Radio's Impact on Public Spending

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Abstract

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 the radio. The main empirical finding is that US 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 the radio are large and highly significant. If other government funds were distributed in a similar fashion, then the introduction of the radio led to a major shift in government policies.

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Knowledge is power.[1]

1. Introduction and summary

The effects of mass media on policy is a neglected area of research. While economists have largely ignored mass media's role in politics, political scientists have mainly been concerned with its effect on voting behavior and public opinion.[2] The few earlier empirical investigations of media effects on policy are mainly case studies, conducted by political scientists and journalists, of how the publication of particular news stories affected policy making; see Cook et al. (1983) and Protess et al. (1985). In contrast, this paper studies how the access to a mass medium, the radio, affected peoples ability to attract government funds. An advantage of this approach is that the increasing use of radio was the result of technical innovation and exogenous to the political process.

If more informed voters receive favorable policies,[3] then mass media will affect policy since they provide the bulk of the information people use in elections.[4] 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, the characteristics of those informed change in the wake of mass-media technology changes.

To discuss in detail how media affects policy, a simple model is developed in Section 2. In this model, individuals who receive political information from radio vote both more frequently, and more accurately for politicians who have treated them well in the past. This creates incentives for politicians seeking re-election to provide favorable policies these voters. The section concludes by deriving three hypotheses, illustrated in Figure 1.1. The first is the hypothesis of a *direct effect*: politicians should allocate more government funds to areas where a larger share of the households have radios, everything else equal. The remaining two hypotheses

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

[2] The classic study is Lazarsfeld, Berelson and Gaudet (1944).

[3] To mention a few, Downs (1957), Baron (1994), and Grossman and Helpman (1996), argue this.

[4] 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).

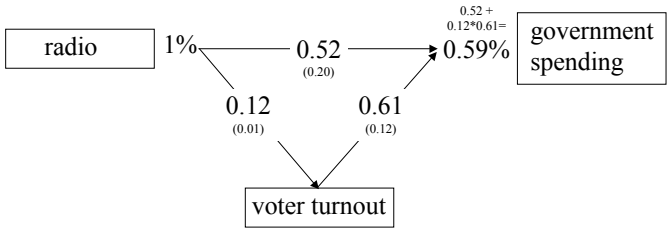

Figure 1.1:

are the building blocks of the *voter turnout effect*: politicians should allocate more funds to areas with higher voter turnout, and voter turnout should be higher where a larger share of the households have radios.

In sections 3 and 4, the hypotheses are tested on the allocation of funds in a main New Deal program – the Federal Emergency Relief Act (FERA). The FERA was a large, new program implemented during a period of rapidly increasing radio use (1933-1935). A cross section of county-level data comprising approximately 3 000 observations is used. The hypothesis that radio use increased voter turnout is tested in a short panel consisting of county level data for the period $1920-1940$. A county-level investigation of all three hypotheses is possible since the 1930 and 1940 Censuses collected county-level data on the share of households with radios.

The empirical results support both a *direct* and a *voter turnout effect*. Figure 1.1 summarizes the main findings. 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 0.59 percent. Of this total effect, 0.52 percent is due to the *direct effect* and the remaining 0.07 percent to the *voter turnout effect*. The numbers in parenthesis are standard errors[5]. The *direct effect* is substantially larger but the two parts of the *voter turnout effect* are more precisely estimated. The effects are not only highly significant statistically, but also economically important. The estimates imply that a one standard-deviation increase in the share of households with radios caused governors to increase spending to the county by 10 percent, on average. The results are robust to the inclusion of a large set of control variables, estimation with instrumental variables, and to specification changes.

[5]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.

Another interesting finding is that less funds were allocated to counties with a large number of illiterates. For every percentage point increase in the illiteracy rate, spending appears to have been cut by on average 2 percent. This finding is highly statistically significant, and also supports the notion that better informed voters receive favorable policies.

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 more significant. The reason is that radio 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.

Section 5 studies the introduction of TV 1950-1960, and finds similar, but weaker, results. Finally, section 6 discusses the results and concludes.

2. Model

How did radio affect FERA spending? Judging from radio addresses at the time, the governors focussed on informing voters that they were responsible, and should be given credit, for the relief funds.[6] Informing voters was probably viewed as a way increase the electoral impact of the relief funds. Considering this, the governors doubtlessly also realized that they could increase this impact by spending more relief funds in areas where voters were likely to be well informed, for example where many voters had access to radio and few were illiterate. These increases could safely be balanced by cuts in areas where voters were less likely to know that the governor was responsible for the cuts.

A simple formalization of this idea will help to structure the empirical analysis. An incumbent governor in state s decides how the state relief budget, I_s, will be allocated across individuals in the state.[7] Each voter i derives utility $u_i(z_i)$ from per capita relief, where

$$u_i(z_i) = k - \frac{a_i}{\frac{1}{\alpha} - 1}(z_i)^{-\frac{1}{\alpha}+1}.$$

The parameter a_i captures the sensitivity of individual i, and the parameter α

[6] See section 3.

[7] Arguably, the governors had a decisive role in determining the within-state allocation of relief funds; see section 3.

captures a general sensitivity to spending within the program. For the utility function to be concave, $a_i > 0$ and $0 < \alpha < 1$. Apart from policy, voters also care about other characteristics of the governor, captured by parameters $\beta_i + \eta$. 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 under the incumbent governor is

$$u_i(z_i) - \beta_i - \eta.$$

Information from radio may affect the voters 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_i and $\xi_i = 0$ otherwise. Voter i follows the simple voting rule to cast his ballot for the incumbent if his utility under incumbent has met some minimum standard $\overline{u}_i$:

$$\xi_i u_i(z_i) - \beta_i - \eta \geq \overline{u}_i;$$

otherwise he votes for the challenger.[8]

The governor tries to assess how his probability of winning the election depends on the allocation of relief expenditures. For each individual i, the governor assigns a probability t_i that the voter will vote, and a probability σ_i that the voter knows that the governor is responsible for this spending program. For simplicity, the voter-turnout probabilities are treated as exogenous. Problems arising from possible endogeneity of voter turnout are discussed in the empirical section. The governor is also uncertain about the voter's exogenous preference for the opponent $\beta_i + \overline{u}_i$; he believes that it is uniformly distributed with mean φ_i and density f_i. Let τ_i equal one if voter i turns out to vote, and zero otherwise. Similarly let υ_i equal one if voter i votes for the incumbent and zero otherwise. The incumbent governor wins the election if he gets more than half of all votes cast:

$$\sum_i \tau_i \upsilon_i > \frac{1}{2} \sum_i \tau_i.$$

[8]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.

This happens with approximate probability[9]

$$\begin{aligned} P^I[z] &= \Pr\left[\sum_i t_i\left(-\varphi_i f_i + f_i(\sigma_i u_i(z_i) - \eta)\right) > 0\right] \\ &= H\left[\frac{1}{\sum_i t_i}\sum_i t_i\left(-\varphi_i f_i + f_i\sigma_i u_i(z_i)\right)\right], \end{aligned}$$

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

$$t_i f_i \sigma_i u_i(z_i) = \lambda_s, \tag{2.1}$$

where λ_s is a positive constant.

Since the empirical data is at the county level, the individual relief allocations, z_i, will be aggregated to that level. Let subscript c and s denote county and state averages. Aggregating to the county level and taking the logarithm yields

$$\begin{aligned} \ln z_c &= \alpha\ln(\sigma_c) + \alpha\ln(t_c) + \alpha\ln(f_c) + \alpha\ln(a_c) \\ &\quad + \ln(z_s) - \ln\left(\sum(\sigma_s t_s f_s a_s)\,\alpha\right) + \ln(\rho_c). \end{aligned} \tag{2.2}$$

The governor allocates more funds to counties where there are many votes to be gained on the margin. These are counties where many attribute an increase in relief spending to the governor (σ_c is high), where many vote (t_c is high), where there are many swing voters who are neither strong supporters of the incumbent governor nor of the challenger (f_c is high), and where the need for relief spending is high (a_c is high).

The term ρ_c contains the error that arise from using county-level data, when the governor had access to individual-level data. It increases with the within-county correlation between being informed, turning out to vote, having a high valuation of relief funds and a high probability density.[10]

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

[10] Formally,

$$\rho_c = \frac{1}{n_c}\sum_{i\in c}\left(\frac{\sigma_i}{\sigma_c}\frac{t_i}{t_c}\frac{a_i}{a_c}\frac{f_i}{f_c}\right)^{\alpha}.$$

The above equation contains two central empirical predictions. First, the coefficient α on the voter turnout variable is positive. This is a more precise formulation of the hypothesis that politicians should spend more money per capita in counties where a larger share of the population votes. Second, σ_c is assumed to be increasing in r_c, since empirical studies have shown that political knowledge is positively related to radio use[11]. Therefore the share of households with radios, r_c, has a positive effect on relief spending which is independent of the effect via voter turnout. This is a more precise formulation of hypothesis that politicians 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 (2.2) 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 (2.2) may also be derived in a model of electoral competition; see Strömberg (1998). In this model, two competing gubernatorial candidates make campaign promises to the electorate, but only a share σ_c of the voters in county c become aware of these promises. This induces the gubernatorial candidates to promise more favorable policies to counties where σ_c is high.

Voter turnout Voter turnout in county c, t_c, is assumed to be a function

$$t_c = b_1 r_c + X_{c2}\beta_2 + \varepsilon_{c2}, \tag{2.3}$$

where r_c is the share of households in the county with radios, and X_{c2} contains other variables related to the costs and benefits of voting which will be specified in the empirical section. The hypothesis of a positive effect of radio on turnout is formally that the coefficient b_1 in the above equation is positive.

Voter turnout is likely to be increasing in the share of households with radios since people who listen to radio are better informed about politics, and since people who are better informed about politics vote more often.[12] Perhaps better informed people vote more often because they feel that they are more likely to makes the right choice in case their vote is pivotal; see Matsusaka (1993), and Feddersen and Pesendorfer (1997). It could also be that people like to fulfill a

[11] See example Delli Carpini and Keeter (1996, p.144)

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

perceived citizen duty (Riker and Ordeshook, 1968) of making an informed choice in the election.

3. Data

This section discusses the link between the theory and the empirics and presents the empirical variables to be used.

The empirical variables used to estimate equation (2.2) are discussed roughly in the order they appear in Table 1. 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. Subscript s denotes state-level aggregation, variables without subscripts are measured at the county level. The exact definitions of the empirical variables are given in Appendix 1.

The hypotheses of media effects on policy were tested on the FERA program. This was a large, new program that was implemented during a period of rapidly increasing radio use. If the radio increased the political strength of certain groups or regions, then one should expect a new, major program to target these groups, to some extent. The FERA program was implemented from 1933 to 1935. It distributed $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, at their peak reaching around 16 percent of all Americans – more than 20 million people. 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 persons whose income was inadequate to meet their needs. The unemployment relief was not confined to the utterly destitute. It also included a substantial portion of professional, clerical, skilled, and semiskilled workers. About 60% of the relief expenditures consisted of cash while the rest was given in kind as coupons for food, clothing, or other goods. Some applicants were required to work in order to receive relief. Of the total relief obligations incurred, 38% was work relief. The work relief projects included work on highways, roads and streets, public buildings, parks, sewer 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 in which the federal government cooperated by making grants-in-aid; see Figure 2.1 for an

organization chart. The FERA provided basic rules concerning 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. Within each county, unemployed people applied for relief. The local administration then determined whether the applicant lacked sufficient resources to cover a minimum standard level of living. 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. Far from all relief requests were granted. In Alabama, for example, it appears that roughly 40 percent of the proposed projects were turned down.[13] Finally, the state administrations applied monthly for funds from the Federal Emergency Relief Administration.

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."[14] The model of this paper suggests 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. Similarly, the federal administrator, Harry L. Hopkins reports regarding the state and local allocation, "Political interference has been a difficulty. I would not say it is serious, but it has been a difficulty. It stops us. It has nothing to do with party. It works on both sides of the fence... If you were to investigate them adequately, it would be a terrific job to prove political interference. How do you do it, anyway? It is not so easy."[15] Further "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."

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

[13] Alabama. Relief Administration, "Two Years of Federal Relief in Alabama", Wetumpka printing company, 1935.

[14] (Ref. What price poor relief? Catherine Dunn, American Public Welfare Association, Chicago, 1936.)

[15] 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

with the standard formal organizational structure of the program as shown in Figure 2.1, 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 FERA program was implemented in the middle of radio's expansion period, 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 minutes hard news broadcasting; 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[16]. By 1937, 70 percent of the American public reportedly depended on the radio for their daily news[17]. Radio was also considered a credible media: 88 percent of the American public thought that radio news commentators truthfully reported the news[18].

Still, in the early 1930s, radio ownership was very unevenly distributed across the United States. Receivers were concentrated in the North East, the Mid-Western cities, and in the Far West. The share of the households in the 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.

In regard to the specific program under study, 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."

[16] For a good discussion of the early history of radio, see Stirling and Kitross (1978).
[17] Gallup (1937).
[18] Gallup (1939)

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.[19] Radio of course made this process more efficient. Radio broadcasts also covered ongoing developments of the programs such as the starting of new corporations to administer FERA programs, and treatment by the State Emergency Relief Administration of county project applications.

As mentioned, the share of informed voters, σ_c, is assumed to be positively correlated with radio use. It is also assumed to be negatively correlated with illiteracy and positively correlated with school enrollment, consistent with recent findings that knowledge about politics is increasing in educational attainment.[20]

The variables t_c and f_c should apply to the gubernatorial elections. The marginal voter density, f_c, has been estimated using data on the variation and mean of county election outcomes 1917-1933.[21] The measure is similar in spirit to the political productivity index of Wright (1974). Another political productivity variable is *share partisans*. It may be the case that a governor knows better who is in need of support among his own supporters, and therefore can allocate money efficiently to this group. Dixit and Londregan (1994) show that the model of this paper could easily be extended to include this feature (see also Cox and McCubbins, 1986).[22] Including *share partisans*, the share of the voters supporting the winning gubernatorial candidate, makes it possible to test whether governors were "taking care of their own" in this way.

The situation in the gubernatorial elections varied greatly. In the South, the Democrats dominated the political scene. In most counties in Georgia and South Carolina, the democrats got *every* vote in *all* elections 1917-1934. In Georgia, Mississippi, and South Carolina, only a few percent of the population voted. In these states, allocating the budget in order to win elections was probably of small

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

[20] See for example Delli Carpini and Keeter (1996).

[21] Election outcomes between 0 and 1 have been mapped to preference shocks between plus and minus infinity using a inverted standard normal distribution. Then the mean and variance of these preference shocks have been calculated and using these the marginal density has been calculated. This corresponds to a maxmimum likelihood estimation of these distribution parameters under some additional assumptions. For further details, please contact the author.

[22] If voter turnout is increasing in relief spending, then an incumbent governor may also wish to spend more in counties where he has larger support to increase turnout in those counties. The model of this paper can be extended to include this feature.

importance in comparison to other aspects not treated in this paper. Therefore, results for a subsample excluding states with winning margins greater than 30 percent will be reported separately. In this sample, votes per capita at the state level was typically around 30-40 percent with a maximum of 50 percent in Illinois. Below, votes per capita will be called "voter turnout" although this term normally denotes average votes per eligible voter.

Clearly, a_c is the theoretical variable which is most loosely tied to any specific empirical variable. The parameter a_c measures the value of the FERA spending to its recipients, and is proportional to the allotment allocated by a social planner to the county. 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 a applicant from a minimum subsistence budget to compute the transfer to which each applicant was eligible.[23] 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 on 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[24] and the per capita value of all crops harvested. Since the ability of friends, family and the community to assist was taken into account, the error by 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 in 1930 and in 1937 are included. Apart from the unemployed, special groups such as 'the aged, mothers with dependent children, youths' are enumerated in the recommendation by 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

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

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

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".[25] 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.

Finally, 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). Still, 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). However, 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 the absence of the fall of income variable, the change in bank deposits, 1930-1934 is included in the regression.

The next step is to specify what variables to include in the regression on voter turnout, equation (2.3). The closeness of the gubernatorial election is included, because it may be related to the benefits of voting (Riker and Ordeshook, 1968). In addition, all of the variables that affect relief spending are included because they may also affect voter turnout. Personal characteristics such as sex, age, race, education and income may influence the costs of voting; see for example Ashenfelter and Kelly (1976), 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 African Americans were at the time 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[26] – are not included. The reason is that there is little time series variation in these variables. In the panel study, county dummy

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

[26] See Wolfinger and Rosenstone (1980).

variables are included to pick up the effects from these variables.

Except for voter turnout in equation (2.2), theory says nothing about which functional forms should be used. The simplest linear form is chosen. To simplify the interpretation of the coefficients, all variables which are not shares are in logs. Thus, one may interpret all coefficients as the percentage response of the dependent variable to a percentage change in the independent variable.

A few further decisions concerning the appropriate sample to test this hypothesis are required. First, there was no contested election in South Carolina in the 1930s until 1938. Therefore South Carolina is excluded from the analysis, which leaves us with 2921 observations. Second, a number of the series contain missing values, notably gas 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 2495 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. A plot suggested that these observations are outliers and they have been omitted. None of the results presented change when these outliers are included in the regressions.

Before a more structured investigation of the data, it may be helpful to look at some simple relations. A number of key variables in this study are positively correlated: having many households with radios, a large share literate, many employed, high average bank deposits and high voter turnout. For example, the share of households with radios has a simple correlation with employment of 0.17, a simple correlation with bank deposits of 0.62, and a simple correlation with voter turnout of 0.64. However, these variables are correlated in different ways to relief spending in the FERA program. Whereas radio use, literacy and voter turnout are strongly positively correlated with relief spending, with correlation coefficients around 0.3, employment is negatively correlated (-0.37) with relief spending and bank deposits is only weakly positively correlated with relief spending.

Data on FERA spending was collected from the final statistical report of this program.[27] Radio data collected from the 1930 Census of the Population.[28] Data

[27] *Work Projects Administration, Final Statistical Report of the Federal Emergency Relief Administration*, Washington: US. Government Printing Office, 1942.

[28] Fifteenth Census Reports, 1930, Population, vol. VI, Families, Table 20.

on voter turnout in gubernatorial[29] and presidential[30] elections was collected from the ICPSR archives. Data on sales at filling stations was collected from the Census of Business,[31] and data on bank deposits was collected from the Federal Deposit Insurance Corporation.[32] The remaining variables are based on US Census data contained in the ICPSR archives.[33]

4. Results

This section presents the empirical results. It also discusses in detail the structure of the econometric problem and the assumptions behind it, the estimation, and potential econometric difficulties and some measures to avoid these.

Substituting the empirical variables from Table 1 into equations (2.2) and (2.3) yields:

$$\ln(z_c) = c_1 r_c + c_2 \ln\left(\frac{t_c}{t_s}\right) + X_{c1}\beta_1 + \varepsilon_{c1}, \tag{4.1}$$

$$t_c = b_1 r_c + X_{c2}\beta_2 + \varepsilon_{c2}. \tag{4.2}$$

Matrices X_1, and X_2 contain the exogenous variables discussed above. The structure of the equations implicitly assumes that voter turnout in 1933-36 is not directly affected by spending within the program. If the errors in the above equations are uncorrelated, then the recursive system may be consistently estimated using equation by equation OLS.

4.1. Spending

Let us first turn to the estimation of equation (4.1), determining FERA spending. The main theoretical predictions are that spending should be high where many

[29] Source: *United States Historical Election Returns, 1824-1968*, ICPSR #1.

[30] Clubb, Jerome M., William H. Flanigan, and Nancy H. Zingale. *Electoral Data for Counties in the United States: Presidential and Congressional Races, 1840-1972* [Computer file]. Compiled by Jerome M. Clubb, University of Michigan, William H. Flanigan, University of Minnesota, and Nancy H. Zingale, College of St. Thomas. ICPSR ed. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [producer and distributor], 1986.

[31] Census of Business: 1935, Retail Trade Survey, US Department of Commerce, Bureau of the Census.

[32] *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?.;

[33] *Historical, Demographic, Economic, and Social Data: The United States, 1790-1970* [Computer file]. Ann Arbor, MI: Inter-university Consortium for Political and Social Research.

households have radios and where voter turnout is high:

$$c_1 > 0, c_2 \in (0,1)\,.$$

The coefficient c_1 is approximately the percentage increase in per capita spending due to a one-percent increase in the share of households with radios.[34] The coefficient c_2 corresponds to parameter α in the utility function, which is restricted to lie in the open interval between 0 and 1.

A number of specifications of equation (4.1) were tested. Table 2 presents the basic results. Columns III and IV presents results only from states where the winning margin was less than 30 percent. This excludes the Southern states and Washington. As discussed, the reason for this exclusion is that it seems unlikely that the FERA money was allocated to influence the election outcomes in states that were completely dominated by one party. The specifications in columns I and III do not allow for state-specific effects while the others do. The rows containing the estimates of c_1 and c_2 are in boldface. Columns A and B contain the theory variables and the expected signs of the coefficients respectively.

The estimate of c_1 is positive and significant at the 1 percent level, except when allowing for state-specific effects and including elections with winning margins greater than 30 percent. In this specification c_1 is significant at the 5 percent level. The estimate of c_2 fall within the predicted interval and is significant at the 1 percent level in all specifications. The measured effects are also economically significant. The estimates of column II in Table 2 imply that an increase in the share of households with radios by 1 percent will increase spending by 0.52 percent, and an increase in voter turnout by one percent will increase spending by $\frac{0.18}{0.30} = 0.61$ percent.[35] Note that the estimated values of c_1 and c_2 are larger when states with winning margins greater than 30 percent are excluded from the sample. This indicates that allocating funds to win the election was less important in states dominated by one party.

A first concern is that the correlation between the share of households with a radio and relief spending may arise simply because counties where many have radios are different from other counties. Counties where many have radios have

[34] Although there is no formal limit to the size of c_1, it is reasonable to expect that it should be lower than 3.4. To see why, consider the extreme case where those and only those with radios receive money from the program. An increase from the average of 29 percent to 30 percent of the households having radios implies that spending increases proportionally, that is, an increase by $1/.29 = 3.4$ percent.

[35] The effect is evaluated at the mean of 0.3 of voter turnout.

lower unemployment, higher wages, higher farm-building values, and fewer urban households. These correlations may produce a downward bias in c_1 if unemployment, wages, etc. are measured with error. There could, of course, also be other omitted variables leading to a positive bias.

If radio use is statistically correlated with spending simply because it proxies for some unobservable characteristic correlated with buying new consumer goods, then one should expect car ownership to behave in a similar fashion as radio ownership. Gas sales per capita is included in the regression since it is likely to be correlated with car ownership (of which data was not collected in the 1930 Census). Gas sales per capita is positively correlated with the same variables as radio: wages, employment, higher farm-building values, etc. Of all variables, the one that shows the strongest simple correlation with gas sales per capita is the share of households with radios. However, gas sales per capita is not significantly correlated with spending in any regression, see Table 2.

To further test whether the OLS-estimates of c_1 are biased, the share of households with radios is instrumented using two instrumental variables. The first instrument is ground conductivity, a geological feature which the Federal Communications Commission uses to predict the propagation of AM-signals across the United States. Since most radios in the 1930s where AM-receivers, this is a most important statistic for radio reception. It also seems, on a priori grounds, unlikely that ground conductivity is correlated with FERA spending, except via radio use. A second instrument intends to measure the local supply of radio broadcasts. It is the sum of the power of all AM-antennas in 1934, weighted by the inverse square root of the distance between the county seat and the antenna. Two sets of instruments are used: IV1 containing only ground conductivity, and IV2 containing both instruments. The instruments are strongly correlated with the share of households with radios in the expected way. The F-statistic on a test for joint significance of the instruments in a regression of the share of households with radio on the instruments and all other exogenous variables are 88 for IV1 and 52 for IV2.

The instrumental-variables regressions are presented in Table 3, columns I and II. The IV-estimates of c_1 are larger than the OLS-estimates; the p-value on a test for no significant difference between OLS and IV-estimates are 0.03 and 0.06 respectively, indicating that the OLS-estimate may be biased downward. A partial test of the exogeneity of the instrumental variables (described in Hausman, 1983) was performed for IV2, see column II. The test indicates whether the variation in the instruments that is not correlated with radio use is correlated with relief

spending. It does not reject the hypothesis that the instruments are exogenous in the regression. When state effects are included (not shown), the standard errors become so large that the IV-estimates are neither significantly different from the OLS-estimates, nor significantly different from zero.

A second concern is that there may be a *simultaneity* problem if FERA spending increased voter turnout 1933-36. This would cause voter turnout to be positively correlated with ε_{c1} and the coefficient estimate of c_2 to be positively biased. To avoid this potential bias, voter turnout 1933-36 is instrumented by voter turnout prior to 1932. This produces small changes; see Table 3, column III.

Another way to deal with the voter-turnout simultaneity-problem is to study a reduced-form equation where voter turnout is not included. Make a first-order Taylor-expansion of log turnout in equation (4.1). Then substitute out voter turnout using equation (4.2). The result is an equation of the form

$$\ln(z_c) = d_{0s} + d_1 r_c + X_c\beta + \varepsilon_{c3}.$$

This formulation avoids the simultaneity problem since both ε_{c1} and ε_{c2} end up in ε_{c3}. An estimation of the above equation also provides a measure of the total effect of radios, d_1. This total effect is the sum of the *direct* and *voter turnout effects.* The result, shown in Table 4, column I, is consistent with the earlier results.

In the above specifications, around 16 percent of the observations are not included due to missing values. In case the omitted observations are not representative, this may create a *sample selection bias.* In column II, a few data series with many missing values have been omitted. With this smaller set of variables, only 8 percent of the observations are omitted due to missing values.

The last two columns of Table 4 show that the effect of radios was significantly larger in rural than in urban counties. This result will be discussed below.

We now return to Table 2, and discuss the effects of the other variables. Of the other variables related to political knowledge, illiteracy is always significantly negatively related to FERA-spending. The school enrollment rate among people aged 7-18 is always positively, and sometimes significantly, related to FERA-spending. The less convincing result for the school enrollment rate variable may be due to the fact that it does not measure the stock of knowledge very well and due to the high correlation between schooling and illiteracy.

The marginal voter density is only significant when state effects are not allowed for. Perhaps this reflects that this variable is not measured correctly. The marginal voter density is not observable. In order to estimate it, strong assumptions

on the distribution of preferences are necessary. The share of voters supporting the winning gubernatorial candidate (partisans) also significantly and positively affects spending to the county in specifications that do not allow for state effects.

Of the variables related to need, a_c, the most important variable explaining FERA-spending is the share of the population that was unemployed. Bank deposits per capita is consistently significantly negatively related to FERA-spending, as is the value of farm buildings. The change in bank deposits is negatively related to FERA spending when the Southern states and Washington are excluded. Unexpectedly, crop value per capita is significantly positively related to FERA-spending in one specification and the share over 65 is sometimes significantly negatively related to FERA-spending.

Although important, the data strongly rejects that *only* variables related to need mattered. The alternative hypothesis that a social planner without political motives allocated the FERA funds is thus rejected.

4.2. Voter turnout

Let us turn now to the estimation of equation (4.2), determining voter turnout. Theory predicts that $b_1 > 0$, and it is reasonable to expect that the coefficient is smaller than 1. 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.

The results from a cross-section using the same data set as was previously used to estimate spending are shown in Table 5. The estimates of b_1 fall within the predicted interval and are significant. The estimates imply that an increase in the share of households with radios of one percent will increase voter turnout by $0.07 - 0.09$ percent.

In this estimation, there may be an important *omitted variable* bias. People in counties where many are interested in politics may be both more likely to have a radio and more likely to vote. To be able to control for this and other county-specific effects, a panel data set was constructed. This panel data set contains most of the important explanatory variables at the county level in 1920, 1930, and 1940, and voter turnout in gubernatorial elections around 1920, 1930, and 1940. The share of households with radios is also instrumented using the same set of instruments as before. Since it is unlikely, on a priori grounds, that ground conductivity and interest in politics are correlated, this estimation procedure may avoid any remaining omitted variable bias.

The results are shown in Table 6. Looking at changes between 1920 and

1930, the fixed effects estimate of b_1 is 0.12, and highly significant, see column I. In columns II and III, the share of households with radios in 1930 is instrumented using the sets of instruments, IV1 and IV2. Instrumenting with ground conductivity only, IV1, does not change the estimate of b_1 significantly. When instrumenting with IV2, the estimate of b_1 is significantly larger than under OLS. However, the test-statistic for exogeneity of the instruments indicate that the instruments in IV2 may not be exogenous in this regression. In the IV-regression, an interaction term between radio and the margin of victory in the presidential election is included. If it was not, the instruments in IV2 would resoundingly fail the exogeneity test. A regression without fixed effects, but including state effects, is shown in column IV. The estimates of b_1 are comparable to the fixed effects estimates.

The similar results from estimations with and without fixed county effects, and with OLS and IV-estimators, indicate that the omitted variable bias might not be a serious problem. In a sense, this is not be surprising. Most of the radio programming time was devoted to entertainment, and people most likely bought radios for entertainment and perhaps practical information such as weather news for farmers. Interest in politics was probably a minor determinant of radio ownership.

Note that while the share of households with radios is positively correlated with voter turnout 1930-1940, the effect is slightly smaller than in 1920-1930; see columns VI and VII.

The most relevant statistic to a governor allocating FERA spending the early 1930s are the estimates of b_1 based on changes from 1920 to 1930, reported in Table 6, columns I, II, and III. Note that 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.

4.3. Discussion of other results

Contemporary observers argued that radio improved information more to rural than urban voters since the latter had better access to alternative information sources such as newspapers, see Brunner (1935). As it was relatively inexpensive

to deliver radio waves to remote areas, the informational advantage of the urban population was diminished by the radio. If the theory of this paper is correct, this should imply larger effects of radio in rural counties. To test this, the share of households with radios was interacted with a dummy variable for the 1419 counties with only rural households. The results indicate that the *direct effect* was significantly higher in rural counties; see Table 4, columns III and IV. Radio's impact on turnout was also significantly higher in rural counties than in urban counties; see Table 6, column V. The estimates imply that radio increased the ability of rural America to attract government transfers. In quantitative terms, radio is estimated to have increased the funds allocated to a rural county relative to an identical urban county by 20 percent.

Another topic deserving discussion is 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. The reason is that these counties have characteristics that make them politically weak. First and foremost, illiteracy rates are high. In 1930, the illiteracy rate among African Americans was ten times that among white, native born, Americans: 16 percent 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 2. This suggests that to understand discrimination one has to understand why these counties had a larger number of illiterates, fewer citizens who voted, and fewer households who used radios. It also suggests measures that would have alleviated this problem: providing people in these counties with better education, eliminating the discretionary use of eligibility rules that were used in the South[36], and giving them access to daily mass media.

Moreover, some results in the regression of voter turnout are worth mentioning. The estimated average effect of the share of households with radios on voter turnout during 1920-1940 is 0.07; see Table 6, column VIII. The aggregate effects of radio on voter turnout are far from negligible. In 1920, less than one percent of the population used radios. By 1940, around 80 percent of the households had radios. The estimate suggest that this would have led to an increase in votes per capita of around 5.5 percent. Between 1920 and 1940, votes per capita in the US

[36] See Ashenfelter and Kelley, (1975).

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 the 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.

The results are consistent with a model where the voter calculates the probability of being pivotal in the election. The winning margin, i.e. the closeness of the election, is negative and significant in some specifications. Furthermore, the coefficient on the interaction term between radio and the closeness of the election is always negative and significant. It thus seems that the effect of radios on turnout is higher when the margin of the election is close. One explanation for these findings is that people are more likely to turn out to vote if 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 would 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, and this makes a larger number of people to vote. This effect would, of course, be larger in areas where more people have radios, creating the interaction effect.

5. Another application: TV in the 1950s

This section presents some evidence that the introduction of television in the 1950s also affected the political influence of different groups. The methodology of the previous sections is now applied on a new data set. There are some features which makes this study less accurate. First, whereas the 1930 Census took place in the middle of the expansion period of the radios (1920-1940), the 1950 Census takes place when the expansion of TV has not started on a large scale, only 9 percent of US homes has TV, and the 1960 Census takes place when most of the expansion is over and 87 percent of the homes has a TV. Second, in this study I do not use one large program with well-defined goals, but all intergovernmental transfers from the states to local governments. This includes spending on education, highways, public welfare, and other purposes. Therefore, it is much more difficult to carefully control for other determinants of spending. Also, it is more difficult to determine what political actors were most important in the allocational decisions as this may

have differed across programs. Unfortunately, Censuses of governments were not performed between 1942 and 1957, therefore one cannot easily relate the changes in intergovernmental transfers 1950-1960 to the expanding use of TV during that period, and I will use a cross-section in 1960.

The role of TV in informing the electorate and promoting political participation is more controversial than that of radio and newspapers. Watching TV news is often not a significant predictor of political knowledge in studies of survey data, see Delli Carpini and Keeter (1996). Critics of TV further claim that instead of stimulating view interest and involvement in social action, TV news may instead spread a political malaise that discourages political participation (Robinson, 1976, Putnam, 2000). For a study that does find positive effects of TV viewing on learning, see Price and Zaller (1993).

While radio was, by contemporary accounts, particularly important for the rural areas, TV was deemed important for people with low education and for African Americans. McCombs (1968) finds that the share who used neither TV nor newspapers extensively *fell* from 71 to 49 percent during 1952-1960 among African Americans with less than high school education. Among whites of the same educational level, the share low media users actually *grew* by 5 percent. Among whites with high school education or more, the share low media users *doubled*, from 16 to 38 percent. I will therefore test whether TV increased the political strength of African Americans and people with low education.

Tables 8 and 9 contain definitions and summary statistics for the variables in this study. Table 10 contains the results from a cross-sectional regression of intergovernmental transfers 1962 on the share of households with TV, voter turnout, and a number of other variables. Regression I shows no direct effect of the share of households with TV on intergovernmental transfers. However, the transfers are positively correlated with voter turnout and with the median school years completed for people aged over 25. The effects of voter turnout on spending are of similar magnitude as in the FERA program. To test whether TV was more important in counties with many African Americans and with low average education levels, TV was interacted with dummy variables indicating whether the share of African Americans was larger than 20 percent, and if the average number of school years completed was less than 9 in 1960, respectively. The result is shown in regression II. TV did have a significantly larger impact on spending in counties with many African Americans, but not in counties with low educational levels.

Table 10 shows the results of the panel regression on voter turnout 1950-1960. Increases in the share of homes with TV is significantly positively correlated with

increases in voter turnout. However, the measured effect of TV on turnout is only about a third as large as that of radio, see Table 5. The positive effect of TV on voter turnout runs counter to arguments that TV discourages political participation. Note that the interaction term between TV and the closeness of the election is significant and negative. This indicates that closer elections increase voter turnout more where many households have TV-sets, thus reproducing the result of radios in Table 5. In regression II of this table, interaction terms between TV and education levels and share African Americans are included. It appears that TV's positive impact on turnout was larger in counties with low median school years, but not significantly different in counties with many African Americans.

Summing up, this section indicates that radios effect on policy was not an isolated historical incident. Data support an indirect effect of TV on government spending: TV had a significant positive effect on voter turnout, and voter turnout was positively correlated with government spending. However, the evidence of a direct effect of TV on spending is weaker and only significant in areas with many African Americans. Further, TV's measured effect on voter turnout was much smaller than that of the radio. There is also weak evidence that people with low education and African Americans gained from the introduction of TV.

6. Conclusion and discussion

Mass media affects politics because it carries 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 mass media. The empirical evidence presented in this paper suggests that such targeting did indeed take place in the US of the 1930s: governors allocated more relief funds to areas where a larger share of the population had radios. The effects are not only highly 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 10 percent, and a change from the lowest to the highest share of households with radios in the sample would increase spending by 65 percent.

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 the 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 other people. The effects of illiteracy are highly significant and considerable. For every percentage point increase in the illiteracy rate, governors cut spending by 2 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 increased redistribution towards the poor.[37] 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 who is responsible for the cuts in welfare, they may cut welfare without risking votes. Given the estimated effects of radio use and illiteracy compared to voter turnout, the expansion of the *informed* voting franchise may be as important for 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 20 percent. In a similar vein, preliminary results also indicate that African Americans, and people with little education, gained from the introduction of TV in the 1950s. Today, the spreading use 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 to measure the political impact of the internet.

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

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Appendix 1: Definitions of Variables

FERA spending/capita:	Cumulative disbursement within the FERA program April 1933 to December 1935/(0.6*population size 1930 + 0.4*population size 1940).
r_c, share hhlds with radios:	families reporting radio sets/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/ number of persons of age 7-18.
t_c, votes/capita:	total votes cast in Gubernatorial elections 1933-1936/((election year-1930)*population 1940 + (1940-election year)*population 1930)/10.
votes/capita in Pres. elect.:	total votes cast in Presidential elections 1932/ (0.2* population 1940 + 0.8*population 1930).
marginal voter density:	see explanation in specification section.
share partisans:	share of voters who voted for the winning gubernatorial candidate.
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/ (0.3*population 1930+0.7*population 1940).
bank deposits/capita:	bank deposits 1934/(0.6*population size 1930 + 0.4*population size 1940).
%Δbank deposits/capita:	(bank deposits/capita 1934 - bank deposits per 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/population 1930.
share 65+:	number of persons 65 years of age or older/population 1930.
share female:	number of females/population 1930.
share black:	number of African Americans/population 1930.
share immigrants:	number of foreign born white persons / number of white persons 1930.
share urban:	total urban population/population 1930.
gas sales/capita:	sales of filling stations in 1934/(0.6*population size 1930 + 0.4*population size 1940).
pop. density:	population per square mile 1930.
population:	0.6*population 1930 + 0.4*population 1940.
vote margin	(votes of winner - votes of runner up)/total votes in gubernatorial election
vote margin at state level	same as vote margin, but all vote data aggregated to state level.

Table 1. Summary statistics

Theoretical variables		Empirical variables	Mean	St. Dev.	Min.	Max.
z_c =		FERA spending/capita	19.98	15.32	0.04	225.67
z_s =		FERA spending/capita$_s$	25.72	11.39	9.50	57.14
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 =	+	votes/capita	0.30	0.17	0.00	0.82
t_s =		votes/capita$_s$	0.28	0.15	0.01	0.50
f_c =	+	marginal voter density	0.22	0.17	0.00	1.03
	+	share partisans	0.64	0.23	0.00	1.00
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	1130	205	333	2800
	-	crop value/capita	137	117	0.01	1272
	+	median rent	1444	683	429	5204
	-	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 urban	0.21	0.26	0.00	1.00
		gas sales/capita	10	8	0	122
		pop. density	1846	19341	1	848778
		population	40609	138268	48	4014611

Table 2. Dependent variable: log *FERA spending/capita*

A	B		I	II	III	IV
		state effects	no	yes	no	yes
		excluding states with winning margins>30% (South)	no	no	yes	yes
σ_c	+	**c_1: share hhlds with radios**	**0.625**** **(3.4)**	**0.519*** **(2.6)**	**0.745**** **(3.5)**	**0.781**** **(3.4)**
	-	share illiterate	-1.997** (-4.2)	-1.459** (-2.8)	-2.023** (-2.7)	-2.610** (-2.7)
	+	school enrollment	0.305 (1.0)	0.724* (2.3)	0.550 (1.3)	0.798 (1.7)
t_c	+	**c_2: log(t_c/t_s)**	**0.208**** **(6.1)**	**0.181**** **(5.1)**	**0.511**** **(5.9)**	**0.449**** **(4.7)**
f_c	+	marginal voter density	0.284** (4.0)	0.145 (1.3)	0.529** (4.2)	0.244 (1.3)
	+	share partisans	0.184* (2.5)	0.080 (0.7)	0.399** (3.8)	0.262 (1.7)
a_c	+	unempl. 1930	8.303** (4.9)	8.697** (5.0)	8.117** (3.8)	9.708** (4.4)
	+	unempl. 1937	9.025** (12.8)	9.887** (12.8)	9.351** (9.9)	9.965** (9.1)
	-	log bank deposits/ capita	-0.079** (-5.1)	-0.093** (-5.4)	-0.068** (-2.7)	-0.116** (-4.2)
	-	%Δbank deposits/capita	-0.005 (-0.4)	-0.014 (-1.1)	-0.117** (-2.6)	-0.134** (-2.8)
	-	log median dwell.value	-0.030 (-0.8)	0.011 (0.3)	-0.027 (-0.5)	-0.029 (-0.4)
	-	log farm value/capita	-0.154** (-4.5)	-0.182** (-4.9)	-0.220** (-5.3)	-0.294** (-6.7)
	-	log retail wage	-0.069 (-0.8)	-0.043 (-0.5)	-0.215 (-1.9)	-0.180 (-1.5)
	-	log crop value/capita	-0.009 (-0.4)	0.025 (1.0)	0.013 (0.4)	0.087** (2.6)
	+	log median rent	0.051 (0.8)	-0.057 (-0.9)	0.049 (0.6)	0.012 (0.1)
	-	share 21+	-2.424** (-5.8)	-1.188* (-2.4)	-2.584** (-4.7)	-1.721 (-2.5)
	+	share 65+	-3.006* (-2.0)	-3.495* (-2.1)	-3.315 (-1.8)	-2.725 (-1.3)
	+	share female	5.308** (5.4)	2.711* (2.4)	4.459** (3.7)	1.839 (1.3)
	+	share black	-0.012 (-0.1)	0.074 (0.6)	-0.665** (-2.6)	-0.384 (-1.2)
	+	share immigrants	0.151 (0.5)	0.583 (1.5)	-0.202 (-0.6)	0.120 (0.3)
con-trol		share urban	0.561** (5.5)	0.563** (5.5)	0.466** (4.3)	0.504** (4.5)
		log gas sales/capita	0.015 (0.8)	0.021 (1.1)	-0.009 (-0.4)	0.006 (0.2)
		log pop. density	-0.029 (-1.4)	-0.074** (-2.7)	-0.042 (-1.7)	-0.085** (-2.7)
		log population	-0.180** (-7.0)	-0.114** (-4.1)	-0.114** (-3.6)	-0.051 (-1.5)
z_s	+	log state FERA spending/capita	0.940** (25.4)		0.865** (19.4)	
		C	1.067 (1.2)		2.140 (1.9)	
		R2	0.57	0.62	0.53	0.58
		# observations	2495	2495	1751	1751

Standard errors are heteroscedastic consistent. T-statistics in parenthesis. ** Significant at 1 percent level. * Significant at 5 percent level.

Table 3. IV-estimates, Dependent variable: log *FERA spending/capita*

A	B		I	II	III
		state effects	no	no	yes
		IV	IV1	IV2	IV3
σ_c	+	c_1: share hhlds with radios	2.599** (2.6)	1.977* (2.1)	0.534** (2.6)
	-	share illiterate	-2.016** (-4.0)	-2.032** (-4.1)	-1.446** (-2.8)
	+	school enrollment	-0.235 (-0.6)	-0.060 (-0.2)	0.737* (2.3)
t_c	+	c_2: $\log(t_c/t_s)$	0.214** (6.0)	0.211** (6.0)	0.177** (2.9)
		⋮	⋮	⋮	⋮
		R2	0.55	0.56	0.62
		# observations	2493	2490	2480
		χ^2(df)		0.35 (1)	0.05 (1)
		F-stat	89	52	98

IV1 contains ground conductivity in the county seat. IV2 contains IV1 and weighted distance from radio antennas in 1934.
All independent variables in Table 2, column II, are included but not displayed in the above regressions.
Standard errors are heteroscedastic consistent. T-statistics in parenthesis. ** Significant at 1 percent level. * Significant at 5 percent level.

Table 4. Alternative specifications, Dependent variable: log *FERA spending/capita*

A	B		I	II	III	IV
		state effects	yes	yes	yes	yes
		excluding states with winning margins>30%	no	no	no	yes
σ_c	+	c_1: share hhlds with radios	0.577** (2.8)	0.535** (2.8)	0.268 (1.3)	0.566* (2.5)
	+	radios*rural			0.575** (4.6)	0.582** (4.3)
	-	share illiterate	-1.732** (-3.4)	-1.701** (-3.5)	-1.320** (-2.6)	-2.137* (-2.3)
	+	school enrollment	0.803** (2.6)	0.627* (2.0)	0.784* (2.5)	0.838 (1.8)
t_c	+	c_2: $\log(t_c/t_s)$		0.207** (5.9)	0.184** (5.2)	0.432** (4.6)
		⋮	⋮	⋮	⋮	⋮
		R2	0.62	0.62	0.63	0.58
		# observations	2495	2675	2495	1751

All independent variables in Table 2, column II, are included but not displayed in the above regressions. In column III, log. median dwell. value, log median rent, and log gas sales/capita have been excluded to reduce the number of observations with missing values. Standard errors are heteroscedastic consistent. T-statistics in parenthesis. ** Significant at 1 percent level. * Significant at 5 percent level.

Table 5. Dependent variable: *votes/capita*

	I	II
year		
elections with winning margins>30% excluded	no	yes
state effects	yes	yes
election year effects		
share hhlds (b_1) with radios	**0.068** **(4.2)**	**0.094** **(4.9)**
share illiterate	-0.073 (-1.4)	-0.332** (-3.2)
school enrollment	0.075** (2.6)	0.170** (3.8)
marginal voter density	-0.079** (-7.4)	-0.078** (-5.4)
share partisans	-0.091** (-7.8)	-0.058** (-4.3)
unempl. 1930	0.115 (0.6)	0.046 (0.2)
unempl. 1937	0.233** (3.4)	0.427** (4.3)
log bank deposits/ capita	0.004** (2.7)	0.011** (4.4)
%Δbank deposits/capita	0.004* (2.3)	-0.005 (-0.8)
log median dwell.value	0.009* (2.2)	0.002 (0.3)
log farm value/capita	-0.002 (-0.7)	0.000 (-0.1)
log retail wage	-0.020* (-2.0)	-0.031* (-2.2)
log crop value/capita	-0.002 (-0.9)	-0.009** (-2.8)
log median rent	-0.034** (-5.3)	-0.034** (-3.6)
share 21+	0.430** (8.0)	0.420** (5.6)
share 65+	0.390* (2.2)	0.191 (0.8)
share female	0.285** (2.8)	0.469** (3.8)
share black	-0.102** (-10.5)	0.034 (1.3)
share immigrants	0.052 (1.6)	0.060 (1.7)
share urban	-0.036** (-4.5)	-0.038** (-3.9)
log gas sales/capita	0.000 (0.2)	0.001 (0.4)
log pop. density	-0.004 (-1.8)	-0.004 (-1.7)
log population	-0.022** (-9.4)	-0.031** (-10.6)
C	0.557** (6.6)	0.438** (4.1)
R2	0.94	0.85
Number of observations	2525	1756

Standard errors are heteroscedastic consistent. T-statistics in parenthesis.
*Denotes significance at 1 percent level. **Denotes significance at 5 percent level.

Table 6. Dependent variable: *votes per capita* 1920-1940.
Fixed effects regressions

	I	II	III	IV	V	VI	VII	VIII
election year effects	yes	yes	yes	yes	yes	yes	yes	yes
time period	1920-1930	1920-1930	1920-1930	1920-1930	1920-1930	1930-1940	1920-1940	1920-1940
IV	no	IV1	IV2	no	no	no	no	no
share hhlds (b_1) with radios	**0.123**** **(11.8)**	**0.173*** **(2.4)**	**0.233**** **(6.5)**	**0.116**** **(12.9)**	**0.120**** **(11.6)**	**0.051**** **(3.5)**	**0.074**** **(9.9)**	**0.068**** **(9.2)**
radios*rural					**0.028**** **(3.2)**			
radio*vote margin at state level	-0.868** (-14.6)	-0.893** (-4.4)	-1.059** (-10.0)	-0.843** (-13.3)	-0.887** (-14.8)	-0.138** (-5.7)	-0.081** (-4.9)	
radio*vote margin at state level (president)		-0.485** (-5.9)	-0.541** (-9.1)					
vote margin at state level	-0.029 (-2.0)	-0.009 (-0.3)	0.020 (1.0)	-0.011 (-0.8)	-0.026 (-1.8)	-0.047* (-2.5)	-0.125** (-10.8)	-0.161** (-18.2)
vote margin at county level	-0.004 (-0.7)	-0.002 (-0.5)	0.001 (0.2)	-0.051** (-12.2)	-0.004 (-0.8)	-0.006 (-1.1)	0.001 (0.2)	-0.001 (-0.2)
share 21+	0.339** (4.3)	0.567** (3.8)	0.682** (7.8)	0.518** (26.4)	0.314** (4.0)	0.211** (2.7)	0.199** (4.2)	0.165** (3.5)
share female	0.725** (5.8)	0.972** (6.0)	1.088** (9.8)	0.661** (12.1)	0.766** (6.1)	-0.154** (-3.8)	-0.056 (-1.6)	-0.054 (-1.5)
share urban	0.007 (0.4)	0.011 (0.8)	0.017 (1.3)	-0.052** (-10.9)	0.003 (0.2)	-0.013 (-0.7)	0.001 (0.1)	0.000 (0.0)
share black	-0.126* (-2.1)	-0.204* (-2.2)	-0.274** (-4.8)	-0.124** (-22.5)	-0.129* (-2.2)	0.212** (2.7)	0.007 (0.2)	0.029 (0.7)
share immigrants	-0.155** (-7.2)	-0.153** (-4.5)	-0.127** (-6.0)	-0.106** (-9.0)	-0.151** (-7.0)	-0.245** (-3.9)	-0.132** (-7.6)	-0.118** (-6.8)
school enrollment	-0.053* (-2.2)	-0.062** (-3.8)	-0.064** (-3.9)	0.015 (1.2)	-0.052* (-2.1)	0.048* (2.4)	0.020 (1.3)	0.016 (1.1)
log retail wage	0.002 (0.4)	0.000 (0.0)	-0.003 (-1.0)	-0.002 (-1.2)	0.002 (0.4)	-0.001 (-0.4)	0.004 (1.3)	0.004 (1.5)
log crop value/capita	-0.008* (-2.4)	-0.006 (-1.2)	-0.002 (-0.7)	-0.004** (-4.5)	-0.008** (-2.6)	0.013** (4.2)	0.004 (1.7)	0.004 (1.7)
log population	-0.056** (-8.0)	-0.050** (-10.5)	-0.050** (-10.3)	-0.020** (-16.0)	-0.052** (-7.4)	-0.034** (-3.1)	-0.046** (-9.3)	-0.049** (-9.9)
R2	0.50	0.50	0.49	0.90	0.50	0.75	0.64	0.64
Number of observations	5224	5215	5215	5224	5224	4695	7537	7537
			$\chi^2_{df:1}$=0.61					

IV1 contains ground conductivity in the county seat. IV2 contains IV1 and weighted distance from radio antennas in 1934.
Regression VII does not include county fixed effects, but includes state effects.
T-statistics in parenthesis. **Denotes significance at 1 percent level. *Denotes significance at 5 percent level.

Table 7: Definitions of Variables for TV study

Local revenue from State/capita	Intergovernmental revenue of local governments obtained from the state government.
TV:	Share occupied housing units with TV-set, 1960.
school years:	Median school years completed of population aged 25 or older, 1960.
t: votes/capita:	total votes cast in 1960 Presidential election/population 1960.
marginal voter density:	absolute deviation from 0.5 in democratic vote share
dem. vote share	democratic vote share in Presidential election 1960
unemployed:	share unemployed in civilian labor force, 1960.
median income:	Median income in 1959 of families (dollars).
share poor:	Percent of families in 1959 with income under $3000.
share rich:	Percent of families in 1959 with income over $10000.
bank deposits/capita:	bank deposits 1960 ($1000)/population 1960.
median rent:	median monthly gross in 1960 (dollars).
share 21+:	share aged 21 years or older 1960.
share 65+:	share aged 65 years or older 1960.
share female:	number of females/population 1960.
share black:	number of African Americans/population 1960.
share immigrants:	share foreign born 1960.
car:	share occupied housing units with automobiles, 1960.
phone:	share occupied housing units with telephone, 1960.
area:	land area (sq. miles)
pop. density:	population per square mile 1960.
population:	population 1960.

Table 8. Summary statistics for TV Study

Theoretical variables	Empirical variables	Mean	St. Dev.	Min	Max
z_s=	Local revenue from State/capita	64.78	33.38	6.84	367.37
σ_s=	TV	0.79	0.12	0.26	1.00
	median school years	9.72	1.43	4.50	12.80
t_c=	votes/capita	0.38	0.12	0.02	0.82
f_c=	marginal voter density	0.11	0.08	0.00	0.45
	share democrats	0.47	0.13	0.12	0.95
	unemployed	0.02	0.01	0.00	0.06
	median income	4265	1266	1260	9317
	share poor	0.35	0.15	0.06	0.78
	share rich	0.08	0.05	0.01	0.45
	bank deposits/capita	68194	369372	0	11730900
	median rent	55	14	20	119
	share 21+	0.59	0.04	0.43	0.73
	share 65+	0.11	0.03	0.01	0.25
	share black	0.09	0.15	0.00	0.83
	share female	0.50	0.02	0.22	0.59
	share immigrants	0.02	0.03	0.00	0.26
	share with cars	0.59	0.08	0.27	0.80
	share with phone	0.67	0.18	0.06	0.96
	area	974	1283	2	20131
	pop. density	157	808	1	16452
	population	57879	198510	894	6038771

Table 9. Dependent variable, local revenue from State/capita

A	B		I	II
		state effects	yes	yes
σ_c		TV	-0.027	-0.048
			(-0.4)	(-0.6)
		black*TV		0.121**
				(4.0)
		median school years*TV		0.005
				(0.3)
		median school years	0.035**	0.034**
			(5.1)	(4.5)
t_c		votes/capita	0.266*	0.261*
			(2.3)	(2.3)
f_c		marginal voter density	0.095	0.096
			(1.5)	(1.6)
		share democrats	-0.021	-0.024
			(-0.5)	(-0.6)
		unemployed	2.655**	2.663**
			(4.1)	(4.1)
		log median income	0.505**	0.407**
			(4.6)	(3.7)
		share poor	1.317**	1.125**
			(5.7)	(4.8)
		share rich	-0.864**	-0.723**
			(-3.9)	(-3.2)
		log bank deposits/capita	-0.049**	-0.051**
			(-3.6)	(-3.7)
		log median rent	-0.087	-0.089
			(-1.2)	(-1.3)
		share 21+	-0.500	-0.565*
			(-1.8)	(-2.0)
		share 65+	0.314	0.454
			(0.7)	(1.0)
		share black	-0.006	-0.190**
			(-0.1)	(-2.9)
		share female	1.462	1.436
			(1.8)	(1.8)
		share immigrants	-0.528*	-0.512*
			(-2.2)	(-2.1)
		share with cars	-0.089	-0.085
			(-1.0)	(-0.9)
		share with phone	-0.406**	-0.394**
			(-5.2)	(-5.1)
		log area	0.089	0.090
			(0.8)	(0.8)
		log pop. density	0.058	0.058
			(0.5)	(0.5)
		log population	-0.130	-0.130
			(-1.2)	(-1.2)
		C	0.469	1.395
			(0.4)	(1.2)
		R2	0.82	0.82
		# observations	2706	2706

Table 10. Dependent variable, votes per capita 1950-1960
Fixed effects regressions

	I	II
year effects	yes	yes
TV	0.025**	0.033**
	(3.8)	(4.9)
black*TV		-0.001
		(-0.2)
median school years < 9*TV		0.016**
		(7.1)
TV*vote margin at state level	-0.116**	-0.117**
	(-5.4)	(-5.5)
vote margin	0.054**	0.051**
	(5.1)	(4.9)
vote margin at state level	-0.023	-0.019
	(-1.5)	(-1.2)
share 21+	0.524**	0.451**
	(15.1)	(12.7)
share 65+	0.477**	0.505**
	(9.3)	(9.9)
log population	-0.015**	-0.018**
	(-4.0)	(-5.4)
median school years	-0.005**	-0.002
	(-4.7)	(-1.4)
log median income	0.003	0.000
	(0.7)	(-0.1)
log bank deposits/capita	-0.004	-0.003
	(-1.8)	(-1.3)
share black	-0.226**	-0.200**
	(-5.9)	(-5.2)
share immigrants	-0.069**	-0.075**
	(-3.6)	(-3.9)
YEAR1	-0.025**	-0.012
	(-3.9)	(-1.8)
R2	0.98	0.98
Number of observations	5670	5670