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Does Median Voter Income Matter? The Effects of Inequality and Turnout on Government Spending

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Introduction

What effect do economic inequality and political participation have on government spending? Greater representation of the poor, usually associated with higher turnout, is thought to increase government spending. Increased inequality increases the incentives for the less well off to demand redistribution from the rich. Both of these expectations are premised on the idea that individual preferences, dictated by economic situation, are translated into policy via participation in the political process. The logic is straightforward: the poorer the decisive voter, the more likely she will demand redistributive policies from government. The income of the decisive voter depends on two parameters: the distribution of income, and who votes. Much empirical work has studied the effects of these two variables- income inequality and turnout- on redistribution, or on government spending. Yet they are thought to be important only in that they impact the income of the median voter. In this paper, I use direct measures, from individual survey data, of the income of the median voter and examine its effect on government spending.

Using data from the Current Population Survey (US Census Bureau, 2006) for the U.S. states from 1978 to 2002, I find that neither the income of the median voter, nor the shortfall between

median voter income and mean income levels, affect the level of welfare spending as predicted by the model. Further, the effects of turnout and inequality, measured at the state level, as has been typical in previous work, retain what effects they have on spending when modeled simultaneously with the direct measures of median voter income. This suggests that they- turnout in particular, as its effect is more consistent in the empirical literature- are operating via a mechanism distinct from any effect on individual preferences. I suggest a number of mechanisms at the state level that might explain the observed correlation without recourse to the median voter logic.

Conventional wisdom holds that, given an income bias in voting, increased participation will increase the representation of the poor, and thus lead to greater government spending. This is consistent with a logic whereby the pivotal position in the income distribution is lower, the more people vote. The Downsian logic of majoritarian democracies implies that it is the median voter who will be pivotal (Downs, 1957) Increased inequality gives the median voter more incentive to vote for redistributive policies since, other things equal, the gains from redistribution are increasing in the difference between mean and median income (Meltzer and Richard, 1981). Further, the two variables should interact: an increase in inequality effectively increases the distance between voters, ordered by income. At higher levels of turnout, this increase is more directly translated into voter preferences. Conversely, an increase in turnout (assuming the increase is concentrated at the lower end of the income distribution) effectively moves the median down the income ranking. The larger the gaps between each individual (that is, the greater is inequality), the bigger an effect on the demand for redistribution each move down the ranking will have.

The existing empirical literature finds (to generalize; see below) fairly consistent effects of turnout on government spending; and more mixed results for the effect of inequality and its interaction with turnout. This empirical analysis is the main focus here, and the major contribution of this paper is to go beyond using turnout and inequality data- assumed to be important due to their impact on the income of the median voter- as proxies, and use the relevant data on the median voters income directly. The results indicate that a reconceptualization of the mechanism by which

turnout affects spending is in order, and I note some possibilities for macro-level explanations. In terms of inequality, this paper continues the in the tradition of failing to find support for the Meltzer-Richard model.

Turnout and redistributive policy

The level of electoral turnout, as well as a class bias in the population of voters, have been found to affect government redistribution both in analyses of American states and in comparisons of developed countries. Much of the literature on the effects of turnout has come from the United States, where the focus has been whether higher turnout would benefit the Democrats (Citrin, Schickler and Sides, 2003; de Nardo, 1980; Nagel and McNulty, 1996) but there is little evidence here that Democratic candidates are disadvantaged by lower turnout. However, there could be effects on policy even without partisan advantage if both parties tailored their platforms in anticipation of a pro-rich bias in voting. Indeed, redistributive public policies are more generous where the poor are better represented (Hill and Leighley, 1992). A parallel comparative literature has also considered whether leftist parties do better as turnout rates increase (Pacek and Radcliffe, 1995), and does find a positive effect. Hicks and Swank (Hicks and Swank, 1992) find that in addition to government partisanship, turnout increases welfare spending in the advanced democracies. Taking a broader view, the increase in turnout associated with the extension of the franchise or transition to democracy leads to a transformation of the role of the state in the economy along more redistributive lines (Boix, 2003), although this conclusion has its critics (Ansell and Samuels, 2010) . Most recently, Vincent Mahler (Mahler, 2006) investigates not only the effect of turnout on redistribution, but also the effect of turnout on the income skew in the electorate. This work finds that turnout is positively related to redistribution, and support (albeit from a limited set of data) that this is due to the different skew induced in the income distribution of voters by different levels of turnout.

These macro-level findings fit well with the empirical literature on the characteristics of voters as compared to non-voters. Those who vote tend to be richer and better educated than those

who do not vote (Verba et al., 1993; Wolfinger and Rosenstone, 1980) . In addition, although the evidence that voters have different policy preferences than non-voters is mixed (Gant and Lyons, 1993; Wolfinger and Rosenstone, 1980) , the most consistent differences are found with respect to redistributive policies (Bennett and Resnick, 1990).

Inequality and redistributive policy

If voter income determines preferences and the choice of policy, then inequality of income will have an impact on redistributive policy. Intuitively, since political resources in a democracy are distributed equally, the poor can use their relative numerical advantage to equalize the distribution of income. The greater the concentration of wealth, the greater the incentive to redistribute. This logic is formalized in the Meltzer-Richard model of redistribution (Meltzer and Richard, 1981). The key parameter is the difference between the income of the median compared to that of the mean, because the median voter is the pivotal political agent (in a majoritarian democracy), while the mean income is the pivotal economic agent- the mean income individual exactly breaks even in a system of lump sum redistribution financed from linear taxation.

Empirical studies of the effect of inequality on redistribution, however, have found at best mixed support for the Meltzer-Richard model. (Perotti, 1996) looks at several different measures of redistribution across countries in an investigation of the mechanisms by which inequality could affect growth, but finds little consistent relationship between inequality and redistribution. (Rodriguez, 1999) tries to test the Meltzer-Richard theory directly using evidence using both time series and cross sectional (across states) data from the United States and finds neither a short- nor a long-term effect of inequality on redistributive spending. Other studies have revealed negative relationships between inequality and redistribution (Lindert, 2004; Moene and Wallerstein, 2001), although this finding tends to be fragile across different model specifications (Iversen, 2005). Recent analyses focused directly on redistribution, rather than government spending measures, have tended to find the predicted, positive, association between inequality and redistribution (Kenworthy and Pontusson, 2005; Milanovic, 2000; Mahler, 2006).

Many, though not all, of these analyses include the level of turnout as a control variable. However, if the median voter mechanism is to be taken seriously, the interaction of turnout and inequality is a critical variable, which has been little studied. Franzese finds that the interaction does have the predicted effect on redistribution in the advanced democracies he studies (Franzese, 1998, 2002); yet other work explicitly considering this interaction term finds no effect (Boix, 2003; Ansell and Samuels, 2010). A further refinement, bringing the empirical analysis closer to the model, investigates not only the effect of turnout but also the skew that inequality in turnout induces in the income distribution of voters. Using macro-level data, there is no evidence that the effect of inequality on redistribution is contingent on voter turnout, and explains this with survey data revealing that low turnout in fact advantages parties of the left (Finseraas, 2008).

These models are all based on a model of the relationship between turnout, inequality and redistribution that is grounded in the micro-logic of the median voter. The accounts of the effects of turnout follow a similar underlying logic where the outcomes of the policy making process depend on the representation of individual preferences in the political system, even if they are less explicit about whose preferences matter. The same logic underpins the accounts as to why each of these independent variables affects redistribution (Larcinese, 2007). It is worthwhile making this model explicit, to best reveal the implications that can be taken to the data.

Inequality, turnout, and individual preferences

The workhorse model in the literature linking inequality to redistributive preferences is the Meltzer-Richard model (Meltzer and Richard, 1981). They propose a model in which the government finances lump-sum per person expenditure with a progressive linear tax rate. The government has no other expenditure or revenue requirements, thus each individual receives

$$b = \frac{\sum^n \tau y_n}{n}, \tag{1}$$

the total tax take divided by the population, in the form of a lump sum grant (n indexes taxpayers, τ is the linear tax rate, and Y is individual income). This is equal to simply the tax rate times mean income,

$$b = \tau \times \frac{\sum^n y_n}{n} = \tau \bar{y} \quad (2)$$

Thus if $Y_i < \bar{y}$, individual i will want a high (100 % in this simple set up) tax rate, while any individual with greater than mean income will want a tax rate of zero. However, adverse incentive effects associated with high tax rates put an upper bound on the preferred tax rate even of the lowest income individual. Thus Meltzer and Richard derive the result that an individuals preferred tax rate is inversely ordered by income. They also show that the further an individuals income below the mean, in dollar terms, the higher tax rate they will choose. Thus individuals will demand redistribution in proportion to the extent by which mean income exceeds their own income. It is this particular kind of inequality which should be associated with rising redistributive demands.

Turnout and the median voter

The translation of these preferences into policy relies upon the median voter as the decisive voter in a majoritarian system. When the median voter is pivotal, the relevant metric for inequality is the difference between median and mean income. Non-participation at the bottom of the income distribution moves the median voters income up, relative to the median income in the population. On a Meltzer-Richard logic this translates directly into lower redistribution. More formally, the demand for redistribution depends on the income of the median voter relative to the mean,

$$R = f(\bar{y} - \hat{y}_m), \quad (3)$$

where \bar{y}_m is median voter income. Assuming that the income distribution remains the same (that is, there are no changes in \bar{y} or y_m , the median in the population), then \hat{y}_m is determined by

the distribution of voting in the population. The maintained assumption in the literature linking turnout to policy outcomes is that as turnout declines, abstention is concentrated among low income voters. Thus

$$\hat{y} = f(\hat{y}_m - y_m) - y_m = g(\text{turnout}) \quad (4)$$

The larger the difference between median voter income and median income, the higher is the income of the median voter, since we are assuming a given distribution of income in the population. The difference in incomes is determined by turnout, with higher turnout bringing \hat{y}_m down closer to y_m .

In many of the analyses of the effect of turnout on redistributive policies, this median voter mechanism is not explicitly cited as the way in which the preferences that are represented in the political process are determined. The notion that no heed need be taken of those who do not participate, however, is clear (Lijphart, 1997). Also, while there are many other ways to influence policy, beyond voting (Schlozman, Brady and Verba, 2008), the effect of turnout itself requires a logic of the representation of individual preferences.

Thus we are left with a model at the macro level whereby redistributive policies are essentially a function of the income of the pivotal voter, relative to mean income. This parameter is determined by two things: the distribution of income, and who votes. The theoretical models either implicit or explicit in the literature outline a logic of individual preferences that therefore imply that

1. the income of the median voter affects the level of redistribution;
2. the effect of turnout on redistribution is mediated by the degree of income inequality;
3. the effect of income inequality on redistribution is mediated by the level of turnout.

It is these hypotheses that the analyses of the interaction of turnout and inequality are essentially testing. In this paper, however, I am able to directly test the step in the logic that is implied by the interaction model, namely that the income of the pivotal voter determines redistributive policies.

Empirical analysis

I proceed with three empirical strategies to investigate the influence of the income of the pivotal voter on redistributive policy, and the implications for turnout and inequality. First, I estimate the effect of the shortfall of the median voters income, relative to the mean, on redistribution. Second, I consider whether the effect of turnout on redistribution is mediated by the income of the median voter, as it should be if the individual level, median voter logic applies as is implied by the model, and also the obverse case, whether the impact of inequality is mediated by turnout. Finally, I consider the effect on interaction models of the inclusion of the measure for the income of the pivotal voter.

Data and Methodology

The analysis that follows is a time-series cross-section* of the 50 U.S. states and the District of Columbia from 1978 to 2002. The drawback of comparing states as opposed to nations is the potential for a lack of variation on the dependent variable. However, the individual U.S. states are responsible for two of the most important programs of redistribution, Supplemental Security Income (SSI) and welfare: Aid to Families with Dependent Children (AFDC) and Temporary Aid to Needy Families (TANF) after 1996. These programs constitute a significant proportion of the redistributive effort made in the United States, and there is considerable variation in the generosity of benefits across states. In 2004 the average monthly TANF payment to a family with three children was \$420, but levels ranged from \$170 to \$923 (Lazere and Tallent, 2006). There is also a large degree of variation in total state expenditure. Since this captures all the services provided by the state, to the extent that these are lump sum benefits paid for through proportional taxation, this is a better measure of the redistribution that the Meltzer-Richard model seeks to explain.

There is some debate as to what the appropriate dependent variable should be when we are considering government redistributive effort, or policy oriented toward the poor. Much of the work on government redistribution uses total government social benefit expenditures as the relevant outcome (Huber and Stephens, 2001; Hicks, 1999; Swank, 2002); while others have focused directly

on the redistributive effects of such policies in terms of reducing the Gini coefficient (Kenworthy and Pontusson, 2005; Iversen, 2005), net government expenditure going to the bottom quintiles, or bottom half (Milanovic, 2000). The focus on taxation and cash transfers is understandable from an empirical point of view, since it is relatively straightforward to see which individuals are receiving such benefits, and to assign them a monetary value. From a theoretical standpoint, however, the exclusion of in-kind transfers and public goods provision does not necessarily give us a better measure of redistribution, since any lump sum benefit that is financed from progressive taxation will be progressive. Indeed, even if the poor do not value the goods provided as highly as do those rich, or do not consume as much of it (such as public highways, for example) the effect of the provision of these good will still be redistributive if the gradient in consumption is less than the gradient of the taxes used to finance the provision.

In light of these ambiguities, I use two difference measures of public spending to capture government redistributive effort. The first, public welfare spending, consists primarily of cash and near-cash benefits, including Medicaid spending. The second, total state expenditure, is a measure total spending, and thus is intended to capture the redistributive potential even of those public good provided in equal quantity to all. Indeed, while using the U.S. states is necessitated by the nature of the data on turnout and income, it also has implications for the dependent variable and the assumptions made by the comparative research design. The assumption that the same factors are important in determining policy in each jurisdiction seems more likely to be justified in the case of the American states than in a cross national context. On the other hand, there may be limited variability across states, not least due to federally mandated constraints on and requirements for policy. Nevertheless, states have a fair amount of discretion over the policies that make up the bulk of the public welfare budget, in particular Medicaid generosity, as well as TANF and SSI.

Thus I estimate the model with these two measures of the dependent variable: per capita state spending on public welfare, and total state government expenditure per capita. The difference between the two measures is that the latter includes many public goods which are not direct transfers

to individuals. Not all of these will benefit the poor, (spending on highways, for example) but the additional spending will tend to be redistributive for two reasons. First, the direct intuition from the Meltzer-Richard model, is that any benefit provided equally to all and financed from a progressive tax is redistributive. Second, the poor may benefit more from public goods which are provided in equal measure to all, since the rich are more likely to take advantage of private substitutes. In particular, expenditures for housing and community development, parks and recreation, education, health and hospitals are expenditures included in total spending which are likely to benefit the poor disproportionately. In terms of adhering directly to the terms of the theoretical model, the total expenditure model is to be preferred, since it is precisely these lump-sum benefits which the model concerns. Public welfare spending, by contrast, is likely to be targeted to those poorer than the median voter, and thus exhibit a different logic with respect to voting and inequality than the one envisaged by the model. However, this latter variable captures everyday notions of redistribution better than the broader expenditure measure, thus I include results for both variables.

I also use the absolute dollar amount of spending divided by gross state product as an alternative measure of redistributive effort to capture the effort going to redistribution. Thus while the absolute dollar amount going to expenditure may be automatically influenced by the income level of the state.

I use data from the November voting supplements of the Current Population Survey on the distribution of income of those who vote and of the population as a whole for the 50 U.S. states and the District of Columbia for the years 1978 to 2002. The CPS data are available each election year (presidential and congressional elections), which yields time-series cross-sectional data for 13 years for 51 states. From the survey responses about family income (which are categorized into bins), I create simulations of the distribution of income by drawing incomes in proportion to the number of people in each income category. This process is repeated for both the population as a whole (all respondents with non-missing income responses) and for those who claim to have voted in the last election. From these simulated distributions I can then calculate characteris-

tics of the income distribution of voters as compared to the population- for example, the distance between mean income and median income (which is the important parameter in determining demand for redistribution in the Meltzer-Richard model), as well as the mean and median incomes of both populations. These two variables can be used as controls for the income of the median voter, which should mediate the effect of turnout in models of the determinants of redistributive spending.

One problem with using CPS data for turnout is that the survey overestimates the level of turnout. In order to ascertain how much of a problem this might be, I used data for those years of the American National Election Study which verified voting reports. Although there is a discrepancy between reported and actual turnout, in these studies at least there was no additional bias across income groups in terms of over-reporting, nor a systematic bias across the states ¹. Thus while turnout may be overestimated, I do not think this seriously compromises the analysis that follows.

I employ two different empirical strategies to investigate the median voter mechanism by which turnout is assumed to affect redistributive outcomes. First, I measure the effect of the income of the median voter directly on redistributive outcomes. If turnout is leading to greater redistribution via its impact on the income of the median voter, then we should be able to capture this effect equally well- or better- by directly considering voter income itself. Secondly, if the impact of turnout- and indeed of inequality- is operating via the purported median voter mechanism, it should be the case that (a) higher turnout (inequality) is associated with higher redistributive spending; (b) the estimate of this effect is dampened by the inclusion of the direct measure of median voter income. This strategy essentially involves leveraging what might otherwise manifest itself as post-treatment bias in the regression: theory suggests that controlling for median voter income should induce post-treatment bias with respect to turnout (and inequality)- that by including the variable which is in itself the result of the independent variables of interest, we would downwardly bias our estimates.

¹Analyses available on request.

In all cases, I estimate multivariate models to control for potentially confounding determinants of redistribution. Specifically, I estimate

$$y_{i,t} = \alpha_i + \beta_1(\text{turnout}_{i,t}) + \beta_2(\text{inequality}_{i,t}) + \beta\mathbf{X}_{i,t} + \gamma y_{i,t-1} + \varepsilon_{i,t}, \quad (5)$$

where \mathbf{X} is a matrix of control variables.

The matrix of control variables in each case includes gross state product per capita; union membership; the unemployment rate; the fraction of the population aged over 65; the fraction of the population that is African American; as well as controls for the party in control of the state executive. Since the estimations are run on data averaged over a four-year election cycle, the differential turnout in different types of election should be averaged out.

Gross state product per capita is included to capture the effects of Wagners Law; that growing government expenditure is the result of economic progress. It is expected therefore that the level of GSP per capita will be positively associated with redistributive spending. This is measured as the sum of costs incurred and incomes earned within a state (data from the Bureau of Economic Analysis (US Bureau Of Economic Analysis)) divided by the population for the same year (data from the Statistical Abstract of the United States (US Census Bureau)).

Unionization levels have been shown to be important in determining levels of redistribution cross-nationally; the differential mobilization of labor being a key explanatory variable in power resources theories of welfare state development (Korpi, 1983). I use Hirsch, MacPherson and Vroman estimates of union density, union members as a percentage of non-agricultural wage and salary workers, including public sector workers, derived from the CPS (Hirsch, Macpherson and Vroman, 2001). The other key variable in the power resources literature is the power of the Left in government (Huber and Stephens, 2001). However, while important in a cross-national context, there are reasons to doubt the effect that this variable will have at the level of the U.S. states.

First, neither of the major American parties is a party of the Left- a social democratic party- which is what is found to be important in comparisons across Western Europe. Second, at the state level, the lack of coherence and cohesion of the American parties (Katz and Kolodny, 1994) means that to be a Democrat in one state may mean something very different to the same party in another state. This is particularly pronounced with regard to the Democratic party in the Southern states at the beginning of the period under study, but more generally, divergence in party strategies makes party incumbency a weaker predictor of outcomes. However, I include a categorical variable in the analysis which differentiates between states where the incumbent governor is a Republican, a Democrat, or an independent to control for possible partisan effects on policy. These data come from the Congressional quarterly database (Congressional Quarterly Online Library, N.d.).

The level of state redistributive spending is also related in mechanical ways to the demographic characteristics of the population. Higher unemployment rates mean that the fraction of the population eligible for unemployment insurance transfers is larger, thus it is included as a control. Similarly the fraction of the population aged over 65 may increase expenditure. Since the major programs that affect the elderly are federal expenditures (Social Security and Medicare), this can be expected to be less pronounced at the state level than it would be at the national, but other programs (Medicaid, in particular) may also be sensitive to the size of the elderly population. Since the elderly are more likely to vote, as well as receiving a high proportion of expenditures, there may be a political mechanism increasing redistribution with the size of the elderly population, as well as the mechanical demographic effect. On the other hand, certain types of spending (education in particular) have been found to be depressed by the size of the elderly population (Poterba, 1998).

Finally I include the proportion of the state population that is African-American, to capture the possibility that this diversity affects redistributive generosity. (Alesina and Glaeser, 2004) maintain that a large part of the difference between European and the American welfare state can be attributed to the greater racial diversity in the United States. This outcome can prevail even if preferences are color-blind, when affirmative action policies are available as an alternative

to redistribution as a policy tool to aid minorities (Austen-Smith and Wallerstein, 2004). There is also a strong relationship in American public opinion which is significantly less supportive of redistributive spending, the more it is associated with African-American recipients (Gilens, 1998).

One other variable that is accorded some importance in the international comparative literature is the degree of openness of the economy to international trade and competition. (Katzenstein, 1985) argues that small, open economies develop industrial policies to shelter workers from the higher risks that this international exposure brings. However, the importance of this factor is disputed at the international level (Rodrik, 1997; Cusack and Iversen, 2000), and while data are available on international trade at the state level, the relevant risks that state policies would need to insure would include interstate trade within the U.S. In the absence of such data I do not include this variable in the analysis.

Since the data are a series of cross sectional observations of the states through time, a number of methodological issues present themselves. First, because the theoretical model implies a relatively long term process, I estimate the empirical models using data that are averaged over a four year time period. Using a longer time period (six- or eight- year averages) does not affect the substantive results.

Standard in the political science literature is to estimate the equation above such that $\alpha_i = \alpha_j = \alpha$ for all units (states), as recommended by (Beck and Katz, 1995). The lagged dependent variable is included to adjust for autocorrelation. Lagrange multiplier tests indicate that including one lag is sufficient to guarantee conditional independence. Further, estimating a model including lags of the independent variables (as well as the dependent variable) reveals only unemployment to have a significant effect. Inclusion of the lagged unemployment variable in the subsequent analyses does not change the substantive conclusions in any way, so the simpler models are reported here. Thus in terms of the dynamic specification, it seems that the lagged dependent model is appropriate.

However, the assumption of a common intercept across states is not necessarily appropriate, and indeed for all of these models traditional hypothesis tests indicate that the set of fixed effects are jointly significant. Estimating the lagged dependent variable model with fixed effects, OLS is no longer unbiased nor consistent in a finite sample. Thus there is a tradeoff to be made, since omitting the state fixed effects will also bias the estimates. While the bias of the coefficient on the lagged dependent variable (γ , above) can be large, Monte Carlo simulations indicate that the bias on the estimates of β is usually small when both the fixed effects and lagged dependent variables are included (Wilson and Butler, 2007). In fact, in this application the substantive implications are the same when the fixed effects are omitted as when they are included; thus again I present the simpler versions of the models here, omitting the fixed effects.

The second common approach when dealing with time-series cross-sectional data draws on the econometrics of panel data. Using the generalized method of moments (Arellano and Bond, 1991) to instrument for potentially endogenous independent variables with their lagged values, and utilize differenced values for estimation yields unbiased parameter estimates under certain general conditions. However, the complexity of the estimation procedure- particularly as the number of time periods (and thus the number of instruments) gets larger- means that the advantages of this method in terms of consistency may be outweighed by its disadvantages from an efficiency standpoint (Beck and Katz, 2004). Thus following (Bawn and Rosenbluth, 2006) I present results from both the GMM and OLS-LDV-PCSE specifications.

Results

Table 1 shows the results of modeling government spending, on public welfare and in total, as a function of the income of the median voter, relative to mean income. The model does not include turnout or inequality since, according to the individual-level theory, both of these should operate via the income of the median voter. I find a positive relationship between this measure of inequality and public welfare spending. However, while statistically significant, it is substantively small:

the coefficient reflects the effect in dollars of per capita spending of a \$1000 dollar increase in the shortfall of median voter income (from mean income). Thus for a thousand dollar increase of this variable, the effect on public welfare spending is on the order of dollars, or perhaps tens of dollars, annually. On the other hand, however, the size of this effect is comparable to that estimated for gross state product per capita, which also increases public welfare spending on the order of a few dollars for every \$1000 difference. This change in the dependent variable resulting from these variables is small relative to the overall level of variation of the outcome: one standard deviation of the public welfare spending variable is equal to \$270.

Considering welfare spending per capita relative to state product as opposed to in absolute terms, again the Meltzer-Richard prediction of a positive point estimate of the median voter income shortfall is borne out in the data, but in this case it cannot be statistically distinguished from zero in either of the two model specifications. State-years with higher state product do spend more on public welfare even in these relative terms, and unemployment is also associated significantly with higher public welfare effort.

Using the total spending measure of government redistribution, the results are more consistent across the absolute dollar measure and the relative effort measure. In both cases, the impact of the median voter's income shortfall is negative, contrary to theoretical prediction but consistent with some of the other findings in the literature. However, again these effects are very small in substantive terms: the \$1000 change in median voter income shortfall (which represents about a change of about 1/5 of a standard deviation of that variable) leading to a change of \$20-\$30 in absolute per capita dollars (one to two percent of the dependent variable's standard deviation), or about 0.05 standard deviations change in dollars relative to state income (models 7 and 8). The control variables in these specifications also conform more closely to theoretical expectations, with both unemployment and state product associated with higher redistribution, and the percentage of the state population that is African American associated with lower spending. The surprising result here is that unionization rates appear to be associated with lower spending effort, in contradiction

of expectations.

(TABLE 1 ABOUT HERE)

The results for the income of the median voter thus provide little support for the theoretical predictions of the median voter based theories. This raises the question as to whether, and why, we would find a positive effect of turnout or inequality on spending levels. Thus I estimate a set of models in which both of these variables are included, and introduce alternatively the median voters income. If the impact of these macro-level variables is as the micro-logic of the model claims (via the income of the median voter), then the inclusion of these controls should diminish the direct effect of turnout and inequality, as at least some of the effect should be picked up as running via these median voter parameters. Table 2 shows the results of this analysis considering public welfare spending, and Table 3 the analogous results for total expenditure.

(TABLE 2 ABOUT HERE)

(TABLE 3 ABOUT HERE)

Before considering the mediating effect of median voter income, are inequality and turnout associated with redistributive spending in the data? For the models of absolute levels of welfare spending, I find no significant association between inequality and spending levels, though the point estimates are negative (counter to theoretical prediction). Turnout is correctly signed, positively associated with spending levels, but reaches conventional levels of statistical significance only in the GMM specification. Similarly for the relative spending levels, the effect of inequality cannot be differentiated from zero; while a positive association between turnout and spending is revealed only in the GMM specification. Introducing the measure of median voter income we see that this measure itself has an impact only on the absolute level of spending (models 10 and 12), and more importantly that it does not significantly change the estimates of the effect of turnout or inequal-

ity. In Table 2, where the outcome is total spending inequality does appear to have a significant positive association with spending both when controlling for the income of the median voter and otherwise, and both in terms of the absolute dollar amounts of redistribution undertaken and the weight of state redistribution in the state economy. Turnout in these models is also signed as per the theoretical expectations and conventional wisdom, but does not reach conventional levels of statistical significance. Interestingly, the inclusion of median voter income in the models for the relative weight of spending in state income (models 22 and 24) inflates the point estimate on turnout somewhat such that the association does seem to be statistically significantly different from zero. However, the change in the coefficient upon the introduction of median voter income is not itself statistically significant.

The control variables in these models are also fairly consistent across the specifications and broadly in line with expectations: GSP per capita is generally positively associated with levels of welfare spending; unemployment appears to be associated with higher levels of welfare spending as a fraction of state product but not with its absolute level, and the racial diversity measure—the proportion of the state’s population that is African American— is associated with lower levels of overall spending (in absolute and relative terms), but not with lower levels of targeted public welfare.

Conclusion

This null finding raises several important questions as to the effects of turnout and inequality on spending patterns. In particular with respect to turnout, since the findings of a positive effect are more robust in the literature, the question now becomes, since it is not by influencing the income of the median voter, how does increasing participation increase the level of spending? While the median-voter logic has theoretical appeal at the micro-level, it is not supported by the data. Further, there are several plausible reasons why, at the macro-level (where studies of the relationship have focused) higher turnout would be associated with higher levels of spending. That is, for reasons other than the income of the median voter, high turnout jurisdictions systematically differ

from low turnout jurisdictions in ways pertinent to redistributive spending.

In cross-national context, the most obvious candidates are differences in electoral institutions. In particular, PR systems lead to greater redistribution than majoritarian ones (Iversen and Soskice, 2006), and they also tend to have higher turnout (Franklin, 2004; Powell, 1986; Jackman, 1987). In a review of what affects turnout, Blais notes that the age of the population affects turnout levels (Blais, 2006); and given the large proportion of government spending that is directly (social security) or indirectly (medical care) linked to the elderly population, this too could induce the observed correlation without the median voter coming in to play.² However, given the effect of turnout is robust to the inclusion of controls for the size of the elderly population, so the relationship is likely not quite this straightforward.

Blais also finds that higher levels of economic development, smaller countries and closer elections result in higher turnout. The first of these may be associated with higher levels of redistributive spending via a Wagners Law effect whereby an increasing government share in the economy results from economic progress. The relationship of country size to redistribution has also been examined in the literature. If a certain absolute number of people is necessary to threaten revolution, and thus secure redistribution to the poor, countries with larger populations will meet this threshold more easily (Campante and Do, 2007). Since this implies higher redistribution in larger countries, it would induce a negative relationship between turnout and redistribution. On the other hand, Alesina and Spolaore equate larger countries with greater heterogeneity, which may limit redistribution (Alesina and Spolaore, 2003; Alesina and Glaeser, 2004), resulting in the positive correlation between high turnout and redistribution. In the case of the American states, larger states (by area) have lower taxation than smaller states (Merrifield, 1991). Although the direction of the effect of country size is unclear, both are plausible accounts of the observed macro-phenomenon which need

²Note that there are two ways in which this could work: one whereby the median voter logic is important, that if the population is older the median voter too is more likely to be old, and if older people favor more redistribution, this operates analogously to the income of the median voter. On the other hand, we can also imagine a situation where (for example) social security benefits are exogenously set at a certain level that is equal across jurisdictions. Then despite there being no median voter mechanism, these payouts will be higher where the elderly are more concentrated; and if the elderly are more likely to vote (as indeed they are) then turnout will also be higher in these same jurisdictions.

not rely on the proposed micro-logic.

Even across American states, where institutional differences are smaller, there are still a number of variable which may induce the observed correlation. Some of the same factors that drive cross-national variation in turnout may also be operative at the state level (such as population age). Research on turnout determinants that focus on the state also find that greater concentration of executive power, a smaller number of legislators as well as increasing their power all increase turnout, as do close elections (Merrifield, 1993). In a second essay on the determinants of taxation levels in the states, many of the same variables prove important. In particular, increasing the number of legislators tends to decrease tax revenues (Merrifield, 1991), which again would lead to a correlation between higher turnout and more redistribution, because both are the result of having a small number of legislators.

A final relationship between turnout and redistribution that hinges on a broader difference between places with high turnout (and high redistribution) and those with low turnout hinges on the role of information and the formation of political preferences. Merrifield begins with the assumption that

Turnout [can be] seen as an indicator of the general level of awareness and of interest in state politics. It was thought that a more active and aware citizenry would lead to a smaller state government sector, especially in the light of the tax revolts and anti-liberal rhetoric of the last decade or so. (Merrifield, 1991), p. 304)

Taking from this the insight that higher levels of turnout may be indicative of greater engagement in politics, we can question the second assumption that this engagement would necessarily translate into anti-redistributive preferences. High turnout may instead lead to (more) voters spending (more) time investing in discovering their material interests. There is some evidence that these interests are not necessarily transparent (Bartels, 2005), and there are costs to becoming politically informed. The key point here is that if the extent to which these material interests are what determines vote choice is influenced by those same factors as influence turnout (awareness and interest,

or social embeddedness (Abrams, Iversen and Soskice, 2005), for example) then the key difference between high- and low- turnout jurisdictions is not so much the interests of the median voter, as determined by their position in the income distribution, as the extent to which these interests translate into policy preferences.

These macro-level alternatives, although untested here, give us reasons to be skeptical of the median voter mechanism explaining the effects of turnout on policy, given that this micro-mechanism has not (to my knowledge) been explicitly tested. This may also help reconcile a number of existing problems that the empirical patterns pose for the median-voter micro-logic. First, as mentioned above, the correlation between turnout and the relative representation of the poor is not very high. Secondly, the evidence that non-voters have different policy preferences than voters is mixed (Gant and Lyons, 1993; Wolfinger and Rosenstone, 1980), although where differences have been found they do tend to center on questions of redistribution (Bennett and Resnick, 1990). In addition, the responsiveness of policy to the preferences of the median income voter, where these diverge from those of the rich, is very low (Gilens, 2005). It is worth noting that the pivotal citizen, in terms of determining policy, may not be the median voter, given that there are many other ways to influence policy. Indeed, there is a good deal of evidence that the income bias in participation is far more pronounced with regard to other forms of participation (Verba et al., 1993; Wolfinger and Rosenstone, 1980) and that the pivotal actor is likely to be much richer than the median income, or even the median voter (Schlozman, Brady and Verba, 2008). If the pivotal political actor is pivotal due to activities other than voting, there is little reason to expect higher turnout to influence policy outcomes.

The effect- or lack of effect- of inequality requires a different approach in light of the mixed evidence, empirically, of its effects. Some of the same factors that may lead to both high turnout and high redistribution may also account for the effect of inequality. For example, proportional electoral systems may be the omitted variable leading to a negative relationship between inequality and redistribution, since they are associated with both greater income compression and more generous public policy (Iversen and Soskice, 2006). However, before such explanations can be advanced,

a better understanding of the relationship that is to be explained is necessary, since there is little consensus on the empirical pattern.

In conclusion then, the hypothesized micro-logic that relates inequality and turnout to government spending is not corroborated by measures that consider the individual level parameters directly, as opposed to relying on jurisdiction-level aggregates. Direct measures of the purported micro-mechanism fail to predict the outcomes as well as not mediating the direct effects of turnout or inequality. While the micro-foundations of the voter income model make sense in theory, then, they do not receive empirical support. I have suggested some avenues for further investigation as to the mechanism, at the macro-level, of the reasons for which higher turnout may be associated with more generous spending.

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