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THE ECOLOGICAL FALLACY REVISITED: AGGREGATE- VERSUS INDIVIDUAL-LEVEL FINDINGS ON ECONOMICS AND ELECTIONS, AND SOCIOTROPIC VOTING*

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ABSTRACT

Several aggregate-level studies have found a relationship between macroeconomic conditions and election outcomes, operating in intuitively plausible directions. More recent survey-based studies, however, have been unable to detect any comparable relationship operating at the individual-voter level. This persistent discrepency is puzzling. One recently proposed explanation for it is that voters actually behave in an altruistic or "sociotropic" fashion, responding to economic events only as they affect the general welfare, rather than in terms of self-interested "pocketbook" considerations.

It is argued here that the discrepencies between the macro- and micro-level studies are a statistical artifact, arising from the fact that observable changes in individual welfare actually consist of two unobservable components, a government-induced (and politically relevant) component, and an exogenous component caused by life-cycle and other politically irrelevant factors. It is shown that because of this, individual-level cross-sectional estimates of the effects of welfare changes on voting are badly biased and are essentially unrelated to the true values of the behavioral parameters of interest: they will generally be considerable underestimates, and may even be of the wrong sign. An aggregate-level time-series analysis, on the other hand, will often yield reasonably good (if somewhat attenuated) estimates of the underlying individual-level effects of interest. Thus, in this case, individual behavior is best investigated with aggregate- rather than individual-level data.

It is also shown that the evidence for sociotropic voting is artifactual, in the sense that the various findings and evidence which ostensibly show sociotropic behavior are all perfectly compatible with the null hypothesis of self-interested "pocketbook" voting.

Political scientists are well aware of the dangers of basing inferences about individual behavior on aggregate data. The well-known works[1] on the "fallacy of ecological inference" have become timeless classics in political methodology, and occupy a secure niche in the syllabus of every introductory research methods course. Aggregate data, while still indispensible in areas such as historical research, is now generally recognized as an inferior substitute for individual-level data, and empirical results based solely on aggregate-level analyses, however elaborate, are normally regarded with due caution until confirmed by a proper individual-level study. The importance of attempting this kind of confirmation is widely appreciated: even negative instances, in which the aggregate results do not stand up at the individual level--real-world examples of the ecological fallacy at work--nevertheless perform an important service, in redirecting scientific attention away from the macro-level findings and on to the real task of analyzing and explaining the individual-level results. Sometimes, indeed, this may lead to major substantive revisions in knowledge.

Recent developments in the continuing flow of studies on the electoral impact of economic conditions provide a current case in point. On the one hand several earlier studies,[2] based on time series analysis of aggregate data, have found some association between macroeconomic conditions and election outcomes (or aggregate popularity measures), operating in intuitively plausible directions. Though there are differences in the detailed natures and magnitudes of the specific effects uncovered in different studies of this kind, the general finding of a plausible and reasonably strong relationship is one which comes through rather persistently in this work. On the other hand, more recent survey-based studies[3] have been unable to detect any comparable relationship between individual voting behavior and personal economic circumstances. Despite considerable experimentation with different variables, models, and hypotheses, such associations as have been found at the individual level are generally quite modest in magnitude, and in particular are too weak to account for the aggregate-level effects typically found in the time-series studies. Moreover, the individual-level effects seem to be quite unstable, varying considerably from one election to the next; often, in fact, they are of the "wrong" sign, pointing to effects which operate in directions which are intuitively implausible, and/or inconsistent with those of well-documented aggregate-level effects.

These discrepencies between the aggregate and individual-level findings pose a challenging intellectual puzzle, and have inspired several explanatory efforts.[4] One particularly interesting explanation is the "sociotropic voting" thesis,[5] based on the premise that voters assess and respond to economic conditions in terms of altruistic or "sociotropic" considerations, according to what is good or bad for the country as a whole, rather than in terms of their own personal self-interests, narrowly defined. The discovery that voters do not simply "vote their pocketbooks," but rather respond to economic events primarily in terms of the collective welfare, is a very interesting one, with potentially important implications for our understanding of contemporary electoral politics, and for democratic theory generally. It therefore deserves a closer look.

Our purpose here is to explore some of the implicit inference problems arising in this work, to try to get some insight into the nature of the empirical evidence and its bearing on the inferences drawn from it. We proceed by assuming that time-series and cross-sectional data are generated by a single, fixed electorate, whose behavior is governed by a specified behavioral hypotheses, and that regression analyses are performed on these two bodies of data in order to estimate the parameters of the underlying behavioral relationship. These estimates are then examined to see how they compare, and how successful each is in measuring the true, underlying behavioral effects we are trying to infer. On the basis of this analysis, we shall conclude the following:

First, there is nothing in the apparently anamolous empirical evidence which requires much by way of substantive explanation. The discrepencies between the macro and micro-level studies are basically a statistical artifact, and do not show any real disagreement about the true values of the underlying behavioral parameters of interest; they arise simply because the time-series and cross-sectional analyses are estimating two quite different derivative empirical relationships, neither of which is a perfect reflection of the real behavioral relationship we are ultimately interested in making inferences about. As we shall show, even when the <u>underlying</u> behavioral relationship

which governs individual voting decisions is the same for every voter in every election, the <u>observable</u> aggregate-level and individual-level empirical relationships between measurable economic variables and votes will still differ considerably from each other. Regression analyses of these two relationships will thus inevitably yield quite different estimates. There is no reason whatever to expect time-series and cross-sectional estimates of the same parameters to be similar in magnitude; they need not even be of the same sign.

Secondly and more importantly, of the two kinds of analyses, it is the <u>aggregate</u> time-series evidence--rather than that based on individual-level survey data--which is most likely to yield valid inferences about the underlying individual-level behavioral effects we are trying to measure. Estimates from either kind of data will be biased. Under plausible data and parameter assumptions, however, the bias in the aggregate-level estimates will be a relatively modest and tractable one, and in many cases they should convey a reasonably accurate idea of the underlying relationship. The individual-level cross-sectional estimates, on the other hand, are hopelessly contaminated. They depend only tenuously on the true parameter values, and in general are so badly and unpredictably biased as to be essentially unrelated to the underlying individual-level behavioral parameters we are trying to estimate.

Thirdly, the evidence for sociotropic voting is artifactual. The various findings and evidence which ostensibly show sociotropic voting cannot, in fact, effectively discriminate between that and self-interested behavior, and are all perfectly compatible with the null hypothesis of self-interested "pocketbook" voting. It is certainly possible, even likely, that American voters are not driven exclusively by self-interest, and that they also respond to altruistic or sociotropic considerations to some degree. However, persuasive empirical demonstration of this, or meaningful estimation of the extent to which these two factors operate in the electorate, will be a formidably difficult task, on conceptual as well as methodological grounds.

More generally, our analyses suggests that individual-level survey data, at least when analyzed with the usual methods, is not really useful for studying the effects of short-term economic fluctuations on individual voting decisions. While it may ultimately be possible to draw valid inferences from such data, this will require careful modeling and specification of the underlying structures and effects to be estimated, and sophisticated estimation techniques which take proper account of the subtleties involved. It is clear that straightforward multiple regression (or probit, or cross-tabulation) analysis of vote intentions on a set of economic and control variables will not do the job, and is likely to yield only spurious estimates which are virtually unrelated to the true effects. It would therefore seem prudent to regard <u>any</u> finding in this area based on individual-level survey data with caution, until and unless it can be shown to be unaffected by the problems described below.

Finally, the analysis here may also have some broader

implications for the general problem of ecological inference. In the problem we consider, it turns out to be an aggregate-level analyses, rather than one based on individual-level data, which is most likely to yield valid inferences about individual behavior. Of course this conclusion is not a general one, since the arguments on which it rests are specific to the particular data and problem at hand. Still, the example is certainly not an isolated one. Moreover, even the specific example may be of some general interest, if only as a useful corrective against unwarranted generalizations baaed on a somewhat uncareful reading of the ecological fallacy literature. There is nothing inherently wrong, or suspect, about aggregate data, and findings based on such data are not <u>per se</u> any more (or less) in need of independent corroboration than those based on any other kind of data. It is true, of course, that aggregate-level findings are typically compatable with a variety of alternative individual-level hypotheses or mechanisms, and in this sense can never completely identify the individual-level effects. But this is true of individual-level findings as well; for an example, see Section 5 below. Identification problems are not unique to aggregate-level analyses, and may be at least as severe when working with individual-level data.

There are situations, clearly, in which certain kinds of individual behaviors can be studied most effectively, or only, with individual-level data of the proper kind. But this does not mean that individual-level data as such is necessarily always well suited for studying the behavior of individuals, or that aggregation is necessarily bad, or undesireable:[7] in many cases--as in the example studied below--aggregation bias may turn out to be a lesser and minor evil, compared to measurement error, response biases, or other problems inherent in the available individual-level data. Intelligent methodological choices can only be made by realistically considering all such sources of error or bias, in the context of the specific problem at hand, and without preconceptions for or against any particular type of data or level of analysis. This, we think, is the proper conclusion to be drawn from ecological fallacy, and from the rather different arguments which follow.

1. INTRODUCTION AND OVERVIEW

It may be useful to introduce the formal argument by first sketching out a simple example which illustrates how an individual-level analysis will fail to estimate the true effects properly, while an aggregate-level analysis may not.

Consider an election (1980?) in which the administration's poor handling of macroeconomic policy causes a substantial and widespread decline in personal incomes, which in turn leads to a general rejection of the incumbent at the polls. Of course, the incumbent will still receive some votes (or positive popularity ratings, which for present purposes we regard as equivalent to votes), since strong partisans of the incumbent party (or those who are favorably influenced by its performance on other, noneconomic issues)

will still tend to favor it, though less strongly than would otherwise be the case: a uniform shift against the incumbent does not mean that everyone votes against it, but rather that the overall distribution of partisan preferences is shifted downward, against the incumbent. The same is true for the independent variable: indeed, even if the net effect of administration policies were to impose precisely identical income losses on every voter, we would still see considerable variation in the observed changes in overall incomes, since individual incomes are also affected in a variety of other nongovernmental influences (entries or retirements from the workforce, promotions or seniority-related raises, inheritances or other windfall gains and losses) which still vary considerably across individuals. Government macroeconomic policies do not eliminate these idiosyncratic differences, but rather simply induce shifts in the overall distribution of individual income changes, upwards or downwards.

Therefore, if we consider the two variables together, it is evident that the main effect operating in this hypothetical election will be a shift in the position of the entire bivariate distribution or scatterplot, downward and to the left of its position in a "normal," nonrecession election, as shown in Figure 1a. It is not clear that there should be any particular effect on the shape of the individual-level scatterplot itself, or indeed what this shape should

[Figure 1 about here]

be in the first place. To consider just one possibility, if the government-induced income losses were precisely the same for every voter, all variation in individual income changes would be due to the exogenous idiosyncratic effects. If these factors should happen to operate so as to redistribute incomes upwards (as is typical in post-war U.S. recessions), the larger changes will tend to fall disproportionately on upper-income voters, who in turn are more likely to be Republicans in their partisan predispositions. Thus with a Democratic incumbent under these conditions we might actually find a modest negative correlation between votes and income changes at the individual level, as shown in the figure. This cross-sectional correlation tells us nothing about the actual effects of economic conditions on individual voting decisions: it is quite spurious, and arises simply because the "lucky" voters who receive the above-average income gains happen (in this case) to be anti-administration Republicans to begin with. In any event, the essential point is that whatever the final shape of the scatterplot, the relationship it represents, between individual-level income changes and votes in the election in question, is quite unrelated to the main effect of interest, the overall shift in the position of the scatterplot itself.

In an aggregate-level statistical analysis we would not work with the individual-level scatterplot, but would replace it by aggregate summary statistics such as V_t and Y_t, the mean vote and income change in election t. In a time-series analysis we are essentially fitting a regression line to the (V_t,Y_t) points over a series of elections. As Figure 1b shows, if there is enough variation in the sample of elections, the regression line will convey a good

idea of how the scatterplot shifts position from one election to another as economic conditions change, so the time series regression coefficient, b^{TS}, may well be a good estimate of how voters respond to variations in government economic performance.

In an individual-level cross-sectional analysis, on the other hand, we would instead fit a regression line to one of the individual scatterplots; since these are well-behaved, with variation in both individual-level variables, we will still obtain a well-defined, and possibly even significant, cross-sectional regression coefficient, b^{CS}. But since this regression coefficient is invariant under changes in the means of the dependent or independent variables, it will totally miss the important effect, the change in the overall position of the scatterplot itself (which is, essentially, a change in means). Thus, whatever the relationship being estimated by the cross-sectional regression, it is clearly not the one we are interested in. This, in essence, is why the individual-level cross-sectional estimates go awry.

We turn now to a more careful and explicit analysis of these and related issues. Before developing the analytical argument in detail, however, it will be well to note two general caveats which should be kept in mind throughout.

First, we take as a given that the substantive question of interest is that of how the government's performance in managing the economy during its term in office affects its fortunes in the subsequent election. Thus, we assume the policies and actions undertaken by the administration eventually result in economic gains or losses to voters; the question to be studied is whether, and how, these gains and losses in turn affect their subsequent voting decisions at election time. We make no specific assumptions as to the nature or form of the relevant effects, e.g., as to whether they are incumbency- or party-oriented in direction, or are primarily self-interested, or "sociotropic," in nature. We do assume, however, that we are ultimately interested only in how real economic outcomes affect actual voting decisions, and not in economic rhetoric or perceptual imagery as such. Thus, a finding to the effect that the popularity of an incumbent administration is correlated with voters' assessments of its success in managing the economy would be relevant for our purposes only to the extent that such popularity ratings and performance assessments are related to actual voting decisions and real economic outcomes. If (to take an extreme case) voters' self-reported assessments of the state of economy turn out to be simply the product of their partisan predispositions or their exposure to an intensive media campaign, and are unrelated to actual, measurable economic events, then the empirical finding in question would have no bearing on the basic question of interest here. This point is particularly relevant to Section 5, where we consider voters' subjective perceptions of the performance of the economy as an explanatory variable; in the earlier sections things are generally posed in "real" terms throughout.

Secondly, within the substantive context described above, the specific issue toward which the analysis is directed is that of cross-

sectional analyses of individual-level (e.g., survey) data, versus time-series analyses of aggregate data. Obviously, other combinations of these categories are logically possible as well, e.g., time-series analyses of individual-level (e.g., panel) data, or cross-sectional aggregate-level analyses (e.g., by states), but we shall not be directly concerned with these. In the interest of brevity we will usually simply refer, interchangeably, to "cross-sectional" or "individual-level" or "micro-level" versus "aggregate-level" or "time-series" variables or estimates, without repeating all of the above qualifications; they are intended throughout, however.

This said, we turn now to the analysis. In section 2 we sketch out a simplified bivariate version of the models used in this area, in which changes in individuals' personal economic wellbeing (as measured by their real incomes) are an important influence on their subsequent voting decisions. Technical details and derivations are relegated to an Appendix (available on request). In Sections 3 and 4 we show that the model, along with plausible substantive assumptions about the variances and covariances of the various economic and political variables involved, predicts the following empirical results: In an aggregate, time-series regression, the time-series estimate b^{TS} will depend directly, though with some moderate attenuation, on the true coefficient β. Hence, if the underlying effect is strong enough, we should expect to find a reasonably strong estimated relationship, in the proper direction, between changes in per capita income and aggregate election returns (or popularity, or whatever). In a regression across individuals, on the other hand, the true effect will be heavily attenuated, and the cross-sectional coefficient b^{CS} will be sensitive to a spurious covariance term, which depends on the (quite irrelevant) correlation between individual income changes and partisan predispositions. These two variables are unlikely to be very highly correlated, so the estimated cross-sectional relationship will generally be weak or nonexistent. But there is also good reason to believe that direction and magnitude of the income change-partisanship correlation--the tail that wags the dog in the cross-sectional estimate--will vary with the stage of the business cycle, the identity of the incumbent, and other factors which change over time. Hence, in addition to a weak relationship, we should also expect to find an unstable one, which varies considerably from one election to the next, perhaps even with some occasional sign reversals. These predictions are, of course, a reasonably accurate if somewhat stylized description of the main features of what the various time-series and cross-sectional survey studies have, in fact, found. The empirical results of these studies thus confirm the basic hypothesis and common-sense assumptions mentioned above, and show little else beyond that.

In Section 5 we turn to a different matter, and consider the evidence for altruistic or "sociotropic" voting. The argument here is severalfold. We first show that the sociotropic voting hypothesis implies essentially the same empirical findings as those predicted by the earlier, "self-interested" model: a reasonably strong aggregate-

level effect, and a weak and unstable relation between votes and personal wellbeing at the individual level. This evidence, therefore, does not distinguish between sociotropic and self-interested behavior. We then consider an additional (and more persuasive) body of evidence on the question, which shows a positive relationship between individuals' vote intentions and their "sociotropic judgments," or perceptions of the administration's overall success in managing the economy at large. We show that with a purely sociotropic electorate these "sociotropic performance ratings" should indeed be positively related to individual votes. This relationship, however, is a spurious one, and is essentially unrelated to the underlying behavioral relationship of interest. The cross-sectional estimate obtained by regressing votes on performance ratings, in particular, is hopelessly biased, and in fact is quite independent of the true value of the underlying sociotropic behavioral parameter. Moreover, this spurious relationship between votes and subjective "performance ratings" would also be observed even with a purely self-interested electorate, in which voters respond solely to personal "pocketbook" issues, and not at all to sociotropic considerations.

Thus none of the currently available evidence on the question is capable of effectively discriminating between sociotropic and self-interested behavior. While sociotropic concerns may indeed be an important influence on voter behavior, the task of demonstrating this, and more generally of obtaining meaningful estimates of their impact, is one which remains to be performed.

2. THE MODEL

To keep things simple, we shall ignore various realistic but inessential complications which arise from stochastic variability, or from the need to control for differing incumbencies or other extraneous variables. To reduce the problem to its essence, consider the following simplified case: A fixed electorate of n voters ($i = 1,2,\ldots,n$) votes in each of T elections ($t = 1,2,\ldots,T$). The same party happens to be incumbent during all elections.[7] Voters vote by expressing a judgment for or against the incumbent party, which we assume to be a continuous variable; we denote by v_{it} the "vote" or judgment of the i^{th} voter in the t^{th} election.[8] The administration's economic policies (and other actions) affect the personal financial wellbeing of voters, and these gains or losses are assumed to influence their subsequent voting behavior in a simple and direct way. In particular, we assume that the relevant financial impacts on voter i can be represented as a change in his real income, and that i's vote in election t is determined by the following simple, purely deterministic relation:

(i) $$v_{it} = \alpha_i + \beta g_{it},$$

where g_{it} is the government-induced change in i's income over the period preceeding the election, and α_i and β are unknown parameters.[9] (We are thus implicitly assuming "self-interested" voters who respond to changes in their own incomes; the alternative possibility, of altruistic or "sociotropic" voters, is considered explicitly in section 5 below.) If g_{it} were zero--i.e., if administration policy had a precisely neutral effect on i's income--then i's vote would be $v_{it} = \alpha_i$; hence the parameter α_i is essentially a measure of i's partisan predisposition, or of the extent to which i is disposed to vote for the administration on the basis of other, noneconomic, issues. The behavioral parameter β, which measures the dependence of votes on administration economic performance, is the one we want to estimate; we assume this is essentially constant across voters (in contrast to the partisanship parameter α_i, which varies from voter to voter).

If we could observe the variables g_{it} and v_{it} over several elections for all voters, it would in principle be a simple matter (at least for the deterministic case we are considering) to estimate the α_i and β parameters. Moreover, even if such individual-level panel data were not available, we could still estimate the parameter of interest, β, in other ways, e.g., with aggregate-level data over a series of elections. In particular, if we form the aggregate-level variables $V_t = 1/n \sum_i v_{it}$ (the aggregate vote for the incumbent in election t) and $G_t = 1/n \sum_i g_{it}$ (the average government-induced income change at t), it is then a simple matter to see that the relation between these aggregate-level variables is

$$V_t = 1/n \sum_i v_{it} = 1/n \sum_i (\alpha_i + \beta g_{it}) \text{ from (i)}$$

$$= 1/n \sum_i \alpha_i + \beta\, 1/n \sum_i g_{it} = \bar{\alpha} + \beta G_t,$$

where $\bar{\alpha} = 1/n \sum_i \alpha_i$ is the mean partisanship in the electorate. Hence if we regress V_t on G_t over a series of elections, the regression line will fit perfectly (in the purely deterministic case we are assuming here), and the slope will be the behavioral parameter β.

An alternative possibility is to use a cross-section of individuals i in a single election t. Note that we can rewrite the individual-level relationship (i) as

$$v_{it} = \alpha_i + \beta g_{it} = \bar{\alpha} + \beta g_{it} + (\alpha_i - \bar{\alpha}), = \bar{\alpha} + \beta g_{it} + u_i,$$

where as before $\bar{\alpha}$ is the mean or average partisanship in the electorate, and $u_i = (\alpha_i - \bar{\alpha})$ is i's partisanship in deviation form. In this relation u_i looks much like the residual disturbance term in a classical bivariate regression. If we now regress individual vote intentions v_{it} on government-induced income changes g_{it}, the least-squares line will not fit perfectly (since the "residuals" u_i are nonzero), but if the usual regression assumptions are satisfied by the underlying relationship, the slope of this cross-sectional regression line will still be an unbiased estimate of the parameter of interest, β. In the present context, the critical assumption required for this is that the "disturbance" u_i be uncorrelated with the explanatory variable g_{it}: if this is so, the cross-sectional estimate will give an accurate idea of the true individual-level effect. If not, however--i.e., if there is some correlation between income changes and partisanship across individuals--then the estimate will be biased to some degree.

Unfortunately, however, all this is irrelevant to the real inference problem we face, since in practice we would be unable to observe the proper explanatory variable g_{it} (or G_t) directly. What we can observe, instead, are the net changes in voters' <u>total</u> incomes. But while these are affected to some degree by governmental activities, they are also influenced by a variety of extraneous or idiosyncratic factors, which operate quite independently of the activities of the incumbent administration. Among these exogenous factors will be some macro-level events whose effects are widespread throughout the economy (or electorate), such as OPEC-induced shocks in energy supplies and prices, major natural disasters, and the like. Many of the relevant factors are much more localized or personalized in impact, however, and will largely cancel each other out at the aggregate level. For example, local variations in weather patterns may seriously affect the incomes of farmers or ski-lift operators in certain geographic areas, but normally will be of only minor importance for the economy at large; the same will be true for sectoral changes in the economy, with losses in declining industries or regions being largely offset by gains in growth industries or areas. At a more individualistic level, life-cycle differences will be a major source of variation in individual income changes: for example, younger workers entering their most productive years will usually enjoy above-average income gains irrespective of macroeconomic conditions (because of upward job mobility, mandated seniority-related salary raises, and the like), while older or retiring workers typically get below-average increases. The addition or departure of a dependent child will have a major effect on a household's financial wellbeing, and individual-level windfall events (inheritance or gifts, casualty losses from fire or accident, etc.) will also play a role. All of these factors produce considerable cross-sectional variation in individual income changes. The net effect at the aggregate level will be much smaller, however, because of the cancellation of offsetting effects

Let e_{it} be the net change in i's income during period t resulting from all these extraneous (and politically irrelevant) factors, and g_{it} be the government-induced change, as before. Then the observable variable y_{it}, the net change in i's total income, will be given by

(ii) $$y_{it} = g_{it} + e_{it}.$$

The behavioral relation (i) we are trying to estimate involves only the government-induced component, but since neither it or the idiosyncratic component e_{it} is observable, we must work instead with the "contaminated" variable y_{it}. This gives rise to some serious estimation problems, to which we now turn.

3. THE AGGREGATE-DATA TIME-SERIES ESTIMATE

To obtain the time-series estimate we form the aggregate-level variables V_t, the aggregate vote in election t, and $Y_t = 1/n \sum_i y_{it}$, the change per-capita income over the period preceeding the election.

Since $y_{it} = g_{it} + e_{it}$ (from (ii)), the aggregate-level variable Y_t can be similarly decomposed into government-induced and exogenous components: $Y_t = 1/n \sum_i(g_{it} + e_{it}) = G_t + E_t$, where G_t is the mean (or per-capita) government-induced income change and E_t is the mean exogenous change. In analyzing the aggregate data we fit a regression line of the form $a^{TS} + b^{TS} Y_t$ to the (V_t, Y_t) points over a series of T elections. The time-series regression coefficient b^{TS} is then our estimate of the underlying behavioral parameter β. It is shown in the Appendix (line 20), that this estimate will in general be related to the true coefficient value as follows:

(iii) $$b^{TS} = \beta \frac{cov^{TS}(G_t,Y_t)}{var^{TS}(Y_t)} = \beta\left[\frac{var^{TS}(G_t) + cov^{TS}(G_t,E_t)}{var^{TS}(G_t) + 2cov^{TS}(G_t,E_t) + var^{TS}(E_t)}\right],$$

where var^{TS}, cov^{TS} are sample variances and covariances of the aggregate-level variables in question, over the T elections. Since the quantity in square brackets is not unity, in general the estimate will be biased; the magnitude of the bias depends on the various variances and covariances involved.

Let us now try to get some sense of their probable magnitudes. G_t is the change in per-capita income arising from government activities. It should be noted that this quantity depends only indirectly on the level of government spending as such: for example, to take an extreme case, if all government expenditure were for purely redistributive direct income transfers, then the individual-level gains of recipients ($g_{it} > 0$) would precisely offset the losses of the "donors" ($g_{it} < 0$), so at the aggregate level the net change G_t would be precisely zero, irrespective of the level of spending. However, if this redistribution involves some deadweight loss to the economy--due to direct administrative costs and waste, or to indirect efficiency losses resulting from incentive distortions and the like--then G_t would be negative (and the magnitude of this deadweight loss will presumably vary with the level of spending). On the other hand, much of government expenditure is for the provision of public goods and services, rather than for direct income transfers as such, and to the extent that such expenditure is productive (in the sense of providing valued goods and services which would be underproduced, or produced less efficiently, by the private sector) the aggregate income increment resulting from such programs will be positive (and presumably will also vary with the level of expenditure). Since the deadweight losses and efficiency gains work in opposite directions, they tend to cancel each other. Moreover continuing multi-year programs and mandated expenditure requirements create great inertia in spending levels, and permit them to respond only slowly, and only with considerable lag, to exogenous economic fluctuations. Hence, all things considered, these spending-level effects are unlikely to contribute significantly to the variance of G_t, or to produce any sizable correlation between G_t and E_t.

The important variations in G_t are thus not tied directly to the level of expenditure, but arise from quite different sources. One important factor, particularly in recent times, is macroeconomic

policy. Thus, if government expenditures are maintained or increased during economic downturns (negative E_t) despite falling tax revenues, this will create a positive G_t, both directly (the gains of recipients or consumers of government programs will not be fully offset by the losses of the taxpaying "donors") and indirectly, because of the effects on the level of economic activity. Thus, to the extent that conscious macroeconomic policy or "built-in stabilizers" attempt to compensate for exogenous economic fluctuations, G_t will tend to be somewhat negatively correlated with E_t. Other factors which produce important fluctuations in G_t operate in a less systematic manner. These include foreign policy developments (grain embargos, negotiation of new trade agreements) which have important economic ramifications, and intended or unintended macro-level consequences of domestic policies in various areas (e.g. safety or environmental legislation; attempts to intervene in behalf of specific sectors, such as farmers or auto workers; attempts to control or decontrol energy prices or wage settlements; and so on). Developments of this kind are typically responses to specific pressures or policy problems, not directly related to macroeconomic targets; hence, in the long run, over several administrations and business cycles, their effects are probably largely uncorrelated with short-term exogenous economic fluctuations.

On balance, then, we should expect G_t to be largely uncorrelated, or perhaps somewhat negatively correlated, with the exogenous term E_t. Moreover, since the government-induced effect G_t may be a sizeable quantity, given energetic economic management or mismanagement, its variance is probably comparable in magnitude to that of E_t.[10]

To see what this means for the bias in the time-series estimate, first consider the simpler case in which E_t and G_t are completely uncorrelated over the sample period. Then the expression (iii) reduces to:

$$b^{TS} = \beta\left[\frac{var^{TS}(G_t)}{var^{TS}(G_t) + var^{TS}(E_t)}\right] = \beta\left[\frac{var^{TS}(G_t)}{var^{TS}(Y_t)}\right]$$

(since $cov^{TS}(G_t,E_t) = 0$). The quantity in square brackets is the proportion of variance of per-capita income changes arising from government-induced changes. Since this proportion necessarily lies between zero and one, b^{TS} will lie between zero and the true value β. The estimate will therefore be of the correct sign, but will somewhat understate the true effect. The degree of understatement depends on the proportion of total variance contributed by the government-induced effects: if they account for half the total, for example, the estimate will be half the correct value, β.

In the more general case, G_t and E_t may be somewhat negatively correlated because of compensatory macroeconomic policies. Let us suppose, in this case, that G_t consists of two components, an uncorrelated component U_t (for which $cov(U_t,E_t) = 0$) and a countercyclical component C_t, which compensates or offsets some portion π of the exogenous income change E_t. Thus $C_t = -\pi E_t$, and $G_t = U_t + C_t = U_t - \pi E_t$. It is then straightforward to show that

$$b^{TS} = \beta\left[\frac{var^{TS}(U_t) + \pi(1 - \pi)var^{TS}(E_t)}{var^{TS}(U_t) + (1 - \pi)^2 var^{TS}(E_t)}\right]$$

Since the numerator and denominator are both positive for any π, the estimate is of the correct sign. Moreover, since $\pi(1 - \pi) < (1 - \pi)^2$ for values of π in the plausible range $0 < \pi < 1/2$, the quantity in square brackets will be less than unity, so the estimate will, again, be somewhat attenuated. To pick some typical values, if $\pi = 1/4$ and the variances of U_t and E_t are approximately equal, we will have $b^{TS} = 19/25\ \beta$, which is rather less attenuation than in the previous case. As π decreases the attenuation increases, with $b^{TS} = 1/2\ \beta$ in the limiting case of $\pi = 0$; conversely the attenuation diminishes as π increases (the estimate would actually exaggerate the true effect for the unlikely case in which π exceeded 1/2).

In summary, then, we should expect the aggregate time-series estimate to be of the correct sign, though probably somewhat attenuated in magnitude. The attenuation is not overwhelming, however, and b^{TS} should give a reasonable order-of-magnitude estimate of the true value of β; a sizable estimate is a valid indication of a (probably still larger) real effect in the underlying behavioral relation.

4. THE INDIVIDUAL-LEVEL CROSS-SECTIONAL ESTIMATE

We now choose a fixed election t, and regress individual votes v_{it} on income changes $y_{it} = g_{it} + e_{it}$ across the n individuals. It is shown in the Appendix (line 21) that the cross-sectional regression coefficient b^{CS} will then be:

$$\text{(iva)} \quad b^{CS} = \beta \frac{cov^{CS}(g_{it},y_{it})}{var^{CS}(y_{it})} + \frac{cov^{CS}(\alpha_i,y_{it})}{var^{CS}(y_{it})}$$

$$\text{(ivb)} \quad = \beta \left[\frac{var^{CS}(g_{it}) + cov^{CS}(g_{it},e_{it})}{var^{CS}(g_{it}) + 2cov^{CS}(g_{it},e_{it}) + var^{CS}(e_{it})} \right].$$

$$+ \left\{ \frac{var^{CS}(\alpha_i,g_{it}) + var^{CS}(\alpha_i,e_{it})}{var^{CS}(g_{it}) + 2cov^{CS}(g_{it},e_{it}) + var^{CS}(e_{it})} \right\}.$$

Here var^{CS}, cov^{CS} denote cross-sectional variances and covariances of individual-level variables, which in general are quite different from the time-series variances and covariances of the corresponding aggregate-level variables. In particular, the idiosyncratic component will be the major source of the cross-sectional variance in individual incomes, and the government-induced component will play a much smaller role.

To see what this means for the estimate, first consider the limiting case in which government policies affect only macroeconomic variables, and do not change the pattern of incidence at all. Then the government-induced effect will be the same for all voters, i.e., $g_{it} = G_t$ for all i, so the variance var(g) and covariances cov(g,e) and cov(g,α) will be all the zero (here, and henceforth in this section, we omit the CS superscripts and it subscripts when no ambiguity will result). The expression (vib) then reduces to:

$$b^{CS} = \beta \left[\frac{0 + 0}{0 + 2 \cdot 0 + var(e)} \right] + \left\{ \frac{0 + cov(\alpha,e)}{0 + 2 \cdot 0 + var(e)} \right\} = \frac{cov(\alpha,e)}{var(e)} .$$

Thus the estimate does not depend on the true value β at all! It is completely determined by a spurious term, which concerns the quite irrelevant correlation between exogenous individual income changes and partisan predispositions. Clearly, in this case, the cross-sectional estimate will tell us nothing whatever about the underlying behavioral relationship of interest.

More generally, even if there is some variation in g_{it} across individuals, its variance will still be small relative to that of the idiosyncratic component. g may be correlated to some extent with e in this case, but the correlation is probably very weak: empirical studies of incidence (in the US, at least) typically find that the net incidence of government spending is more or less uniform (proportionately) across income classes and that the pattern does not change much over time.[11] Hence cov(g,e) is probably small relative to the other variance terms involved. If for simplicity we take it to be zero, then cov(g,y) = var(g), so the expression (iva) reduces to:

$$\text{(va)} \quad b^{CS} = \beta \left[\frac{var(g)}{var(y)} \right] + \frac{cov(\alpha,y)}{var(y)}$$

Since var(e) is large relative to var(g), the β term will be heavily attenuated, and the second spurious term will still have considerable effect on the estimate. To assess this effect, we first rewrite things in terms of more familiar sample statistics. The sample correlation between α and y is $r_{\alpha y} = \frac{cov(\alpha,y)}{\sqrt{var(\alpha)var(y)}}$, so $\frac{cov(\alpha,y)}{var(y)} = r_{\alpha y} \sqrt{\frac{var(\alpha)}{var(y)}} = r_{\alpha y} \frac{S_\alpha}{S_y}$, where S_α and S_y are the sample standard deviations of α and y. Hence (va) can be rewritten as

$$\text{(vb)} \qquad b^{CS} = \beta\left[\frac{\text{var}(g)}{\text{var}(y)}\right] + r_{\alpha y}\frac{S_\alpha}{S_y}.$$

Let us now make the following illustrative quantitative assumptions: suppose that the distribution of individual-level income changes, measured in percentage form, has a standard deviation of around 5 percentage points.[12] Let votes and partisanship be scaled from 0 to 100, and suppose that the distribution of partisanship (i.e., of α) has a standard deviation of 25 points.[13] Further suppose that the government-induced component g accounts for 10 percent of the variance of individual income changes, and that the true value of β is .5 (so that, at the aggregate level, a 10 percent government-induced change in per-capita income would produce a 5 percent shift in votes). Under these assumptions, (viib) becomes:

$$b^{CS} = .5(.10) + r_{\alpha y}\frac{25}{5} = .05 + 5\ r_{\alpha y}$$

Clearly the real effect is essentially washed out, and the estimate depends primarily on the second, spurious term. Although individual income changes are unlikely to be highly correlated with partisanship, some weak accidental correlation is surely inevitable. Even if $r_{\alpha y}$ never exceeded .05 in magnitude--a very modest range--the spurious term could then range from -.25 to +.25, and thus would dominate the estimate.

Moreover, there is good reason to believe that the $r_{\alpha y}$ correlation probably varies in both magnitude and sign from election

to election. Economic upturns tend to redistribute incomea downward, and thus to disproportionately benefit low-income Democrats: hence we might well expect to find $r_{\alpha y} > 0$ in boom-year elections under Democratic administrations, or $r_{\alpha y} < 0$ under Republican incumbents. Recession-year elections should reverse these, since incomes then tend to be redistributed upward. To the extent that these and other effects do cause $r_{\alpha y}$ to vary, the cross-sectional estimates b^{CS} obtained in different elections will be quite unstable and inconsistent, in both magnitude and sign.

Different assumptions might improve things somewhat, but not enough to change the basic conclusion. For example, to make things more favorable for the cross-sectional approach, let us change the above assumptions as follows: suppose that the government-induced component accounts for 25 percent of the variance in individual income changes. Moreover, suppose we introduce some specific measures of partisanship, or other control variables, to reduce the effect of the spurious term. Of course these controls will not be perfect, so there will still be an unmeasured partisanship component which enters into the cov(α,y) term. Let us be quite optimistic, however, and suppose that our controls succeed in accounting for half the variance of partisanship. The unmeasured α component will then represent 50 percent of the total variance, so its standard deviation would then be $\sqrt{.50}(25) = 17.5$. Putting these assumptions into (vb), we find that:

$$b^{CS} = .5(.25) + r_{\alpha y}\frac{17.5}{5} = .125 + 3.5\ r_{\alpha y}$$

Clearly we are still seriously underestimating the true effect ($\beta = .5$). Moreover since the spurious 3.5 $r_{\alpha y}$ term is of the same order of magnitude as the first term, it will still have a major effect on the estimate, and will still create instability (and occasional sign reversals, when $r_{\alpha y}$ gets negative enough) in the cross-sectional estimates obtained from different elections. Moreover, because the bias arising from the $r_{\alpha y}$ term is additive rather than multiplicative, it is not feasible to try to improve the estimate by simply correcting for the attenuation of the first term.

Table 1 below summarizes the results of similar calculations as the two critical quantities, var(g)/var(y) and var(α), are varied from plausible through unrealistically extreme values. Only in the implausible "extreme, extreme" case at the lower right hand corner do we get a reasonably stable estimate of β, and even there the midrange value, $b^{CS} = .25$, is a considerable underestimate. In every other case the spurious $r_{\alpha y}$ term is comparable in magnitude to the β term, so the estimates will be unstable and inconsistent in both magnitude

[Table 1 about here]

and sign. The conclusion therefore seems clear: under any realistic view of the probable empirical magnitudes at work here, the estimates obtained by regressing individual votes on changes in personal wellbeing is essentially unrelated to the real effect of interest.

5. SOCIOTROPIC VOTING

The analysis so far has proceeded on the basis of the behavioral relationship (i). According to this specification, voter i's vote depends directly on g_{it}, the government-induced change in his personal real income during period t. While this can be interpreted in different ways--e.g., it may be that voter i regards g_{it} as a "signal" of the overall success of government's economic policies in improving the general welfare--clearly the simplest and most obvious interpretation is simply that voter i is acting in terms of his own personal self-interest, narrowly defined. In contrast to this somewhat egoistic or selfishly motivated type of voter, we can also consider the possibility of other-regarding or sociotropic voters. As Kinder and Kiewiet, for example, picture him,

> . . . the prototypic sociotropic voter is influenced most of all by the nation's economic condition. Purely sociotropic citizens vote according to the country's pocketbook, not their own. Thus the party in power suffers in the polls during hard times because voters act on their negative assessments of national economic conditions--quite apart from the trials and ribulations of their own lives.
>
> . . . The sociotropic voter asks political leaders not "What have you done for me lately?" but rather "What have you done for the country lately?"[14]

After reviewing findings of other studies and perfoming their own analysis, they conclude that U.S. voters, at least, do behave sociotropically, and respond hardly at all to self-interested "pocketbook" considerations.

To examine the evidence on sociotropic voting, we must first formally characterize sociotropic behavior. A sociotropic voter is one who responds, not to changes in his own personal economic wellbeing, but rather to changes in aggregate or collective wellbeing. A natural and obvious measure of such collective improvement is $G_t = 1/n \sum_i g_{it}$, the government-induced change in average or per capita real income. If we take this as the relevant sociotropic index, a sociotropic voter is then one whose behavior is governed by a relationship such as

(vi) $$v_{it} = \alpha_i + \beta_2 G_t ,$$

instead of the "self-interested" relation in (i). In (vi), β_2 is the true "sociotropic" parameter, to be distinguished from the self-interest parameter considered so far (to avoid ambiguity we henceforth denote the self-interest effect--β of (i)—by β_1).

Let us now see how the estimates considered in the previous two sections would be affected if voters actually behaved sociotropically. At the aggregate level, if we again regress the aggregate vote V_t on Y_t, resulting time-series estimate will be

(vii) $$b^{TS} = \beta_2 \frac{cov^{TS}(G_t, Y_t)}{var^{TS}(Y_t)}$$

(Appendix, line 27). This is essentially identical to (iiia), except that it now involves the sociotropic coefficient β_2, in place of the earlier self-interest parameter. From the discussion and considerations of section 3, we should therefore expect the aggregate

analysis to yield a good, if somewhat attenuated, estimate of the real (in this case sociotropic) effect.

The individual-level cross-sectional regression of votes v_{it} on changes in personal wellbeing y_{it} should not be expected to yield a good estimate of the true sociotropic effect, β_2, since it involves the "wrong" explanatory variable, personal wellbeing. Ideally, in fact, we might hope that the estimate will be zero, since sociotropic voters do not in fact respond at all to personal considerations. As it turns out (Appendix, line (28)) the cross-sectional estimate is

(viii) $$b^{CS} = \frac{cov^{CS}(\alpha_i, y_{it})}{var^{CS}(y_{it})} = r_{\alpha y} \frac{S_\alpha}{S_y}$$

Thus, as expected, b^{CS} does not depend on the true sociotropic effect, β_2; however, the estimated effect is not necessarily zero, for it still involves the spurious $r_{\alpha y}$ term encountered in the previous section. As shown there, under plausible data assumptions the $r_{\alpha y}$ term dominates the estimate even under the "self-interested" hypothesis (i), so the behavior of the estimate is essentially identical under either hypothesis: we should expect generally weak, somewhat unstable estimates, with occasional sign reversals, in either case. The personal wellbeing estimates simply do not discriminate between the sociotropic and self-interest hypotheses.

The more persuasive evidence for sociotropic voting lies not in the nonfindings concerning the role of personal economic circumstances, however, but rather in the further finding, common to several studies, to the effect that individual voting decisions are positively and consistently related to "sociotropic judgments" of various kinds. These self-reported judgments include individuals' assessments of how successfully the government is handling economic problems, or of which party is more competent in economic affairs, and their perceptions of current trends in general business conditions. These measures all perform similarly, and the differences between them are not important for present purposes. Hence we consider a typical performance measure, which we assume can be represented as a continuous or scaled variable, and denote by p_{it} voter i's assessment of the government's sociotropic performance in managing the economy in period t. To investigate the relation between these performance judgments and voting, we do a cross-sectional regression of the form $v_{it} = a + b_p p_{it}$ + error; b_p is then the estimated sociotropic effect.

If we still assume the electorate is composed entirely of sociotropic voters who behave according to the sociotropic relationship (vi), then (Appendix, line (35)) the estimate will be related to the underlying model parameters as follows:

$$\text{(ix)} \qquad b_p = \frac{\text{cov}^{CS}(v_{it}, p_{it})}{\text{var}^{CS}(p_{it})} = \frac{\text{cov}^{CS}(\alpha_i, p_{it})}{\text{var}^{CS}(p_{it})} = r_{\alpha p} \frac{S_\alpha}{S_p}$$

Thus, interestingly, the estimated sociotropic effect b_p does not depend on the true sociotropic parameter β_2 at all!

This is, on reflection, not really so surprising. Since we are ultimately interested in real sociotropic effects--i.e., in the

effect of the government's actual economic performance on votes--the validity of the estimate will depend critically on the relationship between the government's actual sociotropic performance, G_t, and voters' sociotropic perceptions, p_{it}. Define $u_{it} = p_{it} - G_t$ as the discrepancy between perception and reality. Then, equivalently, $p_{it} = G_t + u_{it}$, so we can think of the explanitory variable p_{it} as being composed of two parts, a "real" component G_t, and perceptual "noise" or error, u_{it}. If we now aggregate over individuals, then $P_t = 1/n \sum_i p_{it} = 1/n \sum_i (G_t + u_{it}) = G_t + \bar{u}_t$, where $\bar{u}_t = 1/n \sum_i u_{it}$ is the sample mean of the perceptual errors. From this it follows that $(p_{it} - P_t) = (u_{it} - \bar{u}_t)$. This implies that the cross-sectional variance and all covariances involving the performance rating variable p_{it} depend only on perceptual noise, and not at all on the real sociotropic measure, G_t. Since the cross-sectional estimate is a function solely on these variances and covariances (cf. (ix) above), it is not surprising that it therefore completely misses any actual sociotropic effect which may be present.

The estimate does depend, however, on another term involving $r_{\alpha p}$, the correlation between individual partisanship and performance ratings. This term is also spurious in relation to the real effect we are trying to estimate, but is rather different in nature from the somewhat analogous spurious income-partisanship $r_{\alpha y}$ term encountered earlier. In the present case the term involves the correlation between voters' partisanship and their perceptions of government performance (or actually, as noted above, the errors in such perceptions); it is thus a reflection of perceptual bias, or an inverse measure of cognitive dissonance. There is abundent evidence[15] to the effect that such biases are present, and generally operate so as to reduce dissonance by bringing perceptions into accordance with partisan preferences. Hence, in contrast to the weak and unstable $r_{\alpha y}$ term encountered earlier in the "personal wellbeing" regressions, we should expect the present $r_{\alpha p}$ term to be consistently positive. (As before, to the extent that better explicit controls on partisanship reduce S_α, the spurious term would be lessened in magnitude; in the present case, however, this would simply drive the estimate toward zero, and would not improve it as an estimate of β_2.) Thus we should expect b_p to be positive, and probably sizeable. Clearly, however, this estimate is artifactual, and does not demonstrate any real sociotropic effect.

Let us now consider the situation under the alternative hypotheses of self-interested voting. When all voters act according to the self-interested relationship (i), the sociotropic estimate is

$$\text{(x)} \qquad b_p = \beta_1 \frac{\text{cov}^{CS}(p_{it}, g_{it})}{\text{var}^{CS}(p_{it})} + \frac{\text{cov}^{CS}(\alpha_i, p_{it})}{\text{var}^{CS}(p_{it})} = \beta_1 r_{pg} \frac{S_g}{S_p} + r_{\alpha p} \frac{S_\alpha}{S_p}$$

(Appendix, line (34)). The second term is the same "cognitive dissonance" factor appearing in (ix) above. In addition, however, we now have another term involving the true (self-interest) coefficient β_1. The magnitude and sign of this first term depends on the correlation r_{pg} between individual performance assessments p_{it} and

government-induced changes in personal wellbeing, g_{it}. If the two are uncorrelated, the β_1 term drops out, and (x) becomes identical to (ix). On the other hand, to the extent that individuals do extrapolate or project their own experiences with the effects government policies onto the economy at large, we might expect some positive correlation between g_{it} and individual performance ratings p_{it}. To this extent this is so the expression (x) will be somewhat larger than (ix). This implies that the estimate will be somewhat larger under the self-interest hypothesis than in the sociotropic case; thus, if anything, a large performance-rating estimate b_p should be taken as evidence for self-interested rather than sociotropic voting! (In practice, the β_1 term would likely be small relative to the $r_{\alpha p}$ term, however, so again the realistic conclusion is that the cross-sectional estimate is simply unable to discriminate between the hypotheses.)

Though we have considered only the polar extremes of purely self-interested or purely sociotropic voting, and have looked only at simple bivariate regressions using the two explanatory variables, our conclusions apply quite generally. In particular, suppose we specify a generalized model of the form

$$\text{(xii)} \qquad v_{it} = \alpha_i + \beta_1 g_{it} + \beta_2 G_t$$

which incorporates both self-interest and sociotropic effects, and then to attempt to estimate β_1 and β_2 simultaneously with a multiple regression of v_{it} on y_{it} and p_{it}.

Before considering the multiple-regression estimates let us note in passing how the various bivariate regressions considered above would perform under the more general specification (xii) above. First, in the time-series regression of V_t on changes in per capita income Y_t, the estimate is

$$\text{(xiii)} \qquad b^{TS} = (\beta_1 + \beta_2) \frac{\text{cov}^{TS}(G_t, Y_t)}{\text{var}^{TS}(Y_t)}$$

(Appendix, line (31)). This is closely akin to (iiia) and (vii), except that it now involves both self-interest and sociotropic effects. Thus we should expect the aggregate analysis to yield a reasonable, if somewhat attenuated, estimate of the overall net effect; it is not possible to disentangle the separate self-interest and sociotropic coefficients with this kind of analysis, however. In the individual-level cross-sectional analysis (and omitting the CS superscripts and it subscripts), the regression of votes on personal wellbeing yields an estimate of the form

$$\text{(xiv)} \qquad b_y = \beta_1 \frac{\text{cov}(g,y)}{\text{var}(y)} + \frac{\text{cov}(a,y)}{\text{var}(y)},$$

(Appendix, line (32)), while regressing votes on individual performance ratings gives

$$\text{(xv)} \qquad b_p = \beta_1 \frac{\text{cov}(g,p)}{\text{var}(p)} + \frac{\text{cov}(a,p)}{\text{var}(p)}$$

(Appendix, following line (35)). These expressions are identical to (iv) and (x) respectively, which obtain under the pure self-interest

hypothesis, so once again the estimates will entirely miss any real sociotropic effect β_2 which may be present, and will also fail to yield a meaningful estimate of the self-interest effect, because of the heavy attenuation of the β_1 term.

Now consider a multiple regression of the form

$$v_{it} = a + b_{y.p} y_{it} + b_{p.y} p_{it} + \text{error}.$$

Here $b_{y.p}$ and $b_{p.y}$ are now the multiple regression estimates of the effects of personal wellbeing and sociotropic judgments (as distinct from the corresponding bivariate estimates, b_y and b_p, considered above). From the normal equations for this regression it follows that

$$\text{(xvi)} \qquad b_{p.y} = b_p - b_{y.p}\, r_{yp} \frac{S_p}{S_y}$$

(Appendix, line (38)). Thus the multiple-regression estimate is equal to the bivariate estimate, minus a correction factor which depends on various sample quantities. Results reported by Kinder and Kiewiet show the sample correlation between sociotropic judgments and personal wellbeing to be quite small (the partial correlations run from nil to .13), while the multiple regression estimate of the personal wellbeing effect is even smaller.[16] Since S_p and S_y are of roughly similar magnitudes,[17] the correction factor will be the product of two quite small numbers and one which is close to unity (or at least not overwhelmingly large). Hence, at least for this data, $b_{p.y}$ will be very close to the bivariate estimate b_p, so our various findings and conclusions concerning b_p also apply to the multiple regression estimate $b_{p.y}$. The same will be true for b_y and $b_{y.p}$.

Table 2 below summarizes the main facts concerning the relationship between the regression estimates (whether bivariate or multiple) and the model parameters, under the various behavioral hypotheses considered.

[Table 2 about here]

Several conclusions seem clear. The individual-level cross-sectional analysis yields poor estimates in every case. They completely miss any sociotropic effect that may be present; the sizeable and apparently stable "performance rating" estimates are largely reflections of the spurious "cognitive dissonance" correlation between ratings and partisan predispositions, and do not depend on any true sociotropic effect β_2 at all. While the personal wellbeing estimates do depend to some degree on β_1, the dependence is a very weak one; in practice the behavior of these estimates will depend mainly on the spurious α,y term, in every case. In contrast to these essentially spurious cross-sectional estimates, the aggregate-level estimate is reasonably successful in estimating the net effect of economic circumstances on voting behavior. It cannot separate out the self-interest and sociotropic effects, however; only the net $(\beta_1 + \beta_2)$ effect is identified. Because of this, and because all the cross-sectional estimates behave essentially identically under all three hypotheses, it is clear that none of this evidence effectively discriminates between the various behavioral hypotheses. There is simply no way of determining whether the observable relationships

between economic variables and voting were ultimately generated by sociotropic or self-interested behavior, or by some combination of the two, on the basis of this kind of evidence.

This is not to say that the distinction between sociotropic and self-interested voting is necessarily meaningless or undecideable with respect to other kinds of evidence, however. The individual-level estimation problems described above arise essentially from measurement error. On the other hand aggregation at the economy-wide level, while circumventing many of these problems, nevertheless makes it impossible to distinguish self-interest from sociotropic behavior, since they always operate in tandem at the aggregate level. One possible solution, however, might be to aggregate to some intermediate level, at which self-interest and sociotropic considerations diverge, which would make it possible to separate out and identify the two effects. Thus, for example, in an election in which free trade vs. protectionism become an important issue, we would be particularly interested in examining the behavior of groups or regions suffering from import competition. The net social gains from trade are both sizeable and positive $(G_t > 0)$, but while the benefits are rather diffusely distributed to consumers at large, the costs tend to fall disproportionately on those involved in uncompetitive domestic industries. Such persons provide a clear case in which altruism and self-interest diverge, and their response should thus yield a good test of the sociotropic hypothesis. Such a test certainly seems feasible: indeed, we already have considerable casual empirical evidence on groups such as auto or steel workers threatened by foreign imports, or farmers stuck with large stocks of unsold grain because of a foreign policy decision to embargo grain exports.

It seems likely that a careful analysis of the voting patterns of such groups would reveal that self-interest, even at the expense of the general welfare, has not disappeared as a significant factor in contemporary U.S. politics. This is not to say that only self-interest is important, or that there are not times and places in which citizens may indeed be willing to endure considerable personal sacrifices for the sake of broader social ends. But a picture of the U.S. voter as always acting in this fashion, and never being moved by personal or parochial interests, is surely overdrawn: the broad facts of recent historical experience point differently.

There is a variant of the sociotropism hypothesis which is not so easily falsified. One might argue, for example, that an auto worker who votes for a protectionist candidate may still be voting sociotropically with respect to his fellow workers, if not the nation at large, since protection helps all auto workers, and not simply the voter in question. Thus, in this vein, we could broaden the definition of sociotropism to allow the scope of sociotropic concern to apply to groups or collectivities smaller than the society at large, though still larger than the individual himself.

This reformulation, however, reduces the hypothesis to a near-tautology, and makes the distinction between self-interested and sociotropic voting an essentially empirically meaningless one, with

respect to nearly any imaginable kind of evidence. In a modern mass democracy, most individuals never receive, or expect to receive, purely personal favors or benefits from office holders. Even the most self-interested of voters will therefore not judge candidates in terms of such individual benefits, but will instead favor candidates whose policy positions and actions have benefited him, individually--which is quite a different thing. Since public policies by definition always affect aggregates of individuals, any such voter will always be able to find a coalition of others who have similar interests. We are then faced with the following question: does the voter in question favor the candidate because of genuine altruistic concern for the welfare of those others who have been similarly affected by the candidate's activities and polices? Or is he instead purely but realistically self-interested, and democrat enough to rationalize, perhaps even to himself, his personal concerns in terms of group interests and fellow-feeling? The question is not quite an empirically meaningless one, but it comes very close. Surely it would be exceedingly difficult to devise an empirical test of it.

It seems clear from this that the task of meaningfully distinguishing between and measuring the respective impacts of self-interest and altruism in contemporary electoral politics is a subtle and difficult one, and that in addition to the methodological problems reviewed above, there are also serious conceptual and theoretical issues which will first have to be addressed and resolved before meaningful empirical research on the question will be possible.

FIGURE 1a

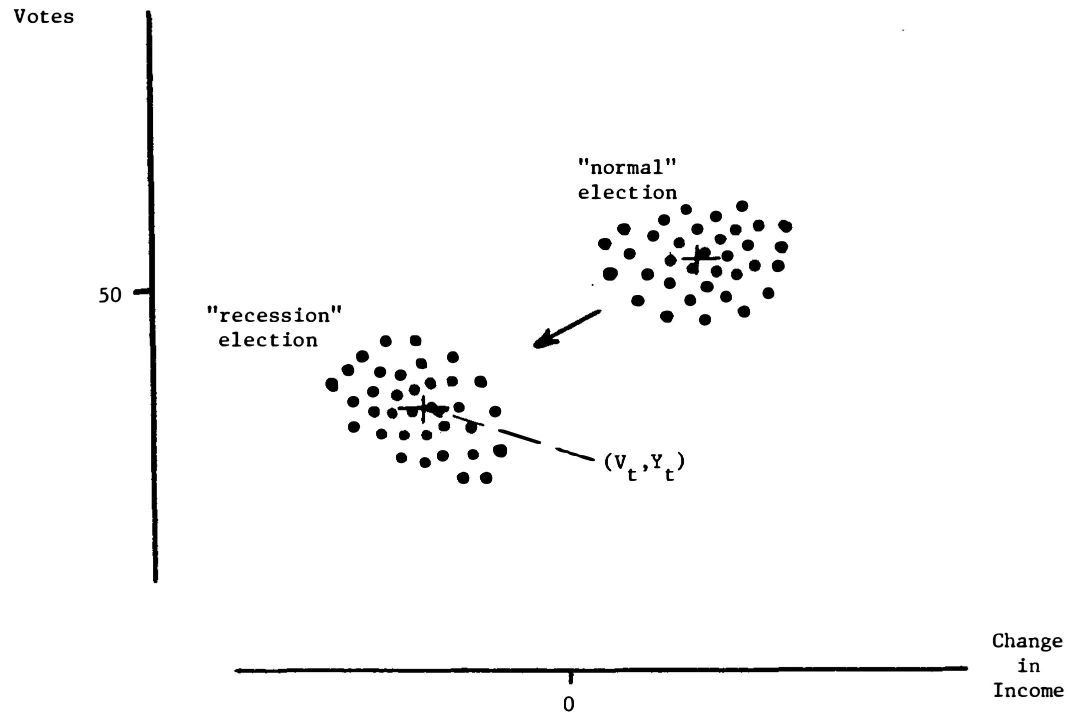

FIGURE 1b

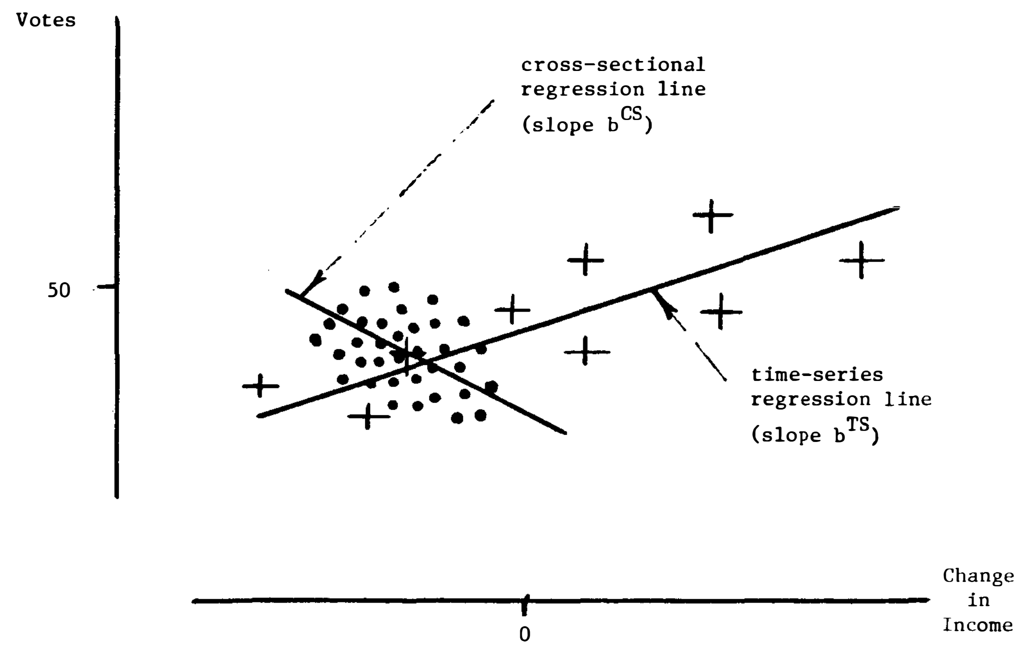

TABLE 1

PREDICTED CROSS-SECTIONAL ESTIMATES AS A FUNCTION OF VAR(G)/VAR(Y) AND OF UNEXPLAINED VARIANCE IN α

Assumptions: $var(\alpha) = 25$, of which 0, 51% or 84% is explicitly controlled; $var(y) = 5^2$, of which var(g) constitutes 10%, 25% or 50%.

		Degree of Control on Partisanship			
$\frac{var(g)}{var(y)}$		None 0%	Good 51%	Extreme 84%	Mean of b^{CS} ($\beta = .5$)
Low	10%	$.1\beta \pm 5r$	$.1\beta \pm 3.5r$	$.1\beta \pm 2r$	.05
High	25%	$.25\beta \pm 5r$	$.25\beta \pm 3.5r$	$.25\beta \pm 2r$	.125
Extreme	50%	$.5\beta \pm 5r$	$.5\beta \pm 3.5r$	$.5\beta \pm 2r$	.25
Range of b^{CS} ($r = \pm .05$)		$\pm .25$	$\pm .18$	$\pm .10$	

TABLE 2

EVIDENCE AND INFERENCES UNDER ALTERNATIVE BEHAVIORAL HYPOTHESES

	Evidence:		
		Cross-Sectional, Using	
Hypothesis:	Aggregate Time-Series: Y_t	Personal Wellbeing: y_{it}	Performance Ratings: p_{it}
Self-Interested Voters: (i)	$\beta_1 \frac{cov(G,Y)}{var(Y)}$	$\beta_1 \frac{cov(g,y)}{var(y)} + \frac{cov(\alpha,y)}{var(y)}$	$\beta_1 \frac{cov(g,p)}{var(p)} + \frac{cov(\alpha,p)}{var(p)}$
Sociotropic Voters: (vi)	$\beta_2 \frac{cov(G,Y)}{var(Y)}$	$\frac{cov(\alpha,y)}{var(y)}$	$\frac{cov(\alpha,p)}{var(p)}$
Both: (xii)	$(\beta_1 + \beta_2) \frac{cov(G,Y)}{var(Y)}$	$\beta_1 \frac{cov(g,y)}{var(y)} + \frac{cov(\alpha,y)}{var(y)}$	$\beta_1 \frac{cov(g,p)}{var(p)} + \frac{cov(\alpha,p)}{var(p)}$
Comment:	Indistinguishable, since estimate is positive and sizeable in all cases. Related to real effect, but unable to disentangle β_1 and β_2.	Indistinguishable, since β_1 term is heavily attenuated and α term dominates. Estimates small, unstable, and essentially unrelated to true β's in every case.	Probably indistinguishable, since β_1 term small relative to α term. Estimate sizeable, positive, but essentially unrelated to true β's, in every case.

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FOOTNOTES

1. Notably Robinson (1950), plus innumberable subsequent articles, monographs, and volumes.
2. E.g., among others, Kramer (1971), Kramer and Lepper (1972), Stigler (1973), Lepper (1974), Tufte (1975), Arcelus and Meltzer (1975), Bloom and Price (1975), Goodman and Kramer (1975), Fair (1978).
3. E.g., Logan (1977), Fiorina (1978), Kinder and Kiewiet (1979a, 1979b). A few studies of Presidential voting have produced less nihilistic findings, e.g., Tufte (1978), Kiewiet (1981).
4. Jacobson and Kernell (1981), for example, suggest that while voters don't actually respond to economic conditions, politicians and party leaders believe they do, and are inspired to mount particularly effective campaign efforts when they think economic trends favor their party, or to retire from the field if things look unpromising economically. They argue that the aggregate-level relation between economic conditions and electoral outcomes is actually produced by these differential campaign efforts, rather than by any direct behavioral response by voters to economic conditions.

   They make a convincing case for differential efforts in several recent elections, and such differentials would of course tend to exaggerate or magnify whatever underlying behavioral effects there may be. As a sole explanation for the aggregate-

   level findings, however, it stretches credulity to suppose that politicians and campaign strategists could remain so persistently misguided over so long a period, if in fact there were no fire beneath the smoke.
5. Kinder and Kiewiet (1979a, 1979b).
6. This point is hardly an original one. Grunfeld and Griliches (1960), for example, reach similar conclusions, on rather different grounds (which are, however, probably also widely applicable to political data).
7. We thus sidestep the question of whether the relevant effects are incumbency-or party-oriented in direction: with this sample of elections, both are equivalent and indistinguishable.
8. We should thus think of v_{it} as a popularity index or "thermometer score" for the incumbent; this reduces things to a relatively straightforward regression problem. Alternatively, if we wish to treat i's vote as a dichotomous variable, then v_{it} can be interpreted as the index in a probit relation; voter i will vote for or against the incumbent according to whether v_{it} lies above or below some threshold value. In this case the estimates would be more complicated and nonlinearities would make the aggregation a bit more complicated, but all of the main points made above would also apply to this probit interpretation.
9. This bivariate formulation is, once again, to simplify and is not essential. All of our points could be made, though in more complicated form, in the context of a more realistic multiple

regression model with other explanitory and control variables. Alternatively, we can suppose that the vote and income measures have already been "purged" of the effects of these other variables by regressing each on all the control variables at an earlier stage of the analysis; if v_{it} and g_{it} are defined as the residuals from these regressions, then as is well known the estimate obtained from a bivariate regression of v_{it} on g_{it} is identical to that which would be obtained in a multiple regression which includes the control variables along with income.

10. In principle these quantities could be estimated from policy simulations with a macroeconomic model. I have not attempted to do this in detail, but some rough calculations from results of Fair's simulations with his model (Fair [1976]) are consistent with the statements above. For example the results reported on his Table 9-1 (p. 170) show that in 1971 a decrease of $5 billion in government purchases of goods and services (approximately 8 percent of the Federal budget) would have reduced GNP by about $12 billion over a one-year period, or by more than 4 percent. Historical variations in this and other policy variables on this order of magnitude are not unlikely, and would produce changes in GNP which are sizeable relative to the historical U.S. growth rate of 3 percent.

11. [Reference].

12. In principle this number is readily estimable, though I have been

unable to locate a published source for it. One researcher who has worked extensively with the Michigan survey of consumer finances has guesstimated it to be in the 4 to 5 percent range (Rachel Willis, personal communication). The lower figure would actually strengthen the argument above.

13. In recent years the distribution of party identification, a rough proxy for α_i, is roughly uniform, though skewed somewhat in favor of the democrats. If the distribution were perfectly uniform, and scaled from 0 to 100, the standard deviation would be about 29 points. The figure given above is a rough-and-ready adjustment from that. As shown in Table 1 below, our conclusions are insensitive to wide downward revisions of this quantity.

14. Kinder and Kiewiet (1979a), pp. 132, 156.

15. E.g., Tufte (1978), Table 5-8, p. 132. Similarly, Table 2, p. 1062 of Markus and Converse (1979) shows voter perceptions of candidates' issue positions are systematically related to the respondents' own policy and political preferences, generally in a dissonance-reducing manner.

16. r_{yp} from (1979b), Figures 1 and 2; $b_{y.p}$ from (1979b), Table 2.

17. Rod Kiewiet, personal communication.

THE ECOLOGICAL FALLACY REVISITED: AGGREGATE- VERSUS INDIVIDUAL-LEVEL FINDINGS ON ECONOMICS AND ELECTIONS, AND SOCIOTROPIC VOTING

APPENDIX

THE MODEL

$i = 1,2,\ldots,n$ voters.

$t = 1,2,\ldots,T$ elections.

v_{it} = i's vote in election t.

g_{it} = the government-induced change in i's income at election t.

e_{it} = the idiosyncratic part of i's income change at t.

(1) $y_{it} = g_{it} + e_{it}$, the total change in i's income at t.

(2) $v_{it} = \alpha_i + \beta g_{it}$, the true, deterministic, behavioral relation between government-induced economic effects and i's vote, to be estimated.

SOME BASIC IDENTITIES AND DEFINITIONS

Aggregate-level variables:

(3) $V_t = 1/n \sum_i v_{it}$, the aggregate vote for the incumbent at election t.

(4) $\bar{V} = 1/T \sum_t V_t$, the mean of V_t over the T elections.

(5) $G_t = 1/n \sum_t g_{it}$, the aggregate or average government-induced income change at t.

(6) $\bar{G} = 1/T \sum_t G_t$.

(7) $E_t = 1/n \sum_i e_{it}$, the aggregate or average idiosyncratic change at t.

(8) $\bar{E} = 1/T \sum_t E_t$.

(9) $Y_t = 1/n \sum_i Y_{it}$, the change in per-capita income at t.

(10) $\bar{Y} = 1/T \sum_t Y_t$.

Aggregating the identity (1), using (5) - (10), we obtain:

(11) $Y_t = 1/n \sum_i [g_{it} + e_{it}] = G_t + E_t$.

(12) $\bar{Y} = \bar{G} + \bar{E}$.

(13) $(Y_t - \bar{Y}) = (G_t - \bar{G}) + (E_t - \bar{E})$.

Similarly, aggregating the behavioral relation (2) and using (3) and (5) we have:

(14) $V_t = 1/n \sum_i V_{it} = 1/n \sum_i [\alpha_i + \beta g_{it}] = \bar{\alpha} + \beta G_t$,

where by $\bar{\alpha}$ we denote the quantity $1/n \sum_i \alpha_i$, the mean partisanship of the electorate. Similarly,

(15) $\bar{V} = 1/T \sum_t V_t = \bar{\alpha} + \beta\bar{G}$,

(16) $(V_t - \bar{V}) = \bar{\alpha} - \beta G_t - (\bar{\alpha} + \beta\bar{G}) = \beta(G_t - \bar{G})$.

Individual-level variables (for a fixed election t and varying across individuals):

(17) $(y_{it} - Y_t) = (g_{it} - G_t) + (e_{it} - E_t)$, from (1) and (11), and

(18) $(v_{it} - V_t) = a_i + \beta g_{it} - (\bar{a} + \beta G_t) = (a_i - \bar{a}) + \beta(g_{it} - G_t)$

from (2) and (14).

Finally, we recall that in a simple bivariate regression of P_j on Q_j, $j = 1,\ldots,J$, the formula for the regression coefficient b is

(19)
$$b = \frac{\sum_j (P_j - \bar{P})(Q_j - \bar{Q})}{\sum_j (Q_j - \bar{Q})^2} = \frac{\mathrm{cov}(P_j,Q_j)}{\mathrm{var}(Q_j)}$$

where $\bar{P}$, $\mathrm{cov}(P_j,Q_j)$, $\mathrm{var}(Q_j)$, etc. denote the sample means, covariances, and variances over the J units of observation.

THE AGGREGATE, TIME-SERIES REGRESSION

Regressing V_t on Y_t over all T elections yields (from 19) the <u>time series</u> regression coefficient b^{TS}:

$$b^{TS} = \frac{1/T \sum_t (V_t - \bar{V})(Y_t - \bar{Y})}{1/T \sum_t (Y_t - \bar{Y})^2}$$

Using (16) we get

$$b^{TS} = \frac{1/T \sum_t \beta(G_t - \bar{G})(Y_t - \bar{Y})}{\mathrm{var}^{TS}(Y_t)}$$

(20a)
$$= \beta \frac{\mathrm{cov}^{TS}(G_t,Y_t)}{\mathrm{var}^{TS}(Y_t)}$$

Equivalently, using (13), we get

$$b^{TS} = \frac{1/T \sum_t \beta(G_t - \bar{G})[(G_t - \bar{G}) + (E_t - \bar{E})]}{1/T \sum_t [(G_t - \bar{G}) + (E_t - \bar{E})]^2}$$

$$= \frac{1/T\ \beta[\sum_t (G_t - \bar{G})^2 + \sum_t (G_t - \bar{G})(E_t - \bar{E})]}{1/T\ [\sum_t (G_t - \bar{G}) + 2\sum_t (G_t - \bar{G})(E_t - \bar{E}) + \sum_t (E_t - \bar{E})^2]}$$

(20b)
$$= \beta \frac{\mathrm{var}^{TS}(G_t) + \mathrm{cov}^{TS}(G_t,E_t)}{\mathrm{var}^{TS}(G_t) + 2\mathrm{cov}^{TS}(G_t,E_t) + \mathrm{var}^{TS}(E_t)}$$

Here var^{TS} and cov^{TS} denote the <u>time series</u> variances and covariances of the aggregate-level variables, over the sample of T elections.

THE CROSS-SECTIONAL, MICRO-LEVEL REGRESSION

For fixed t, regressing v_{it} on y_{it} across individuals yields (from 19) the <u>cross-sectional</u> regression coefficient b^{CS}:

$$b^{CS} = \frac{1/n \sum_i (v_{it} - V_t)(y_{it} - Y_t)}{1/n \sum_i (y_{it} - \bar{Y}_t)^2}$$

$$= \frac{1/n \sum_i [(a_i - \bar{a}) + \beta(g_{it} - G_t)](y_{it} - Y_t)}{1/n \sum_i (y_{it} - Y_t)^2}$$

$$= \frac{1/n \sum_i [(a_i - \bar{a}) + \beta(g_{it} - G_t)][(g_{it} - G_t) + (e_{it} - E_t)]}{1/n \sum_i [(g_{it} - G_t)^2 + 2(g_{it} - G_t)(e_{it} - E_t) + (e_{it} - E_t)^2]}$$

$$= \frac{\beta\ 1/n \sum_i[(g_{it} - G_t)^2 + (g_{it} - G_t)(e_{it} - E_t)]}{1/n \sum_i[(g_{it} - G_t)^2 + 2(g_{it} - G_t)(e_{it} - E_t) + (e_{it} - E_t)^2]}$$

$$+ \frac{1/n \sum_i(\alpha_i - \bar{\alpha})[(g_{it} - G_t) + (e_{it} - E_t)]}{1/n \sum_i[(g_{it} - G_t)^2 + 2(g_{it} - G_t)(e_{it} - E_t) + (e_{it} - E_t)^2]} .$$

Expressing these in terms of variances and covariances, the estimate becomes

(21a) $$b^{CS} = \frac{cov^{CS}(v_{it}, y_{it})}{var^{CS}(y_{it})}$$

(21b) $$= \beta \frac{cov^{CS}(g_{it}, y_{it})}{var^{CS}(y_{it})} + \frac{cov^{CS}(\alpha_i, y_{it})}{var^{CS}(y_{it})}$$

(21c) $$= \beta \frac{var^{CS}(g_{it}) + cov^{CS}(g_{it}, e_{it})}{var^{CS}(g_{it}) + 2cov^{CS}(g_{it}, e_{it}) + var^{CS}(e_{it})}$$

$$+ \frac{cov^{CS}(\alpha_i, g_{it}) + cov^{CS}(\alpha_i, e_{it})}{var^{CS}(g_{it}) + 2cov^{CS}(g_{it}, e_{it}) + var^{CS}(e_{it})}$$

Here var^{CS}, cov^{CS} denote cross-sectional variances and covariances, which in general are quite different from the time-series variances and covariances of the corresponding aggregate-level variables.

ALTRUISTIC OR "SOCIOTROPIC" VOTERS

Voter i is altruistic or "sociotropic" if he bases his vote on G_t, the administration's overall performance in handling the economy in general, rather than on g_{it}, its performance in improving i's individual wellbeing. Thus, instead of (2), the behavioral relation governing i's voting behavior will be

(22) $$v_{it} = \alpha_i + \beta_2 G_t ,$$

where β_2 is the (true) "sociotropic" effect. Aggregating this over individuals we get

(23) $$V_t = 1/n \sum_i(\alpha_i + \beta_2 G_t) = \bar{\alpha} + \beta_2 G_t, \quad \text{so}$$

(24) $$(v_{it} - V_t) = (\alpha_i + \beta_2 G_t) - (\bar{\alpha} + \beta_2 G_t) = (\alpha_i - \bar{\alpha})$$

Similarly, aggregating V_t over the T elections we obtain, from (23) and (6):

(25) $$\bar{V} = 1/T \sum_t(\bar{\alpha} + \beta_2 G_t) = \bar{\alpha} + \beta_2 \bar{G}, \quad \text{so}$$

(26) $$(V_t - \bar{V}) = (\bar{\alpha} + \beta_2 G_t) - (\bar{\alpha} + \beta_2 \bar{G}) = \beta_2(G_t - \bar{G})$$

Suppose we now repeat the various aggregate and cross-sectional regressions for a "sociotropic" electorate. For the aggregate time-series regression, the coefficient will be

$$b^{TS} = \frac{1/T \sum_t(V_t - \bar{V})(Y_t - \bar{Y})}{1/T \sum_t(Y_t - \bar{Y})^2} \qquad \text{from (19)}$$

$$= \frac{1/T \sum \beta_2(G_t - \bar{G})(Y_t - \bar{Y})}{1/T \sum_t (Y_t - \bar{Y})^2} \quad \text{from (26), i.e.}$$

(27) $$b^{TS} = \beta_2 \frac{cov^{TS}(G_t, Y_t)}{var^{TS}(Y_t)}$$

which is the same as (20a). In the cross-section, regressing v_{it} on y_{it} gives

$$b^{CS} = \frac{1/n \sum_i (v_{it} - V_t)(y_{it} - Y_t)}{1/n \sum_i (y_{it} - Y_t)^2} \quad \text{from (19)}$$

$$= \frac{1/n \sum_i (\alpha_i - \bar{\alpha})(y_{it} - Y_t)}{1/n \sum_i (y_{it} - Y_t)^2} \quad \text{from (24), i.e.}$$

(28) $$b^{CS} = \frac{cov^{CS}(\alpha_i, y_{it})}{var^{CS}(y_{it})} \quad .$$

More generally, if voters respond to both personal and altruistic considerations, we can write

(29) $$v_{it} = \alpha_i + \beta_1 g_{it} + \beta_2 G_t,$$

where β_1 is now the true "self-interested" effect (previously defined by β_1 in (2)), and β_2 is again the true "sociotropic" effect. From this it is readily shown that

$$(V_t - \bar{V}) = (\beta_1 + \beta_2)(G_t - \bar{G}) \quad \text{and}$$

(30) $$(v_{it} - V_t) = (\alpha_i - \bar{\alpha}) + \beta_1(g_{it} - G_t),$$

which is identical to (18). Thus the time-series estimate (27) becomes

(31) $$b^{TS} = (\beta_1 + \beta_2) \frac{cov^{TS}(G_t, Y_t)}{var^{TS}(Y_t)} \quad ,$$

while the cross-sectional estimate (28) becomes

(32) $$b^{CS} = \beta_1 \frac{cov^{CS}(g_{it}, y_{it})}{var^{CS}(y_{it})} + \frac{cov^{CS}(\alpha_i, y_{it})}{var^{CS}(y_{it})}.$$

This is identical to (21b).

PERFORMANCE RATINGS

Let p_{it} be individual i's assessment or rating of the administration's overall performance in handling the economy in general, in period t. As usual we define the mean assessment at t,

$$P_t = 1/n \sum p_{it} \ .$$

In a simple bivariate regression of votes on performance ratings, the cross-sectional regression coefficient is then

(33) $$b_p^{CS} = \frac{cov^{CS}(v_{it}, p_{it})}{var^{CS}(p_{it})} = \frac{1/n \sum_i (v_{it} - V_t)(p_{it} - P_t)}{var^{CS}(p_{it})}$$

For a self-interested voter (again denoting by β_1 the "self-interest" parameter β of (2)) this becomes

$$b_p^{CS} = \frac{1/n \sum_i [(\alpha_i - \bar{\alpha}) + \beta_1(g_{it} - G_t)](p_{it} - P_t)}{var^{CS}(p_{it})} \quad \text{from (30)}$$

(34) $$= \beta_1 \frac{cov^{CS}(g_{it}, p_{it})}{var^{CS}(p_{it})} + \frac{cov^{CS}(\alpha_i, p_{it})}{var^{CS}(p_{it})}$$

On the other hand, for a "sociotropic" voter, we have simply

$$b_p^{CS} = \frac{1/n \sum_i (\alpha_i - \bar{\alpha})(p_{it} - P_t)}{var^{CS}(p_{it})} \quad \text{from (24)}$$

(35) $$= \frac{cov^{CS}(\alpha_i, p_{it})}{var^{CS}(p_{it})} .$$

(In the general case (29) of both personal and sociotropic effects, the expression (30) again holds, so the cross-sectional coefficient is still as in (34).)

Finally, suppose we do a multiple regression (across individuals) which includes both individual wellbeing y_{it} and performance ratings p_{it} as explanatory variables. Omitting the CS superscripts and it subscripts, and denoting by $b_{p.y}$ and $b_{y.p}$ the least-squares coefficients of these two variables in the multiple regression, the normal equations for this regression can be written as

(36) $$cov(v,y) = b_{y.p}\, var(y) + b_{p.y}\, cov(p,y)$$

(37) $$cov(v,p) = b_{y.p}\, cov(p,y) + b_{p.y}\, var(p)$$

Solving (37) for $b_{p.y}$ and using (33) we have

(38) $$b_{p.y} = \frac{cov(v,p)}{var(p)} - b_{y.p} \frac{cov(p,y)}{var(p)}$$

$$= b_p - b_{y.p} r_{yp} (\frac{S_p}{S_y})$$

where by r_{yp}, S_p, S_y we denote the sample correlation between, and standard deviations of, y and p, and b_p is the bivariate coefficient obtained by regressing votes on performance ratings as in (33). Similarly, (37) can be rewritten as

(39) $$b_{y.p} = b_y - b_{p.y}\, r_{yp}\, (\frac{S_y}{S_p}) \quad ,$$

when b_y is the bivariate coefficient b^{CS} of (21) or (28) above.