RADIO'S IMPACT ON PUBLIC SPENDING*

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If informed voters receive favorable policies, then the invention of a new mass medium may affect government policies since it affects who is informed and who is not. These ideas are developed in a voting model. The model forms the basis for an empirical investigation of a major New Deal relief program implemented in the middle of the expansion period of radio. The main empirical finding is that U.S. counties with many radio listeners received more relief funds. More funds were allocated to poor counties with high unemployment but, controlling for these and other variables, the effects of radio are large and highly significant.

Knowledge is power[1].

I. Introduction

Understanding why the political demands of some groups are more easily met is a key issue in political economy. This paper widens this debate to include the effects of mass media. There are many reasons why mass media influence government policy making. One reason, which is central in this paper, is that the

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[1]Francis Bacon, Sacred Meditations, [1597].

media provide the bulk of the information people use in elections.[2] Further, mass media are not neutral devices, uniformly distributing information to everyone. Rather, each of the large mass media creates its specific distribution of informed and uninformed citizens, partly because of its specific costs and revenue structure. As a result, in the wake of mass-media technology changes, there are changes in who has access to political information. If better informed voters receive favorable policies,[3] then policy may also change.

To discuss the media's effects on policy making in detail, a simple model is developed. Crucially for a group of voters to defend their political interests, they must vote and they must know whether their elected representatives have done something for them. These two aspects are not unrelated, as better informed citizens are more inclined to vote, and information from the media helps voters with both. Therefore, it is more costly for politicians to neglect voters with access to political information via the media. This line of reasoning contains three testable hypotheses: government spending should be higher on groups where many have access to the media; it should be higher on groups where more people vote; and voter turnout should be higher in groups where many have access to the media.

The next step is to find a suitable data set to test these hypotheses. The effects of the media on policy are probably most easily measured when new media technologies are introduced, since these situations create both dramatic changes in people's access to mass media and large geographical variations in the share of households with access to the new media. A few episodes in U.S. history are exceptional in this regard: the appearance of the penny press 1830-1860, the introduction of radio 1920-1940, the introduction of TV 1950-1960 and, more recently, the introduction of cable TV and the internet. The penny press probably created the most dramatic change, since it was the first real mass media, but the detailed data necessary for an empirical study is limited for this period. The introduction of radio also caused major changes in voter access to mass media, since radio was the first broadcast media with characteristics very different from print media. The introduction of TV probably had less impact, since it shared many of the existing media's characteristics, and because people were already

[2] For example, when a survey organization asked a cross section of American voters about their principal source of information in the 1940 presidential campaign, 52 percent answered "radio", and 38 percent "newspapers" (Gallup, 1940).

[3] To mention a few, this is argued by Downs [1957] Baron [1994], and Grossman and Helpman [1996].

saturated with information from other media.[4] This is even more true for media introduced after TV. For these reasons, this paper will study media effects during the expansion period of radio. More precisely, radio was introduced as a mass medium in the early 1920s, and expanded rapidly to reach a household penetration of around 80 percent by 1940.

Interestingly, this was also an era of rapid changes in economic policy making. In the middle of the expansion period of radio, the New Deal was launched. The main empirical question posed in this paper is whether radio influenced the distribution of funds in one of these early New Deal programs. The program in question provided unemployment relief and was implemented 1933-1935. It was the largest of the early New Deal programs and was administered by the Federal Emergency Relief Administration (FERA).

A cross section of approximately 2500 U.S. counties is used for the study of FERA expenditures. A different data set, a short panel of counties for the period $1920-1940$, is used to explore whether the increasing use of radio increased voter turnout. These county-level investigations are possible thanks to the 1930 and 1940 Censuses which collected county-level data on the share of households with radios (the share of households in 1920 was virtually zero).

Despite seemingly clear-cut reasons to believe that mass media would influence government policy making, media influence on government policies has been a neglected area of research. The issue appears to have fallen in the void between two academic disciplines. Economists have studied economic aspects of the media market, while political scientists have mainly been concerned with media's effect on voting behavior and public opinion.[5] The early empirical investigations of media effects on policy were mainly case studies of how the publication of particular news stories affected policy making, and were made by political scientists and journalists; see Cook et al. [1983] and Protess et al. [1985].

However, subsequent to the first version of this paper, a small economics literature on the effects of media on government policy making has emerged. Strömberg [1999, 2003] argues that increasing returns to scale in news production induces a political bias favoring large groups at the expense of minorities and small organized interests. That work, as well as the present paper, studies how the media affect the conflict between voters for government resources.

Besley and Burgess [2002] instead study if the media can discipline politicians

[4] Strömberg [2001b] indeed finds that the effects of TV are similar to those of radio found in this paper, but weaker.

[5] The classic study is Lazarsfeld, Berelson and Gaudet [1944].

to be more responsive to voters' demands. Since the media help voters infer how responsive the politicians are, politicians have electoral incentives to be more responsive in areas where many have access to the media. Using a panel of 16 Indian states, 1958-1992, they find that Indian state governments' provision of public food and calamity relief expenditure is more responsive to falls in food production and crop flood damage in states where newspaper circulation is high.

Shi and Svensson [2002] further investigate whether the access to the media moderates political business cycles. Such cycles are generated if politicians increase spending just prior to elections in programs directly affecting voters, such as cash transfers, at the expense of activities only affecting voters with a lag, such as budget deficits, see Rogoff [1990]. The media may moderate these cycles by informing voters prior to the election about, in this case, the size of budget deficits, see Strömberg [2001a] and Shi and Svensson [2002]. Using a panel of 123 developed and developing countries over a 21-year period, Shi and Svensson indeed find larger political budget cycles in countries where few people have radios.

The effects of voters having access to the media may be very different if governments limit press freedom or own the media. Brunetti and Weder [2003] and Ahrend [2002] find press freedom and corruption to be negatively correlated across countries. Further, Djankov et al. [2003] find state ownership of the media to be negatively correlated with a number of measures of good governance in a cross section of 97 countries. Naturally, press freedom and state media ownership are not exogenously given. Besley and Prat [2001] argue that a free press is more likely to emerge in places with many media outlets, where the advertising market is large, and political rents are small. However, the effect of more media competition is multi-facetted. While Besley and Prat argue that having more media outlets makes it more difficult for politicians to bribe the media, Mullainathan and Shleifer [2003] argue that greater competition could induce newspapers to write stories confirming readers' prior opinions rather than the facts.

The above literature focusses mainly on the information-providing role of the media. But the media also affect people in other ways. People learn about political norms, rules, and values from the media, and the media may even change the way people interact. For example, Putnam [2000] argues that television made leisure time more private and reduced social interactions, thereby reducing voter turnout and social capital.

This paper proceeds as follows. The next section describes the expanding use of radios in the 1930s and the FERA program. The model is developed in Section III. Section IV describes the empirical specification and presents the data. The

empirical results are then presented in Section V. Section VI concludes.

II. The FERA Program and the Expansion of Radio

The FERA program was implemented in the middle of the expansion period of radio, an ideal time for this type of study. At the beginning of the FERA program in 1933, radio was established as an important mass medium. Already in 1930, NBC Blue had started the first regular – five times a week – 15 minute hard news broadcast; an initiative soon followed by the other networks. In the 1932 presidential election, the two parties spent nearly $5 million on radio campaigns, with 25 percent going to national hookups. Radio covered politics both at the state and the federal level[6]. By 1937, 70 percent of the American public reportedly depended on the radio for their daily news[7]. Radio was also considered a credible media: 88 percent of the American public thought that radio news commentators made truthful reports[8].

Despite this, in the early 1930s, radio ownership was still very unevenly distributed across the United States. The share of households in a county with a radio receiver, r_c, ranged from 1 percent to 90 percent, with a mean of 26 percent and a standard deviation of 18 percent. This exceptional variation in radio use should make it easier to identify effects of radio use on spending, since the variation in government spending due to radio effects should also have been exceptionally large during this period.

As regards the FERA program, radio broadcasts reminded the electorate of the benefits they had received from the incumbent Governors. For example, Governor Lehman of New York states in a broadcast on the WOR network, November 3, 1934: "In 1932, I promised that the State under my administration would recognize that it was its obligation to see that no citizen should be lacking in food, shelter, or clothing. I am proud of the fulfillment of that promise during the two years of my administration. Between November 1931 and August, 1934, we expended $482 000 000 from public funds, Federal, State, and local." The address goes on to take credit for projects such as farm-to-market roads and relief to specific groups, such as home-owners and teachers. Reminding voters of past political favors was apparently regarded as important and the parties reportedly produced textbooks containing this information[9]. Radio, of course, made this process more efficient. Radio broadcasts also covered ongoing developments of the

[6] For a good discussion of the early history of radio, see Stirling and Kitross [1978].

[7] Gallup [1937].

[8] Gallup [1939].

[9] See the New York Times, November 1, 1934. "Hopkins hits back on relief politics".

programs such as the starting of new corporations to administer FERA programs, and treatment by the State Emergency Relief Administration of county project applications.

The FERA program was implemented from 1933 to 1935. It was a very large program, distributing $3.6 billion, which can be compared with total – federal, state, and local – government expenditures which were around $12 billion at the time. The program funds were widely distributed, reaching around 16 percent of all Americans – more than 20 million people – at their peak. At the county level, total FERA spending per capita, z_c, ranged from 4 cents to $226, with a mean of $20 and a standard deviation of $15.

The purpose of the Federal Emergency Relief Administration was to provide assistance to all individuals whose income was inadequate to meet their needs. Unemployment relief was not confined to the those who were completely destitute. It also included a substantial portion of professional, clerical, skilled, and semi-skilled workers. About 60 percent of the relief expenditures consisted of cash, while the rest were given in kind as coupons for food, clothing, or other goods. Some applicants were required to work in order to receive relief, and 38 percent of the total relief obligations incurred were work relief. The work relief projects included work on highways, roads and streets, public buildings, parks, sewage systems, airports, and benefited the whole community and not just the unemployed.

The FERA was not a federal program, but a state and local program where the federal government cooperated by making grants-in-aid; see Figure I for an organization chart. The FERA provided basic rules for eligibility for relief, but state and local emergency relief administrations made the final decisions on who would receive relief and how much relief was to be given. Each month, the county would estimate its total relief needs and request funds from the State Emergency Relief Administration. The state administration would then review the requests and determine the allocation to each county. The state relief administration also received applications for worthwhile projects that might be undertaken by work relief personnel. Not all relief requests were granted. In Alabama, for example, it appears that roughly 40 percent of the proposed projects were turned down[10]. Finally, the state administrations applied monthly for funds from the federal administration.

Figure I about here

[10] Alabama. Relief Administration, "Two Years of Federal Relief in Alabama", Wetumpka Printing Company, 1935.

There were some indications that the FERA funds were used for political purposes. A local relief chairman reportedly laments when the FERA is cutting back its activities, "this is likely to hinder my chances for re-election', since there would undoubtedly be a feeling of bitterness created on the part of a number of people whom he might find it necessary to refuse."[11] The model of this paper will suggest that the chairman would be more reluctant to refuse people who are likely to vote, and people who are likely to know that he was responsible for their refusal. Further, the federal administrator Harry L. Hopkins reports "we have several states where the Governor is acting chairman. I think this business of a man running for office is something else. We have a fellow managing a campaign for one man running for office."[12]

The model focuses on the Governors as the main source of political influence on the *state-to-county* allocation of emergency relief funds. This is consistent with the standard formal organizational structure of the program**,** as shown in Figure I, as well as with contemporary accounts. It is also consistent with the findings of Arnold [1979] that the executive often has an important impact on the allocation of newly started programs. It is likely that other actors such as state legislators, house representatives, and federal administrators also had some influence, but this paper treats the Governor as being the most important political actor.

The county allocation of FERA funds in the South has been studied by Fleck [1999]. More recently, Fishback, Kantor, and Wallis [2002] study the national county-level allocation of FERA spending, as well as the allocation of funds within other New Deal programs. Their study also summarizes results of other studies of New Deal programs.

III. Model

A simple model will help structure the empirical analysis. An incumbent governor in state s allocates the state relief budget, I_s, across counties in the state, subject to the budget constraint

$$\sum_c n_c z_c = I_s, \tag{1}$$

[11] Catherine Dunn, "What price poor relief?" American Public Welfare Association, Chicago, 1936.

[12] US Congress. House. Committee on Appropriations. Federal emergency relief and works program. Hearing before the subcommittee of house committee on appropriations. Seventy-third Congress, second session. H.R. 7527, 1934. p. 28

where n_c is the population and z_c is per capita relief spending in county c. Voters in county c derive utility $u_c(z_c)$ from per capita relief, where

$$u_c(z_c) = k + \frac{a_c}{1-1/\alpha}(z_c)^{1-1/\alpha}. \tag{2}$$

Parameter a_c allows relief spending to be more valuable to voters in some counties than others, while parameter α affects the concavity of the utility function. For the utility function to be concave, $a_c > 0$ and $0 < \alpha < 1$. Besides policy, each voter i also cares about other characteristics of the governor, captured by parameters β_i and η. The parameter $-\beta_i$ represents an individual's ideological preference in favor of the governor and $-\eta$ represents the governor's general popularity. The total utility of voter i, in county c, under the incumbent governor is

$$u_c(z_c) - \beta_i - \eta. \tag{3}$$

Information from radio may affect the voter's decision since only some voters know that the governor was responsible for the relief allocation. Let the variable $\xi_i = 1$ if the voter knows that the governor is responsible for the allocation z_c, and $\xi_i = 0$ otherwise. Voter i follows the simple voting rule to cast the ballot for the incumbent, if the utility under the incumbent has met some minimum standard $\overline{u}_i$:

$$\xi_i u_c(z_c) - \beta_i - \eta \geq \overline{u}_i; \tag{4}$$

and otherwise the challenger is voted for.[13]

III.A. *Allocation of Funds to Counties*

The governor tries to assess how his probability of winning the election depends on the allocation of relief expenditures. For each individual i in county c, the governor assigns a probability t_c that the voter will vote, and a probability σ_c that the voter knows that the governor is responsible for the relief program. The latter probability is increasing in the share of households in the county with a

[13]Recent studies of Congressional elections provide some empirical support for this voting rule. For example, using CPS survey data, Johannes and McAdams [1981] find that people who remember that their U.S. Representative had done something for them (specifically: brought in federal grants, projects, revenue sharing, or flood or disaster relief; had kept defense contracts, had kept jobs, or aided schools, roads or other local projects) were more likely to vote for the incumbent. Similarly, Stein and Bickers [1994] find that voters who remember that their Representative has done something for them are more likely to vote for him or her, controlling for the proportional increase in actual awards to their district. Stein and Bickers also find that people who were better informed about politics in general were more likely to be informed about new projects, controlling for actual new awards.

radio, r_c.[14] I will assume the functional form $\sigma_c(r_c) = \overline{\sigma}_c (r_c)^{c_{\sigma r}}$, where $c_{\sigma r}$ and $\overline{\sigma}_c$ are positive constants. The governor is also uncertain about the voters' exogenous preferences for the challenger $\beta_i + \overline{u}_i$; he correctly believes that these are drawn from a uniform distribution with mean φ_c and density f_c. Let v_i equal one if voter i votes for the incumbent, and zero otherwise. Conditional on η, the probability that v_i equals one is

$$\frac{1}{2} + f_c(\sigma_c u_c(z_c) - \eta - \varphi_c). \tag{5}$$

Since this is true for all voters in county c, it is also the expected share of voters that will vote for the incumbent. Note that a given swing in popularity, η, or a given change in utility from the program will produce a larger swing in the share of voters supporting the governor in counties where the marginal voter density, f_c, is high. The reason is that these counties have many swing voters, that is, voters who are not strongly ideological. This will be used when measuring the marginal voter density. Similarly, let τ_i equal one if voter i turns out to vote, and zero otherwise. Then, the probability that τ_i equals one is t_c.

The incumbent governor wins the election if he gets more than half of all votes cast:

$$\textstyle\sum_i \tau_i v_i > \frac{1}{2} \sum_i \tau_i. \tag{6}$$

This happens with approximate probability[15]

$$\begin{aligned} &P^I[z] = \Pr\left[\textstyle\sum_c n_c t_c f_c (\sigma_c u_c(z_c) - \varphi_c - \eta)\right] > 0 \\ &= H\left[\frac{1}{\sum_c n_c t_c f_c} \sum_c n_c t_c f_c (\sigma_c u_c(z_c) - \varphi_c)\right], \end{aligned} \tag{7}$$

where H is the cumulative density function of the general popularity shock.

The governor allocates relief spending to maximize his probability of being re-elected. Given that the solution is interior, the allocation satisfies the budget constraint and the first-order condition

$$t_c f_c \sigma_c u_c'(z_c) = \lambda_s, \tag{8}$$

where λ_s is a positive constant. Inserting the functional form of the utility function and taking the logarithm yields

$$\ln z_c = \alpha c_{\sigma r} \ln(r_c) + \alpha \ln \overline{\sigma}_c + \alpha \ln(t_c) + \alpha \ln(f_c) + \alpha \ln(a_c) - \alpha \ln(\lambda_s). \tag{9}$$

[14] Empirical studies have shown that political knowledge is positively related to radio use, see, for example, Delli Carpini and Keeter [1996, p.144].

[15] The approximation disregards idiosyncratic individual-level uncertainty. Given the size of the state electorates, the approximation error is extremely small.

The governor allocates more funds to counties with many votes to be gained on the margin. These are counties where many attribute an increase in relief spending to the governor (because r_c is high or $\overline{\sigma}_c$ is high), where many vote (t_c is high), where there are many swing voters (f_c is high), and where the need for relief spending is high (a_c is high).

The above equation contains two central empirical predictions. First, the coefficient α on log turnout is positive, so governors should spend more money per capita in counties where a larger share of the population votes. Second, the coefficient $\alpha c_{\sigma r}$ on log radio penetration is positive, so that governors should spend more money in areas where a large share of the population has a radio.

Note that a Benthamite social planner, maximizing the unweighted sum of utilities, would allocate funds according to equation (9) evaluated at $\sigma_c = t_c = f_c = 1$. Therefore, under the alternative hypothesis that government funds were allocated by a social planner, the allocation should *only* depend on a_c.

Equation (9) can also be derived in a model where the mechanism of radio influence is different, see Strömberg [1999, 2003]. In Strömberg [2003], radio makes voters aware of political campaign promises, and they elect on the basis of the promises made during the election campaign. The model of that paper is richer in that the media's news coverage is endogenous, as are prices for newspapers and advertisements. Simple versions of both models applied to the allocation of FERA funds are found in the Strömberg [1999] version of this paper.

IV. Specification and Data

This section discusses the link between the theory and the empirical evidence and presents the empirical variables.

IV.A. *Specification*

First, we derive an estimable form of equation (9), determining the allocation of FERA spending. Suppose that $\ln \overline{\sigma}_c$, and $\ln a_c$, are linear in the variables contained in vectors $x_{\sigma c}$ and x_{ac}, respectively (to be discussed below), so that

$$\ln \overline{\sigma} = c'_\sigma x_{\sigma c} + \varepsilon_\sigma, \tag{10}$$

and

$$\ln a_c = c'_a x_{ac} + \varepsilon_a, \tag{11}$$

where c_σ and c_a are coefficient vectors. Insert the above equations into equation (9) and re-arrange to get

$$\ln(z_c) = c_1 \ln(r_c) + c_2 \ln(t_c) + \beta_1' x_{c1} + \mu_s + \varepsilon_{c1}, \tag{12}$$

where

$$c_1 = \alpha c_{\sigma r}, \quad c_2 = \alpha, \quad \beta_1 = \begin{pmatrix} \alpha \\ c_\sigma \\ c_a \end{pmatrix}, \quad x_{c1} = \begin{pmatrix} \ln(f_c) \\ x_{\sigma c} \\ x_{ac} \end{pmatrix}. \tag{13}$$

The maintained assumption is that $\ln r_c$, $\ln t_c$, and the variables in x_{c1} are uncorrelated with the error term, ε_{c1}, so that the coefficients can be consistently estimated by OLS. The errors are assumed to be independent across states, but no restrictions are placed on the within state variance-covariance matrix.

This equation is estimated using a pure *cross section* of counties, and allowing for state-specific intercepts, μ_s. The main hypothesis is that c_1 is positive since having more households with radios has a positive effect ($c_{\sigma r}$) on the share of informed voters, and having more informed voters has a positive effect on FERA spending ($0 < \alpha < 1$). The other hypothesis is that turnout is positively correlated with spending, since $c_2 = \alpha$ is positive and smaller than one.

Regarding identification, omitted variables constitute a potential cause for concern. Counties where many people have radios have observable characteristics indicating a low need for unemployment relief (low unemployment, high wages, high bank deposits, high farm-building values), and observable characteristics indicating high political influence (high voter turnout, low illiteracy rates). If radio penetration is also correlated with unobserved determinants of need for relief spending or political influence, then the estimates will be inconsistent. The sign of the omitted variable bias is not predictable. Unobserved factors related to a low need for relief create a downward bias in the estimated effect of radios on spending**,** while unobserved factors related to political influence create an upward bias.

A way of dealing with this problem is to find a set of valid instruments, that is, variables that affect the share of households with radios in the county, but are uncorrelated with unobservable factors determining the need for relief spending or political influence.

The instruments I propose are geological features that affect the quality of radio reception. Most radios in the 1930s were AM receivers. AM waves travel both through the ground and the air, and the quality of radio reception depends on both types of transmission. The ability of AM radio waves to travel through

the ground is measured by *ground conductivity,* and the first instrument I will use is ground conductivity in the county seat. Ground conductivity is determined by the type of terrain. In the United States, it ranges between 0.5 and 30 millimhos per meter, and a higher ground conductivity indicates better AM propagation characteristics. Data was provided by the The Federal Communications Commission which uses ground conductivity to predict the propagation of AM signals across the United States. Second, radio transmission through the air is affected by physical obstacles. Therefore, I will use the share of the county's land area that was woodland in 1930 as a second instrument. The instruments are strongly correlated with the share of households with radios in the expected way, so they fulfill the first condition of being correlated with radio penetration. The second condition, that ground conductivity and the share of woodland are exogenous to relief spending, seems highly plausible. This is important since it is impossible to fully test this condition. In the results section, I show some partial tests of it, namely over-identification tests.

IV.B. *Data*

The empirical variables used to estimate equation (12) are discussed roughly in the order in which they appear in Table I. The first column contains the variables from the theoretical model. The second column contains a sign indicating whether the relation between the theoretical and the empirical variable is positive or negative. The exact definitions and sources of the empirical variables are given in Appendix 1.

Table I about here

The spending data, z_c, used in the cross-sectional analysis is cumulative FERA spending from April 1933 to December 1935, per capita. The share of informed voters, σ_c, is likely to be negatively correlated with illiteracy and positively correlated with school enrollment, in accordance with recent findings that knowledge about politics is increasing in educational attainment.[16] So illiteracy and school enrollment are the variables collected in $x_{\sigma c}$ of equation (10). The data on the shares of households with radios, illiteracy and school enrollment are all from 1930.

[16] See, for example, Delli Carpini and Keeter [1996].

Variables t_c and f_c apply to elections for the office of governor. For each state, one gubernatorial election was included in the sample. If a state held elections in 1934 (33 states) or 1935 (2 states), then this election was chosen. Otherwise, if the state had an election in 1936, this election was chosen (11 states). If no election was held 1934-1936, then the election in 1933 was chosen (1 state). Finally, one state (Georgia) did not hold gubernatorial elections during the period 1933-1937, and is excluded from the sample. Below, votes per capita will be called "voter turnout", although this term normally denotes average votes per eligible voter. As a proxy for marginal voter density in county c, f_c, I use the standard deviation of the Democrats' vote share in all gubernatorial elections 1922-1932 held in the state to which county c belongs. Presumably, counties where the Democratic vote share varies more over time contain more swing voters who are not tied down by ideology but are swayed by, for example, personal popularity to vote for one candidate or the other.

Next, I describe the large set of empirical variables proxying for parameter a_c, collected in x_{ac} of equation (11). Parameter a_c measures the value of the FERA spending for its recipients, and is proportional to the allotment allocated to the county by a social planner. I take this parameter to correspond to how the authorities claimed that they were allocating the FERA funds. The federal administration advised local relief agencies to subtract the income of an applicant from a minimum subsistence budget to compute the transfer for which each applicant was eligible.[17] The investigation of an application should include a visit to the home, inquiry as to real property, bank accounts, and other financial resources of the family, and a determination of the ability and agreements of family, relatives, friends, churches and other organizations to assist. The estimates of income were to include wages and other cash income, farm and garden produce, and all other resources.

To capture real property values, I use data on the median value of owner-occupied dwelling units and the per capita value of farm buildings. To capture bank accounts, and other financial resources, I use data on bank deposits. To capture wages and other cash income, farm and garden produce, I use the average wage in the retail sector[18] and the per capita value of all crops harvested. Since the ability of friends, family and the community to assist was taken into account,

[17] See 'Final Report On the WPA Program, 1935-43'.

[18] The simple correlation between the average wage in the retail sector and per capita personal income at the state level, where income data exists, is 0.8. The reason why the average wage in manufacturing is not used is that there are many observations missing from this series.

the error from using county-level aggregates is likely to be diminished. Not only average income, but also the distribution of income, may be important. Therefore, the share of the population that was unemployed is included. Apart from the unemployed, special groups such as 'the aged, mothers with dependent children, youths' are enumerated in the recommendation by the FERA as groups of needy persons. The share of the population aged over 65, the share of females, and the share aged below 21, are used for measuring the occurrence of these special groups. The share of African Americans and the share of immigrants may be correlated with need aspects not captured by the other variables, and these variables are also included.

Concerning the minimum subsistence levels, a study by the FERA finds that "The greatest similarity in major budget group costs, which together constitute the cost of living as a whole, was found in combined food prices, and the greatest difference, in rents".[19] The correlation between total costs of living and housing costs is 0.81. Therefore, I use the median monthly rent to capture variations in the cost of living.

A number of control variables will be included in the regression. The above set of variables is substantially larger than that used in earlier studies of the *federal-to-state* allocation of New Deal money; see Arrington [1969], Wright [1974], Anderson and Tollison [1991], and Wallis [1984, 1991, and 1998]. However, two potentially important variables are not included because county-level data has not been found: the share of federal land in the state, and the fall in income 1929-1933 (Reading, 1973). Since the federal government had no formal control over the allocation of FERA grants within the states, it is not clear that the share of federal land is important in this study. To compensate for the absence of the fall in income variable, the change in bank deposits 1930-1934 is included in the regression. Finally, it may be the case that governors spend more money in counties where they have more supporters. Including *share partisans*, the share of voters supporting the winning gubernatorial candidate, makes it possible to test whether governors were "taking care of their own" in this way; see Dixit and Londregan [1994] and Cox and McCubbins [1986].

Most of the data is from 1930, and collected in the census of that year. The exceptions are the political outcome variables, unemployment, which is from both 1930 and 1937, and bank deposits and gasoline sales which are from 1934.

Except for voter turnout and the share of households with radios, theory says

[19] Margaret Loomis Stecker, "Intercity differences in costs of living in March 1935, 59 cities". US Government Printing Office, 1937.

nothing about which functional forms should be used in estimating equation (9). The simplest linear form is chosen. To simplify the interpretation of the coefficients, all variables in x_{c1} which are not shares are in logs. Thus, all coefficients may be interpreted as the percentage response of the dependent variable to a percentage change in the independent variable.

There are a few further issues concerning the selection of the sample. As mentioned, there was no contested election in Georgia in the 1930s until 1938. Therefore, Georgia is excluded from the analysis, which leaves us with 2921 observations. Second, a number of the series contain missing values, notably gasoline spending per capita, crop value per capita, median value of owner occupied dwelling, and monthly rent. The exclusion of all observations with missing values leaves us with 2492 observations. The possible selection bias from this narrowing of the sample is discussed below. Further, in some areas, voter turnout was reportedly higher than 100 percent of the population. This was true for St. Louis, Missouri, in gubernatorial elections, and for St. Louis, Missouri; Loving, Texas; and Baltimore, Maryland, in presidential elections. These observations have been omitted, although none of the results presented change when they are included in the regressions.

Before a more structured investigation of the data, it may be helpful to look at some simple correlations. A number of key variables indicating high socioeconomic status are positively correlated: having many households with radios, a large share of literate, many employed, high average bank deposits and high voter turnout. However, these variables are correlated to relief spending in the FERA program in different ways. Whereas radio use, literacy and voter turnout are strongly positively correlated with relief spending, with correlation coefficients around 0.3, employment is negatively correlated (-0.33) with relief spending and bank deposits are only weakly positively correlated with relief spending.

V. Results

V.A. *Baseline results*

This section presents the empirical results. Table II presents the OLS estimation of equation (12) determining spending across counties. Column I contains the baseline case. In column II, a few data series with many missing values have been omitted. With this smaller set of variables, only 13 percent of the observations are

omitted due to missing values as compared to 20 percent in the specification of column I. All regressions include state-specific effects. Further, in all regressions, the variance-covariance matrix is estimated allowing for arbitrary correlations within state (clustered by state).

Table II about here

To understand the coefficient signs, it helps to first notice the general pattern. Factors indicating low socioeconomic status are *positively* correlated with spending if they indicate a need for income assistance (high level of unemployment, low bank deposits, low house values), and *negatively* related to spending if they indicate low levels of political participation and information (low voter turnout, high levels of illiteracy, low radio use).

The main theoretical predictions are that spending should be high where many households have radios and where voter turnout is high, that is,

$$c_1 > 0, c_2 \in (0, 1), \tag{14}$$

in equation (12). Turning first to the effect of radio ownership on spending, the estimate of c_1 is positive and significant at the 1 percent level. The estimated effect of turnout on spending, c_2, falls within the predicted interval and is significant at the 1 percent level. The estimated effects are economically significant. Since the variables are in logs, the coefficients measure the elasticity of relief spending with respect to the share of households with radios and with respect to voter turnout. The estimates of column I in Table II imply that an increase in the share of households with radios by 1 percentage point will increase spending by $\frac{0.14}{0.26} = 0.54$ percent, and an increase in voter turnout by one percentage point will increase spending by $\frac{0.17}{0.30} = 0.57$ percent, evaluated at the means of 0.26 and 0.3 for share of households with radios and voter turnout, respectively.

The other variables related to political knowledge are also correlated with relief spending in the expected way. Illiteracy is negatively related to FERA spending, while the school enrollment rate among people aged 7-18 is positively related to FERA spending.

As concerns the variables related to need, a_c, the most important variable explaining FERA spending is the share of the population that was unemployed in 1937. Bank deposits per capita are significantly negatively related to FERA spending, as is the value of farm buildings. Spending is also negatively related to the share of people aged above 21, and positively correlated with the share of females in the population.

As a short side-track, consider the apparent discrimination of African Americans in the FERA program. In counties with a large share of African Americans, income was lower than average, and unemployment (in 1930) was higher than average. Still, the simple correlation between the share of African Americans and relief spending is negative (-0.28). The reason is that these counties have characteristics that make them politically weak. First, the illiteracy rate among African Americans was ten times that among white, US-born, Americans: 16 percent as compared to 1.6 percent. Second, the voter turnout rate is low; and, third, few households had radios in counties with many African Americans. Interestingly, there is no remaining discrimination once illiteracy, voter turnout, and radio use have been accounted for; see Table II. This suggests that discrimination was mediated through poor education, low turnout (partly because of discretionary use of eligibility rules in the South[20]), and poor access to political news.

V.B. *Regressions on subsamples*

Next, two auxiliary hypotheses are tested on subsamples: that the political effects are larger where elections are competitive and that the effects of the radio are larger in rural ares. The results are displayed in Table II, columns III and IV.

Column III contains observations from states where the gubernatorial elections were competitive, with a winning margin of less than 30 percent. Some elections in the full sample are quite non-competitive. For instance, the Democrats dominated the political scene in the South. In most counties in Georgia and South Carolina, the Democrats got *every* vote in *all* elections 1917-1934. In non-competitive states, allocating the budget in order to win elections was probably of small importance in comparison to other aspects not treated in this paper. Therefore, the average partial effect of both the share of households with a radio and voter turnout on relief spending should be larger when non-competitive states are excluded. This turns out to be true. In fact, the estimated effects of radio penetration and voter turnout are about twice as large in the sample of competitive states as in the full sample. The hypotheses that the estimated coefficients are the same in the competitive and non-competitive states is rejected at the 1 and 5 percent level for radio and voter turnout, respectively.

Contemporary observers argued forcefully that radio improved information more for rural than urban voters. The reason was that urban voters had better access to alternative sources of information, such as newspapers, see Brunner [1935]. As it was relatively inexpensive to deliver radio waves to remote areas,

[20] See Ashenfelter and Kelley, [1975].

the informational advantage of the urban population was diminished by radio. If the theory of this paper is correct, this should imply larger effects of radio in rural counties. To test this, a separate regression was run on the subsample of counties with only rural households**,** which is displayed in Table II, column IV. The estimated effect of radio is 46 percent higher in rural areas than in the full sample. The hypothesis that the estimated effect of radio is the same in rural and non-rural counties is rejected at the 10 percent significance level. The estimates imply that radio increased the ability of rural America to attract government transfers. In quantitative terms, radio penetration going from the sample minimum of 1 percent to the sample mean of 26 percent is estimated to have increased the funds allocated to a rural county relative to an identical non-rural county by around 50 percent.[21]

V.C. *IV regressions*

To explore whether the OLS estimates suffer from an omitted variables bias, the share of households with radios is instrumented with ground conductivity and the share of woodland. This bias may arise if radio penetration is correlated with an unobserved need for relief or political influence. The regressions where radio penetration is instrumented are presented in Table III, columns I-III. For the full sample, the IV estimate of the radio's effect on spending, c_1, is larger than the OLS estimate, but insignificant. For the subsample of competitive states, the estimated effect of radio is significant at the 10 percent level, and for the subsample of rural counties, it is significant at the 5 percent level. The IV estimates of the effect of radio are larger than the OLS estimates, but only significantly so in the subsample of rural counties.

Table III about here

The instruments pass standard tests of validity. The F-test for the joint significance of the instruments in the first-stage regression is not below 20 in any of the specifications, so the instruments are not weakly correlated with radio

[21] FERA spending in the subsample of rural counties is not significantly correlated with unemployment in 1930. The reason is that the depression hit the rural areas later. Consequently, unemployment in rural areas 1933-1935 is probably not well predicted by unemployment in 1930.

penetration. The overidentifying restrictions all pass at the 10 percent level, using a J-statistic, consistent in the presence of arbitrary intra-state (intra-cluster) correlation.

The larger IV estimates of c_1 are an indication of a downward asymptotic bias in the OLS estimates. The bias could be the result of unobserved economic affluence that is positively related to radio penetration and negatively related to FERA relief. This effect seems to dominate the possible bias in the opposite direction, caused by unobserved political strength that is positively correlated with both radio penetration and FERA relief. However, the evidence of a positive bias is not overwhelming, and I will use the more conservative OLS estimates in discussing the effects of radio.

Another concern is that there may be a simultaneity problem if FERA spending increased voter turnout 1933-1936. This would violate the assumption that $\ln t_c$ is uncorrelated with ε_{c1}, and lead to inconsistent estimates. To avoid this potential problem, voter turnout 1933-1936 is instrumented by voter turnout prior to 1932. This produces small changes in the estimate of c_2; see Table III, column IV. A Hausman test rejects the hypothesis that voter turnout is endogenous to spending.

V.D. *Other robustness checks*

If radio use is statistically correlated with spending simply because it proxies for some unobservable characteristic correlated with buying new consumer goods, car ownership should be expected to behave in a similar fashion as radio ownership. Gasoline sales per capita is included in the regressions shown in Table II since it is likely to be correlated with car ownership (for which data was not collected in the 1930 Census). Gasoline sales per capita is positively correlated with the same variables as radio penetration: wages, employment, higher farm-building values, etc. Of all variables, the one showing the strongest simple correlation with gasoline sales per capita is the share of households with radios. However, gasoline sales per capita is not significantly correlated with spending.

The results are not sensitive to the effects of small counties or outliers. Limiting the sample to counties with a population over 5000, or over 10 000, reduces the sample by 3 and 16 percent, respectively. The estimated coefficient on radio, c_1, in the restricted samples is 0.12 and 0.11, respectively, and significant at the 5 percent level. Similarly, if the sample is weighted by the log county population, the estimate of c_1 is 0.13 and significant at the 5 percent level. To explore the effects of outliers, equation (12) was estimated using a procedure which puts less weight on observations with large estimated residuals (Stata command rreg).

This produced estimates of the coefficient c_1 that were smaller than the OLS estimate in the full sample (0.7 as compared to 0.14), similar to the OLS estimate in the subsample of competitive states, and larger than the OLS estimate in the subsample of rural counties (0.33 as compared to 0.20). The c_1 estimate in the full sample is significant at the 5 percent level, and the estimates in the subsamples are significant at the 1 percent level. Estimates using the Least Absolute Deviations (LAD) estimator, which puts less weight on outliers than OLS since it minimizes the absolute rather than the squared deviations, produce results similar to the rreg estimates.

V.E. *Radio's effect on voter turnout*

The above estimates of c_2 indicate that governors allocated more funds to areas with a high voter turnout. If radio ownership increases the probability of turnout, then radio can also affect the allocation of relief spending via turnout. This subsection investigates whether radio increased voter turnout.

Voter turnout is likely to be increasing in radio penetration since people who listen to the radio become better informed about politics and therefore also more likely to vote.[22] Perhaps better informed people vote more often because they feel that they are more likely to make the right choice in case their vote is pivotal; see Matsusaka [1993], and Feddersen and Pesendorfer [1997]. It could also be the case that people like to fulfill a perceived "citizen's duty" (Riker and Ordeshook, 1968) of making an informed choice in the election.

The following specification is used to test whether radio increased turnout,

$$t_{ct} = b_1 r_{ct} + X_{ct2}\beta_2 + \mu_c + \mu_t + \varepsilon_{ct2}. \tag{15}$$

This equation is estimated using a *panel* of counties 1920-1930, including county fixed effects, election year effects, and a set of exogenous variables. The coefficient b_1 measures the percentage change in votes per capita due to an increase of one percent in the share of households with radios. We are interested in testing whether this is positive, $b_1 > 0$. If the errors in the equations (12) and (15) are uncorrelated, then the recursive system may be consistently estimated using equation by equation OLS.

The dependent variable, t_{ct}, is voter turnout in gubernatorial elections. Turnout in one election around 1920, and one election around 1930 was used. The independent variables are taken from the 1920 and 1930 censuses. The closeness of

[22] For empirical evidence, see for example Palfrey and Poole [1987] and Delli Carpini and Keeter [1996]

the gubernatorial election is included, because a vote is more likely to affect the outcome, if the election is close (Riker and Ordeshook, 1968). Further, the share of households with radios is interacted with the closeness of the gubernatorial election, since radio ownership may make voter turnout more sensitive to the closeness of the election.

Controls include the share aged above 21, the share of females, the share of blacks, the share of illiterates, the share of people aged 7-18 that attend school, the share who are urban, the share of immigrants, log population, the absolute difference between the vote share of the winner in gubernatorial election and the vote share of the runner up, and the log crop value per capita. Personal characteristics such as sex, age, race and education have been found to be correlated with voter turnout; see, for example, Ashenfelter and Kelley [1975], Wolfinger and Rosenstone [1980], and Teixeira [1992]. Sex and race are also likely to be more important in the 1930s than in these more recent studies. The extension of the franchise to women was fairly recent (1920) and, at the time, African Americans were disenfranchised in the South. Immigration is included because of residence requirements for voting. The urban share of the population, population density, and unemployment, are included because they may affect the cost of voting. However, a number of institutional features which have been found to be important for voter turnout – poll taxes, literacy tests and registration laws[23] – are not included. This is because there is little time series variation in these variables. In the panel study, county dummy variables are included to pick up the effects of these variables.

The results are shown in Table IV. The estimated effect of the share of households with radios on voter turnout, b_1, is 0.12, and significant at the 1 percent level, see column I. An increase in county radio penetration of 10 percent is thus estimated to increase voter turnout by 1.2 percent.[24]

Table IV about here

[23] See Wolfinger and Rosenstone [1980].

[24] Note: the negative coefficient on the interaction term between the share of households with radios and the vote margin at the state level implies that the effect of the share of households with radios on voter turnout is smaller than b_1, when the election is not close. However, a governor who cares about winning the election only cares about how high the turnout is when the election is close, since only then can a re-allocation of relief funds affect the election outcome. The effect of radios on voter turnout in this case is b_1.

As a robustness check, I instrumented radio penetration in 1930, and the interaction term between radio and closeness of the election, using the same set of instruments for radio penetration as in Table III. The results are displayed in Table IV, column II. The IV estimate of b_1 is 0.20 and significant at the 10 percent level (p-value .06). The IV estimate of the radio coefficient is larger than the OLS, but not significantly so. The similar results from estimations with OLS and IV estimators indicate that there is no serious omitted variable bias. All time-constant unobserved heterogeneity between counties is controlled for by the fixed effects, and time-varying unobserved heterogeneity in turnout does not seem to be correlated with radio use.

We can now determine the estimated effect of radio on spending via voter turnout. An increase in the share of households with radios by one percent increases turnout by 0.12 percent. Since every percent increase in turnout increases spending by 0.57 percent, the effect on spending is 0.12*0.57 = 0.07 percent.

Before we close this section, two additional topics merit our attention. First, the negative coefficient on the interaction term between the share of households with radios and the vote margin at the state level implies that voter turnout is more sensitive to the closeness of the election in counties where many people have radios. One explanation is that people turn out to vote when they think that it is more likely that their vote will change the outcome of the election. In areas where many people have radios, a larger share of the voters would know when the election was going to be close, thus causing the interaction effect. An alternative explanation is the following. People who know the names and platforms of political candidates are more likely to vote. Close elections are followed more extensively in the media. Therefore, more people learn about names and platforms of the candidates in close elections, which makes a larger number vote. Naturally, this effect would be larger in areas where more people have radios, thereby creating the interaction effect.

Second, radio's estimated total impact on voter turnout in the United States is huge. In 1920, less than one percent of the population used radios. By 1940, around 80 percent of households had radios. The estimated average effect of the share of households with radios on voter turnout during 1920-1940 is 0.07; see Table IV, column III. (In this specification, the interaction effect between radio and the closeness of the election has been removed to measure the average effect across different levels of closeness.) The estimate suggests that the increasing use of radio led to an increase in votes per capita of around 5.5 percent. Between 1920 and 1940, votes per capita in the United States increased by about 12 percent, from

25 to 37 percent, in both gubernatorial and presidential elections. According to the estimates, the increase would only have been about half as large without radio. The estimates are based on time-series variation using year dummy variables, so they are not merely picking up the time trend in both series.[25]

VI. Conclusion and discussion

The mass media affect politics because they carry politically relevant information to the voter. This makes media users better at holding politicians accountable, and more likely to vote. For these reasons, politicians should target voters using the mass media. The empirical evidence presented in this paper suggests that such targeting did indeed take place in the United States in the 1930s; governors allocated more relief funds to areas where a larger share of the population had radios. The effects are not only statistically significant, but also economically important. The estimates of this study imply that for every percentage point increase in the share of households with radios in a certain county, the governor would increase per capita relief spending by 0.6 percent. A one standard-deviation increase in the share of households with radios would increase spending by 9 percent, and a change from the lowest to the mean share of households with radios in the sample increases spending by 60 percent.

The results are illustrated in Figure II. The total effect of an increase in the share of households with radios by one percentage point is an increase in state FERA-spending to the county by 0.61 percent. The lion's share of this total effect, 0.54 percent, is due to a *direct effect* of radio on spending. The remaining 0.07 percent is due to radio increasing voter turnout and voter turnout increasing spending. The numbers in parenthesis are standard errors[26]. The results are

[25] More generally, the media's impact on voter turnout is likely to be conditional on media ownership. Oberholzer Gee and Walfogel [2002] find that changes in the number of black-owned, black-targeted radio stations (due to local station ownership restrictions being lifted) significantly affected black voter turnout in the US 1994-1998.

[26] The standard error on the effect of voter turnout on government spending is a linear transformation of the estimated standard error of the coefficient estimate of the logarithm of voter turnout.

robust to the inclusion of a large set of control variables, estimation with instrumental variables, and specification changes.

Figure II about here

The findings do not suggest that FERA money went to rich counties, where many happened to have radios and few were illiterate. In fact, including income and wealth variables in the regression makes the estimate of the coefficient on radio penetration more significant. The reason is that radio penetration is positively related to income and wealth which are, in turn, negatively related to the need for relief funds. Excluding income and wealth from the regression introduces a downward bias in the estimate of the radio coefficient. Further, IV estimates where radio penetration is instrumented with ground conductivity and the share of woodland in the county, indicate a remaining downward bias in the OLS estimates of the effects of radio penetration on government spending in the subsample of rural counties.

The effect of illiteracy is another piece of evidence suggesting that information creates strong incentives for politicians. The governors did allocate less relief funds to areas with a large share of illiterate people. Like radio, illiteracy may hurt voters because illiterates are less likely to be informed about who is responsible for cuts in the programs from which they benefit. But illiteracy also indirectly hurts voters because illiterates vote less frequently than others. The effects of illiteracy are highly significant and considerable. For every percentage point increase in the illiteracy rate, governors cut spending by more than one percent, on average.

The above findings point to the need for an information-augmented theory of the growth of government. In Meltzer and Richard's [1981, 1983] classical theory, the enlargement of the voting franchise to the poorer segments of the population leads to an increased redistribution towards the poor.[27] The findings in this paper support the idea that groups with a high voter turnout are more successful in attracting redistributive spending. However, this paper also finds that people without a radio, and people who were illiterate, were less successful in attracting redistributive spending, over and above the effect via voter turnout. This implies that although allowing the poor the right to vote is important, it does not grant them equal political power. If politicians understand that the poor do not know

[27] For a recent test of this hypothesis, see Husted and Kenny [1997].

who is responsible for the cuts in welfare, they may cut welfare without risking votes. Given the estimated effects of radio use and illiteracy as compared to voter turnout, the expansion of the *informed* voting franchise may be as important in explaining the growth of government as the expansion of the total voting franchise.

The innovation of new mass media influences the political strength of different groups by affecting who is informed and who is not. The results of this paper indicate that radio improved the relative ability of rural America to attract government transfers, as the estimated radio effects are significantly larger in rural areas. In total, radio is estimated to have increased the funds allocated to a rural county, relative to an identical urban county, by around 50 percent. In a similar vein, preliminary results in Strömberg [2001b] also indicate that African Americans, and people with little education, gained from the introduction of TV in the 1950s. Today, the spread of the internet is likely to have a similar political impact, creating losers and gainers. An interesting topic for future study would be to apply the methodology developed in this paper to identify these groups and measure the political impact of the internet.

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APPENDIX 1: DEFINITIONS AND SOURCES OF VARIABLES

Variable name	Definition
FERA spending/capita	cumulative disbursement within the FERA program April 1933 to December 1935/population 1934.
share hhlds with radios	families reporting radio sets 1930/total number of families 1930.
share illiterate	number of persons ten years of age and over who are illiterate 1930 /population 1930.
school enrollment	number of persons 7-18 years of age attending school 1930/ number of persons of age 7-18 1930.
voter turnout	total votes cast in the gubernatorial election/((election year-1930)*population 1940 + (1940-election year)*population 1930)/10.[#]
marginal voter density	the standard deviation of the county democratic vote shares in gubernatorial elections, 1922-1932.
unempl. 1930	total number of persons out of a job, able to work, and looking for a job 1930/population 1930
unempl. 1937	number of totally unemployed persons registered 1937/population 1937
bank deposits/capita	bank deposits 1934/population 1934
%Δbank deposits/capita	(bank deposits/capita 1934 - bank deposits/capita 1930)/(bank deposits/capita 1930).
median dwell.value	median value of owner-occupied dwelling units 1930
farm value/capita	value of farm buildings 1930/population 1930
retail wage	total full-time and part-time payroll of retail establishments 1930/number of full-time employees of retail distribution stores 1930.
crop value/capita	total value of all crops harvested 1929/population 1930.
median rent	median monthly contract rent of tenant-occupied dwelling units 1930.
share 21+	number of persons 21 years of age or older 1930/population 1930.
share 65+	number of persons 65 years of age or older 1930/population 1930.
share female	number of females 1930/population 1930.
share black	number of African Americans 1930/population 1930.
share immigrants	number of foreign-born white persons 1930 / number of white persons 1930.
share partisans	share of voters who voted for the winning gubernatorial candidate[#]
share urban	total urban population 1930/population 1930.
rural dummy	takes value 1 for counties where share urban equals zero, and value 0 otherwise
gas sales/capita	sales of filling stations in 1934/population 1934.
pop. density	population per square mile 1930.
population	0.6*population 1930 + 0.4*population 1940.
ground conductivity	ground conductivity in the county seat
share woodland	number of acres of woodland 1930/approximate land area (in acres) 1930

[#]For each state, one gubernatorial election was included in the sample. If a state held elections in 1934 (33 states) or 1935 (2 states), then this election was chosen. Otherwise, if the state had an election in 1936, this election was chosen (11 states). If there was no elections 1934-1936, then the election in 1933 was chosen (1 state). Finally, one state (Georgia) did not hold gubernatorial elections during the period 1933-1937, and is excluded from the sample.
Population in non-census years are linear interpolations of the 1930 and 1940 population estimates.

Variable name	Source
FERA spending:	Work Projects Administration, Final Statistical Report of the Federal Emergency Relief Administration, Washington: US. Government Printing Office, 1942.
share households with radios:	Fifteenth Census Reports, 1930, Population, vol. VI, Families, Table 20.
voter turnout in gubernatorial elections, marginal voter density, share partisans:	Inter-university Consortium for Political and Social Research, Study no. 1, . UNITED STATES HISTORICAL ELECTION RETURNS, 1824-1968 [Computer file]. Ann Arbor, MI: Inter-university Consortium for Political and Social Research.
sales at filling stations:	Census of Business: 1935, Retail Trade Survey, US Department of Commerce, Bureau of the Census.
bank deposits:	Federal Deposit Insurance Corporation Data on Banks in the United States, 1920-1936 [Computer file]. ICPSR ed. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [producer and distributor], 196?.;
ground conductivity:	Federal Communictions Commission, Media Bureau, Audio Division, M3 Map of Effective Ground Conductivity in the USA.
remaining variables	US Census data: Historical, Demographic, Economic, and Social Data: The United States, 1790-1970 [Computer file]. Ann Arbor, MI: Inter-university Consortium for Political and Social Research.

TABLE I
SUMMARY STATISTICS

Theoretical variables		Empirical variables	Mean	St. dev.	Min.	Max.
z_c =		FERA spending/capita	19.98	15.32	0.04	225.67
σ_c =	+	share hhlds with radios	0.26	0.18	0.01	0.90
	-	share illiterate	0.04	0.04	0.00	0.44
	+	school enrollment	0.74	0.06	0.05	1.00
t_c =	+	voter turnout	0.30	0.17	0.00	0.82
f_c =	+	marginal voter density	0.09	0.06	0.00	0.31
a_c =	+	unempl. 1930	0.01	0.01	0.00	0.09
	+	unempl. 1937	0.04	0.02	0.00	0.14
	-	bank deposits/ capita	115	169	0.45	5345
	-	%Δbank deposits/capita	-0.25	0.52	-1.00	11.61
	-	median dwell.value	2582	1357	536	20000
	-	farm value/capita	189	148	0.03	849
	-	retail wage	1131	203	333	2800
	-	crop value/capita	137	117	0.01	1272
	+	median rent	1448	720	429	14000
	-	share 21+	0.55	0.06	0.36	0.83
	+	share 65+	0.04	0.02	0.00	0.09
	+	share female	0.48	0.02	0.23	0.54
	+	share black	0.11	0.18	0.00	0.86
	+	share immigrants	0.05	0.06	0.00	0.50
controls	+	share partisans	0.64	0.23	0.00	1.00
		share urban	0.21	0.26	0.00	1.00
		rural dummy	0.46	0.50	0	1
		gas sales/capita	10	8	0	122
		population	40609	138268	48	4014611
		pop. density	1846	19341	1	848778
instruments		ground conductivity	9.15	8.07	0.5	30
		share woodland	0.06	0.08	0.00	0.42

TABLE II

OLS ESTIMATES, DEPENDENT VARIABLE: LOG *FERA SPENDING/CAPITA*

A	B		I	II	III	IV
σ_c	+	**c_1: log share hhlds with radios**	**0.138**** **(2.6)**	**0.145**** **(3.0)**	**0.264**** **(3.8)**	**0.201**** **(3.6)**
	-	share illiterate	-1.111* (-2.1)	-1.216* (-2.2)	-2.133 (-1.9)	-1.577** (-2.6)
	+	school enrollment	0.856* (2.3)	0.789* (2.3)	.877 (1.6)	0.847 (1.9)
t_c	+	**c_2: log voter turnout**	**0.165**** **(2.9)**	**0.189**** **(3.3)**	**0.389**** **(2.9)**	**0.120*** **(2.4)**
f_c	+	marginal voter density	0.034 (0.1)	0.137 (0.4)	0.077 (0.1)	-0.185 (-0.4)
a_c	+	unempl. 1930	7.837** (3.9)	6.493** (3.2)	8.449** (3.7)	0.088 (0.03)
	+	unempl. 1937	9.750** (10.6)	9.153** (10.6)	10.165** (6.5)	8.248** (5.6)
	-	log bank deposits/ capita	-0.093** (-3.6)	-0.088** (-3.2)	-0.125** (-3.0)	-0.122** (-2.9)
	-	%Δbank deposits/capita	-0.013 (-0.9)		-0.116 (-1.8)	-0.009 (-0.9)
	-	log median dwell.value	-0.0004 (-0.01)		-0.043 (-0.6)	-0.002 (-0.03)
	-	log farm value/capita	-0.144* (-2.5)	-0.151** (-2.6)	-0.261** (-3.4)	-0.128 (-1.8)
	-	log retail wage	0.016 (0.2)	-.092 (-1.1)	-0.157 (-1.2)	-0.014 (-0.1)
	-	log crop value/capita	0.017 (0.5)	.008 (0.2)	.093 (1.8)	.033 (0.9)
	+	log median rent	-0.063 (-0.9)		-0.029 (-0.2)	-0.034 (-0.3)
	-	share 21+	-1.908** (-2.7)	-2.053** (-3.3)	-2.501* (-2.6)	-3.189** (-3.3)
	+	share 65+	-2.181 (-1.0)	-2.525 (-1.2)	-1.075 (-0.4)	1.936 (0.6)
	+	share female	1.923* (2.2)	1.760* (2.0)	1.016 (0.8)	0.988 (0.4)
	+	share black	0.105 (0.9)	0.156 (1.4)	-0.317 (-0.9)	.061 (0.4)
	+	share immigrants	0.319 (0.5)	0.207 (0.4)	-0.035 (-0.1)	0.066 (0.1)
con-trol	+	share partisans	0.052 (0.2)	0.085 (0.5)	0.194 (0.8)	-0.011 (-0.04)
		share urban	0.994** (6.5)	1.002** (6.9)	1.008** (4.5)	
		rural dummy	0.253** (5.8)	0.257** (5.9)	.263** (4.3)	
		log gas sales/capita	0.015 (0.6)		-0.009 (-0.3)	0.021 (0.8)
		log pop. density	-0.064* (-2.4)	-0.069* (-2.3)	-0.087* (-2.4)	-0.042 (-0.7)
		log population	-0.092* (-2.4)	-0.106** (-2.7)	-0.020 (-0.5)	-0.302 (-5.4)
		state effects	yes	yes	yes	
		R^2	0.63	0.63	0.59	0.69
		Number of observations	2492	2679	1749	984
		p-value, H_0: $c_{1\ subsample} = c_{1,\ full\ sample\ -\ subsample}$			0.00	0.08
		p-value, H_0: $c_{2\ subsample} = c_{2,\ full\ sample\ -\ subsample}$			0.04	0.67

T-statistics in parenthesis. ** Significant at 1 percent level. *Significant at 5 percent level. Variance-covariance matrix estimated allowing for arbitrary correlations within state (clustered by state). Column I contains the baseline specification, Column 2 omits a few variables to increase sample size. Column III contains the subsample of states with competitive gubernatorial elections, that is, with winning margins of less than 30 percent. Column IV contains the subsample of counties with only rural households. The last two rows report p-values of F-test of the hypotheses that the coefficients on radio and turnout are different in competitive and noncompetitive states, and in rural and non-rural counties.

TABLE III

IV-ESTIMATES, DEPENDENT VARIABLE: LOG *FERA SPENDING/CAPITA*

	I	II	III	IV
sample	full sample	competitive states	rural counties	full sample
instrumented variable	radio	radio	radio	turnout
c_1: log share hhlds with radios	0.238	0.617	0.717*	0.143*
	(1.0)	(1.9)	(2.3)	(2.7)
c_2: log voter turnout	0.162**	0.331**	0.060	0.120
	(2.9)	(2.8)	(0.8)	(1.9)
⋮	⋮	⋮	⋮	⋮
state effects	yes	yes	yes	yes
R^2	0.63	0.58	0.66	0.63
Number of observations	2490	1748	981	2470
F-stat, instruments, 1st stage	36	22	22	148
Over-id restrictions, χ^2_{df} (p-value)	$2.42_1(0.12)$	$1.4_1(.23)$	$0.43_1(.51)$	$1.71_3(0.64)$
Hausman test for endogeneity, p-value	0.67	0.26	0.07	0.35

Voter turnout is instrumented by turnout and log turnout in the last gubernatorial election in each state, prior to 1933, and turnout in the 1928 presidential election. Radio is instrumented by ground conductivity in the county seat, and the land area share that is woodland. All independent variables in Table 2, column I, are included but not displayed in the above regressions. T-statistics in parenthesis. ** Significant at 1 percent level. * Significant at 5 percent level. Variance-covariance matrix estimated allowing for arbitrary correlations within state (clustered by state).

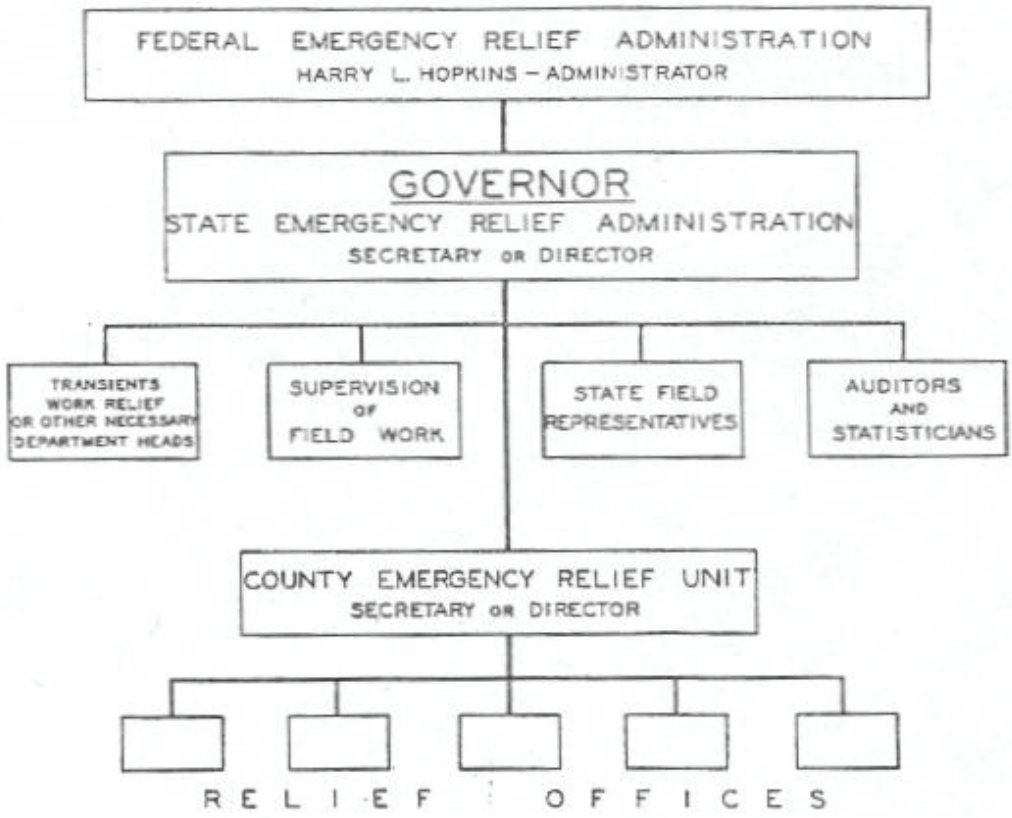
TABLE IV

ESTIMATED EFFECTS ON TURNOUT, DEPENDENT VARIABLE: *VOTES PER CAPITA*.

	I	II	III
	OLS	IV	OLS
	1920-1930	1920-1930	1920-1940
share hhlds with radios (b_1)	0.117**	0.200	0.073*
	(3.0)	(2.0)	(2.1)
radio*vote margin at state level	-0.932**	-1.329**	
	(-3.5)	(-4.0)	
vote margin at state level	-0.043	0.027	-0.150*
	(-0.7)	(0.4)	(2.3)
controls	yes	yes	yes
county fixed effects	yes	yes	yes
election year effects	yes	yes	yes
R^2	0.48	0.45	0.54
Number of observations	5740	5726	9007
Over-id restrictions, χ^2_{df} (p-value)		1.14_1(.29)	
Hausman test for endogeneity, p-value		0.22	

In column II, radio is instrumented by ground conductivity in the county seat, and the land area share that is woodland. Controls in columns I and II include share illiterate, share aged 7-18 attending school, share 21+, share female, share urban, share black, share immigrants, log population. The regression in column III includes the same controls, excluding the share illiterate due to lack of data for 1940. T-statistics are in parentheses. **Denotes significance at 1 percent level. *Denotes significance at 5 percent level. Variance-covariance matrix estimated allowing for arbitrary correlations within state (clustered by state).

Figure 2.1 FERA Organization Chart
Source: United States Congress, Hearing before the Subcommittee of Appropriations, HR 7527, US Government Printing Office, 1934



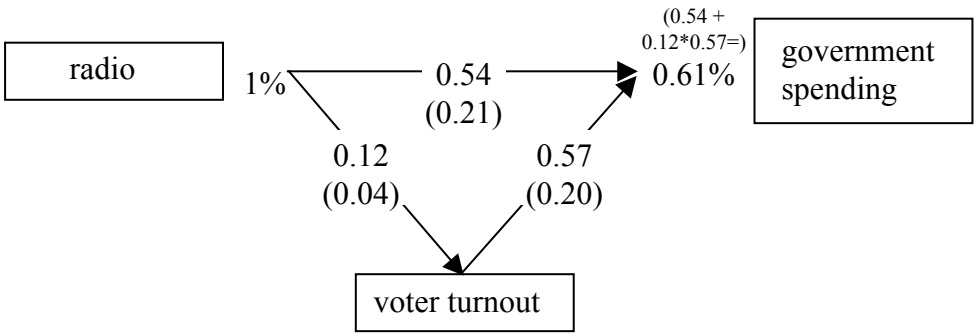


FIGURE II
Total estimated effect of radio on FERA spending