



Communications

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Communications

Comment on Przeworski and Wallerstein (Vol. 76, June 1982, pp. 215-238)

Adam Przeworski and Michael Wallerstein aim to provide a simple explanation of the falsification of Marx's prophecy about irreconcilable class struggle. In pursuing this objective, the authors relativize the Marxian zero-sum game assumption of class conflict, introducing a long-term non-constant sum component in the game between capitalists and workers. Thus, they accept the Marxian assumption that the total output of the economy is divided into profits and wages, which means that each one of them is a decreasing function of the other. On the other hand, they consider what they call the "underlying logic of compromise," that wages increase as a percentage of current profits. This assumption introduces common interests between workers and capitalists concerning the survival of capitalism.

Subsequently, workers seek to maximize their wages W^* , and capitalists their consumption C^* , by manipulating their weapons: worker militancy r and the ratio of savings s , respectively, subject to the constraint that the opponent will choose the best strategy. This maximization is done over a time horizon h , with time discount rates a and b for workers and capitalists respectively. Specifically, the capitalists choose each time (for each r) their best s (s^* = the one that maximizes C^*) and in their turn, the workers choose from these pairs of r and s^* the one that maximizes W^* : (r^{**} , s^*). If r^{**} and s^* take the extreme permitted values, then there is no possible class compromise. Przeworski and Wallerstein use the term "workers dominance" for this procedure, and subsequently I focus on this case, although the arguments (under a weaker form) hold also for the dual case of "capitalist dominance."

The graphic representation of the function $W^*(r)$ is given in Figure 1. (Compare p. 229.)

This figure shows that the value of r that maximizes W^* is in the extreme right of the diagram. In the original article, however, the curve $W^*(r)$ is shown as dashed beyond the local minimum. Przeworski and Wallerstein make some additional assumptions to justify this choice (p. 227). They involve cost of militancy, political reasons, and history, and they summarize:

When workers are highly uncertain and capitalists relatively certain, a compromise *may* [emphasis added] be established at a point at which workers are kept from increasing their militancy by capitalists' threat of disinvestment, whereas capitalists' optimal rate of investment is positive. (p. 230)

The problem with this model arises from its sensitivity and instability. As we can verify from Figure 1, $W^*(r)$ presents a local maximum (at r^{**}), and a local minimum (at $0.25 - b$). However, beyond a certain level of militancy r_{lim} , $W^*(r^{**})$ becomes a suboptimal solution. The smaller the difference $r_{lim} - r^{**}$, the more implausible becomes the authors' claim that compromise may be the dominant solution for workers. Therefore, $r_{lim} - r^{**}$ is an indicator of the fragility of the compromise solution.

Table 1 replicates Table 1 (p. 231) of the original article. We have added two columns containing the values of r_{lim} , and $r_{lim} - r^{**}$, respectively. Differences in values result from technical reasons.¹

For $a < 0.16$ there is no local maximum of the curve, which means that under these assumptions compromise solutions are not possible.

Table 1 indicates that the "underlying logic of compromise" is not enough to assure class compromise, especially when workers are sure about the future (low a). Indeed as the future discount rate a decreases, $r_{lim} - r^{**}$ decreases dramatically. However, the results of the model seem to imply that class compromise *might* occur when workers are highly uncertain about their future conditions. There is one important problem with this conclusion. If a class is uncertain about the future, then it is not likely to maximize its objective function over a long time horizon. In algebraic terms, the future discounted wages decrease steeply, so that the workers receive in a very small number of years almost the same amount that they would receive in an infinitely long future. It seems then that under these conditions there is no point in pursuing the maximization. Table 2 illustrates this property. (The values of a , r^{**} , s^* , and W^*_{30} are taken from Table 1 of the original article (p. 231).)

The table shows that in these cases the dominant class has exceeded 95% of the target value in 12 years. If we combine this remark with Przewor-

I would like to thank J. Sprague, B. Salert, J. Alt, and K. Shepsle for their useful comments.

¹We maintain the condition $1/[(1+a)(1+b)] = 0.7$, and we tested a range of r [0.0, 0.35] and s [0.21, 0.99] with a 0.005 increment.

Figure 1.

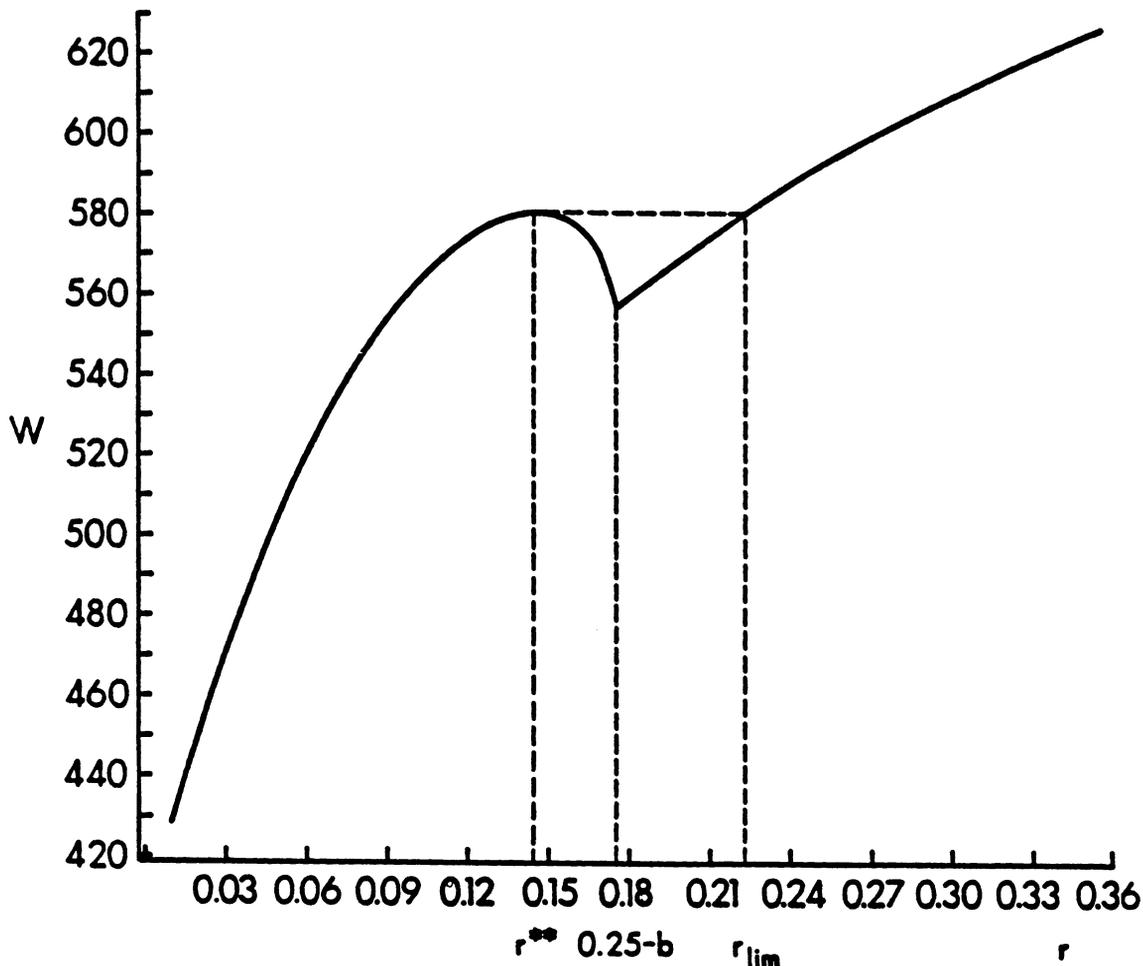


Table 1.

$h = 30 \quad 1/[(1+a)(1+b)] = 0.7$

a	r^{**}	W^*	r_{lim}	$r_{lim} - r^{**}$
0.421	0.210	488.8	0.305	0.095
0.331	0.150	580.6	0.225	0.075
0.231	0.065	732.9	0.110	0.045
0.161	No local maximum			

Table 2.

a	r^{**}	s^*	W^*_{30}	W^*_{12}	% of W^* attained after 12 years
0.43	0.215	0.655	483	470.6	97.4
0.35	0.16	0.615	557	532.2	95.5

Table 3.

$h = 50, \rho = 80\%, 1/(1+a)(1+b) = 0.7$								
a	r^{**}	s^*	W^*	C^*	h_w	h_c	r_{lim}	$r_{lim} - r^{**}$
0.421	0.23	0	97.5	384.5	5	26	0.43	0.20
0.331	0.16	0.73	487.9	416.4	7	26	0.41	0.25
0.231	0.06	0.78	684.2	541.2	14	34	0.29	0.23
0.161	0.01	0.62	744.8	426.4	17	20	0.05	0.04

ski's and Wallerstein's statement that for $h < 12$ "there is no compromise solution," we come to the conclusion that also under these conditions class compromise is extremely unstable. Intuitively, under high uncertainty about the future, the Keynesian aphorism about long-term conditions and the Marxian expectations become jointly relevant.

To sum up: The original idea of finding some relatively simple structural conditions that would explain the failure of Marx's prophecy is very interesting. But the "brake" proposed for irreconcilable class struggle is not enough because, in the case of low future discount rates, the local maxima reported in the article have a very limited range of dominance and, in the case of high future discount rates, maximization over a long time period is unlikely to occur.

In light of these remarks, there is a technical modification that increases the stability of the model. One can assume that both classes will maximize over a long *nominal* time horizon h , but will stop the process in shorter *actual* time horizons when they reach a certain percentage ρ of their long-term target. Under this assumption, both workers and capitalists will not choose r^{**} and s^* that maximize W^* and C^* over a long time horizon, but the ones that will give them $\rho\%$ of these targets in a smaller time interval.

Let us call h_w and h_c these actual time horizons for workers and capitalists respectively. For the original model $\rho = 100\%$ and $h_w = h_c = h$. However, as ρ decreases, an additional trade-off mechanism between long-term considerations and militancy arises. The actual time horizon becomes endogenous to the model (function of a, b, r, s , and ρ). Since the number of terms in both W^* and C^* is a function of this endogenous actual time horizon, each actor may be able to get more by increasing this actual time horizon than by increasing his militancy. Table 3 demonstrates the results of incorporating these ideas for $h = 50$ and $\rho = 80\%$.

Table 3 demonstrates that the more uncertain workers are about the future, the more this actual horizon decreases. By comparing the column $r_{lim} - r^{**}$ of Table 3 with the correspond-

ing column in Table 1, we can understand the stabilizing effect of the new assumption. Although the initial assumption was about a 50-year nominal time horizon, the actual time horizon of each class exceeds 30 years only once. Indeed, the additional "brake" operates by increasing the nominal time horizon and consequently "transposing" to more limited actual time horizons equilibria that would otherwise (in the unmodified model) need a nominal time horizon of 50 years to take place. But the stabilizing action of the ρ threshold is not limited to this effect. An additional trade-off mechanism between actual time horizons and militancy has been installed. Even for a nominal time horizon $h = 30$ (actual horizons h_w and h_c less than 20 years) the stability of the model increases substantially.

In summary, an additional mechanism was introduced that increased the plausibility of compromise solutions between workers and capitalists. This mechanism has a double effect. First, it permits more weight to the compromise conditions through the increases in the nominal time horizon whereas the actual time horizon remains limited. Second, it installs for both classes a trade-off between actual time horizons and militancy that has an autonomous effect on the intensity of class struggle. A comparison of Tables 1 and 3 shows that $r_{lim} - r^{**}$, which is the indicator of the stability of the model, has at least been doubled. However, it is incorrect to consider the problem resolved. On the contrary, the original challenge remains, and a more credible model must be advanced to explain the failure of the Marxian prophecy.

GEORGE TSEBELIS

Washington University

Reply

Unfortunately, the preceding comment on "The Structure of Class Conflict in Democratic Capitalist Societies" is based on an erroneous

understanding of our original argument and contributes little of substance. Because the blame for some of the confusion may be ours, we are happy to clarify.

Professor Tsebelis states that in our model: "the underlying logic of compromise" is not enough to assure class compromise, especially when workers are sure about the future (low a)." This is simply incorrect. In our model, a compromise solution, that is, a solution in which capitalists invest at a positive rate and workers refrain from claiming the entire product, always exists when either class bears relatively low risk. The only case with no compromise solution is the case in which the risk facing both classes is relatively high (specifically, when the rates of discount, a and b , both exceed the productivity of capital, $1/c$).

How did Tsebelis draw the opposite conclusion? It appears by misreading our Table 1, reproduced as Table 1 in the "Comment," in two ways. To explain, we must review the nature of our solutions. They are Stackelberg solutions in which one class leads and the other class follows. The follower responds with its best reply, that is, with its optimal strategy given the actions taken by the leader. Since the follower's best reply is predictable, it is anticipated by the leader when deciding which action to take. The actor that leads is often called the dominant player. It is important to note that there is no general advantage to being the dominant player. In our model, each class is better off if the other is forced to lead.

Our results, then, were the following: There exists a compromise solution with workers as the dominant player when capitalists' risk is low (when $b < 1/c$) and another solution with capitalists as the dominant player when workers' risk is low (when $a < 1/c$). (There does not exist a compromise Nash solution under any circumstances.) Professor Tsebelis's error was to conclude that the solution with workers dominant ceases to exist in Table 1 because the workers' rate of discount a falls below 0.16. In fact, the existence of this solution depends in no way upon workers' discount rate. What he missed is that with our specified relationship between a and b , $1/[(1+a)(1+b)] = 0.7$, $b > 1/c$ when $a < 0.16$, and it is the increase in the capitalists' rate of discount to a level at which capitalists refuse to invest regardless of workers' actions that destroys the compromise with workers dominant.¹

Tsebelis's second misunderstanding stems from failing to consider the entire Table 1, including the part not replicated in his "Comment." What the other half of Table 1 shows is that when $a < 1/c$ the solution with capitalists as the dominant player exists and that this solution is a much better deal for workers. Finally, we must emphasize that the particular relationship assumed in Table 1 between a and b was chosen for illustrative purposes only. In general, there is no reason why reductions in the risk facing workers must always be accompanied by increases in the risk for capitalists.

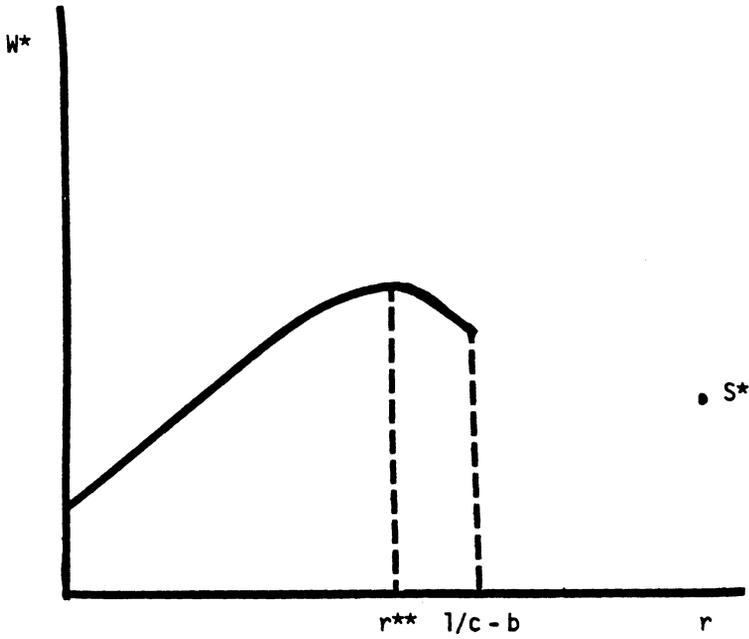
There is another, nontechnical, and more important misunderstanding in the "Comment." This concerns the comparisons of workers' welfare at the local maximum under capitalism, $W^*(r^{**})$ in Figure 1, with workers' welfare when they are highly militant (when r is large). If the line we drew on the right side of $0.25 - b$ in Figure 1 was a dashed one, it was for good reasons, reasons that were stated on page 227 and discussed in the section entitled "Beyond Capitalist Democracy" (pp. 233-235). To keep the discussion as simple as possible, let us assume that $a > 1/c$ and $b < 1/c$ so that the solution illustrated in Figure 1 is the only possible compromise. If workers increase r beyond r^{**} , capitalists respond by reducing investment sharply, leaving workers worse off. If workers continue to increase r until they reach $r = 1/c - b$, capitalists respond by disinvesting at the maximum rate possible. Beyond this point workers have nothing to lose and will only gain if they grab a greater and greater share of the profits, provided the laws governing the normal class relations of democratic capitalist societies, in particular equation (7) (p. 217) of our text, continue to hold. But it is implausible that such laws would continue to hold in a crisis as capitalists disinvest, and workers become increasingly militant. To the right of $0.25 - b$ lies either a socialist transformation or an authoritarian capitalist restoration. In Figure 1, the function $W^*(r)$ is no longer defined for r in this region.

To illustrate the point, we have redrawn Figure 1 as we should have drawn it in the article to avoid confusion. $W^*(r^{**})$ is the best workers can do under capitalism, and S^* is the expected value to workers of a revolutionary strategy as defined on page 234 of our text. (S^* includes the risk of an attempted socialist transformation being aborted by a counterrevolution.) If $W^*(r^{**}) > S^*$, the local maximum under capitalism is, in fact, a global

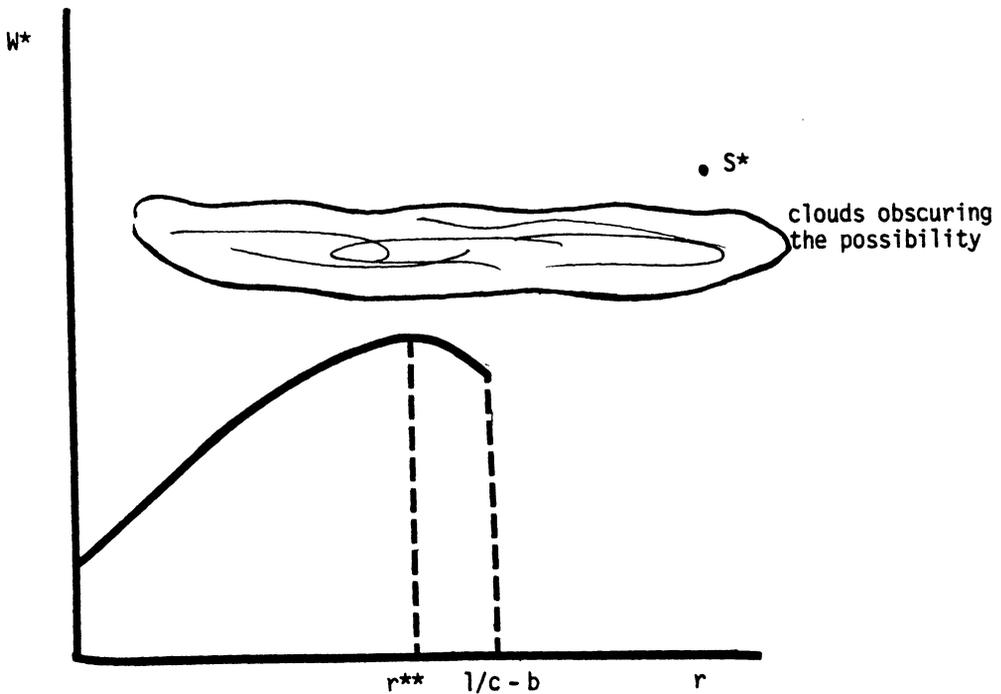
¹There is a technical confusion in our text. With $1/[(1+a)(1+b)] = 0.7$, $b > 0.23$ when $a < 0.16$, whereas we assume throughout that $1/c = 0.25$. This discrepancy is due to the difference between our analytic results, which assume an infinite horizon, and

our simulated results in which the horizon was taken to be 30. Formally and correctly, the proper threshold, call it T , should have been written $T = 1/c - \epsilon(h)$, where $\epsilon(h) > 0$, $\epsilon'(h) < 0$, and $\epsilon(h) \rightarrow 0$ as $h \rightarrow \infty$.

Figure 1. If the Expected Value of Socialism Is Lower than the Capitalist Maximum



If the Expected Value of Socialism Is Higher than the Capitalist Maximum



maximum. It is the best workers can do under the circumstances in any system. Because we do not know how to assess the value of S^* , we left it as a possibility, perhaps a temptation, that workers who have maximized their capitalist fate by choosing r^{**} might yet opt for a revolutionary strategy. The distance $r^{**} - r_{lim}$ is of no significance whatsoever. The choice facing workers around r^{**} is not whether to move into the region of high r . Once they go past r^{**} they will have decided to go all the way to socialism or barbarism. The only relevant comparison is of $W^*(r^{**})$ and S^* , and who knows how to make this comparison.

ADAM PRZEWORSKI
MICHAEL WALLERSTEIN

University of Chicago

Comment on Kramer
(Vol. 77, March 1983, pp. 92-111)

Gerald Kramer has played a pivotal role in the study of economic voting. His 1971 article was a watershed; it directed research on economic voting away from using "rather simple statistical methods. . ." toward methodological sophistication (1971, p. 133). With aggregate-level data, Kramer was able to identify a relationship between economic conditions and vote. Some subsequent studies using individual-level data, however, "have been unable to detect any comparable relationship between individual voting behavior and personal economic circumstances" (Kramer, 1983, p. 92). In "The Ecological Fallacy Revisited" Kramer attributes the apparent incongruity between these two methods of studying economic voting to a statistical artifact in the individual-level studies. He concludes that the aggregate-level studies provide better estimates of economic voting than the individual-level research. This comment critically examines Kramer's "Ecological Fallacy" article, and particularly its basic assumptions.

Kramer bases his study on three assumptions which are untenable and which point to conclusions concerning aggregate research that are similarly not supportable. First, Kramer is interested in how "real economic outcomes affect actual voting decisions and not in economic rhetoric or perceptual imagery" (1983, p. 95). Kramer's measure of real economic outcomes is change in per capita income. Kramer's second assumption is that economic voting is correctly studied by estimating the effect of government-induced economic changes on voting behavior. Non-government-induced changes bias the estimation of economic

voting. Change in one's income, Kramer suggests, results from both government-induced and "politically irrelevant," "extraneous" factors. Large extraneous effects, the OPEC embargo for example, have national implications and bias both the aggregate and individual studies. However, in the individual studies bias occurs because the local extraneous effects cannot be separated from those that are government-induced. Extraneous factors bias both the aggregate and individual estimates of economic voting with the greater bias, a consequence of local economic changes, affecting the individual research.

Third, Kramer assumes that the "effect of interest" (1983, p. 95) is not individual elections, but rather the pattern of elections over time. Individual elections may present counterintuitive or "spurious" results that would not be evident in the aggregate studies.

Kramer's assumptions permit a mathematical demonstration of how the less biased aggregate estimates of economic voting are superior to the individual-level approach. While all of this works well within the economic world that Kramer creates, his assumptions are unrealistic in the political world. Assumptions that more correctly reflect political reality would result in different conclusions.

Kramer's first assumption is that the economic vote is based on a change in per-capita income. Unfortunately, he does not describe the cognitive process by which voters calculate their net income change. Except in the most general terms, of course, voters are not able to determine the net change in their income. A voter's economic situation is largely determined by subjective interpretations of many individual and societal factors. Per-capita income is merely an imprecise measure of only one aspect of personal financial situation.

Individual-level studies use survey data that assess the economic status of respondents by asking whether their financial situation has gotten better, worse, or remained the same. The survey data, which measure subjective interpretations of objective factors, better represent voters' financial situation than income change which taps only the objective conditions. There is empirical evidence to support the superiority of survey data as a measure of economic condition. Inflation, for example, is an important factor affecting income levels. Yet Hibbs (1979, p. 712) finds that "less tangible subjective and psychological factors are more important than objective costs in explaining widespread aversion to inflation." Fiorina (1981, p. 29) discovered that family financial data (SRC survey) were a better predictor of presidential and congressional elections than income change. Logically and empirically the survey data better measure personal financial condition than income

change, and therefore more accurately estimate economic voting.

Kramer's second assumption is that only government-induced impacts on the economy should be considered when estimating economic voting. It is unlikely, especially in our complex economy, that voters are able to distinguish accurately between effects that are or are not government induced. The closing of a steel factory could be the result of outmoded production processes, but unemployed workers may blame the administration's high interest rates that limit auto purchases. It is not the source of financial discomfort that predicts economic voting, but rather the attribution of blame to the government. Feldman (1982, p. 463) demonstrates that self-interested political behavior is most likely to result when societal conditions or political actors are seen as responsible for economic grievances. It is the individual-level data that can best unravel the complex relationship between personal financial condition and political actions. Again, both logically and empirically, the individual-level data are more likely to provide better estimates of economic voting.

Kramer's third assumption excludes the analysis of individual elections which may, he suggests, exhibit spurious results. Kramer implies that there is a constant linear relationship between economic conditions and vote over all elections; this may be wrong. Economic voting may not have been evident in the prosperous years of 1964 and 1966 even among the disadvantaged who could not blame the in-party for their misfortune when the economy was booming (Alford, 1982, p. 17). Similarly, Bloom and Price (1975, p. 1241) found economic voting in a downturn "while economic upturns yield no corresponding benefits." Since, as demonstrated previously, Kramer's model cannot adequately measure personal financial situation nor detect "bias," he cannot determine if a particular election result is spurious. The counter-intuitive findings evident in specific elections are not likely the result of methodological artifacts, but rather reflect real political events.

Assumptions that differ from Kramer's, and more accurately mirror political reality, would lead to the conclusion that individual-level studies can provide accurate estimates of economic voting. This is not to argue that we should cease in our efforts to improve individual-level research; we need to increase the precision of our estimates of economic voting. Nor is this intended to be a general attack on aggregate research of economic voting. Aggregate research, especially studies based on realistic assumptions about the political world, can yield important information. We should discard the notion that a competition exists between aggregate or individual-level studies; each approach contributes in different

ways to our understanding of economic voting. Aggregate studies permit us to see patterns and change in economic voting, whereas individual-level research allows a more complete understanding of political factors and vote performance in each election. A fruitful task would be to examine accurately the strengths and shortcomings of each approach and then to assess their ultimate contribution to our knowledge of economic voting.

MICHAEL J. SCICCHITANO

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Reply

Michael Scicchitano feels that my conclusions rest on three assumptions that are "untenable" and "unrealistic in the political world."

Taking his points in reverse order, the third seems based on a misunderstanding of the logic of the argument. I used the assumption that the same relationship operates in all elections for two purposes: First, to show that, even so, individual and aggregate-level analyses would still yield different results, and second, that the latter will often yield a reasonably good estimate of the true relationship. The assumption played no role whatever in the demonstration (pp. 100-102 and 109) that an individual-level cross-sectional analysis will fail to detect the true (individual-level) relationship. If, as Scicchitano suggests, this true relationship varies from election to election, individual-level

analyses will simply fail to measure *any* of them correctly. Of course, an aggregate-level analysis would also fail under these conditions (unless the differences were of the Bloom-Price type or exhibited some other regularity, in which case an aggregate-level analysis of the proper kind might again succeed), but in no sense does this resurrect or justify individual-level cross-sectional analysis as a viable alternative.

With respect to the second assumption, Scicchitano argues that it is "unlikely . . . in our complex economy, that voters are able to distinguish accurately between" government-induced and other effects. I agree. But he evidently feels that, since voters can't make these distinctions *accurately*, they don't make them at all. Thus, Scicchitano would presumably maintain, for example, that the strain on a family's financial well-being that would arise from sending a child to an expensive private college or from having their uninsured home burn down is just as relevant politically as a tax increase of equal magnitude. This seems to me an untenable assumption for the political world I perceive. I think most voters would react quite differently to these events, and that in general they can and do recognize, at least roughly, the difference between government-induced and other, nongovernmental effects.

Scicchitano's remaining point concerns the distinction between self-reported subjective perceptions versus objective measures of personal economic well-being; he feels the former "more accurately estimate economic voting." Perhaps this is simply a linguistic or terminological problem. For example, consider a scenario in which the economy hummed along evenly under a single incumbent over a series of elections with absolutely no measurable differences in any aspect of any individual's economic condition from one election to the next. Suppose, at the same time, there was considerable variation in individual voting behavior over the period and that it were found that voters (perhaps having been conditioned by Reagan's 1980 "ask yourself. . ." slogan) invariably reported being better or worse off financially according to whether they were going to vote for or against the incumbent. Scicchitano would presumably interpret this as evidence for "economic voting." I, on the other hand, would say this shows nothing at all about how economic circumstances affect voting behavior (it couldn't, because there was no variation in any of the economic variables), although it may reveal something interesting about the way in which voters rationalize their choices or form perceptions. I suppose Scicchitano is free to define "economic voting" as he chooses, but I must say it seems to me greatly preferable to recognize and preserve the distinction between subjective psychological

and objective economic variables, in the interests of both linguistic clarity and scientific understanding.

I agree with Scicchitano on the desirability of more detailed election-specific (or group-specific) studies. However, I'm afraid I remain unpersuaded that cross-sectionally oriented individual-level studies (especially those based on subjective measures) can contribute meaningfully to that goal.

GERALD H. KRAMER

California Institute of Technology

Reply to Hill and Cassel
(Vol. 77, December 1983, pp. 1011-1012)

Our article (Abramson & Aldrich, 1982) demonstrated that weakening party loyalties and eroding beliefs about government responsiveness played a major role in accounting for the decline of turnout from 1960 to 1980. Hill and Cassel argue that our research is misleading. Their central claim is that "the goal of identifying a simple and elegant model of declining participation has been perverted by statistical and *ceteris paribus* gymnastics which seek to maximize the explanatory power of this simple model at the expense of completeness and necessary complexity" (Hill & Cassel, p. 1011). They argue that we left out important social and demographic variables, that our model is "fundamentally misspecified" and that, therefore, we cannot claim "to have solved Brody's (1978) much discussed 'puzzle' of declining electoral participation" (p. 1011). As examples of misspecification, they maintain we should have included residential mobility and education in our estimates.

Because we do not include any social or demographic variables in our analysis, we obviously exclude some that are relevant to understanding changes in postwar turnout, along with many that are not. We discuss at some length our research strategy of focusing on two attitudinal variables (Abramson & Aldrich, 1982, p. 504; note 20, pp. 511-512). We emphasize that our "analysis has not attempted a comprehensive explanation of the changes in attitudes themselves" (p. 519), a step necessary for any full theoretical account. We discuss other attitudinal variables, especially declining concern about electoral outcomes, but do not directly incorporate them in our estimates. Most important, we acknowledge that our analyses

“do not discriminate between alternative theoretical views of voting behavior” (p. 519).

We obviously never claimed to have developed a fully specified model of the decline of turnout, and nowhere did we claim to have “solved” the puzzle of declining electoral participation. Indeed, our basic conclusion that “the combined effect of the decline in partisan strength and the decline in beliefs about government responsiveness appears to account for between two-thirds and seven-tenths of the decline in presidential turnout” (p. 519) makes it clear that the puzzle remains unsolved. We do claim that the two variables we analyzed go a long way toward explaining why turnout declined. But given the ranges of the values we regularly report, it should be apparent that we consider our numerical estimates as indications of the approximate degree of explanation.

Although we do not provide unbiased point estimates, we have seen no evidence that our equations yield misleading approximations. Hill and Cassel offer no empirical evidence that the purported misspecification has any impact on actual estimates. Let us look at the two variables they suggest might have an impact—residential mobility and education.

Hill and Cassel state that “without a variable for voter mobility in the Abramson and Aldrich analysis, one does not know whether the apparent contribution of their two variables to the decline is undermined by spurious correlations” (1983, p. 1011). They assert that their earlier work demonstrated that “the increasing mobility of voters made a significant contribution to declining turnout.” But this is not what they actually found. Their earlier analysis of SRC-CPS data (Cassel & Hill, 1981) showed that residential mobility was significantly related to turnout in both 1964 and 1976 (p. 187). But, according to their own estimates, increased residential mobility accounted for only 0.5% points of the 10% point decline in non-Southern turnout (pp. 189, 191). A variable with such a weak impact would not affect the contribution of party identification and feelings of political efficacy very much, even if it were highly correlated with these attitudes. In fact, it is not. Cassel and Hill (1980, Table 1A) report that in 1976 the zero-order correlation between residential mobility and partisan strength was $-.09$, whereas the correlation between mobility and their measure of political efficacy was $.04$.

Turning to level of education, Hill and Cassel (1983, p. 1011) “cannot help but wonder how much of the explanatory power of the authors’ two-variable model would evaporate if the offsetting power of education were introduced into the model.” Of course, including education would be necessary to fully explain variation in

postwar turnout, and we have demonstrated elsewhere that increased educational levels played a major role in impeding the decline of turnout (Abramson, Aldrich, & Rohde, 1982, pp. 84-85). But given the increases in educational levels between 1960 and 1980 and the positive relationship between education and political efficacy and education and turnout, one would expect in the abstract that including education as a “control” would strengthen the impact of the decline of efficacy on the decline of turnout.

The actual effect of controls is ultimately an empirical question, and the evidence shows that controls for education do not cause the explanatory power of party identification and feelings of political efficacy to “evaporate.” In his recent book, Kleppner (1982, pp. 122-130) studies declining turnout among non-Southern whites, using algebraic standardization procedures very similar to ours. Using SRC data for the 1952 through 1980 period, he estimates the impact of weakening party loyalties and feelings of political efficacy (measured as we did). Kleppner’s calculations include simultaneous controls for education, income, age, and sex. Despite these controls, his results for non-Southern whites are very close to ours for the entire white electorate. We find that between a fourth and three-tenths of the decline in turnout from 1960 to 1980 can be accounted for by weakening partisan loyalties, whereas Kleppner’s estimate is 28.5%. We find that just over half the decline can be attributed to eroding feelings of “external” political efficacy, whereas Kleppner’s estimate is 52.6%. Kleppner’s research not only supports our position that these two attitudinal variables made a substantial contribution to the decline of turnout, but also shows that their impact persists when controls for social and demographic variables are introduced.

Our goal was to demonstrate that attitudinal change made an important contribution to the decline of turnout. Including additional attitudinal variables would affect our estimates, as would including relevant social and demographic variables. Extant research suggests, however, that including residential mobility would have very little effect on our estimates, and that including level of education would not weaken them. Knowing what other variables to include in order to attain unbiased point estimation requires proper specification. Attaining such specification is basically a theoretical task, and only theory can unravel the puzzle of declining turnout. What is really important at this stage is not questioning the specific importance of particular variables, but doing the hard theoretical work needed to sort out the relationships among explanatory variables. By demonstrating the importance of two key attitudinal variables, and by clarifying some of the con-

ceptual confusion that has characterized their study, we hope we contributed to that larger goal.

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Errata

In "Changes in the Vote Margins for Congressional Candidates: A Specification of Historical Trends" by James C. Garand and Donald A. Gross (Volume 78, pp. 17-30), Figures 2 and 3 were inadvertently transposed. The caption for Figure 2 (p. 21) should have been placed with the graph on p. 27, and the caption for Figure 3 should have appeared over the graph on p. 21. We regret the error.

"A Method of Estimating the Personal Ideology of Political Representatives" by Richard T. Carson and Joe A. Oppenheimer (Vol. 78, pp. 163-174) contained several errors. In Table 3,

Senator Leahy (Vt.) should have been identified as a Democrat with an I* of .17, and Senator Stafford (Vt.) as a Republican with an I* of -.29. These errors did not affect the other data as reported. On pp. 173 and 175, Senator Leahy is cited as an example of a "Republican" given a boost as a liberal who bucked his party and who is given a high liberalism score; the references should be to Senator Weicker. Also in Table 3, Senator Reigle should have been listed as from Michigan and Senator Pryor as from Arizona.

Forthcoming Articles

The following articles have tentatively been scheduled for publication in the December 1984 issue.

B. A. Anderson and B. D. Silver, "Equality, Efficiency, and Politics in Soviet Bilingual Education Policy, 1934-1980"

M. K. Jennings and G. B. Markus, "Partisan Orientations over the Long Haul: Results from the Three-Wave Political Socialization Panel Study"

D. Mason, "Individual Participation in Collective Racial Violence: A Rational Choice Synthesis"

M. Midlarsky, "Political Stability of Two Multiparty Systems: Probabilistic Bases for the Comparison of Party Systems"

P. F. Nardulli, R. B. Flemming, and J. Eisenstein, "Unraveling the Complexities of Decision Making in Face-to-Face Groups: A Contextual Analysis of Plea Bargained Sentences"

R. G. Niemi, "The Problem of Strategic Behavior under Approval Voting"

L. M. Preston, "Freedom, Markets, and Voluntary Exchange"

L. Ragsdale, "The Politics of Presidential Speechmaking, 1949-1980"

D. R. Sabia, "Political Education and the History of Political Thought"

J. A. Segal, "Predicting Supreme Court Cases Probabilistically: The Search and Seizure Cases, 1962-1981"

R. M. Siverson and M. R. Tennefoss, "Power, Alliance, and the Escalation of International Conflict 1815-1965"

S. S. Ulmer, "The Supreme Court's Certiorari Decisions: 'Conflict' as a Predictive Variable"