Why Did the Electorate Swing Between Parties During the Great Depression?

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Abstract: The Democratic Party's electoral success during the 1930s has long intrigued politicians and scholars. To gain new insight into the factors underlying that success, this paper examines the striking heterogeneity in county-level support for Roosevelt. Even though the Depression's effects and the New Deal's benefits were famously widespread, only some parts of the country responded with large and durable partisan shifts. One reason is that several factors, including pre-New Deal economic hardship, Dust Bowl conditions, and New Deal spending, appear to have had effects that were largely transitory (i.e., faded by 1940). A complementary reason is that swing electorates can, and did, swing both ways: Compared to other counties, those with a history of greater electoral variability experienced bigger swings toward Roosevelt in 1932 and 1936, and then bigger back-swings in 1940. By contrast, several other variables – notably economic and demographic factors discussed the previous literature – are related to relatively durable shifts: Controlling for Roosevelt's 1932 vote share (and other factors), Roosevelt's 1940 vote share was higher in urban and manufacturing-oriented counties and lower in farming counties and counties with large German-born populations. Finally, heterogeneity in marginal responses may have mattered greatly to national-level Democratic success, because pre-New Deal economic hardship appears to have had a larger marginal effect on vote shares in the most populous areas (where most voters lived). By demonstrating which factors were transitory and which were more durable, this paper illuminates the New Deal Realignment and, more generally, the influence of economic conditions and distributive policy on voter behavior.

I. Introduction

Although many econometric studies have examined the way voters respond to macroeconomic fluctuations, government spending, and other factors, only a few – most notably Wright (1974) and Brown (1988) – have focused on the 1930s.[1] The fact that little attention has been given to the 1930s is quite striking, given the momentous events of that decade: a disastrous downturn in the U.S. economy (the Great Depression), an unprecedented rise in non-military federal spending (the New Deal), and a dramatic swing of voters from the Republican Party to the Democratic Party (the New Deal Realignment).

By using county-level data to estimate the marginal effects of Depression-related hardship, New Deal spending, and other factors on Democratic electoral success, this paper yields new insight into the political economy of the 1930s and, more generally, the influence of economic conditions and distributive policy on voter behavior. County-level data are particularly useful because they exhibit substantial heterogeneity in Democratic electoral gains: During the Depression, some parts of the country swung a long way toward the Democrats, others swung a small way toward the Democrats, and still others swung in the opposite direction.

What Explains the Heterogeneity in Democratic Electoral Gains (and Losses)?

The natural starting point is to consider factors related to the government's handling of the economy. Growing economic hardship increased the public's demand for a more active federal response to the economic crisis, and voters in 1932 thus delivered a landslide to the Democrats, who responded with the widely popular New Deal (e.g., Brady 1988; Fleck 1999a, 2008). In this context, some of the most obvious potential explanatory factors for voting patterns are pre-New Deal

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[1]Fishback, Kantor, and Wallis (2012) have work in progress that examines the effect of New Deal spending on election results.

1

economic hardship, Dust Bowl conditions, and New Deal spending. As this paper shows, however, the econometric evidence that these factors mattered much, especially with respect to causing *long run* changes in voting patterns, is quite modest. Although more severe economic hardship at the onset of the Depression increased support for the Democratic Party, the estimated effects for the typical county appear moderate in size for 1932 and 1936, fading somewhat by 1940. And while the historical literature has long pointed to suffering caused by drought and the Dust Bowl as an important impetus for New Deal policy (e.g., Schlesinger 1958), counties that experienced severe droughts do not tend to show upswings in Democratic support, ceteris paribus.[2] Even in the extreme cases – counties with Dust Bowl conditions – the electoral gains for the Democrats appear to be transitory (large in 1936, mostly gone by 1940). The marginal effect of New Deal spending appears likely to have been substantial (yet not huge) in the short run (i.e., in the 1936 election), then diminished by the 1940 election – though a prudent interpretation of the available data precludes firm conclusions.[3]

The econometric analysis reveals several other key features of the electorate's response to Depression-era hardship and policy. Very importantly, the data suggest substantial between-county variation in the electorate's sensitivity to economic conditions. That is, while the estimated effect of additional economic hardship on the predicted Democratic vote share – when estimated using data

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[2]As Schlesinger (1958, 336-337) explains, the combination of an economic crisis with drought and dust is what allowed New Dealers to justify a variety of programs:

> Against the backdrop of drought and dust, Roosevelt hoped to awaken in the American people a sense of urgency about their ultimate basis in nature. "Unlike most of the leading Nations of the world," he observed a trifle bitterly in 1934, "we have so far failed to create a national policy for the development of our land and water resources." Here plainly was a major objective for his administration, even if it had to take second place to the war against depression.

[3]The newly assembled spending data used by Kantor, Fishback, and Wallis (2012) will allow additional analysis and further insight into the effects of New Deal distributive programs on voting behavior.

from all counties – is only moderate, the magnitude of the estimated effect depends on the sample of counties. More specifically, the pre-New Deal economic downturn's marginal effect on the Democratic vote share (conditional on other pre-New Deal conditions) appears to have been greater in more populous counties than in less populous counties. This helps to explain how the Democrats gained so much *national-level* support despite the typical county exhibiting only moderate effects of the Depression's onset and New Deal spending: The counties where the Depression's onset caused the largest increase in the Democratic vote share were those with more people and, hence, more votes. Similarly, the estimated marginal effect of New Deal spending on Democratic vote shares appears to have been larger in counties with more people and, hence, more votes.

Another important insight into the nature of Depression-era electoral gains and losses comes from a mechanism highlighted in Wright's (1974) seminal work: the behavior of swing electorates. Between the pre-Depression elections (1928 and prior) and Roosevelt's two landslide victories (1932 and 1936), the swing toward the Democratic Party was larger in historically swing counties than in other counties; that is, larger in counties with more inter-temporal variation in pre-1932 Democratic vote shares. Then, from 1936 to 1940, the partial back-swing away from Roosevelt was larger in historically swing districts than in other districts. In short, counties that had previously exhibited a swing nature continued to do so during the 1930s and, hence, contributed disproportionately to the era's partisan shifts.

Several other economic and demographic variables show a strong relation to changes in voting patterns over the course of the 1930s. Manufacturing-oriented counties and urban counties increasingly supported the Democrats, while farming counties increasingly opposed the Democrats, ceteris paribus; this provides evidence of electoral support shifting in line with the mid-to-late 1930s changes in congressional alignments (e.g., Patterson 1967) and in the coalition supporting farm

3

policy (e.g., Alston and Ferrie 1999). A larger German-born population is associated statistically with inter-temporal declines in support for Democrats, ceteris paribus. This econometric role of the German population could reflect, as has long been hypothesized (e.g., Lubell 1951; Wright 1974), the increasing prominence of international issues – particularly the question of how to respond to German military expansion. An important point to recognize, however, is that an empirical link between a larger German population and weakening support for Roosevelt is already apparent when comparing voting patterns from 1936 to those from 1932. The German-related post-1932 reduction in Democratic vote shares is larger in 1940 than in 1936, consistent with onset of World War II affecting voting patterns, but the existence of German-related changes in voting patterns between 1932 and 1936 provides reason for caution with respect to inference: What appears on the surface in 1940 data to be a World War II driven shift in voting could be, at least in part, the result of changes that started in the early 1930s (when domestic policy issues dominated politics).

Contribution to the Literature

By improving the understanding of Depression-era voting behavior, the findings outlined above contribute to several branches of scholarly research. In his seminal analysis, Wright (1974) develops a theoretical model of distributive politics, then tests it using state-level data on partisan vote shares, New Deal spending, and economic conditions. My estimated effects of spending on county-level vote shares add to what Wright (1974, 35) refers to as his "modest evidence to the effect that the spending patterns of the New Deal did indeed have an effect on the vote."[4] I also confirm Wright's empirical findings with respect to the size of the German-heritage population, though my results suggest an important caveat (mentioned above) with respect to inferring causality. In

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[4]By using a county-level data set (instead of Wright's state-level data), I can control better for a variety of other factors that may obscure the causal relationship between spending and voting.

addition, my results complement Wright's work (and the literature he inspired) by demonstrating the

extent to which swing electorates actually did swing during the Depression era, and by showing that

swing counties swung first to, and later away from, the Democrats.[5]

This paper adds substantially to the literature on the role that "critical elections" (such as the

Democratic landslide of 1932) play in long term partisan realignments and policy changes.[6] Viewed

as a whole, the literature suggests myriad ethnic, economic, and demographic factors correlated with

(and potentially related systematically to) partisan leanings and realignments, and leads to the

conclusion that the New Deal Realignment involved an extremely broad-based increase in support

for Democrats. Within this literature, Brown (1988) is a particularly important antecedent to my

paper, because he examines mass voting behavior and conducts an econometric analysis of county-

level presidential election data from 1928, 1932, and 1936.[7] Much of the critical elections literature

focuses on the way partisan shifts affect congressional policymaking, and this yields conclusions

complementary to the arguments in Wright (1974) and the subsequent economic literature (e.g.,

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[5]Wright's (1974) empirical analysis has two parts. In the first part, he tests hypotheses with respect to the determinants of spending: whether swing states (relative to loyally partisan states) will obtain more federal spending, and whether small-population states will obtain more federal spending per capita (because the apportionment of Electoral College votes and Senate seats gives those states more electoral weight per capita). In the second part of his empirical analysis, Wright tests whether spending changed voting outcomes. The determinants of spending (the topic of the first part of Wright's empirical analysis) has been the focus of the large literature that Wright inspired (e.g., Wallis 1987, 1998, 2001; Anderson and Tollison 1991; Fleck 1999a, 1999c, 2001a, 2001b, 2008; Bateman and Taylor 2003a, 2003b; Fishback, Kantor, and Wallis 2003), as well as the early work by Arrington (1969) and Reading (1973). In this paper, I revisit the second part: the electoral effects of spending.

[6]See, e.g., Key (1955), Burnham (1970), Ginsberg (1972, 1976), Sundquist (1973), Sinclair (1977, 1985), Clubb, Flanigan, and Zingale (1980), Brady and Stewart (1982), Brady (1988), Brown (1988), Miller and Schofield (2003).

[7]Brown (1988) concludes that (I) the Democrats' 1932 gains came principally from previously Republican voters switching over to cast their ballots for Democrats, (ii) the Democrats' 1936 gains came principally from previous nonvoters going to the polls and voting for Democrats, (iii) in 1936, Republicans gained support in farming areas, (iv) the Democrats' large gains in counties with urban populations and many manufacturing workers came in 1936, not in 1932.

Fleck 1999a, 2008): After the Democrats initially gained power as a result of the Depression, they used the New Deal as a logrolled set of policies to secure a broad coalition (i.e., the "New Deal Coalition"). Brady (1988, 113-114) provides a particularly clear explanation of the link between the critical election of 1932 and the New Deal Realignment, "The coalition responsible for passing most of the New Deal legislation combined both Northern and Southern, rural and industrial interests. The Democrats were able to force this coalition by essentially logrolling these disparate elements around the issue of trying to alleviate the effects of the depression."[8] My analysis complements the critical elections literature by investigating econometrically the magnitudes and timing of the effects that economic hardship, drought, the Dust Bowl, and New Deal dollars had on voter behavior.

My analysis also illuminates the nature of the late-1930s decline in electoral support for Democrats. This decline and the corresponding rise of a long-lasting "Conservative Coalition" in Congress has been studied in a large literature, with contributions from historians, political scientists, and economists.[9] Again, this paper makes a new contribution by estimating marginal effects of multiple factors on the electorate's behavior, demonstrating the substantial but fading effects of the pre-New Deal economic decline, the perhaps transitory effects of New Deal spending, the lack of any substantial long term gains generated by drought-related hardship, the gains and losses linked to demographic factors, and the behavior of swing counties.[10]

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[8]Also see, e.g., Patterson's (1967) classic analysis of the weakening of that coalition in the late 1930s. The historical literature on the New Deal is, of course, enormous (e.g., Schlesinger 1958; Leuchtenberg 1963; Freidel 1965; Kennedy 1999).

[9]See, e.g., Patterson (1967), Brady and Bullock (1981), Shelley (1983), Brady (1988), Poole and Rosenthal (1997), Fleck (1999a, 2002, 2011).

[10]Although there is a substantial empirical literature seeking to explain the rise of the Conservative Coalition in *Congress* (e.g., Brady 1988; Poole and Rosenthal 1997; Fleck 1999a, 2002), this paper's econometric results seek to explain systematically the late-1930s back-swing in the *electorate*.

Finally, this paper contributes to the more general literature on the determinants of electoral success. By estimating the magnitudes of the electorate's response to the Great Depression, I contribute to the debate over the way macroeconomic events influence voter behavior.[11] And, by estimating the magnitude (and demonstrating the plausibly transitory nature) of the electorate's response to a huge increase in federal spending, I contribute to the debate over the electoral effects of distributive policy.[12] The fact that this paper presents relatively modest estimated effects (at least for the typical county) has some broader lessons: If data from the 1930s yield only modest evidence with respect to the typical county's electoral response to economic decline and federal spending, scholars probably should not expect that data sets with milder macroeconomic shocks and/or small-stakes pork barrel spending will necessarily yield large, precisely estimated effects.

II. History, Hypotheses, and Data

The events of the 1930s make the time period a particularly rich one for studying how the electorate responds to economic hardship and to government spending. As noted earlier, the era had the combination of a disastrous economic downturn and a dramatic increase in government spending. Furthermore, the timing of the swings in voter behavior assists this paper's econometric analysis.[13] More specifically, because the Republicans dominated policymaking when the Depression began,

[11]See, e.g., Kramer (1971), Stigler (1973), Arcelus and Meltzer (1975a, 1975b), Bloom and Price (1975), Goodman and Kramer (1975), Fair (1978), Peltzman (1987), Erikson (1990a, 1990b), Jacobson (1990b), Grier and McGarrity (1998), and Strumpf and Phillippe (1999).

[12]See, e.g., Jacobsen (1978, 1990a), Weingast, Shepsle, and Johnsen (1981), Feldman and Jondrow (1984), Green and Krasno (1988, 1990), Stein and Bickers (1994), Levitt and Snyder (1997), and Sellers (1997).

[13]In 1928, Hoover (a Republican) beat Smith (a Democrat) by a large margin: 58.2% to 40.8%. Four years later, Roosevelt beat Hoover by a large margin: 57.4% to 39.6%. In 1936, Roosevelt beat Landon by an even larger margin: 60.8% to 36.5%. In 1940, Roosevelt beat Willkie 54.7% to 44.8%. See, e.g., Woolley and Peters (2007).

the electoral consequences of the onset of the Depression were negative for the Republican Party and positive for the Democratic Party. In plain language, voters blamed the Republicans and, consequently, put the Democrats in power. Then, after the 1932 landslide election, the Democratic Party dominated policymaking. Even though many Republican politicians supported parts the New Deal, voters viewed the New Deal as a principally Democratic set of policies. The combination of these facts makes empirical analysis of electoral data far more straightforward than if, say, the Depression era had been a period of continuously divided government. In short, the sharp change in party dominance makes it reasonable to assume that, to the extent that voters responded negatively at the margin to early-Depression economic hardship and positively at the margin to higher New Deal spending, the Democratic Party received more votes.

Another advantage of studying voting in the 1930s is the growing wealth of data. Most critically, thanks to work by Fishback, Kantor, and Wallis (2003a, 2003b), county-level New Deal spending data are now available for the entire country, and these data can be matched accurately to electoral and Census data. Of course, this is not to say that all the variables one would wish for are available for the 1930s. For example, there are no reliable county-level unemployment data for the years between the 1930 Census and the 1940 Census, and county-level income data for the Depression era do not exist.[14]

Sorting out the direction of causality typically poses a serious challenge in empirical work

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[14]Even at the state level, many basic variables (e.g., annual unemployment data) do not exist for the 1930s. For clarity, I should note that county-level unemployment data are available from the 1937 Census of Partial Employment, Unemployment, and Occupations. But those data have two serious drawbacks. The first is measurement error: The Census obtained the 1937 data from voluntarily submitted "Unemployment Report Cards." The potential under-counting problem (i.e., some cards that should have been returned to the Census were not) is reason for skepticism regarding the accuracy of the 1937 data (e.g., Howard 1943). Second, the 1937 data (as well as the 1940 data) are a function of the way the New Deal allocated relief employment across counties (Fleck 1999b). On the endogeneity of New Deal era unemployment statistics, also see Darby (1976) and Kesselman and Savin (1978).

on the relationships between economic fluctuations, government spending, and voter behavior. The problem is particularly conspicuous for the 1930s, in view of the many econometric studies that have examined causality running in other directions – that is, seeking to explain how electoral variables influenced the allocation of New Deal spending and how New Deal spending affected recovery from the Depression.[15] To address the endogeneity problems, this paper does two things. The first is to estimate the effects of economic hardship on voting by using economic variables from the Depression's onset – which was prior to the New Deal. The second is to use an instrumental variable to estimate the effects of spending; more specifically, the instrument relies on the fact that the criteria for allocating New Deal funds among states differed from those used to allocate spending among counties within states.

I will now turn to a more detailed discussion of the data used in this paper. Note that Appendix A lists definitions and data sources for all variables used in this paper. Appendices B and C provide descriptive statistics.

Electoral Data Used as Dependent Variables

To ensure that my electoral variables can be compared meaningfully across counties, I use data from presidential general elections.[16] As dependent variables, I use Democratic vote shares

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[15]For example, Wright (1974), Wallis (1987, 1998, 2001), Anderson and Tollison (1991), Fleck (1999a, 1999c, 2001a, 2001b, 2008), Bateman and Taylor (2003a, 2003b), and Fishback, Kantor, and Wallis (2003) seek to explain how electoral variables influenced the allocation of New Deal spending. Fleck (1999b) and Fishback, Horrace, and Kantor (2005b, 2006b) seek to explain how New Deal programs affected economic outcomes.

[16]Analyzing partisan vote shares from other contests, such as gubernatorial or congressional elections, is an excellent topic for future research, but will be complicated by the need to control for cross-sectional (i.e., between-state or between-district) heterogeneity among candidates. For example, southern Democrats differed systematically from northern and western Democrats, as did eastern Republicans from western Republicans (e.g., Key 1949; Patterson 1967; Poole and Rosenthal 1997). With presidential general election data, the set of candidates running is (for a given year) effectively uniform across the entire country.

from the elections of 1932, 1936, and 1940 (all of which Roosevelt won). The 1932 variable will reflect the effects of the Depression's onset and, specifically, voters' dissatisfaction with Republican policies and voters' expectations of Democratic policy. The 1936 variable will reflect not only the effects of the Depression, but voters' responses to the early New Deal. In 1936, the New Deal could not be classified as an obvious success or an obvious failure as a response to the Depression, but the extreme landslide of 1936 showed that the New Deal won support among voters. The county-level 1936 Democratic vote share (especially when controlling for the 1932 Democratic vote share) will thus reflect cross-sectional variation in the support the New Deal won (and in many places lost) among voters. By the 1940 election, more than a decade had passed since the onset of the Depression, and while the Democrats remained the dominant party, a substantial number of voters were less enchanted with the New Deal than they had been in 1936.[17] The 1940 variable will reflect this disenchantment. It will also reflect the degree to which changes in New Deal spending patterns caused changes in voters' behavior. Most notably, the high per capita allocations to swing states (including the western states that received so much land-oriented spending) declined in the late 1930s (Fleck 1999a). And, of course, the 1940 vote share will reflect the growing importance of international concerns following the outbreak of World War II.

Electoral Data Used as Explanatory Variables

The proper interpretation of the estimated effects of economic hardship and New Deal spending on the Democratic vote share in 1932, 1936, and 1940 depends critically on what electoral

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[17]Disenchantment with the New Deal arose from a variety of factors, including Roosevelt's infamous "court packing" plan to add justices to the Supreme Court, reports of corruption in the allocation of relief funds and relief jobs, the lack of full recovery from the Depression, a recession (which came after the 1936 election), and conservative Democrats' displeasure with labor legislation (e.g., Key 1949; Patterson 1967; Brady 1988; Seltzer 1995; Fleck 1999a, 2002).

variables are included as explanatory variables. Because I seek to identify how the Depression, the New Deal, and other factors *changed* voter behavior, I include historical election data in all my specifications. The set of historical election variables differs from specification to specification, because different specifications have different purposes.

Three pre-Depression vote share variables are common to all my specifications. To control for each county's history of supporting the Democratic Party, I use the mean Democratic vote share from 1896 to 1928 (from Fishback, Kantor, and Wallis 2003a, 2003b). As in Wright (1974) and much subsequent work on the political economy of the New Deal, the variable uses the 1896 election as a starting point because it marks the most recent major partisan realignment prior to that of the 1930s (e.g., Brady 1988). The second variable is the square of the first variable. I include the squared term because the relationship between the historical Democratic vote share and support for the Democratic Party during the New Deal is unlikely to be linear over the entire relevant range.[18] The third variable proxies for the propensity of a county's electorate to switch with respect to the party it supported. The variable, which follows the basic logic of a state-level variable Wright (1974) developed, is the standard deviation in the Democratic vote share in presidential elections, 1896-1928 (my source of the variable is Fishback, Horrace, and Kantor 2005a, 2005b). To the degree that this variable proxies accurately for the extent to which counties had swing electorates, the variable should predict greater inter-temporal changes in vote shares, ceteris paribus.

In each specification, I include one of three other presidential election variables, *depending*

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[18]Note, for example, that New Deal era Democratic vote shares would be concave functions of the historical Democratic vote share if the historically less partisan counties (i.e., more evenly divided electorates) had many voters shift toward the Democrats, while Republican strongholds and Democratic strongholds did not. (And, of course, among counties with historical vote shares approaching unanimous support for the Democratic Party, the maximum possible Depression-era increase in the Democratic vote share was necessarily small.)

11

on the purpose of the specification. For regressions with the 1932 Democratic vote share as the dependent variable, I include the 1928 Democratic vote share as an explanatory variable. Thus, the estimated effects of economic hardship at the onset of the Depression are based on changes in the Democratic vote share from 1928 to 1932 (controlling, as discussed above, for the longer run history of support for the Democratic Party). In regressions with the 1936 Democratic vote share as the dependent variable, I include either the 1928 Democratic vote share or the 1932 Democratic vote share; the estimated effects of economic hardship at the onset of the Depression will thus be based on changes in the Democratic vote share from 1928 to 1936 or from 1932 to 1936. I run three sets of specifications when using the 1940 Democratic vote share as the dependent variable. The first set includes the 1928 Democratic vote share, the second includes the 1932 Democratic vote share, and the third includes the 1936 Democratic vote share. The first set thus estimates how the variables of interest influenced the net gain in Democratic support over essentially the entire Depression era, while the second set considers the time period from when New Dealers took power until 1940; the third estimates how the variables of interest influenced the decline in Democratic support that occurred during the later years of the New Deal.

In all specifications, I add another pre-Depression electoral variable as a control: turnout in the 1928 election (Clubb, Flanigan, and Zingale 2005). This is important principally because the southern political system's disenfranchisement of African Americans (along with many poor whites) was related to the Democratic vote share before, during, and after the Depression (e.g., Key 1949). (As discussed later, I also control for the size of the African American population and, in robustness tests, exclude the South.)

Proxies for Economic Hardship

The key issue here is to choose proxies for aspects of economic hardship that may have

caused voters to shift from supporting Republicans to supporting Democrats. Given the dramatic rise in unemployment and decline in income prior to the New Deal, a measure of unemployment and a proxy for income decline are obviously appropriate. Because the only Depression-era, pre-New Deal unemployment data are from the 1930 Census, I use 1930 unemployment; the variable is from Fishback, Kantor, and Wallis (2003a, 2003b) and measured as a fraction of gainfully employed workers. Given the nonexistence of county-level income data, I follow Fishback, Kantor, and Wallis (2003a) in using the growth rate in retail sales from 1929 to 1933 as a proxy for the drop in income.

To control for pre-Depression levels of economic activity, I use retail sales in 1929, again following Fishback, Kantor, and Wallis (2003a, 2003b). Note that any time-invariant effects of pre-Depression income (or retail sales) levels on voting should be reflected in my pre-Depression voting variables (discussed earlier). Thus, the principal purpose of including 1929 retail sales is to control for the way in which a starting-point level of retail sales (as distinguished from the 1929 to 1933 decline) was potentially related to voters' reactions to the Depression and the New Deal.

As a supplement to the measures of unemployment and the decline in retail sales, I follow Fishback, Kantor, and Wallis (2003a, 2003b) once again by using the percentage of farmland with crop failures in 1929. This is useful as an additional proxy for economic hardship because unemployment and retail sales data may provide relatively little information about farming communities: A seriously poor harvest (as indicated by a high percentage of land with crop failures) will generally cause hardship, but it may not lead to much measured unemployment among self-employed farmers, and retail sales may not accurately reflect economic activity in areas with few retailers.

I also include variables to proxy for the county-level importance of two additional causes of Depression-era hardship: severe drought in general, and the Dust Bowl in particular. One variable

13

is "months of extreme or severe drought in the 1930s" as calculated by Fishback, Horrace, and Kantor (2005a, 2005b). The other is a dummy indicating Dust Bowl counties, as classified by Hansen and Libecap (2004).

New Deal Spending

To measure spending, I use per capita grants by New Deal programs, March 1933 through June 1939 (Fishback, Kantor, and Wallis 2003a, 2003b).[19] In view of the potential endogeneity issues mentioned earlier, I examine the estimated effects of spending by using state-level per capita land area as an instrument for spending. Using state-level land area to instrument for county-level spending makes use of fact that the allocation of New Deal funds across states depended on different criteria than did the allocation of spending across counties within states. Specifically, state-level per capita spending is highly correlated with state-level per capita land area, in part because of the federal government's use of formula-based allocations (Fleck 2001b, 2008, 2012).[20] Because within-state allocations across counties did not follow the among-state allocation criteria, any given county's allocation of funds depends positively on the land area in the rest of the state, even

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[19]Note that the per capita grants variable does not include loans. In robustness tests discussed in Section III, I add per capita loans as an explanatory variable.

[20]Using state-level data, Fleck (2001b, 299-300) reports that "per capita total land area has a 0.923 correlation with per capita spending by New Deal programs and a 0.994 correlation with per capita spending by the Bureau of Public Roads." For an explanation of the role of various criteria, including land area, in formula-based spending in the 1930s, see Key (1937, 316-366). For an economic analysis of why governments often rely on formulas, see Johnson and Libecap (2003). Fleck (2012) develops a theoretical model that characterizes the political choice between criterion-based (e.g., formula-based) spending a discretionary spending, then applies the model to the New Deal. Also see Levitt and Snyder's (1995) analysis of congressional district level data, from which they conclude, "It appears that parties in the United States can, given enough time, target *types* of voters, but they cannot easily target individual districts" (p. 961). That describes the basic aspect of distributive policy that I exploit here: New Deal per capita spending was high throughout the entire set of states that had much land per capita.

controlling for a variety of county-level variables (including per capita land area).[21]

There are three important caveats to mention with respect to using land area as an instrument. First, because I am using only one instrument, I can estimate the effects of overall New Deal spending, but not program-by-program effects for the New Deal's many programs.[22] Second, New Deal programs differed from each other in terms of how funds were allocated across states; thus, when using land area to instrument for overall spending, one should interpret the results as reflecting the way elections depended on the types of spending for which land area predicts allocations to states. Third, using a state-level variable as an instrument provides only 48 independent observations for that instrument; to account for this, I report t-statistics calculated from robust standard errors based on clustering by state.[23] It would of course be desirable if I could include additional instruments (ideally, strong instruments observed at the county level), but it is not obvious what variables would be valid for that purpose.[24]

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[21]The fact that the criteria used to allocate funds among states differed from the criteria used to allocate funds among counties within states is expected from historical accounts (e.g., Key 1937; Howard 1943) and demonstrated empirically in Section IV.

[22]Although it would be interesting to estimate how each major New Deal program influenced election outcomes, doing so would require a large number of instruments (at least as many instruments as programs).

[23]Reporting non-robust t-statistics would overstate the strength of the instrument.

[24]For example, Levitt and Snyder's (1997) instrumenting strategy would not be valid for my purposes. To estimate the effects of district-level spending, Levitt and Snyder instrument by measuring spending outside the district but within the state. That makes sense for their purpose, which is to address the omitted variable bias caused by representatives' unobserved effort to obtain federal spending – effort that presumably increases when representatives are more likely to lose an election to a challenger. Levitt and Snyder's strategy would not address the bias of concern in my paper, because the available economic data for the 1930s provide no good controls for economic performance during the New Deal, and one would expect that the (necessarily omitted) economic performance of one county would (i) influence the level of New Deal spending in that county and (ii) be correlated with the economic performance of, and hence New Deal spending in, the rest of the state. Other variables, such as those that indicate local geographical characteristics (e.g., Hoxby 2000), may have influenced New Deal spending, but they would typically be correlated with unobservable factors (e.g., transportation costs, characteristics of firms) correlated with exogenous economic conditions – this would render such variables invalid as instruments. Similarly, if a

15

Proxies for International Concerns: German Population, Foreign Born Population

By the 1940 election, international policy – especially regarding war in Europe – had become a major political issue. For this reason, Wright (1974, 37) included a variable measuring the number of German-Americans (as a percentage of population) in his analysis of (differenced) state-level election data. He found a large and highly statistically significant negative estimated effect of the German-American variable on the change in Roosevelt's vote share from 1936 to 1940. In addition to Wright's state-level findings, previous (non-econometric) analysis of county-level data has identified a relationship between changes in vote shares and the size of the German-heritage population (Lubell 1951).[25] In my analysis, I include the size of the German-born population, measured as a percentage of the county's population (Haines and ICPSR 2005). This variable serves two purposes. First, it is a variable of interest when explaining voting patterns in the 1940 election. Second, it is important as a control, because the distribution of German immigrants across counties is correlated with other variables of interest.[26]

I also include the size of the foreign born population excluding German-born, again measured as a percentage of the county's population. This variable can identify the extent to which changes in voter behavior differed between counties with many (non-German) immigrants and other counties.

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demographic or political variable can predict spending, it will likely also be correlated with electoral changes – and not purely through its effect on New Deal spending. Again, this would render such variables invalid as instruments.

[25]As Lubell (1951, 132) reports, "Throughout the country in 1940, Roosevelt's proportion of the major party vote dropped 7 per cent from 1936. There were twenty counties where his loss exceeded 35 per cent – five times the national average. Nineteen of these counties are predominantly German-speaking in background." Lubell also finds that counties where Roosevelt's vote share dropped 20-34 percent had largely German backgrounds.

[26]For example, the size of the German population is negatively correlated with the historical Democratic vote share and positively correlated with drought during the 1930s (Appendix C).

In the context of the Depression era, the variable may reflect both a general native-born versus immigrant political division, as well as specific concerns about foreign policy. Most importantly, however, the variable provides a useful control, especially to ensure that the German variable does not proxy merely for the overall number of immigrants.[27]

Additional Economic, Demographic, and Geographical Variables

As emphasized in the historical literature, the electoral response of farm communities to the onset of the Depression differed from other communities, particularly those with many manufacturing workers and/or predominantly urban residents.[28] Thus, I include variables for the extent of farming (the percentage of the county's land that was devoted to farms), the number of manufacturing workers (as a percentage of the adult population), and the extent to which the population was urban (the percentage of the county's population living in cities of more than 2500 people). There are three reasons why including these variables is important. First, my proxies for economic hardship at the onset of the Depression will fail to capture some aspects of pre-Depression hardship specific to farm communities.[29] Second, farm families (and some other rural families) had the ability to produce a substantial portion of their own food; thus, the extent to which income-related variables proxy for economic hardship differs between farming and urban areas. Third, some New Deal policies (including price supports and regulations) attempted specifically to help farmers,

[27]The German-born population tended to be larger where the non-German foreign-born population was larger (Appendix C).

[28]These are the three key factors Brown (1988) considers in his econometric analysis of the levels and changes in county-level voting behavior in 1928, 1932, and 1936.

[29]As noted earlier, the unemployment variable conveys less information about farmers than about, say, industrial workers. Also, the post-1929 drop in retail sales may be based on an already depressed starting point in farming communities, many of which did not thrive with the rest of the country during the 1920s. Furthermore, although the crop failure variable measures undesirable conditions for farmers, crop failures were only one source of hardship among farmers.

while others (such as wage and hours regulations) mattered more for manufacturing and urban communities. To the extent that these policies influenced voting, there will be farming-manufacturing-urban differences with respect to how well the New Deal spending variable proxies for the electoral effects of the New Deal.

An additional concern is controlling for factors related to both poverty and politics. As a proxy for need related to low human capital, poor educational opportunities, and potentially the degree to which voters were informed, I include the percentage of the population that was illiterate in 1930. Another obvious factor is the economic and political position of the African American population. Most importantly, Africans Americans in the South were, relative to the average American, disproportionately poor and disenfranchised, and the southern political system was dominated by the Democratic Party before and during the 1930s (e.g., Key 1949). For this reason, I include a variable measuring the size of the African American population.[30]

Finally, because I use state-level per capita land area as an instrument for spending, I include county-level per capita land area. The purpose is to control for factors (other than spending) that are correlated with both per capita land area and changes in voting patterns. This is an important part of my econometric approach, because the inclusion of the county-level variable ensures that the instrument is operating through its effect on allocations to states as in Fleck (2001b, 2008, 2012) – and not through the direct effect of county-level land area, which may predict spending but has no

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[30]For interpreting this paper's econometric results, the reader should keep in mind that the African American variable indicates counties with many African Americans, and thus tells us how electorates in counties with many African Americans behaved, but it need not tell us anything about how African Americans *themselves* voted. Indeed, in many counties with large African American populations, very few African Americans voted (e.g., Key 1949; Kousser 1974). In this light, I use the African American variable principally as a control, and I do not attempt to infer the extent to which Democrats gained support among African American voters. It is, of course, clear from the historical literature that the New Deal brought the Democrats new support among many African Americans (e.g., Freidel 1965; Sundquist 1973; Sitkoff 1978); for an economic explanation of how this worked, see Fleck (1999a, 2008, 2011).

clear justification as an instrument.

III. Empirical Results

This section presents the econometric results based on the variables discussed in the previous section.

Post-1928 Gains and Losses, Not Conditional on New Deal

The three regressions in Table 1 provide a useful starting point, because they indicate the way voting patterns changed relative to the pre-Depression era. First, note that the electoral variables' performance across the estimated equations indicates substantial long run stability in the locations of strong Democratic support. For example, consider two hypothetical districts: one that voted 60% for Democrats prior to the Depression (i.e., Dem 1928 and Dem Mean both equal to 60), the other that voted 40% for Democrats (i.e., Dem 1928 and Dem Mean both equal to 40). The estimated coefficients on Dem 1928, Dem Mean, and Dem Mean Squared in Equation 1 would predict that the pre-Depression gap of 20 would shrink to roughly 14.9. So what the data show here is not a reversal in locations of party strength.[31] Rather, we are looking at a realignment in the sense that the electorate as a whole swung toward the Democrats, but with (as will become clearer in the following discussion) substantial between-county heterogeneity in the extent of, and even direction of, the swinging.[32]

Now consider the variables that proxy for the severity of economic hardship at the onset of

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[31]In the very long run, there has been a striking reversal, with the old Democratic South now a Republican stronghold (e.g., Miller and Schofield 2003). See Fleck (1999a, 2008, 2011) on the New Deal origins of this long run realignment.

[32]From 1928 to 1932, the Democratic presidential vote share increased in almost all (2834 of 2861) counties in my data set. Yet from 1932 to 1936, it decreased in the majority (1685) of counties. Then from 1936 to 1940, it again decreased in a majority (2485) of counties. Over the entire Depression era (1928 to 1940), the Democratic vote share increased in the vast majority (2602) of counties.

the Depression: Unemployment 1930, ΔRetail 1929-1933, Crop Failures 1929. All three variables have estimated coefficients of the expected sign, with greater hardship leading to a larger Democratic vote share, ceteris paribus. Yet, while the estimated effects are nontrivial, the magnitudes of the coefficients and levels of statistical significance are rather moderate in the context of the sweeping political changes of the 1930s.

To illustrate, consider what the point estimates indicate with respect to the predicted Democratic vote percentage. An increase in the value of Unemployment 1930 from 0 to its sample mean would increase Dem 1932 by .40, Dem 1936 by 1.04, and Dem 1940 by .56.[33] A drop in the retail sales growth rate from 0 to the sample mean of ΔRetail 1929-1933 would increase Dem 1932 by 1.52, Dem 1936 by 1.52, and Dem 1940 by 1.01. An increase in crop failures from 0 to the mean of Crop Failures 1929 would increase Dem 1932 by .32, Dem 1936 by .17, and Dem 1940 by .09.[34] The sum total of these effects (i.e., moving all three variables from 0 to their sample means) would increase the predicted values of Dem 1932 by 2.24, Dem 1936 by 2.72, and Dem 1940 by 1.66. Such changes in vote percentages would clearly matter to politicians, but alone would be insufficient to account for the era's landslide changes in voting patterns.

Now consider tests of statistical significance. For 1932, two of the three variables (ΔRetail 1929-1933, Crop Failures 1929) reach marginal levels of statistical significance. For 1936, two of

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[33]Obviously, zero unemployment would not be typical even in the absence of the Depression, but it provides a useful baseline for illustrating the relatively small magnitudes of the estimated effects. For interpreting this illustration, note that the unemployment variable's mean (2.87) and standard deviation (2.22) are similar in their rough order of magnitude.

[34]Note that the mean value of Crop Failures (which is measured as a percentage of total farmland) does not differ greatly between counties with much farmland and counties with little farmland. (For counties with Farmland > 50, the mean of Crop Failures is 3.02. For counties with Farmland < 50, the mean of Crop Failures is 3.63.) Therefore, the illustration discussed above (which is based on the sample mean of Crop Failures) can be viewed as reasonably representative of an illustration that uses the mean of Crop Failures among farming-oriented counties.

the three variables (this time Unemployment 1930 and ∆Retail 1929-1933) reach marginal levels of statistical significance. For 1940, none of the three variables has a statistically significant effect. Jointly, the three variables are statistically significant for 1936 only (with p=.1459 for 1932, p=.0655 for 1936, and p=.3695 for 1940). In short, the statistical relationships between Democratic vote shares and proxies for economic hardship are not particularly strong.

What about the effects of severe drought and the Dust Bowl? The estimated effects of the Dust Bowl variable are positive. Most notably, for 1936 the Dust Bowl effect is statistically significant (t=3.50) and substantial, with a positive Dust Bowl dummy predicting Dem 1936 to be higher by 5.08. By contrast, the estimated effects of the Drought variable are negative. To gain some insight into the empirical role of these variables, consider the combined estimated effects of changing the values of both variables from zero to their sample means. The combined effect is, for the country as whole, negative for 1932 and 1940, and very near zero for 1936, because the Drought effects offset the Dust Bowl effects.[35] Even within the sample of Dust Bowl counties, the combination of increasing both variables from zero values to their sample means (i.e., Dust Bowl county sample means) increases the predicted Democratic vote share by a large amount only in 1936.[36] Overall, the incidence of severe drought and the Dust Bowl appears not to have shifted counties very far toward Roosevelt. Of course, given the purpose of my analysis (as discussed in Sections I and II), the substantive question here does not involve whether rain or dust per se actually shaped partisan alignments. Rather, what the results indicate is that voters did not overwhelmingly

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[35]Moving from zero values to the sample means *decreases* the predicted value of Dem 1932 by .45, *increases* the predicted value of Dem 1936 by .03, and *decreases* the predicted value of Dem 1940 by 1.39.

[36]For the sample of Dust Bowl counties, which had a mean Drought value of 39.3, moving from zero to the sample means of the two variables increases the predicted value of Dem 1932 by 1.02, increases the predicted value of Dem 1936 by 4.99, and decreases the predicted value of Dem 1940 by .62.

flock to Roosevelt in locations where a strong federal response to drought and dust would have been particularly valuable.

The German variable suggests a realignment over the course of the 1930s. More specifically, the variable has a substantial positive estimated coefficient (1.23, with t=3.37) for 1932, a near-zero estimated coefficient (.05, with t=.13) for 1936, and a substantial negative estimated coefficient (-2.05, with t=4.61) for 1940. For interpreting these results, recall that the German variable is based on the number of each county's residents who were *born* in Germany. Of course, measuring the number of German-born residents per se is not the point of the variable. Rather, it is a proxy for a broader form of German affiliation within the electorate in each county. With that in mind, consider a hypothetical county with, say, 2% of its residents born in Germany, compared to an otherwise identical county with only 1% of its population born in Germany. For 1932, the county with 2% born in Germany would have a predicted Dem 1932 that was 1.23 greater than that of the other county. But for 1940, the county with 2% born in Germany would have a predicted Dem 1940 that was 2.05 *less* than that of the other county.[37]

Given the political environment of the 1930s, how should one interpret the realignment related to the size of the German population? The negative estimated effect of the German variable on Dem 1940 is consistent the escalation World War II reducing support for Roosevelt in German-background counties (because of Roosevelt's position on the war relative to the typical Republican position). That said, one must recognize that the estimated coefficient on the German variable declined from 1932 to 1936, not just from 1936 to 1940. This suggests that the decline from 1936 to 1940 may be the result of some change that started in the early 1930s (when domestic policy still

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[37]For interpreting this illustration, note that the German variable has a mean of .82 and a standard deviation of 1.12.

dominated U.S. politics). Thus, while the data show that German-background counties exhibited a reduction in their support for Roosevelt, confirming Lubell (1951) and Wright (1974), it is unclear how much of that reduction can be attributed to international concerns.

The Farmland, Manufacturing, and Urban variables also provide insight into the nature of the electoral realignment that took place during the Depression. For 1932, the estimated coefficients are .065 (t=3.29) on Farmland, -.109 (t=1.88) on Manufacturing, and -.054 (t=3.46) on Urban. But those estimated effects are mostly gone by 1936, and then reversed in sign by 1940, with estimated coefficients of -.018 (t=.88) on Farmland, .129 (t=2.19) on Manufacturing, and .029 (t=1.46) on Urban. Thus, manufacturing-oriented and urban counties showed inter-temporally increasing support for Roosevelt, while farming counties showed inter-temporally decreasing support, ceteris paribus.

Post-1932 Gains and Losses, Conditional on New Deal Spending

I will now turn to an analysis of Roosevelt's post-1932 gains and losses. In Table 2, the first four specifications control for Dem 1932, and either add Spending as an explanatory variable or use State Land to instrument for Spending. The first two specifications seek to explain Dem 1936, and the second two seek to explain Dem 1940.

What do the results in Table 2 add to those in Table 1? First, consider the econometric role of Spending. In Equation 1, the OLS coefficient on Spending is .0044 (t=1.71). This point estimate predicts that having the average value of Spending ($153), compared to no spending, would yield a .67 increase in Dem 1936 (conditional on Dem 1932 and the other explanatory variables). Of course, the potential endogeneity of Spending with respect to political variables implies that the

estimator of that coefficient could be biased upward or downward.[38] Equation 2 presents 2SLS

results that use State Land as an instrument to address the problem of potential bias. (For the first-

stage results, see Appendix D.) Although the effect of Spending remains imprecisely estimated, the

point estimate does increase substantially, to .0279 (t=1.03). This predicts that having the average

value of Spending ($153), compared to no spending, would yield a 4.26 increase in Dem 1936.

Although the .0271 standard error for the 2SLS coefficient on Spending is too large to allow any

clear conclusions, the results are consistent with a substantial effect of New Deal spending on the

Democratic vote share for 1936.[39]

By 1940, the effect of Spending may have diminished. The OLS specification in Equation

3 yields an estimated coefficient of .00034, which is small and statistically insignificant (t=.18). The

2SLS specification in Equation 4 yields an estimated coefficient of .0109, which is smaller than for

1936, but again imprecisely estimated (t=.44).[40]

For comparing how the other variables perform in Equations 1-4 of Table 2 with how they

perform in Table 1, the critical point to remember is that the equations in Table 2 control for Dem

1932 (as well as Spending). Hence, any apparent realignment effects in Table 2 are those that

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[38]For example, the bias could be upward if New Dealers allocated funds to accelerate vote gains in counties where support for Democrats was increasing exogenously, while the bias could be downward if New Dealers allocated more funds where the per vote cost of vote-buying was high.

[39]As discussed in Section II, one consequence of my identification strategy is that I have only 48 independent observations for my instrument (State Land). This limits the precision of my 2SLS estimator. Note, for example, that if I reported non-robust t-statistics (i.e., if I ignored the fact that using State Land as an instrument generates non-spherical disturbances), the reported confidence intervals would be much narrower, generating t=3.21 for the 2SLS coefficient on Spending (instead of t=1.03, as reported in Equation 2 of Table 2). Similarly, the non-robust first-stage t-statistic on State Land would be 6.33 (instead of 2.17, as reported in Equation 1 of Appendix D).

[40]Using the point estimate, the average value of Spending ($153), compared to no spending, would yield a 1.66 increase in the predicted value of Dem 1940.

occurred after the New Dealers came to power. The results again demonstrate shifting support among German-affiliated, manufacturing-oriented, urban, and farming counties, with German and farming counties showing decreasing support for Democrats, and manufacturing-oriented and urban counties showing increasing support for Democrats, ceteris paribus.[41]

Post-1936 Gains and Losses, Conditional on New Deal Spending

Equations 5 and 6 in Table 2 repeat the specifications in Equations 3 and 4, but with one change: including Dem 1936 instead of Dem 1932 as an explanatory variable. The purpose is to test more directly what the previous specifications suggest about the net drop in support that occurred between the overwhelming Democratic landslide of 1936 and the more modest Democratic victory of 1940. The estimated effects of Spending are now negative, with an OLS coefficient of -.0038 (t=1.91) in Equation 5, and a 2SLS coefficient of -.0143 (t=1.06) in Equation 6. This suggests (consistent with Equations 3 and 4) that the effect of Spending at the margin may have been transitory, with modest gains for 1936 fading by 1940. Consistent with the results discussed earlier, German-affiliated counties moved away from the Democrats between 1936 and 1940, as did farming counties, with manufacturing-oriented and urban counties moving toward Democrats, ceteris paribus.

Did the Swing Counties Swing?

Did the Democrats experience large electoral gains in swing counties and, if so, when?

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[41]Two other results are suggestive, though not conclusive, regarding the nature of the findings in Table 1. First, Equation 2 in Table 2 (2SLS for Dem 1936) yields a negative estimated coefficient on Dust Bowl (as well as on Drought). This suggests that the 1936 positive Dust Bowl effect, as estimated in Equation 2 of Table 1, may have worked through the value of New Deal programs in those counties. Second, there is an additional reason for skepticism with respect to interpreting the German variable as a proxy for voter preferences over international policy: The first stage of the 2SLS estimation indicates that a larger German population predicts lower spending (Equation 1 in Appendix D). It thus appears that the German variable is correlated with some omitted variable that influenced New Deal policy. Hence, it is plausible that when the German variable predicts declining support for Democrats, it may be proxying, at least in part, for something other than preferences over international policy.

Controlling for pre-Depression electoral variables (Table 1), the estimated coefficient on Dem SD is positive for all three elections, yet shrinking from one election to the next: .481 (t=3.66) for Dem 1932, .400 (t=3.47) for Dem 1936, and .184 (t=1.70) for Dem 1940. These results show that swing counties did more swinging toward the Democratic Party in 1932, but that the magnitude of the swing-county effect declined over the course of the next two elections.[42]

This leads to a related question: Did the electoral swing *away* from the Democrats between the 1936 and 1940 elections occur disproportionately in swing counties? The answer is yes. The results discussed in the previous paragraph suggest this, but the question is better addressed by Equations 5 and 6 in Table 2, because these specifications control for Dem 1936. The estimated coefficients on Dem SD are -.133 (t=2.03) for the OLS specification and -.126 (t=1.91) for the 2SLS specification. In sum, swing counties tended to move disproportionately toward the Democrats from the pre-Depression period to the early New Deal years, but then tended to move disproportionately away between 1936 and 1940.

What Happened Outside the South?

Given that the Democratic Party's pre-Depression electoral strength was so heavily concentrated in the South, it is clear that the Democrats' rise to national-level dominance required gaining many votes outside the South. For this reason, when attempting to explain econometrically the Democrats' electoral gains in the Depression era, it is essential to consider the extent to which the econometric results actually reflect what happened outside the South. To address this issue, I excluded the South (defined here as the eleven Confederate states) from the data set, then re-

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[42]To illustrate the magnitude of these effects, consider a .481 coefficient, which is the peak (i.e., 1932) estimated effect (Table 1, Equation 1). Increasing the value of Dem SD by 4.91 (a one-standard-deviation change in the variable) would increase the predicted value of Dem 1932 by 2.36.

estimated the equations in Tables 1 and 2.

Given the relative lack of inter-party competition in the South, perhaps the most obvious question is whether the estimated effects of Dem SD depend on between-region differences. Excluding the South from the equations in Table 1 generates positive, but smaller, estimated coefficients for 1932 and 1936, and a near-zero estimated coefficient for 1940: .281 (t=2.33) for Dem 1932, .282 (t=1.90) for Dem 1936, and -.011 (t=.08) for Dem 1940. Turning to the specifications that examine the swing away from Roosevelt between 1936 and 1940 (Table 2, Equations 5 and 6), excluding the South roughly doubles the estimated effects, with coefficients of -.251 (t=1.93) for the OLS specification and -.263 (t=3.04) for the 2SLS specification. In sum, it appears that for the non-South, as for the country as a whole, the historically (i.e., pre-New Deal) swing counties did more Depression-era swinging, both toward and away from the Democrats.

Three additional points are worth noting. First, excluding the South does not substantially change the estimated effects of the early Depression's severity.[43] Second, the results show clearly that the realignment related to the German, Farmland, Manufacturing, and Urban variables is not an artifact of between-region differences involving the South (with its disproportionately fewer immigrants, fewer urban residents, and unique political institutions) and the rest of the country. Third, turning to the post-1932 and post-1936 effects, the South-excluded results indicate no major reinterpretations of the previous tables. Some of the estimated effects of spending are modestly larger outside the South (and again have standard errors too large to allow firm conclusions), but they

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[43]With the South excluded, I repeated my earlier exercise to examine jointly the effects of Unemployment 1930, ΔRetail 1929-1933, and Crop Failures 1929. Moving all three variables from 0 to their South-included sample means increases the predicted values of Dem 1932 by 2.83, Dem 1936 by 3.15, and Dem 1940 by .91. Tests of joint significance for the three variables generate p=.0333 for Equation 1, p=.1454 for Equation 2, and p=.8304 for Equation 3.

do not suggest that the inclusion of the South (a low spending region with unique political institutions and electoral history) in Table 2 masks a major non-southern vote-buying effect. In short, excluding the South confirms the results in Tables 1 and 2.

Heteroskedasticity and Heterogeneous Responses

Before discussing several issues related to heteroskedasticity and heterogeneous responses, a preliminary point is in order. Recall that the t-statistics reported in Tables 1 and 2 are based on robust standard errors clustered by state. Hence, the estimation method has already dealt with (i) the generic problem of heteroskedasticity rendering the standard errors (and t-statistics) inconsistent and (ii) the problem of assuming that shocks are independent across county-level observations when a single shock (e.g., state-level policy) might affect an entire state.[44]

Population-Related Heteroskedasticity

Now consider another potential concern related to heteroskedasticity: The coefficients estimated in Tables 1 and 2 are based on small population counties having the same econometric weight as large population counties. Yet, given the units in which the regressions are specified (with vote shares rather than total votes cast on the left hand side, and appropriately matched variables on the right), small population counties might have larger expected squared errors. Furthermore, they might also be unrepresentative of the entire country. One cannot rule out the importance of this issue ex ante, because county population ranges so widely: from 241 individuals to nearly 4 million. To address these concerns, I tested for population-related heteroskedasticity using the OLS regressions

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[44]My reporting of robust test statistics is the prudent approach. First, heteroskedasticity is clearly present: For each of the OLS regressions reported in Tables 1 and 2, I used a Breusch-Pagan test for heteroskedasticity related to the full set of explanatory variables, and in each case the test rejected homoskedasticity with $p<.00005$. Second, relying on non-robust estimates of standard errors would lead to a reliance on dubiously narrow confidence intervals (substantially so for some of the main variables of interest).

reported in Tables 1 and 2, re-estimated those regressions using population-weighted least squares,

and then tested the WLS regressions for population-related heteroskedaticity.

This yielded two conclusions. First, comparing each WLS equation to its OLS counterpart,

there is stronger evidence of heteroskedasticity in the WLS equation.[45] The finding that weighting

by population appears to exacerbate population-related heteroskedasticity supports my decision to

report in Tables 1 and 2 the results of unweighted regressions with robust t-statistics. Second, there

is an interesting difference between the weighted and unweighted regressions: The weighted

regressions yield larger estimated coefficients and associated t-statistics on the two principal proxies

for the severity of the Depression's onset: Unemployment 1930 and ΔRetail 1929-1933.[46] This

leads to the next econometric issue.

Population-Related Heterogenous Responses

The WLS results suggest that larger-population counties had substantially stronger marginal

responses to the onset of the Depression. To test this more directly, I re-estimated the equations in

Table 1, but restricted the sample to the set of counties with populations above the sample mean.

This confirmed the heterogeneous responses suggested by the population-weighted regressions:

Restricting the sample to counties with above-mean populations amplifies the estimated effects of

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[45]In the WLS specifications, a Breusch-Pagan test rejects the null hypothesis (of no heteroskedasticity related to population) very strongly, with p<.00005 in each case. In the OLS specifications, the null hypothesis can be rejected in only some specifications; the p-values range from strong rejection (p=.0013) to no rejection (p=.9195).

[46]When the equations in Table 1 are re-estimated using WLS, the estimated coefficients on Unemployment 1930 are .477 (t=2.55) for Dem 1932, .727 (t=3.18) for Dem 1936, and .738 (t=3.12) for Dem 1940. The coefficients on ΔRetail 1929-1933 are -4.59 (t=2.20) for Dem 1932, -6.42 (t=3.12) for Dem 1936, and -5.44 (t=3.24) for Dem 1940. When the South is excluded, the estimated coefficients on Unemployment 1930 are .479 (t=2.84) for Dem 1932, .681 (t=2.77) for Dem 1936, and .691 (t=3.08) for Dem 1940. For ΔRetail 1929-1933, they are -9.54 (t=5.07) for Dem 1932, -9.87 (t=3.86) for Dem 1936, and -6.64 (t=3.53) for Dem 1940.

Unemployment 1930 and ΔRetail 1929-1933. Similarly, restricting the sample to counties with below-mean populations generally shrinks the estimated effects of Unemployment 1930 and ΔRetail 1929-1933.[47]

This type of population-related heterogeneity in responses matters for understanding the *national-level* Democratic gains generated by the Depression: Because large population counties generally had more voters than did small population counties, between-county heterogeneity in the marginal effects of economic hardship led to greater gains in vote totals (compared to a homogeneous response equal to the among-county average response). For example, the 537 counties with above-mean populations are fewer than 19% of the counties in the data set, yet had more than 65% of the population.[48] To illustrate the effects estimated from the restricted sample (i.e., from counties with above-mean populations), consider the how the predicted Democratic vote shares increase as a result of moving the values of both Unemployment 1930 and ΔRetail 1929-1933 from 0 to their entire-country sample means: Dem 1932 increases by 4.10, Dem 1936 by 4.62, and Dem 1940 by 3.28.[49] If the exercise is conducted by moving the values of both variables from 0 to the

[47]Re-estimating the equations in Table 1 using the 537 counties with above-mean populations, the estimated coefficients on Unemployment 1930 are .700 (t=3.16) for Dem 1932, .607 (t=2.20) for Dem 1936, and .407 (t=1.45) for Dem 1940. Using the 2324 counties with below-mean populations, the estimated coefficients are smaller: .033 (t=.16) for Dem 1932, .215 (t=1.21) for Dem 1936, and .069 (t=.31) for Dem 1940. Turning to the estimated effects of ΔRetail 1929-1933, restricting the sample to above-mean populations yields the following estimated coefficients: -4.39 (t=1.90) for Dem 1932, -6.05 (t=1.96) for Dem 1936, and -4.39 (t=1.43) for Dem 1940. Again the sample with below-mean populations yields smaller effects: -3.28 (t=2.22) for Dem 1932, -2.68 (t=1.89) for Dem 1936, and -1.52 (t=1.11) for Dem 1940.

[48]Recall that from 1932 to 1936, Roosevelt's national-level vote share increased from 57.4% to 60.8%, while his vote share declined in most (1685 of 2861) counties. Yet among the 537 counties with above-mean populations, Roosevelt's vote share increased in 384. These simple descriptive data make clear the importance of Roosevelt's gain in vote shares among the populous counties. And that, in turn, points to the importance of explaining why his vote share increased in those populous counties.

[49]Tests of joint significance for the two variables generate p=.0028 for Equation 1, p=.0082 for Equation 2, and p=.0329 for Equation 3.

restricted-sample mean values, the corresponding increases in the Democratic vote share are even

larger (5.13, 5.44, and 3.84), because the more populous counties had higher unemployment. In

plain language, the Democrats had bigger hardship-driven gains in counties where there were more

voters.

Do the Democratic Party's post-1932 gains and losses also reveal population-related

heterogenous responses? Yes. To investigate this, I re-estimated the equations in Table 2, with the

sample restricted to counties with above-mean populations. Once again, this causes a fundamental

change in the estimated equations – but this time in the estimated effects of Spending. There is an

important caveat here: Dropping low-population (or high-population) counties from the sample

leaves only pieces of states in the data set.[50] That said, the estimated effects of Spending on Dem

1936 and Dem 1940 (controlling for Dem 1932) are much larger than for the entire-country sample.[51]

It thus appears likely that New Deal spending had a larger marginal influence on Democratic vote

shares in counties where the Democratic vote share had greater weight (i.e., more votes) in winning

national elections.

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[50]More specifically, the caveat with respect to running 2SLS on a subset of counties is that my 2SLS
regressions reflect the estimated effect of state-level spending, but may attribute the (potentially large)
electoral effects of high spending *throughout* a state to spending in the restricted group of counties (which
may not have spending as high as in the omitted counties). In other words, if very high spending *throughout*
a state (not just in the counties in the restricted sample) influenced voting patterns in the restricted-sample
counties, then my 2SLS estimator of the coefficient on Spending will reflect, in essence, the effects of more
funds than the Spending variable actually counts. In this case, the expected 2SLS coefficients will correctly
indicate the direction of the spending effect, but overstate the magnitude of that effect.

[51]For 1936, the estimated coefficients on Spending are .0230 (t=2.31) for Equation 1 (OLS) and
.2411 (t=2.36) for Equation 2 (2SLS). For 1940, the estimated coefficients are .0152 (t=1.87) for Equation
3 (OLS), and .2089 (t=2.12) for Equation 4 (2SLS). In the specifications that predict Dem 1940 controlling
for Dem 1936, the estimated coefficients are negative (-.0037 with t=.75) in Equation 5 (OLS) or positive
(.0166 with t=.29) in Equation 6 (2SLS). Using the sample of counties with below-mean populations, the
estimated coefficients on Spending in Equations 1-6 are: .0040 (t=1.63), .0169 (t=.73), .0001 (t=.05), .0048
(t=.21), -.0037 (t=1.88), and -.0106 (t=.94).

Additional Controls

Although I have included in Tables 1 and 2 the full set of available variables that, in my view, should be included on a priori grounds, it is useful to add controls to test the robustness of the results. First, while the Spending variable used in Table 2 measures the main component of New Deal distributive policy, it does not include an important secondary component: loans. Were loans an important influence on voting patterns and/or does controlling for loans cause any major changes in the estimated effects of other variables? To address these questions, I re-estimated the OLS specifications in Table 2 with the addition of per capita loans as an explanatory variable.[52] In each specification, the estimated coefficient on the loan variable was negative and statistically insignificant (t<.20), and the addition of the loan variable produced only very minor changes in the other results.[53]

Second, in view of the fact that the marginal effects of two key variables (Unemployment 1930, ΔRetail 1929-1933) differ substantially between the set of large-population counties and the set of small-population counties, it is worth asking whether adding population directly as an explanatory variable has any important effects. For this reason, I re-estimated the equations in Tables 1 and 2, with the addition of two explanatory variables (the county's population, the inverse of the county's population) to proxy for factors related to population that might be correlated with

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[52]To estimate 2SLS regressions that instrumented for both spending and loans, I would need at least two instruments, and I have only one (State Land).

[53]In further robustness tests, I added the loan variable to the regressions estimated for each of the three restricted samples discussed earlier: the South excluded, counties with above-mean populations, and counties with below-mean populations. Again, the loan variable was statistically insignificant and caused only minor changes in the other results.

spending, economic hardship, and electoral behavior.[54] The population variables do not indicate any clear relationship between population and changes in the Democratic vote share, and, most importantly, controlling for the population variables produces only minor changes in the other results.[55]

Remaining Questions: Types, Timing, and Effectiveness of New Spending

Before concluding, consider what this paper does and does not do. To keep the analysis tractable, the empirical focus has been on exploiting heterogeneity in county-level presidential vote shares in order identify the factors underlying durable (versus transitory) shifts in partisan leanings. This adds much to the previous literature (Wright 1974; Brown 1988), but it also points to additional questions regarding the electoral effects of New Deal policy. First, to what extent did different types of New Deal spending, which were allocated in different patterns (e.g., Reading 1972; Fishback, Kantor, and Wallis 2003a) and had different economic effects (Fishback, Horrace, and Kantor 2005b), have different electoral effects? Second, did temporary increases in swing-state spending (Fleck 1999a) give rise to electoral effects that were substantial but of short duration, or did swing-state voters' growing disenchantment with Democrats cause a reduction allocations to swing states? Third, rather than spending itself being the direct influence on voters, might the more important causal factor be the effectiveness of spending in alleviating suffering and, as Wright (1974)

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[54]Adding the pair of variables (population and the inverse of population) allows for nonlinear effects of population (e.g., there could be important differences between small-population and medium-population counties, but not between medium-population and large-population counties, or vice versa). There is a only modest correlation (-.135), not a high correlation, between population and the inverse of population (Appendix C).

[55]In further robustness tests, I added the population variables to the regressions estimated for each of the three restricted samples discussed earlier: the South excluded, counties with above-mean populations, and counties with below-mean populations. Again, the population variables fail to indicate any clear relationship between population and changes in the Democratic vote share. And again, controlling for the population variables produces only minor changes in the other results.

emphasizes, in providing employment? These questions are necessarily beyond the scope of this paper because addressing them will require the development of identification strategies that can overcome the lack of data (state-level and county-level) on economic conditions during the New Deal years.[56]

IV. Conclusion

Why did the electorate swing so far in the Democratic Party's favor during the 1930s? It has long been obvious that two factors mattered: The Depression began under a Republican regime, and Democrats implemented popular policy. In this paper, I estimate how much county-level Democratic vote shares responded *at the margin* to economic hardship, New Deal spending, and other factors. Examining these marginal effects is particularly important because the Democrat Party's electoral success did not come from uniform gains across the county. My econometric analysis provides new insight into why some counties swung a long way toward the Democrats, others swung a small way toward the Democrats, and still others swung in the opposite direction.

Where did the Democratic Party experience the greatest gains? Pre-New Deal economic hardship mattered: Democratic gains were greater in counties where the Depression hit harder. And the hardship-induced gains were substantial in counties with relatively large populations (where most people, and most voters, lived). The level of spending may have mattered appreciably for the 1936 election, but the effects appear transitory (quite plausibly gone in the typical county by 1940). Like

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[56]The paucity of data has long been the principal obstacle for scholars seeking to conduct econometric studies of voting, public policy decisions, and economic outcomes during the 1930s. As noted in Section II, identifying valid instruments for spending is challenging. One potential strategy for sorting out which programs had larger or smaller effects on voting (and how the timing of spending mattered) is to make use of the differences between criterion-based spending and discretionary spending, as identified in (Fleck 2012). My preliminary findings (in which I am not yet confident enough to report) suggest that having more land-oriented spending (which would include highway spending and reclamation projects) in a state is associated with smaller (and more transitory) electoral effects.

the economic hardship effects, spending effects may have been larger in more populous counties. Counties that experienced severe droughts did not show large upswings in Democratic support. Even counties with Dust Bowl conditions showed a transitory vote effect (large in 1936, mostly gone by 1940). Counties that had swing electorates prior to the Depression had relatively large swings toward Democrats initially, then subsequently had back-swings that exceeded those in other counties. Urban and manufacturing-oriented counties show increasing Democratic support over the course of the New Deal, while farming counties show decreasing Democratic support, ceteris paribus. A larger German-born population also predicts falling Democratic support, ceteris paribus; this is consistent with Wright's (1974) and Lubell's (1951) findings, but it may or may not reflect the increasing prominence of international policy as fighting escalated in Europe.

Overall, this paper's results fit what one should expect from a rational electorate. It would be highly surprising if a huge economic downturn did not harm the electoral success of the party in power – voters can rationally interpret a huge downturn as a negative signal (though not a perfect signal, of course) about the quality of policy. Yet the electorate would appear rather myopic if enormous numbers of voters switched their support from Republicans to Democrats in the *long run* solely because their own county was hit particularly hard by a nationwide macroeconomic downturn that started under a Republican regime but lasted a decade. The electorate would also appear myopic if enormous numbers of voters switched parties in the long run because of local drought conditions, or because of largely temporary (e.g., Fleck 1999a) regional favoritism in the distribution of federal funds.

Finally, what does the electoral success of the New Dealers tell us about the prospects for major political realignments? Despite recurrent macroeconomic downturns and politicians' incentives to engineer electoral gains, large and durable political shifts are rare (e.g., Sundquist 1973;

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Brady 1988; Poole and Rosenthal 1997). The results in this paper help to explain why. Even for the 1930s – the quintessential example of an extreme downturn with a massive increase in non-military spending – the evidence that the electorate responded in a large and durable manner to economic hardship and local spending is modest. These new findings complement previous arguments that what we often refer to as the "New Deal Realignment" can be viewed as a fundamental change in the way multiple policy issues – including civil rights and regulation of the economy – were logrolled into partisan positions (Sundquist 1973; Brady 1988; Fleck 1999a, 2002, 2008, 2011). Moreover, the process of realignment did not end in the 1930s (e.g., Miller and Schofield 2003).

Thus, history tells us that a major realignment will likely require more than a severe downturn combined with an aggressive policy response to that downturn. In this light, it is not surprising that modern politicians – including those who had hoped the recent recession would lead to a "New New Deal" and a durable partisan shift – still look back to Roosevelt with envy.

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Variables Used in Table 1, Table 2, and Appendix D

Dem1928, Dem1932, Dem1936, and Dem1940: Each variable is the county's Democratic vote share in a presidential general election (either 1928, 1932, 1936, or 1940), measured as a percentage. From Clubb, Flanigan, and Zingale (2005).

Dem Mean: The county's mean Democratic vote share in presidential general elections over the period 1896 to 1928, measured as a percentage. Directly from Fishback, Kantor, and Wallis's (2003a, 2003b) mean9628.

Dem Mean Squared: The square of Dem Mean.

Dem SD: The standard deviation in county's Democratic vote share in presidential general elections over the period 1896 to 1928, measured as a percentage. Directly from Fishback, Horrace, and Kantor's (2005a, 2005b) std9628.

Turnout: Turnout in the 1928 presidential general election, measured as a percentage of the estimated number of eligible voters. From Clubb, Flanigan, and Zingale (2005).

Unemployment 1930: Number of unemployed workers as a fraction of the gainfully employed, 1930. Directly from Fishback, Kantor, and Wallis's (2003a, 2003b) pctunem.

ΔRetail 1929-1933: Growth rate in retail sales per capita, from 1929 to 1933 (measured as the difference in log retail sales). Directly from Fishback, Kantor, and Wallis's (2003a, 2003b) rldf3329.

Retail Sales 1929: Retail sales per capita in 1929. Directly from Fishback, Kantor, and Wallis's (2003a, 2003b) rtsapc29.

Crop Failures 1929: The percentage of farmland with crop failures in 1929. Directly from Fishback, Kantor, and Wallis's (2003a, 2003b) pctfail.

Drought: Fishback, Horrace, and Kantor's (2005a, 2005b) "months of extreme or severe drought in the 1930s" (mxsd30s).

Dust Bowl: A dummy variable equal to one for Dust Bowl counties, as identified by Hansen and Libecap (2004). My source of the variable is Fishback, Horrace, and Kantor's (2005a, 2005b) dustbowa.

Spending: County-level per capita expenditure on New Deal grants, from March 1933 through June 1939 (based on 1930 population). Calculated from data in Fishback, Kantor, and Wallis (2003a, 2003b) as follows: ndexp/pop30.

State Land: State-level square miles of land per capita (using 1930 population). Calculated from two of Fishback, Kantor, and Wallis's (2003a, 2003b) county-level variables: areapop and pop30.

German: The size of the German-born population, measured as a percentage of the county's population. More precisely, the number of whites born in Germany, divided by the county population, put in percentage terms: (pbwgerm/totpop)*100, where pbwgerm and totpop are both 1930 Census variables from Haines and ICPSR (2005).

Other Foreign Born: Percent of population foreign born, excluding those born in Germany. I calculated this variable using pforb3, which is the 1930 Census "sum of foreign born white males and females" measured as a percentage of the population (Fishback, Horrace, and Kantor 2005a, 2005b), and the German variable (described above) that I calculated from 1930 Census data. More precisely: Other Foreign Born = pforb3 - (pbwgerm/totpop)*100.

Farmland: The percentage of the county's land that was devoted to farms. Calculated from Fishback, Kantor, and Wallis's (2003a, 2003b) pctfrmln. I made one change to that variable: For the observations coded with more than 100% of the county's land devoted to farms, I reset the variable equal to 100; this is consistent with Fishback, Horrace, and Kantor's (2006a, 2006b) treatment of the variable.

Manufacturing: Manufacturing employees in1929, measured as a percentage of the adult population, from 1930 Census. My source of the variable is Fishback, Horrace, and Kantor's (2005a, 2005b) manemp3a.

Urban: The percentage of the county's population that lived in cities with more than 2500 people (1930). Directly from Fishback, Kantor, and Wallis's (2003a, 2003b) pcturb3.

Illiterate: The percentage of the population that was illiterate in 1930. Directly from Fishback, Kantor, and Wallis's (2003a, 2003b) pctill30.

African American: The percentage of the county's population that was African American, measured in 1930. Directly from Fishback, Kantor, and Wallis's (2003a, 2003b) pctblk30.

County Land: County-level per capita square miles of land (using 1930 population). Directly from Fishback, Kantor, and Wallis's (2003a, 2003b) areapop.

Variables Used in Robustness Tests

Loans: County-level per capita New Deal loans, from March 1933 through June 1939 (based on 1930 population). Calculated from data in Fishback, Kantor, and Wallis (2003a, 2003b) as follows: loan/pop30.

Population: The county's population in 1930. Directly from Fishback, Kantor, and Wallis's (2003a, 2003b) pop30.

<u>1/Population</u>: (1/Population)(100,000), with Population as defined above. Directly from Fishback, Kantor, and Wallis's (2003a, 2003b) invpop.

Sample Coverage

  I included counties in my data set if and only if all of following hold true. First, the ICPSR codes match between my four data sources (Fishback, Kantor, and Wallis 2003a, 2003b; Fishback Horrace, and Kantor 2005a, 2005b; Clubb, Flanigan, and Zingale 2005; Haines and ICPSR 2005). Second, the 1930 Census population data match between the three data sets that include population data (Fishback, Kantor, and Wallis 2003a, 2003b; Fishback, Horrace, and Kantor 2005a, 2005b; Haines and ICPSR 2005). Third, there are no missing variables. This yields 2861 observations.

Appendix B
Descriptive Statistics

<u>Notes</u>: Sample is 2861 counties. See Appendix A for definitions of variables.

	Mean	Std. Dev.	Min	Max
Dem 1940	59.36225	20.26942	8.3	100
Dem 1936	65.77648	17.83105	10.9	100
Dem 1932	67.53146	17.81709	15	99.8
Dem 1928	43.92555	17.98882	3.3	100
Dem Mean	48.75178	18.51749	7.466274	99.04321
Dem Mean Squared	2719.513	2088.18	55.74524	9809.558
Dem SD	10.20236	4.913345	1.136188	44.73776
Turnout	53.56326	23.00847	2.5	98.9
Unemployment 1930	2.871053	2.215195	0	17.88321
ΔRetail 1929-1933	-.4758138	.2283588	-1.548113	1.019282
Retail Sales 1929	285.0972	135.5142	5.870956	945.1987
Crop Failures	3.185411	4.059659	0	90.90909
Drought	23.37065	17.11282	0	85
Dust Bowl	.0157288	.1244459	0	1
Spending	152.6159	138.5802	20.60106	3903.034
State Land	.0514717	.1100161	.001552	1.206056
German	.8170569	1.120277	0	8.586957
Other Foreign Born	4.155663	5.424875	0	31.43746
Farmland	64.83386	27.20587	0	100
Manufacturing	5.603081	7.375567	0	73.66907
Urban	21.69013	24.83222	0	100
Illiterate	5.168558	5.813269	0	50.2906
African American	10.34579	17.85533	0	85.82867
County Land	.1028238	.2688503	.0000623	4.586112
Loans	71.35709	67.6504	0	1511.274
Population	38412.19	122022	241	3982123
1/Population	8.62267	14.07391	.0251122	414.9377

Appendix C
Correlation Matrix

Notes: Sample is 2861 counties. See Appendix A for definitions of variables. In the table below, the variable names are abbreviated to save space, but match the order of the variables as listed in Appendix A.

	Dem 40	Dem 36	Dem 32	Dem 28	DemMean	DemMeanSq	Dem SD
Dem 40	1.0000						
Dem 36	0.9453	1.0000					
Dem 32	0.8456	0.9040	1.0000				
Dem 28	0.7284	0.7492	0.7886	1.0000			
DemMean	0.8544	0.8347	0.8402	0.7890	1.0000		
DemMeanSq	0.8452	0.8262	0.8238	0.8034	0.9831	1.0000	
Dem SD	0.0807	0.1963	0.1838	-0.0593	-0.0017	-0.0243	1.0000
Turnout	-0.8224	-0.8008	-0.7791	-0.6179	-0.7540	-0.7706	-0.1145
Unempl30	-0.1429	-0.1492	-0.2913	-0.2464	-0.2492	-0.2582	0.0867
ΔRetail	-0.0669	-0.1191	-0.1390	-0.0652	-0.0920	-0.1019	-0.0317
Retail29	-0.3392	-0.2697	-0.3785	-0.3767	-0.3890	-0.3892	0.1799
CropFail	0.0820	0.1041	0.1012	0.0282	0.0811	0.0559	0.1315
Drought	-0.4869	-0.3791	-0.3404	-0.3194	-0.4167	-0.4123	0.2065
Dust Bowl	-0.0027	0.0378	0.0191	-0.0715	0.0146	0.0070	0.0492
Spending	-0.1752	-0.0795	-0.0864	-0.1597	-0.2023	-0.2032	0.2374
StateLand	-0.0458	0.0157	-0.0256	-0.0474	-0.0798	-0.0981	0.2943
German	-0.4672	-0.3199	-0.2526	-0.2075	-0.4445	-0.4210	0.1973
Foreign	-0.3443	-0.3030	-0.4115	-0.2569	-0.5339	-0.4849	0.2376
Farmland	-0.2465	-0.1885	-0.0432	-0.1235	-0.0979	-0.1049	-0.1922
Manufact	0.0817	0.0059	-0.1380	-0.0053	-0.0319	-0.0178	-0.1298
Urban	-0.0631	-0.0865	-0.2451	-0.1476	-0.1630	-0.1663	-0.0493
Illit	0.5995	0.5437	0.5227	0.5944	0.5937	0.6130	-0.0677
Afr Am	0.6965	0.6676	0.6317	0.6913	0.7140	0.7464	-0.0522
Co Land	-0.0366	0.0100	-0.0184	-0.0713	-0.0472	-0.0613	0.3251
Loans	-0.1457	-0.0553	-0.0444	-0.1443	-0.1538	-0.1471	0.2671
Pop	-0.0099	-0.0260	-0.1076	-0.0169	-0.0794	-0.0732	-0.0399
1/Pop	-0.0548	-0.0087	0.0102	-0.0862	-0.0206	-0.0259	0.2913

	Turnout	Unempl30	ΔRetail	Retail29	CropFail	Drought	Dustbowl
Turnout	1.0000						
Unempl30	0.2478	1.0000					
ΔRetail	0.0680	0.0388	1.0000				
Retail29	0.3569	0.4626	-0.1535	1.0000			
CropFail	-0.0569	0.1120	-0.0291	0.0837	1.0000		
Drought	0.4989	-0.0033	-0.0808	0.2251	-0.0402	1.0000	
Dust Bowl	-0.0056	-0.0452	-0.1691	0.1348	0.0497	0.1176	1.0000
Spending	0.2178	0.0773	-0.1069	0.2109	0.1452	0.2882	0.2758
StateLand	0.0964	0.1309	-0.0376	0.1782	0.2149	0.1473	0.0256
German	0.3981	0.0854	0.0147	0.3659	-0.0801	0.3706	-0.0439
Foreign	0.4129	0.4262	0.0818	0.4493	-0.0413	0.2472	-0.0665
Farmland	0.2078	-0.2730	-0.0878	0.0479	-0.0883	0.2632	0.0583
Manufact	-0.0229	0.2942	0.1553	0.1823	-0.0973	-0.1573	-0.0847
Urban	0.1068	0.5288	0.0961	0.6124	-0.0335	-0.0400	-0.0369
Illit	-0.6681	-0.2585	0.0358	-0.4723	0.0425	-0.4141	-0.0863
Afr Am	-0.7363	-0.2774	0.0239	-0.3971	-0.0397	-0.3651	-0.0683
Co Land	0.0695	0.0745	-0.0180	0.0647	0.3303	0.0997	0.0694
Loans	0.1528	0.0185	-0.1607	0.3225	0.0588	0.2816	0.2200
Pop	0.0373	0.2604	0.0418	0.2454	-0.0188	-0.0369	-0.0317
1/Pop	0.0765	-0.0007	-0.0303	-0.0704	0.1756	0.1353	0.1182

```
              Spending StateLand German  Foreign  Farmland Manufact  Urban

Spending        1.0000
StateLand       0.3776   1.0000
German          0.1221   0.0589   1.0000
Foreign         0.1838   0.1946   0.3868   1.0000
Farmland       -0.0475  -0.2552   0.2038  -0.2364   1.0000
Manufact       -0.1985  -0.1553   0.0105   0.2218  -0.1938   1.0000
Urban          -0.1079  -0.0797   0.1405   0.3092  -0.0378   0.5176   1.0000
Illit          -0.2165  -0.0517  -0.3821  -0.3350  -0.2570   0.0183  -0.1726
Afr Am         -0.2571  -0.1483  -0.3448  -0.3625  -0.1765   0.0667  -0.1064
Co Land         0.4228   0.6039  -0.0012   0.1163  -0.2954  -0.1721  -0.1997
Loans           0.3937   0.1861   0.2117   0.1831   0.0951  -0.1264   0.0338
Pop            -0.0534  -0.0736   0.1079   0.2606  -0.1013   0.2602   0.4015
1/Pop           0.3327   0.2699   0.0168   0.0270  -0.1677  -0.2255  -0.3103


              Illit    Afr Am   Co Land  Loans     Pop      1/Pop

Illit           1.0000
Afr Am          0.7018   1.0000
Co Land        -0.0174  -0.1542   1.0000
Loans          -0.2317  -0.2131   0.1536   1.0000
Pop            -0.0417  -0.0173  -0.0901   0.0556   1.0000
1/Pop          -0.0394  -0.1255   0.6248   0.1594  -0.1347   1.0000
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49

Appendix D
First-Stage Results for 2SLS Estimations in Table 2

	1 Spending (OLS)	2 Spending (OLS)
State Land	157.7343	152.4945
	(2.17)	(2.09)
Dem 1932	2.742979	
	(4.72)	
Dem 1936		2.497076
		(4.84)
Dem Mean	-5.298457	-4.599626
	(3.89)	(3.53)
Dem Mean Squared	0.0308432	0.0263186
	(3.30)	(2.76)
Dem SD	0.5954679	0.6914153
	(0.58)	(0.59)
Turnout	1.145629	1.055318
	(4.28)	(3.97)
Unemployment 1930	1.70197	0.9864835
	(0.71)	(0.39)
ΔRetail 1929-1933	1.957971	-0.5634911
	(0.13)	(0.03)
Retail Sales 1929	0.1446209	0.1193747
	(2.48)	(2.11)
Crop Failures 1929	-0.0155817	0.1800458
	(0.01)	(0.15)
Drought	0.9388175	0.8998331
	(3.09)	(2.76)
Dust Bowl	235.1854	225.6528
	(5.68)	(5.53)
German	-9.674611	-4.976507
	(3.01)	(1.52)
Other Foreign Born	-0.2949234	-0.5586545
	(0.22)	(0.39)
Farmland	-0.171487	-0.028713
	(1.00)	(0.15)
Manufacturing	-1.053401	-1.439228
	(2.87)	(3.77)
Urban	-0.3971021	-0.5468792
	(1.77)	(2.37)
Illiterate	-0.447851	-0.5352307
	(0.83)	(1.01)
African American	-0.2307754	-0.384696
	(1.08)	(1.78)
County Land	130.9422	126.8317
	(1.87)	(1.82)
Constant	21.47776	29.19786
	(0.64)	(0.90)
R^2	.3631	.3613
observations	2861	2861

Robust t-statistics in parentheses, with clustering by state.

Table 1
Post-1928 Gains and Losses, Not Conditional on New Deal

	1 Dem 1932 (OLS)	2 Dem 1936 (OLS)	3 Dem 1940 (OLS)
Dem 1928	0.3806766	0.2376599	0.1624354
	(5.70)	(4.11)	(2.06)
Dem Mean	0.8453462	0.8065282	0.9103322
	(5.03)	(4.90)	(5.13)
Dem Mean Squared	-0.0047946	-0.0035974	-0.0036987
	(3.61)	(2.94)	(2.73)
Dem SD	0.4812265	0.4003802	0.1838731
	(3.66)	(3.47)	(1.70)
Turnout	-0.333892	-0.3271912	-0.3369301
	(6.75)	(7.65)	(7.49)
Unemployment 1930	0.1388471	0.3622898	0.1937554
	(0.72)	(2.03)	(0.93)
ΔRetail 1929-1933	-3.194427	-3.190257	-2.12409
	(2.09)	(2.16)	(1.56)
Retail Sales 1929	-0.0072399	-0.0004602	-0.0073792
	(2.25)	(0.11)	(1.74)
Crop Failures 1929	0.1015005	0.0521094	0.0283603
	(1.69)	(0.84)	(0.50)
Drought	-0.0206039	-0.0021093	-0.0609293
	(0.87)	(0.07)	(2.00)
Dust Bowl	1.835029	5.076249	1.775914
	(1.57)	(3.50)	(1.16)
German	1.227198	0.0462496	-2.047409
	(3.37)	(0.13)	(4.61)
Other Foreign Born	0.1083649	0.4083346	0.5048798
	(0.95)	(3.10)	(3.61)
Farmland	0.065138	0.0125664	-0.0175436
	(3.29)	(0.75)	(0.88)
Manufacturing	-0.1085996	0.0136087	0.128816
	(1.88)	(0.23)	(2.19)
Urban	-0.0542792	0.0026304	0.0292822
	(3.46)	(0.15)	(1.46)
Illiterate	-0.2971601	-0.2223461	-0.1628781
	(4.50)	(3.40)	(2.25)
African American	-0.0562505	0.0308482	0.0186056
	(2.15)	(0.91)	(0.53)
County Land	-0.6459834	1.373315	-0.0367161
	(0.71)	(1.38)	(0.04)
Constant	34.18041	34.62398	35.92761
	(6.39)	(6.11)	(6.22)
R^2	.8708	.8318	.8495
observations	2861	2861	2861

Robust t-statistics in parentheses, with clustering by state.

Table 2
Post-1932 and Post-1936 Gains and Losses, Conditional on New Deal

	1	2	3	4	5	6
	Dem 1936	Dem 1936	Dem 1940	Dem 1940	Dem 1940	Dem 1940
	OLS	2SLS[A]	OLS	2SLS[A]	OLS	2SLS[B]
Spending	0.0044212	0.0279296	0.0003412	0.0108983	-0.0037905	-0.0143317
	(1.71)	(1.03)	(0.18)	(0.44)	(1.91)	(1.06)
Dem 1932	0.6427145	0.5756925	0.5196482	0.4895501		
	(11.09)	(6.04)	(6.39)	(4.70)		
Dem 1936					0.8552469	0.8830695
					(25.56)	(18.38)
Dem Mean	0.2644551	0.3802959	0.4419623	0.4939839	0.1802615	0.1353276
	(2.15)	(1.99)	(2.52)	(2.17)	(2.11)	(1.29)
Dem Mean Squared	-0.0005705	-0.0012136	-0.0011942	-0.001483	-0.000552	-0.0003101
	(0.65)	(0.98)	(0.94)	(0.92)	(0.72)	(0.37)
Dem SD	0.0882828	0.0742273	-0.0489892	-0.0553012	-0.1327276	-0.1255169
	(1.13)	(0.84)	(0.46)	(0.48)	(2.03)	(1.91)
Turnout	-0.1138079	-0.1414249	-0.1641923	-0.1765945	-0.0569671	-0.0454212
	(3.23)	(2.99)	(3.39)	(3.16)	(2.40)	(1.61)
Unemployment 1930	0.2682361	0.2245368	0.1364441	0.1168197	-0.0915181	-0.0799384
	(1.81)	(1.30)	(0.66)	(0.51)	(0.69)	(0.57)
ΔRetail 1929-1933	-1.063068	-1.124182	-0.3246534	-0.3520982	0.7206061	0.7222378
	(1.03)	(0.90)	(0.27)	(0.27)	(0.89)	(0.92)
Retail Sales 1929	0.0037241	-0.0002762	-0.003086	-0.0048824	-0.0058213	-0.0043109
	(1.17)	(0.05)	(0.88)	(0.91)	(2.26)	(1.27)
Crop Failures 1929	-0.015495	-0.0146758	-0.0282362	-0.0278684	-0.019107	-0.0173654
	(0.37)	(0.34)	(0.49)	(0.42)	(0.38)	(0.43)
Drought	0.0065845	-0.0177047	-0.0512828	-0.0621906	-0.0561788	-0.0457498
	(0.25)	(0.50)	(1.76)	(1.74)	(3.47)	(2.33)
Dust Bowl	2.904191	-2.487401	0.8769675	-1.54428	-1.538078	0.7752332
	(2.02)	(0.38)	(0.60)	(0.25)	(1.84)	(0.25)
German	-0.752011	-0.5189467	-2.797677	-2.693013	-2.229095	-2.282512
	(2.04)	(1.13)	(4.47)	(3.74)	(6.03)	(5.81)
Other Foreign Born	0.3256906	0.3262193	0.4129444	0.4131818	0.1202629	0.1167189
	(2.82)	(2.65)	(3.12)	(3.05)	(1.60)	(1.66)
Farmland	-0.0289627	-0.0231132	-0.0511982	-0.0485714	-0.0283529	-0.0293912
	(1.79)	(1.28)	(2.81)	(2.69)	(2.89)	(3.29)
Manufacturing	0.091581	0.1212988	0.1894693	0.2028149	0.114993	0.0975279
	(2.08)	(2.13)	(3.80)	(3.37)	(4.28)	(3.05)
Urban	0.0399631	0.0503573	0.0571092	0.061777	0.0241716	0.0178851
	(2.61)	(2.71)	(2.85)	(2.54)	(2.15)	(1.08)
Illiterate	-0.0309484	-0.0256115	-0.0212394	-0.0188427	0.0097128	0.0063509
	(0.56)	(0.45)	(0.27)	(0.23)	(0.19)	(0.12)
African American	0.0667555	0.0725201	0.0417994	0.0443882	-0.0156547	-0.0199544
	(2.77)	(2.81)	(1.35)	(1.32)	(0.89)	(1.06)
County Land	1.074073	-2.76559	0.2425129	-1.481797	-0.6004889	1.063771
	(1.33)	(0.54)	(0.29)	(0.35)	(1.05)	(0.39)
Constant	12.18223	12.07502	18.06285	18.0147	6.632237	6.753393
	(3.01)	(2.76)	(3.75)	(3.63)	(2.49)	(2.59)
R[2]	.8894	.8679	.8791	.8757	.9457	.9423
observations	2861	2861	2861	2861	2861	2861

Robust t-statistics in parentheses, with clustering by state.
[A]2SLS with State Land as instrument for Spending. See Equation 1 in Appendix D for first stage results.
[B]2SLS with State Land as instrument for Spending. See Equation 2 in Appendix D for first stage results.