 by the Census of Manufactures. Manufacturing output is a value added measure; it is the value of all manufacturing outputs minus the value of the inputs used in production. It represents manufacturing's contribution to overall GDP. Higher values are indicative of greater production. These data are incomplete, as the Census Bureau does not report data for counties with few manufacturers. I have complete data for 1,852 counties (60%). Analyses below using these should be viewed with the caveat that only a subset of (often larger) counties are included.

Figure 3: County Economic Conditions in the 1930s

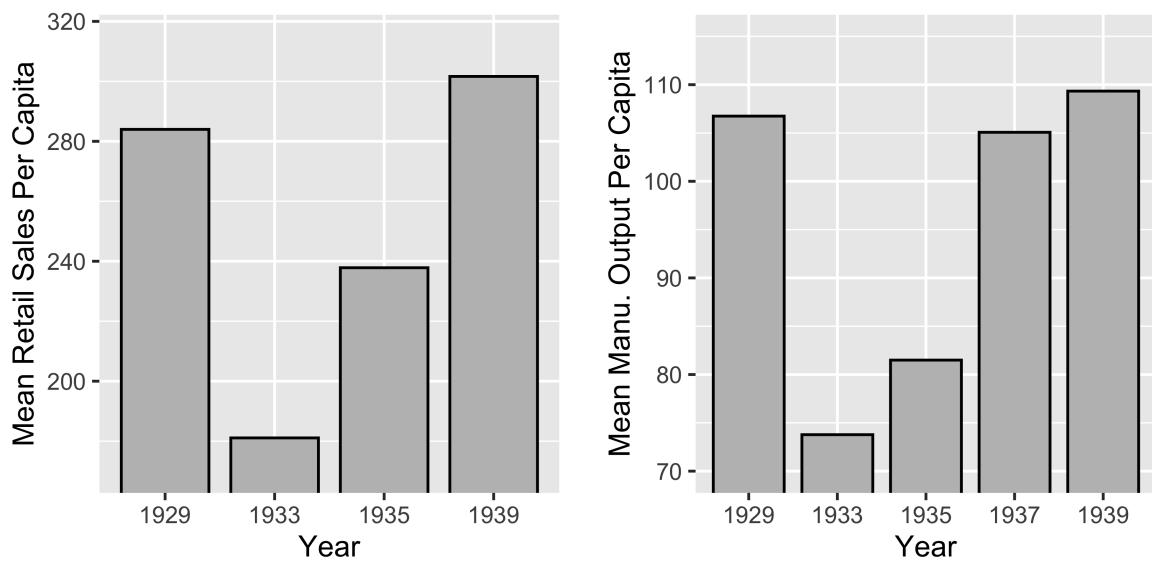


Figure 3 plots the average value of both measures by year.⁷ As expected, retail sales activity and manufacturing output dropped significantly from 1929 to 1933 as the Depression reached its trough. Average retail sales decreased by 44% between 1929 and 1933. Likewise, manufacturing output decreased by 36%. Consumption and production improved year-by-year between 1933 and 1939, and by 1939, conditions had recovered to 1929, pre-Depression levels. In fact, both retail sales and manufacturing output were marginally higher in 1939

⁷As with the New Deal spending data, all values are in 1944 dollars.

than 1929. All told, while these measures are not standard in the literature on economic voting, both appear to be reasonable proxies for local economic conditions during this time period.

4 Did New Deal Spending Affect Economic Conditions?

Whether spending had observable impacts on the economy, and in which direction, is integral to my test. Let's assume that voters *are* responsive to policies as well as policy outcomes. If these policies were successful in boosting the economy, we should expect voters to reward the incumbent party. However, if these policies were had no observable economic impact, or perhaps even led to further economic decline, then we should expect policy-attuned voters to punish the incumbent party. Though past work suggests that relief and public works spending had positive economic effects (e.g., Courtemanche and Snowden 2011; Fishback et al. 2011; Fishback, Horace, and Kantor 2005), I test for economic effects of the three specific programs under study here, as well.

Table 3 regresses the county-level change in retail sales and the change in manufacturing output on county spending. Both outcomes are measured as the log ratio of 1939 to 1933, allowing me to predict growth as a function of spending over the same time period. In some specifications, I also adjust for the three county-level characteristics predictive of spending — % black, % urban, and % unemployed — and include state fixed effects, meaning that these estimates reflect within-state differences in economic outcomes between counties receiving more or less money. As intended, spending appeared to have positive impacts on economic outcomes. Each 1% increase in spending increased economic growth over the same time period between 0.02% to 0.04%. While far from causal, these models provide some baseline evidence that the New Deal had some of its intended effects on both consumption and production, and motivate my expectations for the remainder of the paper.

Table 3: **New Deal Spending and Economic Change, 1933-1939**

	<i>DV: Δ Retail Sales</i>		<i>DV: Δ Manu. Output</i>	
	(1)	(2)	(3)	(4)
log(Spending)	0.02** (0.01)	0.02** (0.01)	-0.02 (0.02)	0.04* (0.02)
% Black		-0.001** (0.0003)		0.002* (0.001)
% Urban		-0.0001 (0.0002)		-0.001 (0.001)
% Unemployed		0.001 (0.002)		-0.01* (0.01)
Intercept	0.42*** (0.02)	0.43*** (0.04)	0.56*** (0.07)	0.40*** (0.11)
State FEs	No	Yes	No	Yes
Observations	3,065	3,065	1,967	1,967
R ²	0.003	0.23	0.001	0.12

Notes:

*p<0.05; **p<0.01; ***p<0.001

5 Empirical Strategy: Spending and Voting

I merge my data on New Deal spending and economic conditions with county-level presidential elections voting returns from 1924 to 1944. Note that in the New Deal data, some counties are reported as one. For instance, spending in each of New York City's five borough's — each of which is in a different county — were collapsed into one geographic unit. The data are reported similarly for many counties and incorporated cities in Virginia (e.g., Alexandria and Arlington County). In these cases, I also aggregated votes into one “county.” In sum, my dataset includes data on spending, economic conditions, and voting behavior for 3,067 “counties.” To estimate the effects of spending and the economy on voting, I use a generalized difference-in-differences design. I estimate the following linear model:

$$\% \text{ Democrat}_{cst} = \beta_1 \log(\text{Spending})_{cs} + \beta_2 \Delta \text{ Local Economy}_{cs} + \theta_{cs} + \alpha_{st} + \gamma_{cst} + \lambda_{cst} + \kappa_{cst} + \epsilon_{cst}$$

where $\% \text{ Democrat}_{cst}$ is the Democratic share of the two-party vote in county c and state s at time t . $\log(\text{Spending})_{cs}$ is logged New Deal spending per capita in county c and state s . For all t prior to 1936, the value of $\log(\text{Spending})_{cs}$ is 0. $\Delta \text{ Local Economy}_{cs}$ is either the change in retail sales per capita from 1933 to 1935, or the change in manufacturing output per capita from 1933 to 1935 in county c and state s .⁸ As before, both economic measures are calculated as the log ratio.

θ_{cs} are county fixed effects, and α_{st} are state-year fixed effects. In all specifications, I also cluster the standard errors by county. County fixed effects mean that my estimates reflect within-county changes in voting patterns before and after New Deal spending. State-year fixed effects mean that my model uses only other counties in the same state as the counterfactual comparison group for each county. Combined, the inclusion of these fixed effects means that I am comparing the within-county change in Democratic support in counties with large New Deal allocations to the within-county change in Democratic support in same-state

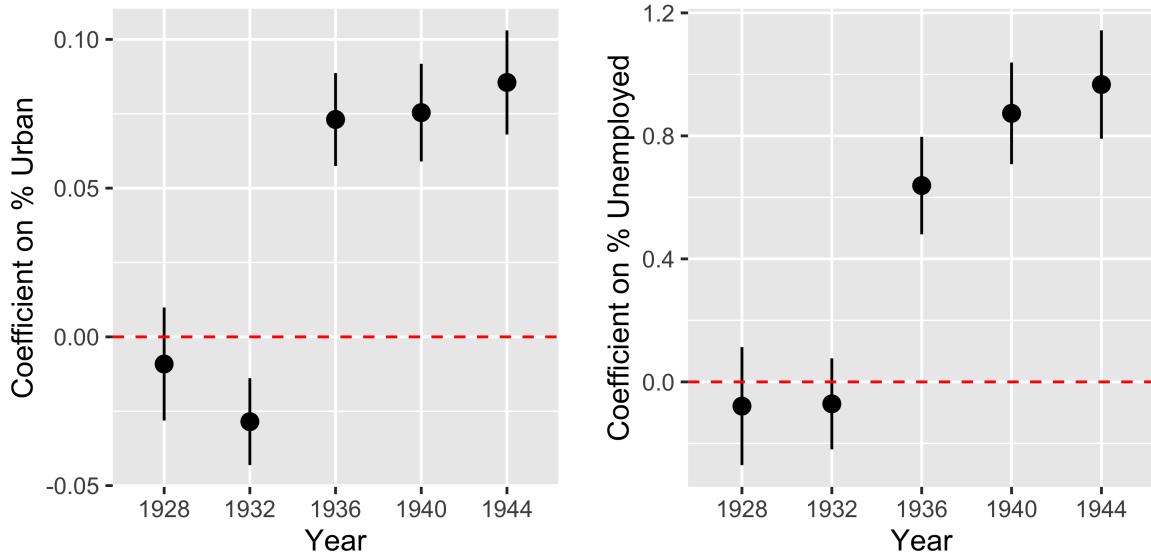
⁸I use the the change from 1933 to 1935 to capture the effects of initial changes in the economy as spending began.

counties with smaller New Deal allocations.

For the β_1 to be causal, we must assume parallel trends. Parallel trends means that the vote share in counties with smaller New Deal allocations must provide valid counterfactuals for the trends *we would have observed* in same state counties with larger New Deal allocations had these counties instead had smaller New Deal allocations. It cannot be the case that counties that received larger New Deal allocations were already trending in the direction of the Democratic Party faster than counties that later received smaller allocations in the years. If this is the case, we cannot attribute the estimated effect to the effect of spending itself.

Is parallel trends a reasonable assumption? We may be concerned that factors correlated with how money was distributed may also be predictive of voting, and in particular, *trends* in voting. Table 2 shows that more urban counties, counties with fewer Blacks, and counties with higher unemployment as of 1930 received more New Deal spending. These specific county factors may bias the effect of spending on voting if these same variables also predict trends in voting over the same time period.

Figure 4: Time Trends in the County Demographic Predictors of % Democrat



To test for this possibility, I regress county % Democrat in each of 1928-1944 on the county

% urban and county % unemployment as of 1930. The models also include state fixed effects. Figure 4 plots the coefficients for each year. As is clear, counties are shifting in their voting behavior based on these characteristics. For instance, prior to 1936, urban counties leaned Republican, but in 1936 and beyond, more urban counties were more Democratic. These demographic trends in voting suggest that we may worry about parallel trends, as counties who received more New Deal money (such as urban counties) may have been increasingly likely to vote Democrat even in the absence of receiving more money. If this is the case, the estimate of the effect of spending on votes will be biased.

To adjust for these confounders, I place each county into a *within-state* quartile for each of the three county characteristics examined above, and include each as a fixed effect by year. γ_{cst} are % Black quartile-year fixed effects, λ_{cst} are % urban quartile-year fixed effects, and κ_{cst} are % unemployed-year fixed effects. The inclusion of these fixed effects means that my counterfactual comparison group is computed not just among counties within the state, but among same state counties of a similar pre-treatment urbanity, Black population, and unemployment rate. Moreover, I allow the effect of each of these demographic characteristics to vary by election. Taken together, these fixed effects allow me to “net out” from the effect of spending the changing effects of urbanity, racial composition, and unemployment on voting. The estimates should more closely reflect the actual effect of spending itself.

6 Results: Immediate Effects

To estimate the immediate effect of New Deal spending, I take two data approaches: a 1924-1944 panel, and a 1932-1944, FDR-only panel. The former includes election years 1924 through 1944. It estimates the effect of spending as the within-county change in the average support for the Democratic Party from 1924-1932 to 1936-1944. The second approach removes 1920-1928 from the analysis, and estimates the effect of spending on Democratic voting relative *only* to support for FDR in 1932. This set-up allows me to see whether New

Deal programs increased FDR's support above and beyond his county-level support in his landslide 1932 victory.

Table 4: New Deal Spending and Presidential Voting

	<i>DV: Democratic Vote Share (0-100)</i>					
	(1)	(2)	(3)	(4)	(5)	(6)
Data: 1924-1944						
log(Spending)	2.55*** (0.28)	2.44*** (0.24)	3.30*** (0.30)	1.04*** (0.29)	0.88*** (0.23)	1.49*** (0.30)
Δ Retail Sales ₃₅₋₃₃		2.42** (0.81)			2.62*** (0.77)	
Δ Manu. Output ₃₅₋₃₃			0.15 (0.29)			0.32 (0.27)
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
% Black-Year FEs	No	No	No	Yes	Yes	Yes
% Urban-Year FEs	No	No	No	Yes	Yes	Yes
% Unemployed-Year FEs	No	No	No	Yes	Yes	Yes
Observations	18,364	18,355	15,172	18,364	18,355	15,172
R ²	0.94	0.94	0.95	0.95	0.95	0.95
Data: 1932-1944						
log(Spending)	2.23*** (0.24)	2.13*** (0.21)	2.74*** (0.27)	0.86*** (0.25)	0.74*** (0.21)	1.14*** (0.26)
Δ Retail Sales ₃₅₋₃₃		1.19 (0.73)			1.52* (0.70)	
Δ Manu. Output ₃₅₋₃₃			0.01 (0.25)			0.09 (0.23)
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
% Black-Year FEs	No	No	No	Yes	Yes	Yes
% Urban-Year FEs	No	No	No	Yes	Yes	Yes
% Unemployed-Year FEs	No	No	No	Yes	Yes	Yes
Observations	12,252	12,243	9,060	12,252	12,243	9,060
R ²	0.97	0.97	0.97	0.97	0.97	0.97

Notes:

*p<0.05; **p<0.01; ***p<0.001

Table 4 reports the results. The top panel reports the results using the 1924-1944 panel, while the bottom panel uses the FDR-only panel. Columns (4)-(6) estimate my model as described above. Columns (1)-(3) estimate my model *without* the county characteristic quartile-year fixed effects. I do so as a simple point of comparison, as if those three pre-treatment county characteristics are in fact confounding the effect of spending on voting, we should see the effect of spending attenuate as we account for these factors explicitly in Columns (4)-(6). The evidence is consistent with this expectation.

Across both datasets, I find significant effects of New Deal allocations on voting *net of the effect of changes in county macroeconomic conditions..* Consider Column (4). I find that a 1% increase in New Deal spending increases Democratic support by about 0.01 percentage points. Even restricting the data to just the FDR years, we see a positive effect similar in size. A similar 1% change in retail sales from 1933 to 1935 nets an additional increase in Democratic vote share. In the 1932-1944 panel, a 1% increase in retail sales boosted Democratic support by about 0.015 percentage points. I do not, however, find an effect of changes in manufacturing output on voting. The size of these coefficients are small, and very noisy. Taken together, the evidence thus far lends a different conclusion than traditional models of retrospective voting would predict. Here, I have found evidence that voters are responsive to the policy actions that politicians take. Counties that received more benefits via New Deal policies and programs were more supportive of the Democratic Party than those counties that received less money *no matter the economic conditions.* In addition to whether times are good or better, voters appear to care about whether politicians took action to address economic needs. The magnitude of these effects are also notably on par with the magnitude of the effects of positive changes in those macroconditions, too.

7 Heterogeneity in Policy Effects

The above models test the aggregate effect of spending, combining WPA, HOLC, and PWA spending into one measure. Of course, while these programs had similar end-goals and objectives, each provided a different form of relief and recovery. As noted, the WPA focused on providing work to those without it, the HOLC refinanced struggling homeowners' mortgages, and the PWA sought to boost private sector production, particularly in the construction and manufacturing sector. Below I assess whether these three programs had differential effects on voting.

To do so, I reestimate the same model but disaggregate spending into three variables. Here, I use just the 1932-1944 panel, as these estimates give the most immediate effect, as well as the most conservative estimates. I also only present the results adjusting for the county confounders. Table 5 presents the results. It appears that most of the aggregate effect of New Deal spending can be attributed to the WPA and the HOLC, and especially the WPA. In contrast, the coefficients for the PWA are small in magnitude, and statistically indistinguishable from zero.

Table 5: New Deal Spending and Presidential Voting, 1932-1944 — WPA vs. HOLC vs. PWA

	DV: Democratic Vote Share (0-100)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
log(WPA)	0.83*** (0.21)	0.78*** (0.19)	1.30*** (0.23)						
log(HOLC)				0.43* (0.17)	0.41* (0.17)	0.63** (0.22)			
log(PWA)							0.15 (0.10)	0.12 (0.10)	0.11 (0.13)
Δ Retail Sales ₃₅₋₃₃		1.74* (0.70)			1.49* (0.6921)			1.41* (0.69)	
Δ Manu. Output ₃₅₋₃₃			0.13 (0.23)			0.07 (0.23)			0.10 (0.23)
County FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
% Black-Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
% Urban-Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
% Unemployed-Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12,252	12,243	9,060	12,252	12,243	9,060	12,252	12,243	9,060
R ²	0.97	0.97	0.97	0.97	0.97	0.97	0.97	0.97	0.97

Notes:

*p<0.05; **p<0.01; ***p<0.001

Why do the WPA and the HOLC appear to exert the strongest immediate substantive effect on voting? Theory suggests that two conditions must be met for voters to observe new public policies: visibility and traceability (Arnold 1990; Pierson 1993). First, voters must see or experience direct benefits from a program or policy. And second, voters must be able to trace those benefits or the effects of those policies back to government action (see also Soss and Schram 2007). That is, they must hold government responsible for the policy or benefit. Indeed, policy traceability is akin the “clarity of responsibility” in economic policy outcome voting (Powell Jr. and Whitten 1993; Rudolph 2003; Samuels 2004). Voters can only hold an incumbent accountable for policy or policy outcomes if they perceive the incumbent as deserving of credit or blame.

But recent work questions the traceability of many government programs. Mettler (2011) in particular documents the pervasive “submerged state,” referring to the invisibility of many government policies hidden within the tax code, in government-private sector partnerships or that are otherwise “contracted out” to non-governmental agencies (see Kettl 1988 and Morgan and Campbell 2011 for similar arguments). One way that these three programs differed is in their potential traceability to government. The HOLC and WPA offered *direct* financial assistance and benefits, as the government refinanced mortgages for struggling homeowners and gave government jobs to those without work. In contrast, though the PWA certainly created jobs for many, PWA money flowed not from government to voters, but first from government to businesses. In the empirical literature, there is also some evidence that voters are most responsive to direct government aid, as voters increase support for the incumbent party in response to direct disaster relief benefits but not to collective relief benefits for a geographic area (Healy and Malhotra 2009). While intriguing, my results are only suggestive of potential policy design effects.

8 Exploring Mechanisms: Media

How did voters learn about New Deal programs and more specifically, spending in their county? Many voters, of course, may have benefitted directly through jobs or a new mortgage. But still others may have learned about the New Deal and spending in their county through indirect means, such as the media. It is well-established that the news media are one of the primary institutions linking voters to political information. The media have been shown to increase political knowledge and voter turnout (Gentzkow, Shapiro, and Sinkinson 2011; Hayes and Lawless 2015, 2018), and can persuade voters through the framing of the news and candidates (Berinsky and Kinder 2006; Druckman and Parkin 2005; DellaVigna and Kaplan 2007; Ladd and Lenz 2009). There is also evidence that economic news coverage, and the way it is framed, facilitates economic retrospective voting (Garz and Martin 2020; Hetherington 1996; Mutz 1992, 1994). That is, economic news coverage makes voters more responsive to policy outcomes in the voting booth.

In my setting, then, we may expect voters to be more aware of and subsequently more politically responsive to New Deal program spending in counties with a stronger information environment and especially in counties that saw more media coverage of those programs. I test for media effects in two ways. First, I use 1930 Census data on radio access. The measure simply gives the share of families who report owning a radio in 1930. On average, 25.71% of families had radio access in 1930. Radio, of course, was the primary way that FDR communicated with the public through his now-famous “fireside chats.”

Second, I created an original dataset of county-level newspaper mentions of the WPA, HOLC, and PWA. To do so, I scraped [newspapers.com](#). [Newspapers.com](#) is the largest online news archive available right now, with over 606 million pages of text transcribed from almost 18,000 newspapers from the 1700s onward. Their archive has been used in recent work documenting political power in the 1800 and 1900s (Ban et al. 2019). For every newspaper in their archive from 1933 to 1939, I scraped the number of newspaper pages that included the terms “Works Progress Administration,” “Home Owners’ Loan Corporation,” or

"Public Works Administration." Figure 5 shows two examples of New Deal-spending specific newspaper coverage.

Figure 5: Examples of New Deal-Specific News Coverage

<p>Work Program Will Employ All on Dole; County Officials Sure of Early Action</p> <p>(Continued From Page One)</p> <p>on relief rolls in the county have been put into effect in the plains. The figure \$4,000 per month and absolute, however, John F. Laboon, local head of the Works Progress Administration, explained yesterday that, if necessary, the fund will be "juggled around" to give more permanent work.</p> <p>If the difficulties with the local governments are all ironed out, however, the Works Progress Administration still has one more high hurdle.</p> <p>Organized labor and the relief clients themselves are complaining that the wages to be paid are too low. The minimum wage is now \$55 a month for the 38,000 unskilled laborers to be employed, and run to a maximum of \$94 a month for the 1,000 skilled workers on the tentative list.</p> <p>Union leaders call these rates "welfare shop wages," and maintain that they will tend to force down other wages. They also point out that in progress in New York, are threatened here.</p> <p>In answer, Works Progress Ad-</p> <p>County Relief Situation in Brief</p> <p>Here, in brief, is the work-relief situation in Allegheny County, with 234,000 persons (75,000 families) due to go off the relief rolls by August 15.</p> <p>The PUBLIC WORKS PROGRESS ADMINISTRATION—to provide 4,000 jobs, with expenditure of \$47,000,000, if—can pull up \$8,000,000 to be raised by bond issue.</p> <p>County puts up \$7,000,000 to be raised by bond issue. Wages to average \$55 month for common labor to \$94 for technicians.</p> <p>The PUBLIC WORKS ADMINISTRATION—to provide 4,000 jobs, with expenditure of \$34,165,000, if present Allegheny County Authority projects go through. Of this, 70 per cent is a Federal contribution, and the balance is to be furnished by the county, which has scaled down to as low as \$8,600,000, with elimination of toll projects. In a substitute plan, with the outcome still in doubt, it was proposed that the county furnish \$55 a month for the 38,000 unskilled laborers to be employed, and run to a maximum of \$94 a month for the 1,000 skilled workers on the tentative list.</p> <p>FUNDS AVAILABLE FOR RELIEF of those not taken care of by the Works Progress Administration and the Public Works Administration are ample, and sufficient to meet the needs like that in progress in New York, are threatened here.</p> <p>County Poor Board—which has "ample" but unestimated funds to be used only for persons not fit for employment, outside of the city.</p>	<p>6 PWA Projects Now Under Way Through Nevada</p> <p>Six PWA projects calling for the expenditure of approximately \$1,174,000 were under construction yesterday at the first of the month, Wright L. Felt, acting state director for the agency, said yesterday.</p> <p>The projects, three located in Reno, two in Las Vegas and the Pioche-Boulder dam power line, call for the expenditure of \$695,541 in federal funds, released through the PWA.</p> <p>In Reno work is being done on the Lake and Sierra street bridge, with the total cost of the two projects amounting to \$71,336. PWA grants to the projects total \$36,000. The \$68,851 remaining pools in Silverwood park were also constructed with PWA assistance, the amount allocated being a grant of \$30,831 to the city.</p> <p>The other projects include the Pioche-Boulder dam transmission line, which is being built at an estimated cost of \$900,000, and a grant of \$326,000; the North Las Vegas water system towards which PWA grants of \$90,000 and \$100,000, estimated total cost of \$34,245, and street improvement work in Las Vegas, estimated to cost \$76,344 and which has been given a PWA grant of \$34,364.</p>	<p>11,547,220 Hours Of Work Created in Nevada by PWA</p> <p>A total of 11,547,220 man-hours of direct labor has been created by the PWA on various federal and non-federal projects in Nevada, Wright Felt, acting state director of the organization, said yesterday.</p> <p>Of this total, 11,040,844 man-hours of work on federal projects, while non-federal jobs accounted for 506,376 hours.</p> <p>The estimated cost of projects in Nevada amounts to \$2,595,025, of which \$1,072 was furnished by the applicants and \$1,029,381 given in grants by the Public Works Administration.</p> <p>5-Billion Hours Of Work Created</p> <p>Work approximating five billion man hours at the construction site or in production, fabrication and transportation of required public works materials has been created by the federal and non-federal Public Works Administration projects.</p> <p>This was revealed today by statistics announced as PWA passed its fifth anniversary. The figure does not include a huge amount of additional labor created in the manufacture of goods and services for consumers' goods and services.</p> <p>The five billion man-hours figure was reached as PWA passed its fifth anniversary. The figure does not include a huge amount of additional labor created in the manufacture of goods and services for consumers' goods and services.</p> <p>Non-federal applicants furnished a total of \$1,000,000,000 in grants over the four years period.</p>
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(a) The Pittsburgh Press on August 11, 1935

(b) Nevada State Journal on June 20, 1937

Between 1933 and 1939, newspapers.com has text from 1,990 newspapers in 770 counties.

The county-level median number of mentions to the three programs combined over the seven year period is about 755. The average value is 3,760. In modeling the effect of media mentions below, I log-transform this variable to account for the right-skew in mentions. Several important caveats apply to these data. The most important is that these data represent only a sample of newspapers and only a sample of news coverage about these three programs. The latter limitation is true even among those newspapers included in the archive, as newspapers.com may only currently have access to a small sample of newspapers clippings. In short, we should expect there to be measurement error in the data. These limitations aside, these data allow me to test for whether policy coverage conditions the effect of policy spending on voting. Combined, my two measures — radio access and New Deal-specific coverage — should offer some at least suggestive evidence on the role of the information environment and media in encouraging and facilitating policy-based retrospective evaluations.

I reestimate the same specifications as before, but in each model, include an interaction term between spending and each media measure. I treat each media measure as time-invariant, meaning that the direct, unconditional effect of media cannot be estimated independently of the county fixed effects. As a result, the main effect of media is excluded, and absorbed into the the fixed effects. Here again, adjusting for the county characteristics is critical, as we should expect more urban counties, for instance, to have greater radio access, more newspapers, and in turn potentially more program-specific coverage.

Table 6: New Deal Spending and Presidential Voting, 1932-1944 — Media

	DV: Democratic Vote Share (0-100)					
	(1)	(2)	(3)	(4)	(5)	(6)
log(Spending)	0.52 ⁺ (0.27)	0.38 ⁺ (0.22)	0.80** (0.29)	0.82*** (0.25)	0.70*** (0.21)	1.07*** (0.26)
log(Spending) × % Radio	0.01*** (0.00)	0.02*** (0.00)	0.01** (0.00)			
log(Spending) × log(News Mentions)				0.02 ⁺ (0.01)	0.02 ⁺ (0.01)	0.02* (0.01)
Δ Retail Sales ₃₅₋₃₃		1.59* (0.69)			1.50* (0.70)	
Δ Manu. Output ₃₅₋₃₃			0.06 (0.23)			0.11 (0.23)
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
% Black-Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
% Urban-Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
% Unemployed-Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12,252	12,243	9,060	12,252	12,243	9,060
R ²	0.96813	0.96864	0.97332	0.96803	0.96852	0.9733

Notes:

⁺p<0.10; *p<0.05; **p<0.01; ***p<0.001

Table 6 presents these results. I find that greater radio access increased the positive effect of New Deal spending on Democratic support. I also find positive interaction effects of spending and spending mentions, though these effects are statistically weaker. Again, these estimates should be seen as suggestive, not conclusive, given the limitations of data. Still, consistent with a large body of work on media effects, the media appears to be one way that voters were able to respond to policy actions.

9 Do Policy and Policy Outcome Effects Persist?

Throughout this paper, I have focused on the immediate differences in voting patterns pre- and post-spending. For instance, the estimates in the top panel of Table 4 tell us whether Democratic voting patterns changed — and by how much — just prior to spending (1924-1932) and just after spending (1936-1944). The estimates from these models therefore give average differences before and after spending.

Recent research looks for persistent effects of policies, historical institutions, and significant events on public opinion and elections over time. For instance, Acharya, Blackwell, and Sen (2016) find that counties that had large slave populations in 1860 are more Republican today than those counties that had smaller slave populations. Southerners living in these counties today are also more racially resentful than Southerners in Southern counties with a weaker slaveholding history. Along the same lines, Mazumder (2018) shows how civil rights protests in 1964 affect contemporary racial attitudes and county election outcomes.

One obvious question is whether the effects of New Deal spending were substantively large during FDR’s re-election campaigns in 1940 and 1944, and weak or potentially even non-existent in subsequent elections and for candidates other than FDR. The answer to this question also addresses ongoing debates in work on explanations for the New Deal realignment (Achen and Bartels 2016; Caughey, Dougal, and Schickler 2020). To test for persistent effects, I reestimate the same model as before — inclusive of county-confounder fixed ef-

fects — but expand the dataset to 2016, and interact New Deal spending with each election year. Doing so gives me a separate coefficient for the effect of spending (relative to 1924–1928) for every election cycle. As before, because of the fixed effects, I make counterfactual comparisons only among similar counties (pre-treatment) in the same state.

I make two other changes to the specification. First, in this model, I also treat 1932 as a treatment election. If I find a positive and significant effect of spending on voting in 1932, then the effects of spending in 1936 and beyond may reflect the strategic placement of spending in Democratic counties rather than the influence of spending on voting behavior. Second, I estimate two models, one for the South (11 Confederacy states) and one for the non-Southern states. The simple reason is because the South underwent a significant transformation from a Democratic to Republican stronghold over this time period.

Figure 6: Long-Run Effects of New Deal Spending

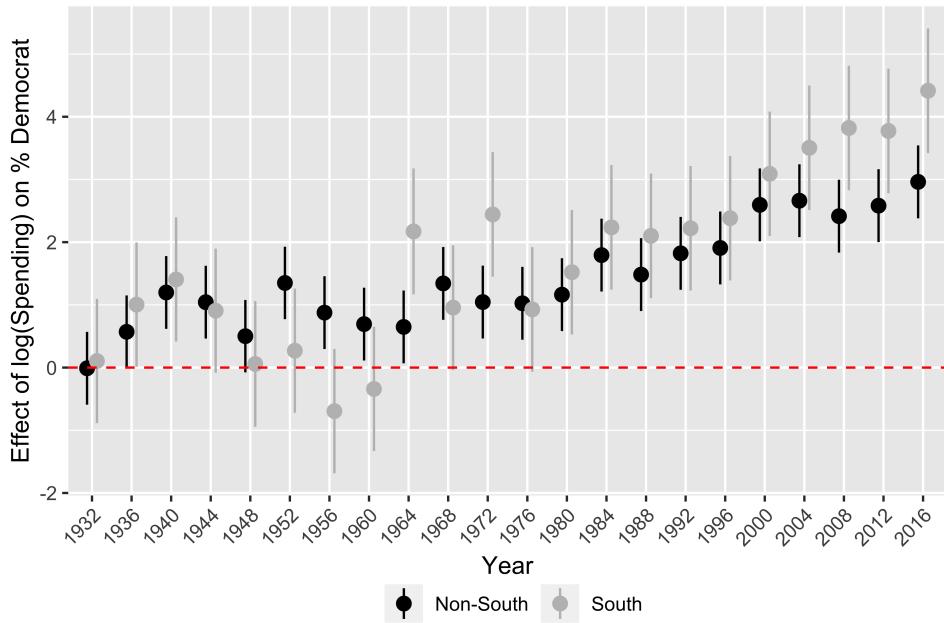


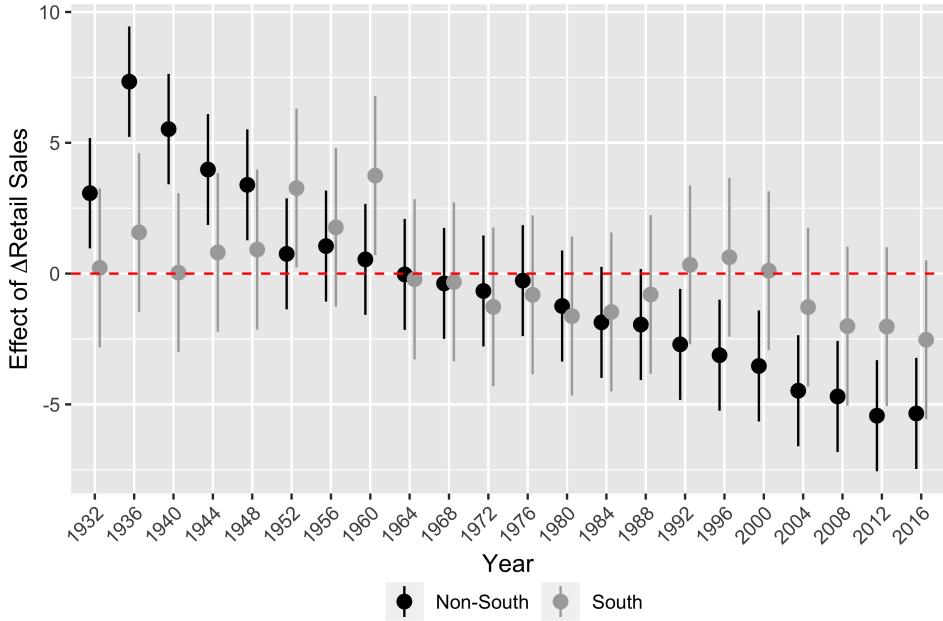
Figure 6 presents the results. In the non-South, I find positive and significant effects of New Deal spending in every election cycle through 2016, suggesting that variation in New Deal allocations has permanently affected voting behavior. To be sure, estimating

persistence across such a long time-series is not easy, and we should view the estimates as suggestive. The fact that the magnitude of the effects are increasing in recent years should not be taken as evidence that voters today have taken notice of the WPA-built structures in their county, and are responding by supporting the Democratic Party today. More likely is that confounders have been introduced over time that bias these estimates upward. Beyond estimation, outlining a theory that can account for the persistence of effects, even effects that appear substantively reasonable, is challenging.

As a result, I focus on the presence of persistence, rather than the magnitude of the persistence estimated here. That is, while the magnitude of these effects should not be written home about, what we can say with more certainty is that the effects of New Deal spending on county voting patterns have not quickly converged back to zero over time. It appears that what incumbents do matters to voters, so much so that it can fundamentally change the partisan tendencies of the places who feel the impact of government action the most.

As a point of comparison, I also estimate the persistent effects of retail sales growth between 1933 and 1935, using the same specification as for spending. Can improvement in the macroeconomy have long-lasting effects, too? Figure 7 tests this claim further. As is clear, retail sales growth has very large and immediate effects. Indeed, in 1936, a 1% change in retail sales growth from 1933 to 1935 in non-Southern states increased Democratic support by 0.07 percentage points. By 1944, that effect decreased to 0.03 percentage points, and by 1948, there is no longer a statistically discernible effect of post-Depression growth on voting. In contrast to the effects of policy, the effects of policy outcomes appear to be much more transient. In short, while “highs” in broader conditions can have large immediate effects on the fortunes of politicians presiding over growth, it appears that the policies introduced to produce those “highs” can have long-lasting impacts on partisanship and voting. Again, this is suggestive of the potential importance and power of policy in the minds of voters and as a source for significant political change.

Figure 7: Long-Run Effects of Retail Sales Growth



10 Conclusion

In 2001, George W. Bush and the Republican Congress passed a tax relief bill. Soon thereafter, almost 100 million taxpayers received a letter from the IRS stating the following: “We are pleased to inform you that the United States Congress passed and President George W. Bush signed into law the Economic Growth and Tax Relief Reconciliation Act of 2001, which provides for long-term tax relief for all Americans who pay income taxes.”⁹

After passage of the CARES Act in response to COVID-19, Americans received a similar letter alerting them to the arrival of their \$2,400 stimulus check: “As we wage total war on this invisible enemy, we are also working around the clock to protect hardworking Americans like you from the consequences of the economic shutdown.”¹⁰ The letter was signed by President Trump.

⁹https://www.cnn.com/2001/ALLPOLITICS/06/19/tax.letter.text/?mod=article_inline

¹⁰<https://www.usatoday.com/story/news/politics/2020/04/28/coronavirus-trump-letter-stimulus-check-reci3040031001/>

These are just two examples of incumbent politicians promoting their policy actions. Both policies were designed to put more money in the hands of Americans and boost the economy. Yet, though it is no secret that politicians often promote their accomplishments (Mayhew 1974), political science theory would predict that these policy decisions and actions will not affect subsequent voting behavior. Rather, it is expected that voters would only evaluate the incumbent on the basis of the state of the conditions these policies sought to affect.

In this paper, I test this claim using data from three of the largest New Deal-era economic relief programs. These programs put millions of Americans to work, kept struggling families in their homes, and stimulated private sector production. Yet, some counties received much more of these benefits than others. Using a difference-in-differences approach designed to rule out alternative explanations, I find robust evidence that voters rewarded the Democrats for New Deal spending in their county above and beyond the effect of improvements in county macroeconomic conditions. These results suggest that the *means* matter: voters do not only care about whether policymakers were able to boost the economy, but whether and how they *tried* to boost the economy. I also find that the WPA and HOLC — the two programs that offered direct financial assistance to voters — were most effective at increasing Democratic vote share. Finally, I offer some suggestive evidence that media coverage facilitated policy-based retrospective voting, and that — unlike the effects of macroeconomic changes — the positive effects of policy on voting can last for decades. In short, while more work is needed to probe the persistence of these effects, it appears that policy can powerfully and perhaps even permanently affect the partisan proclivities of places.

My findings have significant implications for democratic representation. Fox and Shotts (2009) develop a theory of when elected officials will behave as trustees or delegates, and argue that whether officeholders pursue the interests of the public (delegate) or pursue their own interests (trustee) depends on how voters vote. When voters prioritize competence in their voting calculus, politicians will tend to act as trustees. But if voters are policy-oriented, politicians will emphasize ideological congruence with their constituents. In finding that

voters are responsive to policy actions, my results suggest that politicians may be more constrained in their choices than theories of retrospective voting allow. Indeed, making the wrong policy choice, or failing to act at all, may hurt the incumbent just as much as failing to improve the economy itself.

My study also speaks to the challenge — and potential opportunity — of governing in times of crisis. Past work shows that voters punish and reward politicians for their responses to natural disasters (Bechtel and Hainmueller 2011; Gasper and Reeves 2011; Healy and Malhotra 2009). My paper builds on this work, showing how policy action in the wake of economic catastrophe influences voters in a similar way — and potentially for many decades thereafter. These findings suggest that politicians may be able to mitigate the negative electoral effects of economic collapse by pursuing policies and offering provisions that may stem the tide (see also Margalit 2011). Simply put, voters care about whether and how leaders respond to crisis.

The purpose of this paper was to provide some much-needed baseline evidence on the direct effects of both policy and policy outcomes. The next step is to extend my design to other policy areas where voters tend to make retrospective evaluations (e.g., school performance and crime). As noted, the Depression and the subsequent New Deal was a unique event in American history, and we may not expect voters to be responsive to *all* policy actions, particularly those that may go completely unnoticed by voters or be significant part of an incumbents re-election story. Nevertheless, extending my framework across a variety of policy areas will allow for a more complete assessment for the mechanisms behind retrospective voting, and in particular, a better understanding of how, when, and why policy actions matter to voters.

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