B.J.Pol.S. **31,** 179–223
Printed in the United Kingdom
Copyright © 2001 Cambridge University Press

Notes and Comments

The Political Implications of Higher Turnout

BENJAMIN HIGHTON AND RAYMOND E. WOLFINGER*

Rich Americans, far more likely to vote than their poorer fellow citizens, also differ in how they vote and what policies they favour. These undisputed facts lead to the widespread belief 'that if everybody in this country voted, the Democrats would be in for the next 100 years.'[1] The gist of this conclusion, which seems to follow ineluctably from our opening sentence, is accepted by almost everyone except a few empirical political scientists. Their analyses of survey data show that no objectively achieved increase in turnout – including compulsory voting – would be a boon to progressive causes or Democratic candidates. Simply put, voters' preferences differ minimally from those of all citizens; outcomes would not change if everyone voted.

This conclusion assumes that non-voters' preferences would not shift if they were to vote. In his presidential address to the American Political Science Association, Arend Lijphart challenged this assumption with an explanation that would reconcile research findings and conventional wisdom about the likely consequences of higher turnout:

> Nonvoters who are asked their opinions on policy and partisan preferences in surveys are typically citizens who have not given these questions much thought, who have not been politically mobilized, and who, in terms of social class, have not developed class consciousness. It is highly likely that, if they were mobilized to vote, their votes would be quite different from their responses in opinion polls.[2]

Because 'who votes, and who doesn't, has important consequences for who gets elected and for the content of public policies,'[3] Lijphart advocated compulsory voting as a cure for class inequality in turnout.

Lijphart's argument has the great merit of being formulated in testable propositions.

* Department of Political Science, University of California, Davis; and Department of Political Science, University of California, Berkeley, respectively. The datasets we analysed are part of the holdings of UC Data Archive and Technical Assistance (UC DATA) of the University of California, Berkeley. We were assisted in data acquisition by Ilona Einowski and John Stiles of UC DATA. The datasets were obtained from the Inter-University Consortium for Political and Social Research. Some of these were gathered by the National Election Studies of the University of Michigan Center for Political Studies under grants from the National Science Foundation. None of these organizations has any responsibility for our analyses and interpretations. We are grateful for comments at a presentation of our research at the Hoover Institution at Stanford University. A previous version of this Note was presented at the annual meeting of the American Political Science Association, Boston, 1998.

[1] John Kenneth Galbraith, Interview in *California Monthly*, February 1986, p. 11.

[2] Arend Lijphart, 'Unequal Participation: Democracy's Unresolved Dilemma', *American Political Science Review*, 91 (1997), 1–14, p. 4.

[3] Lijphart, 'Unequal Participation,' p. 4.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

We conduct such tests with data from the American National Election Studies and find very modest support for his hypotheses. Then we show that the absence of a consequential link between outcomes and turnout can be explained by answering a hitherto neglected question: who does not vote? As a prologue to our data analysis we review the literature on the political consequences of higher turnout, beginning with empirical studies and proceeding to speculative denials.

TURNOUT AND OUTCOMES

One research genre directly explores whether, in past elections, Democrats fared better when aggregate turnout was higher. The pioneer in this school, James DeNardo, found that sometimes high turnout helped Democratic candidates and sometimes it did not.[4] This conclusion was affirmed in a later exchange with critics and then repeated in a study of gubernatorial and senatorial elections.[5]

These articles have limited relevance to our topic. For one thing, they consider turnout varying only between the upper and lower bounds of recent experience, without reference to any contemplated changes in registration procedures. Hence this genre can illuminate neither the 'disenfranchisement of large sectors of the working class'[6] nor the differential consequences of proposed changes in registration laws. Moreover, 'higher turnout' is an imprecise term; the issue is how that turnout might be brought about, i.e., what scenario one envisions about future change.

The least modest scenario assumes that everyone would vote and compares respondents who voted to an entire survey sample. Until now this comparison served only heuristic purposes, to suggest the political consequences of unstated measures to raise turnout and to show the upper bounds of such effects. It gains significance with Lijphart's proposal to make voting compulsory. The earliest estimate of universal turnout showed that if everyone had voted in 1972, the proportion of Democrats would have been unchanged, while the representation of Independents would have increased nearly 4 percentage points at the expense of Republicans.[7] This trifling difference was the largest political gap between voters and the entire sample. On a range of issues, 'voters are virtually a carbon copy of the citizen population. Those most likely to be underrepresented are people who lack opinions.'[8] Other scholars reported similar

[4] James DeNardo, 'Turnout and the Vote: The Joke's on the Democrats,' *American Political Science Review*, 74 (1980), pp. 406–20.

[5] James DeNardo, 'Rejoinder to Tucker and Vedlitz', *American Political Science Review*, 80 (1986), 1298–304; Harvey J. Tucker and Arnold Vedlitz, 'Does Heavy Turnout Help Democrats in Presidential Elections?' *American Political Science Review*, 80 (1986), 1291–8; Jack H. Nagel and John F. McNulty, 'Partisan Effects of Voter Turnout in Senatorial and Gubernatorial Elections,' *American Political Science Review*, 90 (1996), 780–93.

[6] Frances Fox Piven and Richard A. Cloward, *Why Americans Don't Vote* (New York: Pantheon Books, 1988), p. 8.

[7] Raymond E. Wolfinger and Steven J. Rosenstone, *Who Votes?* (New Haven, Conn.: Yale University Press, 1980), 109–14.

[8] Wolfinger and Rosenstone, *Who Votes?* p. 109.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

findings for elections since 1972.[9] Universal turnout would have produced bigger margins for Republican candidates in 1984 and 1988.[10]

REBUTTALS

The most common rebuttal to empirical findings suggesting that outcomes seldom depend on turnout speculates that some people would come to have different views about candidates and issues if they were to switch from abstaining to voting:

> But the opinions elicited by surveys reflect the underdevelopment of political attitudes resulting from the historic exclusion of low-income groups from active electoral participation. In other words, what survey data cannot reveal is the dynamic dimension of politics. Political attitudes would inevitably change over time if the allegiance of voters from the bottom became the object of partisan competition, for then politicians would be prodded to identify and articulate the grievances and aspirations of lower-income voters in order to win their support, thus helping to give form and voice to a distinctive class politics.[11]

One reason to expect such change is found in claims that 'we don't vote because we are ignored by the two-party system' whose candidates 'are unlikely to run on tickets that address our concerns'.[12] Therefore, so the argument goes, with universal turnout, the campaign's content (its 'dynamics') would change as politicians addressed issues of interest to previously inert citizens. This expectation can be evaluated by comparing the priorities of contemporary voters and non-voters, which we do in our data analysis.

Although few speculations about the future can be tested empirically, several observations occur to us. One is the historical parallel to the 'hidden Republican voter', the once-popular conviction that mid-century Republicans lost elections because their 'me-too' centrist presidential candidates offered true conservatives no reason to vote.[13] A second observation is our belief that non-voters' 'grievances and aspirations' offer ample opportunity for exploitation by political figures whom leftists would deplore. For example, in 1968 the segregationist candidate George Wallace was preferred by almost twice as many non-voters as voters,[14] which leads us to doubt whether underprivileged

[9] Ruy Teixeira, *The Disappearing American Voter* (Washington, DC: The Brookings Institution), pp. 87–97; Paul R. Abramson, John H. Aldrich and David W. Rohde, *Change and Continuity in the 1996 Elections* (Washington, DC: CQ Press, 1998), 86–89; Steven Earl Bennett and David Resnick, 'The Implications of Nonvoting for Democracy in the United States', *American Journal of Political Science*, 34 (1989), 771–802, p. 795. One scholar has suggested that such studies are flawed because they rely on respondents' claims about voting (see Walter Dean Burnham, *The Current Crisis in American Politics* (New York: Oxford University Press, 1982), p. 170). But much of this research relies on inspection of respondents' voting records. Our own analysis of such data from the 1988 American National Election Study, the last NES to verify respondents' reports of registration and turnout, shows that voters preferred Bush to Dukakis by 52 to 47 per cent, not very different from the 51 to 47 per cent verdict of all respondents.

[10] William Schneider, 'The November 6 Vote for President: What Did It Mean?' in Austin Ranney, ed., *The American Elections of 1984* (Durham, NC: Duke University Press, 1985); 'Who were the voters and nonvoters?' *New York Times*, 21 November 1988, A8.

[11] Piven and Cloward, *Why Americans Don't Vote*, pp. 20–1.

[12] Laurel Rosen, 'Why generations X, Y and Zero don't vote', *San Francisco Chronicle*, 25 October 1998, p. 8.

[13] Philip E. Converse, Aage R. Clausen and Warren E. Miller, 'Electoral Myth and Reality: The 1964 Election,' *American Political Science Review*, 59 (1965), 321–36. Such beliefs have their counterparts on the left. A claim that '90 or so million Americans' would stay home in 1988 'as an indictment of the Democratic Party's intelligence and courage' was titled 'Democrats shun their hidden electorate' (Hodding Carter III, *Wall Street Journal*, 7 January 1988, p. 19).

[14] John R. Petrocik, 'Voter Turnout and Electoral Preference: The Anomalous Reagan Elections', in Kay Lehman Schlozman, ed., *Elections in America* (Boston, Mass.: Allen & Unwin, 1987), pp. 239–59, at p. 250.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

people today would be interested only in politically correct scapegoats. If more people voted, 'class politics' might not be the only result; Republicans would not limit themselves to reiterating conservative economic doctrine. The more plausible scenario would be greater emphasis on issues that distract voters of all sorts from attention to economic concerns: flag burning, term limits, school prayer, gun control, gays in the military, gays generally, abortion, immigration, racial preferences, furloughs for felons, school vouchers, and the National Endowment for the Arts.[15] For example, during the 1998 election campaign Republicans seemed to be going out of their way to attack homosexuals; 'Senator Trent Lott of Mississippi, the majority leader, has compared homosexuality to kleptomania, and Republican leaders in the House have been citing the Bible in an effort to prove that homosexuals are sinners.'[16]

HOW REPRESENTATIVE ARE VOTERS?

We revisit differences between voters and all citizens for three reasons: (1) in order to demonstrate a method of making this comparison that takes account of disparities between aggregate turnout figures and turnout estimates from surveys. Our method is more sensitive to differences between voters and non-voters and therefore is less likely to show that voters are a microcosm of the entire citizen population;[17] (2) to see if voters' issue priorities resemble those of the entire sample; (3) in response to a suspicion that findings about the representativeness of voters in the 1970s might be outdated by political realities in the l990s.[18]

Voting in the general election was reported by 76.1 per cent of respondents in the 1992 National Election Studies and 71.8 per cent in the 1996 NES. This is far above the most commonly-used figure, which we will call the 'official turnout': the number of votes cast for presidential candidates divided by the Census Bureau's estimate of the voting-age population in November. Official turnout was 55.1 per cent in 1992 and 48.9 per cent in 1996.[19] Part of the discrepancy between the NES and official estimates reflects the different bases of the percentages. The NES sample is limited to citizens; the voting-age population includes non-citizens. Removing non-citizens from the base increases the official turnout to 58.3 per cent for 1992 and 52.6 per cent for 1996.[20]

The NES estimates are still almost 20 per centage points higher than these adjusted

[15] On a visit to the Berkeley campus in 1996, former Speaker Thomas Foley said that when he campaigned in union halls in his district he found few members interested in discussing wages, working hours, workplace safety or collective bargaining. Their great fear was gun control and they warmed up to Foley when he mentioned that he was endorsed by the National Rifle Association. In 1994 he was defeated for re-election when the NRA endorsed his opponent.

[16] Timothy Egan, 'Uphill race despite lesbian's past success,' *New York Times*, 22 July 1998, p. A1.

[17] Some of the hypotheses we will test refer to differences between voters and non-voters. In this instance, however, references to differences between voters and non-voters do not accurately specify the problem. The consequences of higher turnout (no matter how achieved) are correctly estimated by contrasting voters to a hypothetically larger voting population.

[18] Gary C. Jacobson, *The Politics of Congressional Elections*, 4th edn (New York: Longman, 1997), pp. 87–8.

[19] US Bureau of the Census, *Statistical Abstract of the United States 1997*, 117th edn (Washington, DC: US Government Printing Office, 1997), p. 289.

[20] We adjusted the 1992 estimate with the aid of a report showing that 5.4 per cent of the adult population in 1990 were non-citizens (US Bureau of the Census, *1990 Age, Nativity, and Citizenship for the United States, States, and Counties (CPH-L-114)*, Ethnic and Hispanic Branch, Population Division, 1990 Special Tabulations (Washington, DC, n.d.). For 1996 we used the 1996 Voter Supplement of the Current Population Survey, which provided an estimate that 7.1 per cent of the voting-age population were not citizens (US Bureau of the Census, *Current Population Survey: Voter Supplement File, 1996* [Computer File]. ICPSR version. Washington, DC: Bureau of the Census (producer). Ann Arbor, MI: Inter-university Consortium for Political and Social Research (distributor)).

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

official figures, which poses a problem for attempts to compare voters to all citizens. If voters and non-voters differ and non-voters are underrepresented, comparing voters to all respondents would overweight the responses of voters, at the expense of non-voters, with the size of the effect depending on the relative size of each group. Because NES samples underestimate the size of the non-voting population, they can lead to unrealistically small estimates of the effects of universal turnout. For example, in 1996, when an estimated 47 per cent of all adult citizens stayed home, non-voters accounted for just 28 per cent of the NES sample. There is a simple solution to this problem. We reweighted the NES samples to bring the proportions of voters and non-voters into line with the adjusted estimates of the official turnout.

One consideration remains. The biggest cause of the high NES turnout is respondents who reported voting but in fact did not do so. This would not be a problem if misreporting were random, but it seems to be positively related to education.[21] Reclassifying misreporters from voters to non-voters would make the two groups appear more similar. Lacking information to do this because verification of respondents' reports of voting was last done in the 1988 NES, we can only note that the misreporting will exaggerate estimated differences between voters and non-voters. The result is to *overestimate* any differences between self-reported voters and the entire sample. Therefore differences reported in Table 1 should be regarded as the upper limits of the effects of universal turnout.

Table 1 displays various comparisons between voters and all respondents in 1992 and 1996. It shows that our reweighting does not amend the usual conclusion for 1992 and only slightly modifies it for 1996. In other words, even when samples are adjusted to avoid underrepresenting non-voters, they remain essentially similar to voters. The differences between the two years, which we consider substantively modest, lead us to discuss each election separately.[22]

If everyone had voted in 1992, Bill Clinton's share of the vote would have shrunk by 1.2 percentage points, compared to a loss of 2.5 points by President George Bush. Clinton's margin over Bush would have risen from 13.7 to an even 15 points. Ross Perot would have picked up a few more votes and marginal candidates would have done marginally worse. Nearly 4 per cent of the sample had no candidate preference. Both Republicans and Democrats were just barely more numerous among voters than in the total population. The weaker representation of Independents among voters reflects their generally lower level of civic involvement.[23]

By and large, voters were representative of the entire sample on most of the dozen policy questions listed in Table 1. Voters were, by 5 percentage points, more conservative than the whole sample about whether 'it is important for the government to provide many more services [in areas such as health and education] even if it means an increase in spending.'[24] Employment preferences for blacks were more popular

[21] Brian D. Silver, Barbara A. Anderson and Paul R. Abramson, 'Who Overreports Voting?' *American Political Science Review*, 80 (1986), 613–24.

[22] To be sure, elections can be decided by small margins, but the propositions we are testing allege that the distribution of ballot choices would be significantly different if everyone voted.

[23] Throughout this Note we define Independents as respondents who choose this identity and deny they are closer to either party; those who initially call themselves Independents and then acknowledge closeness to a party are combined with the outright adherents of that party. Such 'leaning' Independents mostly vote for the party to which they concede being closer, while most 'pure' Independents vote for the winning presidential candidate (Bruce E. Keith *et al.*, *The Myth of the Independent Voter* (Berkeley, Calif.: University of California Press, 1992), chap. 3.

[24] We used all NES policy questions measured with the traditional seven-point scale and selected additional questions to provide a wide range of issues over which there is substantial political conflict. The wording of all

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

TABLE 1 *Preferences of Voters and Entire NES Sample Compared*

	1992		1996	
	Voters	Entire Sample	Voters	Entire Sample
		Presidential Choice (%)		
Clinton	47.3	46.1	53.1	59.5
Bush	33.6	31.1	—	—
Dole	—	—	37.6	26.3
Perot	18.8	19.0	7.6	9.3
Other	0.3	0.2	1.7	1.0
No preference*	0.0	3.6	0.0	4.0
		Party identification (%)		
Strong Democrat	20	18	22	17
Weak Democrat	17	17	18	22
Leaning Democrat	14	15	12	15
Pure Independent	9	12	6	11
Leaning Republican	12	13	11	12
Weak Republican	15	15	16	14
Strong Republican	13	11	16	10
		Percentage with 'liberal' preference†		
Government services	35	40	29	34
Government medical insurance plan	51	51	34	43
Government guaranteed job/std. of living	29	32	23	29
Government assistance for blacks	23	23	16	19
Approach to crime reduction	—	—	27	30
Environmental regulations	—	—	52	56
Defence spending	48	46	31	32
Women's role	77	75	77	78
Abortion	64	59	57	55
Homosexuals in the armed forces	59	58	69	68
Death penalty	19	20	20	22
Hiring/promotion preferences for blacks	17	21	17	20
School prayer	13	11	12	11
Handgun control	—	—	45	47
English as official language	29	29	—	—

*Non-voters who expressed no preference when asked for whom they would have voted, but who rated one of the three candidates higher than the other two on the candidate feeling thermometers, were coded as preferring the highest rated candidate.
†See Appendix on the web version of this Note for question wording and definitions of 'liberal' preferences.
Sources: 1992, 1996 National Election Studies.

among the entire sample, 21 per cent of whom supported this policy, compared to 17 per cent of voters. By 3 percentage points voters were less favourable about the federal government guaranteeing a job and a good standard of living to everyone. On the other

(F'note continued)
questions and our coding of liberal responses are in the Appendix available on the web version of this Note. Respondents who did not express an opinion on any question were excluded from the base on which liberal responses for that issue were calculated. These excluded cases will be the principal topic in the following section of this Note.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

hand, voters were more sympathetic to abortion rights. On all other issues, differences in either direction did not exceed 2 percentage points.

In 1996, universal turnout would have expanded Clinton's share of the vote from 53.1 to 59.5 per cent, chopped Bob Dole's vote more than 11 points, and doubled Clinton's winning margin.[25] Changes among other candidates were trivial and 4 per cent of the sample had no candidate preference. In contrast to 1992, Democrats were slightly less numerous among voters and Republicans somewhat less so. As always, Independents were scarcer among voters than in the general public.

The pattern of differences on issues was somewhat more pronounced in 1996. This was particularly the case on redistributive economic questions, the first three lines in the issue section of Table 1. By anywhere from 5 to 9 percentage points, voters were more conservative than the whole sample. These are, we believe, the largest such differences found in such analyses of any recent election. Smaller gaps in the same direction are apparent on several other issues. Voters were barely more liberal on just three topics: abortion, gays in the military, and school prayer.

We also compared responses to the familiar NES question about whether spending should be 'increased, decreased, or kept about the same' for each of more than a dozen federal programmes. In almost every spending category the whole sample was more generous than just the voters, but the differences were never great, averaging about 2 per cent in 1992 and 5 per cent in 1996.

These findings are incontestable evidence that on some major issues voters were more conservative than the entire adult population in 1996. We defer for the moment trying to appraise the importance of this recent tilt to the right, other than to note that the differences between voters and the entire electorate, while indisputable, are relatively slight; none is as high as 10 percentage points.[26] These modest differences do not suggest that universal turnout would have brought success to the Clinton health insurance plan or prevented enactment of 'welfare reform'.

To investigate whether universal turnout would expand the mainstream to include political leaders currently on the fringes, we looked at assessments of Jesse Jackson, Pat Robertson, Pat Buchanan and Louis Farrakhan. The average feeling thermometer scores for the first three are in the 40s for both voters and the entire sample. Farrakhan scored 25 with voters and 27 for the entire sample.

We also explored the possibility that universal turnout would bring about a change in policy priorities by comparing voters and the entire sample with regard to the standard NES question on 'the most important problem' facing the country. This is a question that has been ignored in previous empirical research. Roughly a third of the entire sample in 1992 and 1996 volunteered a social welfare issue as the most important problem.[27] In both years, voters were less likely to mention social welfare issues by only 4 percentage points. On racial, environmental, women's and explicit class issues, the

[25] The anomalous nature of this finding is suggested by comparing this change to those in other election years. Not only are the differences smaller in 1992, but the largest difference in candidate preference between voters and an entire survey sample for every presidential election from 1960 to 1988 is 2.6 percentage points. See Teixeira, *The Disappearing American Voter*, p. 96.

[26] These results are consistent with Lijphart's characterization of previous research that 'the usual finding is that there are only *small* differences instead of *no* differences'. See Lijphart, 'Unequal Participation,' p. 4, fn. Small differences, however, do not lead to the conclusion that the content of public policy would be significantly changed if everyone voted.

[27] We recoded the NES important problem mastercodes in the following manner: social welfare (6, 10, 13, 20, 30, 40, 50, 60, 91), environment (150, 151, 153, 154, 160), racial (300), women (45, 330), class/workers (401, 403, 404, 405, 433, 440, 441, 442, 451).

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

TABLE 2 *Prevalence of 'No Opinions' among Voters and Non-voters**

	1992		1996	
	Voters	Non-voters	Voters	Non-voters
Government services†	13	33	12	22
Government medical insurance plan†	10	22	10	13
Government guaranteed job/ standard of living†	9	20	8	13
Government assistance for blacks†	8	14	7	13
Approach to crime reduction†	—	—	4	8
Environmental regulations†	—	—	16	31
Defence spending†	9	24	10	24
Women's role†	3	7	4	6
Abortion	1	1	1	0
Homosexuals in the armed forces	4	4	2	2
Death penalty	2	2	3	2
Hiring and promotion preferences for blacks	3	7	1	3
School prayer	0	2	0	0
Handgun control	—	—	1	1
English as official language	8	12	—	—

*The table shows the percentages in each group who 'don't know' or 'haven't thought much about it'. See Appendix on the web version of this Note for question wording.
†Question explicitly asks if respondent hadn't 'thought much about this.'
Sources: 1992, 1996 National Election Studies.

differences are all less than 1 percentage point, with fewer than 3 per cent of all the respondents identifying any one of these as the most important problem.

'NO OPINIONS' AMONG VOTERS AND NON-VOTERS

'Nonvoters who are asked their opinions on policy … are typically citizens who have not given these questions much thought.'[28] The data on attitudes towards 'government services' in Table 1 are based on responses to the following item, which is typical of many NES questions:

Some people think the government should provide fewer services, even in areas such as health and education, in order to reduce spending. Suppose these people are at one end of the scale at point number 1. Other people feel it is important for the government to provide many more services even if it means an increase in spending. Suppose these people are at the other end, at point 7. And, of course, some other people have opinions somewhere in between at points 2, 3, 4, 5, or 6. Where would you place yourself on this scale, or haven't you thought much about this?

Opinions on the first eight issues in Table 1 were elicited with questions that included this last clause. On other questions, while this option was not explicitly offered, respondents who volunteered that they did not know were not pressed to provide an answer. It is not difficult to test Lijphart's proposition about non-attitudes simply by tabulating the proportions of voters and non-voters who declined to offer an opinion to

[28] Lijphart, 'Unequal Participation', p. 4.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

each of the issue questions in Table 1. The results of this exercise are displayed in Table 2.

Table 2 shows the proportion of non-attitudes on a total of twenty-six issue questions asked in 1992 and/or 1996. Two conclusions are immediately apparent: While non-voters are indeed less likely than voters to say they have a preference, this is not their typical response; on every issue the vast majority of non-voters do produce an opinion. On nineteen out of twenty-six opportunities more than 80 per cent of non-voters have something to say. The average percentage of respondents without an opinion on a question about policy preferences was 5.7 for voters and 11 for non-voters. Even on topics like defence spending and environmental policy, which might be less than pressing concerns to people struggling to make ends meet, a substantial majority of non-voters expressed an opinion. Other than these two issue areas, non-voters were least willing to offer opinions on those redistributive policies that provide a safety net of sorts to the poor.

This was not the case, however, on questions about spending levels for particular programmes. Voters and non-voters alike rarely hesitated to offer their views about how much money the federal government should spend on programmes ranging from helping the homeless to supporting science and technology. Few respondents – never more than 3 per cent – professed not to know about how much to spend 'if you had a say in making up the federal budget this year' on any of the dozen-plus programmes mentioned.

We also compared voters and non-voters on more general partisan and ideological orientations. In both 1992 and 1996 at least 20 per cent of non-voters, compared to under 10 per cent of voters, denied any affinity for either political party. Most of these non-voters were Independents; a few, still further removed from political consciousness, were classified as non-political by the NES. Differences in ideological awareness were substantially greater. In both years at least 40 per cent of non-voters said they 'haven't thought much about it' or just did not know when asked to place themselves on a seven-point ideology scale ranging from very liberal to very conservative. Less than half as many voters declined this invitation to locate themselves on the left–right ideological spectrum. To our surprise, non-voters were not much more likely to call themselves 'moderates'.

In short, we found that non-voters typically do express policy, partisan and ideological preferences when asked. At the same time, non-voters are slightly less opinionated on these sorts of questions.

CLASS CONSCIOUSNESS

We used various measures to test Lijphart's assertion that non-voters 'have not developed class consciousness'.[29] One NES question tells respondents that 'There's been some talk these days about different social classes. Most people say they belong either to the middle class or the working class. Do you ever think of yourself as belonging in one of these classes?' Thirty-one per cent of non-voters in 1992 said they did not identify with either the working class or the middle class, which is not very different from the 26 per cent of voters who refused one of these two labels.[30]

Assuming that weak class consciousness is basically a workers' infirmity, we then conducted a more detailed analysis of respondents with blue-collar occupations.[31] Were

[29] Lijphart, 'Unequal Participation', p. 4.
[30] This question about subjective social class was not asked in 1996.
[31] Specifically, we defined those occupations coded 6–13 in the NES collapsed occupation codes as 'blue collar'.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

TABLE 3 *Class Closeness among Voters and Non-voters**

	1992		1996	
	Voters (%)	Non-voters (%)	Voters (%)	Non-voters (%)
Class closeness – all respondents				
Working	17	27	21	29
Both	47	36	42	36
Middle	24	17	22	14
Neither	13	20	16	21
Class closeness – blue-collar respondents only				
Working	23	34	29	32
Both	48	33	40	33
Middle	14	12	15	10
Neither	16	21	16	25

*See Appendix on the web version of this Note for question wording.
Sources: 1992, 1996 National Election Studies.

blue-collar voters more likely to identify with the working class than their non-voting counterparts? Not at all. Forty-six per cent of the former and 48 per cent of the latter said they thought of themselves as belonging to the working class.

Yet another measure of class consciousness can be derived from a question asking all respondents if they feel 'particularly close to' working-class people, and an identical question about the middle class. Responses to these questions by voters and non-voters are displayed in the top part of Table 3. The most common pattern was affirmative answers to both questions, given by nearly half of all voters and just over a third of non-voters. Non-voters were also a bit more likely to respond negatively to both questions and to say they felt closer to the working class. When we restricted the analysis to people with blue-collar occupations, a similar pattern emerged. Blue-collar non-voters were *more* likely to express closeness to the working class than their counterparts who went to the polls. All in all, then, we could find few signs that non-voters suffered more than other Americans from undeveloped class consciousness.

POLITICAL MOBILIZATION

We found solid confirmation of Lijphart's proposition that non-voters 'have not been politically mobilized', which we defined as being canvassed either by telephone or in person. Respondents were asked if someone from a political party or anyone else had contacted them about the campaign or about voting for a particular candidate. In 1992, 31 per cent of voters and 10 per cent of non-voters were canvassed. The figures for 1996 were 38 per cent of voters and 14 per cent of non-voters.

We pause to take stock of the evidence for Lijphart's propositions. Non-voters do differ from voters in the directions he predicted. They are less likely to have opinions on issues, identify with a party, express an ideological preference, be class conscious, or be stimulated by campaign activists. Yet except for the last point the differences are generally trivial.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

SIMULATING UNIVERSAL TURNOUT

How might one estimate what non-voters would do if they were to vote? We sought an empirical approach to this question that would give Lijphart's propositions the most sympathetic hearing. We began with his belief that being activated to vote includes heightened awareness that develops class consciousness and crystallizes political opinions. Hence we disregarded non-voters' stated preferences. Instead, we attributed to them the opinions of people who had already been mobilized. Of course, all voters are not identical; the question is identifying cognate voters. In view of Lijphart's concern with economic differences, we attributed to non-voters the perspectives of voters in their respective income quintile. We assigned to poor non-voters the political preferences of poor voters, rich non-voters the characteristics of rich voters, and so on.

This process required three steps in order to see what the entire sample would look like if non-voters were to have the preferences of voters with similar incomes. First, we arrayed the distribution of each variable in Table 1 for voters in each of the five income quintiles. Secondly, we attributed these opinions to non-voters in the corresponding quintile. Thirdly, the preferences of voters and non-voters were combined to produce an estimate for the entire sample, which we will call the 'simulated' population. The results of this last step are displayed in Table 4, our estimate of what American voters would be like with universal turnout.

If everyone had voted in 1992, Clinton would have gained 2.5 per cent more of the vote and Bush would have lost 1.5 per cent; Clinton's lead would have gone from 13.7 percentage points to 17.7 points. In terms of party identification, Democrats would have gained and Republicans lost about 3 per cent of the voters.[32] The results would have been about the same in 1996, except for a slightly larger rise in Clinton's vote share. *In the 1990s*, universal turnout would have been a slight benefit to Democratic presidential candidates.

Discussions of the consequences of higher turnout often assume or assert that public policy reflects the distribution of voters' preferences; the more liberal the opinions of those who choose leaders, the more liberal will be the measures enacted into law in Washington: 'the inclination of many politicians to give short shrift to the interests of the young, the poor, the working classes, the black and the brown, has been encouraged by the consistently poor voting records of those groups.'[33] Table 1 provided faint encouragement for the belief that universal turnout would have produced a somewhat more supportive constituency for liberal policies, particularly on redistributive economic issues. The data in Table 4 are a dose of cold water for that opinion. Although the simulated population is indeed more liberal than voters on half of the twenty-six comparisons of issue attitudes in Table 4, *the largest such gap is only 2 percentage points*. There is no difference at all on nine more comparisons. And voters are just barely more liberal on abortion and the role of women. To the extent that voters' issue preferences are more important than their choices of candidates, the lower part of Table 4 modifies the effect of the upper part.

[32] This is consistent with the finding that 'Citizens marginal to the electoral process, for example, such as chronic non-voters, give responses to the party identification item which are very unstable and which seem to move dynamically in tune with whatever party the respondent would vote for at the moment (assuming interest enough to get to the polls, which is usually absent)' (Philip E. Converse and Roy Pierce, 'Measuring Partisanship', *Political Methodology*, 11 (1985), 143–66, p. 150).

[33] Bob Herbert, 'Untapped power', *New York Times*, 7 October 1996, p. A10.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

TABLE 4 *Preferences of Voters and the Simulated Voting Population Compared*

	1992		1996	
	Voters	Simulated population	Voters	Simulated population
		Presidential choice (%)		
Clinton	47.3	49.8	53.1	55.6
Bush	33.6	32.1	—	—
Dole	—	—	37.6	35.1
Perot	18.8	17.8	7.6	7.5
Other	0.3	0.3	1.7	1.8
		Party identification (%)		
Strong Democrat	20	22	22	24
Weak Democrat	17	17	18	19
Leaning Democrat	14	14	12	12
Pure Independent	9	9	6	6
Leaning Republican	12	11	11	10
Weak Republican	15	14	16	15
Strong Republican	13	12	16	15
		*Percentage with 'liberal' preference (%)**		
Government services	35	37	29	31
Government medical insurance plan	51	53	34	36
Government guaranteed job/std. of living	29	31	23	25
Government assistance for blacks	23	24	16	16
Approach to crime reduction	—	—	27	27
Environmental regulations	—	—	52	52
Defence spending	48	48	31	32
Women's role	77	76	77	76
Abortion	64	62	57	55
Homosexuals in the armed forces	59	59	69	69
Death penalty	19	21	20	22
Hiring/promotion preferences for blacks	17	18	17	19
School prayer	13	13	12	12
Handgun control	—	—	45	46
English as official language	29	29	—	—

*See Appendix on the web version of this Note for question wording and definitions of 'liberal' preferences.
Sources: 1992, 1996 National Election Studies.

WHO DOES NOT VOTE?

Why is it surprising that with universal turnout voters would differ so little from those who actually go to the polls? The answer may reflect election commentary that dwells on the turnout *rates* of different economic groups, almost to the exclusion of their relative sizes and at the expense of other demographic dimensions:[34]

[34] Turnout has 'a serious claim on our attention' because 'it is those who most need the vote who tend to use it least, and universal suffrage fails to provide the political counterweight to the power of property and wealth in the way that was intended by its more radical proponents' (Ivor Crewe, 'Electoral Participation', in David Butler, Howard R. Penniman and Austin Ranney, eds, *Democracy at the Polls* (Washington, DC: American Enterprise Institute, 1981), pp. 216–53, at p. 262.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

TABLE 5 *Characteristics of Non-voters*

Characteristic	1992 (%)	1996 (%)
Two years or less at current address	43	43
Age 18 to 29	34	33
Less than high school	30	25
Poor*	30	27
Non-white or Latino	24	24
Two years or less at current address *or* age 18–29	57	55
Less than high school *or* poor *or* non-white *or* Latino	54	51

*Family income less than $12,500 in 1992 and $15,000 in 1996. The *Statistical Abstract* (US Bureau of the Census 1997a (476) reports that in 1992 the official poverty threshold for a family of three was $11,186, and for a family of four it was $14,335. Figures for 1996 were not available; the respective numbers for 1995 were $12,158 and $15,569. *Sources:* 1992 and 1996 Current Population Survey Voter Supplements.

The single most important characteristic of voting in the United States is the economic bias of turnout patterns … those at the top end of the income scale turn out in far larger numbers than those at the bottom end … politicians are responsive to those who vote; voters determine who is elected; nonvoters do not.[35]

This passage embodies two ways analysts can go astray. The first is simply that most Americans are neither rich nor poor, hence it is misleading to ascertain the relationship between income and turnout by comparing the top and bottom ends of the income distribution.[36] The more serious error is leaping from the turnout rates of a chosen low-turnout group to a conclusion that people with that characteristic account for most non-voters. For example, 'In fact, the poor make up about three-fourths of the 'party of nonvoters'.[37] This mistake can be compounded by attributing a political preference to the group: 'Granted the demographics and the class composition of the "party of nonvoters", there seems little reason to doubt that these would be largely Democratic voters, had the Democratic party been interested in, or capable of, the mobilizing incentives to reach them'.[38]

We explain why this prediction is incorrect with a simple but unusual exercise: describing non-voters. Table 5 displays the most common demographic characteristics of non-voters. No single characteristic is shared by a majority of those who did not vote in either 1992 or 1996; the 'party of non-voters' is rather diverse. The two most common

[35] Thomas B. Edsall, *The New Politics of Inequality* (New York: W. W. Norton, 1984), p. 179.

[36] For evidence on this point, see Wolfinger and Rosenstone, *Who Votes?*, pp. 25–6.

[37] Walter Dean Burnham, *Democracy in the Making: American Government and Politics*, 2nd edn (Englewood Cliffs, NJ: Prentice-Hall, 1986), p. 3. Another estimate is that 'poor and low-income Americans … comprise two thirds of the disenfranchised' (Jordan Moss, 'Motor Voter: From Movement to Legislation', *Social Policy*, 24 (1993), 21–31, pp. 29–30).

[38] Walter Dean Burnham, 'Elections as Democratic Institutions', in Kay Lehman Schlozman, ed., *Elections in America* (Boston, Mass.: Allen & Unwin, 1987), pp. 27–60, at p. 47.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

demographic features of non-voters are their residential mobility and youth; in both 1992 and 1996, fully 43 per cent of non-voters had moved within two years of the election and one third were under the age of 30. Neither the residentially mobile nor young people are politically distinctive, let alone a Mother Lode of votes for Democratic candidates. In 1992, for example, 56 per cent of voters who identified with one of the political parties were Democrats, compared to 57 per cent of people who had recently moved and 58 per cent of the young. Movers and young people held only slightly more liberal policy positions than all voters in 1992.

The more commonly mentioned groups of non-voters are smaller. People without a high school diploma, the poor and minorities each comprise between 24 and 30 per cent of the non-voters.[39] Only when these three groups are combined does one find a majority of putatively liberal non-voters. And even in this case, they represent a bare majority, somewhat smaller than the number of non-voters who are either residentially mobile or young. Thus the notion that non-voting is concentrated among a single group or a set of related groups is incorrect.

CONCLUSION

Would election outcomes and the substance of public policy in the United States change dramatically if more people voted? Contrary to the expectations of many others, we have found that universal turnout would bring modest changes. Two approaches lead to this conclusion. First, when the stated preferences of non-voters are aggregated with those of voters, little change is observed. Secondly, to address the possibility that non-voters' preferences would be different if they were mobilized to vote, we attributed the preferences of voters to cognate non-voters and again aggregated the two groups. This approach provided even *smaller* estimates of change. In other words, by addressing Lijphart's critique of the first method, we found less support for the conventional wisdom that higher turnout would be a boon for the Democrats.

The explanation for the modest changes may be found in the characteristics of non-voters. To be sure, the poor, less educated and minorities are overrepresented among non-voters. But the young and the transient are even more numerous. By themselves, none of these groups constitutes even a majority of non-voters. Combined, they barely do so. What our findings have demonstrated is that the 'party of non-voters' is heterogeneous. Taken as a whole, non-voters appear well represented by those who vote.[40]

[39] The data in Table 5 are from the 1992 and 1996 Current Population Survey (CPS) Voter Supplements. The CPS is better than the NES for demographic descriptions because of higher response rates and substantially larger numbers of respondents. Like the NES, the CPS turnout measure is not validated. If the nature of misreporting among CPS respondents is similar to that of the NES, then the percentages of non-voters who are less educated, poor and non-white reported in Table 5 are overestimates. Recall that misreporting in the NES is positively associated with educational attainment. As a result, properly classifying misreporters would produce a better educated group of non-voters; the percentage of less educated citizens among non-voters would fall. Because the poor and minorities tend to be less educated, their representation among non-voters would also decline.

[40] Generalizing to other forms of political participation on the basis of the findings reported here clearly is unwarranted. Turnout is the least unequal form of political participation in the United States (Steven J. Rosenstone and John Mark Hansen, *Mobilisation, Participation, and Democracy in America* (New York: Macmillan, 1993), p. 237).

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

APPENDIX: QUESTION WORDING AND CODING

Government services: Some people think the government should provide fewer services, even in areas such as health and education, in order to reduce spending. Suppose these people are at one end of the scale at point number 1. Other people feel it is important for the government to provide many more services even if it means an increase in spending. Suppose these people are at the other end, at point 7. And, of course, some other people have opinions somewhere in between at points 2, 3, 4, 5 or 6. Where would you place yourself on this scale, or haven't you thought much about this? 'Liberal' response defined as 5, 6 or 7.

Government medical insurance plan: There is much concern about the rapid rise in medical and hospital costs. Some people feel there should be a government insurance plan which would cover all medical and hospital expenses for everyone. Others feel that all medical expenses should be paid by individuals, and through private insurance plans like Blue Cross or other company plans. Where would you place yourself on this scale, or haven't you thought much about this? 1. Government insurance plan; 7. Private insurance plan. 'Liberal' response defined as 1, 2 or 3.

Guaranteed job/standard of living: Some people feel the government in Washington should see to it that every person has a job and a good standard of living. Others think the government should just let each person get ahead on their own. Where would you place yourself on this scale, or haven't you thought much about this? 1. Government see to job and good standard of living; 7. Government let each person get ahead. 'Liberal' response defined as 1, 2 or 3.

Government assistance for blacks: Some people feel that the government in Washington should make every effort to improve the social and economic position of blacks. Others feel that the government should not make any special effort to help blacks because they should help themselves. Where would you place yourself on this scale, or haven't you thought much about this? 1. Government should help blacks; 7. Blacks should help themselves. 'Liberal' response defined as 1, 2 or 3.

Approach to crime reduction: Some people say that the best way to reduce crime is to address the social problems that cause crime, like bad schools, poverty and joblessness. Other people say the best way to reduce crime is to make sure that criminals are caught, convicted and punished. Where would you place yourself on this scale, or haven't you thought much about this? 1. Social problems that cause crime, like bad schools, poverty and joblessness; 7. Make sure criminals are caught, convicted and punished. 'Liberal' response defined as 1, 2 or 3.

Environmental regulations: Some people think we need much tougher government regulations on business in order to protect the environment. Others think that current regulations to protect the environment are already too much of a burden on business. Where would you place yourself on this scale, or haven't you thought much about this? 1. Tougher regulations on business are needed to protect the environment; 7. Regulations to protect the environment already too much of a burden on business. 'Liberal' response defined as 1, 2 or 3.

Defense spending: Some people believe that we should spend much less money for defense. Others feel that defense spending should be greatly increased. Where would you place yourself on this scale, or haven't you thought much about this? 1. Greatly decrease defense spending; 7. Greatly increase defense spending. 'Liberal' response defined as 1, 2 or 3.

Women's role: Recently there has been a lot of talk about women's rights. Some people feel that women should have an equal role with men in running business, industry, and government. Others feel that women's place is in the home. Where would you place yourself on this scale, or haven't you thought much about this? 1. Women and men should have an equal role; 7. Women's place is in the home. 'Liberal' response defined as 1, 2 or 3.

Abortion: There has been some discussion about abortion during recent years. Which one of the opinions

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

on this page best agrees with your view? You can just tell me the number of the opinion you choose. 1. By law, abortion should never be permitted. 2. The law should permit abortion only in case of rape, incest or when the woman's life is in danger. 3. The law should permit abortion for reasons other than rape, incest, or danger to the woman's life, but only after the need for the abortion has been clearly established. 4. By law, a woman should always be able to obtain an abortion as a matter of personal choice. 'Liberal' response defined as 3 or 4.

Homosexuals in the armed forces: Do you think homosexuals should be allowed to serve in the United States Armed Forces or don't you think so? 1. Yes, think so; 2. Don't think so. 'Liberal' response defined as 1.

Death penalty: Do you favor or oppose the death penalty for persons convicted of murder? 1. Favor; 2. Depends; 3. Oppose. 'Liberal' response defined as 3.

Hiring preferences for blacks: Some people say that because of past discrimination, blacks should be given preference in hiring and promotion. Others say that such preference in hiring and promotion of blacks is wrong because it gives blacks advantages they haven't earned. What about your opinion – are you for or against preferential hiring and promotion of blacks? 1. For; 5. Against. 'Liberal' response defined as 1.

School prayer: Which of the following views comes closest to your opinion on the issue of school prayer? Just give me the number of your choice. 1. By law, prayers should not be allowed in public schools. 2. The law should allow public schools to schedule time when children can pray silently if they want to. 3. The law should allow public schools to schedule time when children, as a group, can say a general prayer not tied to a particular religious faith. 4. By law, public schools should schedule a time when all children would say a chosen Christian prayer. 'Liberal' response defined as 1.

Handgun control: Do you favor or oppose a ban on the sale of all handguns, except those that are issued to law enforcement officers? 1. Favor; 5. Oppose. 'Liberal' response defined as 1.

English as official language: Do you favor a law making English the official language of the United States, meaning government business would be conducted in English only, or do you oppose such a law? 1. Favor; 3. Neither favor nor oppose; 5. Oppose. 'Liberal' response defined as 5.

Ideological identification: We hear a lot of talk these days about liberals and conservatives. Here is a 7-point scale on which the political views that people might hold are arranged from extremely liberal to extremely conservative. Where would you place yourself on this scale, or haven't you thought much about this?

Class consciousness: There's been some talk these days about different social classes. Most people say they belong either to the middle class or the working class. Do you ever think of yourself as belonging in one of these classes?

Class closeness: Here is a list of groups. Please read over the list and tell me the letter of those groups you feel particularly close to – people who are most like you in their ideas and interests and feelings about things. [Both 'middle-class people' and 'working-class people' were included on the list.]

Party mobilization: The political parties try to talk to as many people as they can to get them to vote for their candidate. Did anyone from one of the political parties call you up or come around and talk to you about the campaign this year?

Other mobilization: Other than someone from the two major parties, did anyone else call you up or come around and talk to you about supporting specific candidates in this last election?

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

Expectations and Aspirations in Dilemma Interactions

MATTHEW MULFORD*

Incentive compatibility solutions to social dilemmas change individual consequences of non-co-operative and co-operative acts such that self-maximizers will rationally choose to act in ways that promote the public good, i.e. to *co-operate*. The traditional selfish egoism assumption of rational choice requires dilemmas to be solved by either adding some likely punishment to defection or reward to co-operation. Dilemma co-operation can also, of course, be explained by relaxing the narrow self-interest assumption. By allowing for the possibility that outcomes of interdependent interactions are subjectively evaluated by reference to the actor's own as well as others' interests, or that people get additional utility (or avoid disutility) by following certain normative codes, co-operation in finitely repeated dilemmas can be rational.[1]

By relaxing the self-interest assumption, subjective expected utility (SEU) choice models can explain co-operation in finitely repeated dilemmas. This is consistent with observations of co-operation in the absence of formal egoistic incentives from the earliest experimental work in dilemma behaviour.[2] This is also consistent with reported positive correlations between expectations of others' likely defection and defecting choices.[3] As long as people do not have dominant strategies, then expectations should affect choice. Notice, however, that the implied decision process of the 'relaxed' SEU model remains largely untested.[4]

* Department of Government, London School of Economics and Political Science. Thanks are due to Robyn Dawes, Keith Dowding, Patrick Dunleavy, Colin Mills, Tomonori Morikawa, John Orbell, Lars Skalnes, Paul Slovic and the *Journal*'s anonymous referees for providing substantive and methodological suggestions at various stages of this project. The research was partly supported by National Science Foundation Grants No. SES 9008157 and SES 9208534.

[1] For examples of evidence of subjective transformations of given payoff matrices, see James Andreoni, 'Giving with Impure Altrusim: Applications to Charity and Recardian Equivalence', *Journal of Political Economy*, 97 (1989), 447–51; and Michael D. Kuhlman and Alfred F. J. Marshello, 'Individual Differences in Game Motivation as Moderators of Preprogrammed Strategic Effects in Prisoner's Dilemma', *Journal of Personality and Social Psychology*, 32 (1975), 922–31.

[2] For reviews of this literature, see Linda Caporael, Robyn Dawes, John Orbell and Alphons van de Kragt, 'Selfishness Examined: Cooperation in the Absence of Egoistic Incentives', *Behavioral and Brain Sciences*, 12 (1989), 683–98; Robyn M. Dawes, 'Social Dilemmas', *Annual Review of Psychology*, 31 (1980), 169–93; Robyn M. Dawes and Richard H. Thaler, 'Anomalies: Cooperation', *Journal of Economic Perspectives*, 2 (1988), 187–97; and Dean G. Pruitt and Merek J. Kimmel, 'Twenty Years of Experimental Gaming: Critique, Synthesis, and Suggestions for the Future', *Annual Review of Psychology*, 28 (1977), 363–92.

[3] It is obviously the case that (for game theorists) prisoners' dilemmas are defined, in part, by a preference for free-riding over co-operation. To avoid confusion I refer to games where the material payoffs, taken alone, define a prisoners' dilemma as a 'dilemma interaction'. For examples of the expectations/choice correlations, see Robyn M. Dawes, Jeanne McTavish and Harriet Shaklee, 'Behavior, Communication, and Assumptions about Other Peoples' Behavior in a Common Dilemma Situation', *Journal of Personality and Social Psychology*, 31 (1977), 1–11; Harold H. Kelley and Anthony Stahelski, 'Social Interaction Basis of Cooperators' and Competitors' Beliefs about Others', *Journal of Personality and Social Psychology*, 16 (1970), 66–91; and Kuhlman and Marshello, 'Individual Differences in Game Motivation'. For a meta-analysis of experimental findings, see David Sally, 'Conversation and Cooperation in Social Dilemmas: A Meta-Analysis of Experiments from 1958 to 1992', *Rationality and Society*, 7 (1995), 58–92.

[4] For a notable exception, see Michael W. Morris, Damier L. H. Sim and Vittorio Girotto, 'Distinguishing Sources of Cooperation in the One-Shot Prisoner's Dilemma: Evidence for Cooperative Decisions Based on the Illusion of Control', *Journal of Experimental Social Psychology*, 34 (1988), 494–512.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

Utility functions are defined exogenously and actions are chosen if and only if the product of one's subjective probability estimate of the likelihood of outcomes associated with a given strategy and the value placed on those outcomes yields a higher expected utility than does any other perceived alternative strategy.

The simplest version of an SEU model of choice uses objectively defined end states having defined individual payoffs. Each payoff is multiplied by the subjective probability of the state occurring. A rational agent chooses a strategy that maximizes the sum of these products. If correct, single-shot anonymous dilemmas should lead to 100 per cent defection. As noted above, this is clearly not supported by the data.

A more complex version of the SEU model holds expectations and utility to be exogenously defined but independent choice elements. In this relaxed version of the SEU model, subjective evaluation can be different from objective individual payoffs. Dilemma co-operation can be a rational act as long as people evaluate the outcomes in certain ways.

This model is certainly more empirically and intuitively plausible. The relaxed version of SEU implies a decision calculus where objective individual payoffs are one of many factors that influence the evaluation of an outcome. Objective definitions of end states exist but, just as a visual image can be distorted by a mirror, the utility assigned to them reflects 'distortions' caused by internal subjective evaluation processes. The choice of a strategy is a function of these altered values and the subjective perception of the probability of their occurrence.

Note that these two elements are still completely independent. The model predicts no contamination across these stages of choice.[5] While expectations of others' behaviour affect the expected utility of a *strategy* they do not affect the utility of a given *outcome*. As traditionally applied to dilemma interactions, a relaxed SEU model maintains that one's expectations of another's behaviour can have no effect on whether one aspires to mutual co-operation or free-riding.[6]

This Note tests this assumption by analysing data from two experimental studies of dilemma behaviour. To anticipate, the relaxed SEU model does not account for a significant number of co-operative choices. Specifically, it fails to predict that both would-be co-operators and self-maximizers respond to high expectations of others' co-operating by co-operating themselves.

A number of possible explanations for co-operation in the absence of any incentive compatibility are proposed. Each is partially supported by the data. The first explanation relies on feelings of obligation imposed by a truster on a trustee. A player expecting another to co-operate implies the belief that the other is trusting the player to reciprocate, i.e. that the other is putting him/herself at risk of some loss based on expectation that the player will forgo temptation for narrow personal gain. The second explanation holds that evaluations of the dilemma outcomes are not independent of expectations of others' likely behaviour. The more subjects expect others to defect, the less they *aspire* to mutual co-operation.

[5] For a discussion of the theoretical consequences of a using one's own choice as a guide to others' likely behaviour, see John Orbell and Robyn Dawes, 'A "Cognitive Miser" Theory of Cooperators' Advantage', *American Political Science Review*, 85 (1991), 515–28.

[6] In most classic texts, expected utility theory is introduced in comparison to expected value (EV) calculations. Typically the EV of A is defined as $p(V_A)$. Then the concept of utility is defined as $p(U_A)$ – a simple replacement of the objectively defined value with a subjectively defined utility. While utility is defined exogenously, there is no implication that the probability of the outcome becomes an integral part of the utility function. The p remains fixed both in notation and in the implicit structure of the theory.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

EXPERIMENTAL DESIGN

Study 1

Subjects were recruited by advertisements in a university newspaper and the local metropolitan daily. The advertisement offered 'between $5 and $40' to people participating in two studies that would require about two hours to complete. The exact amount of pay would depended on 'decisions you make and the simultaneous decisions of others in the experiment.' Subjects were randomly assigned to conditions and replications. Efforts were made to prevent people who knew each other from participating in the same replication.

Since it was important for subjects to face the possibility of losing money, and since – for obvious reasons – subjects could not leave the experiment poorer than they entered, each study had two parts. The first involved pencil-and-paper responses to personality inventory questions with the subjects being asked to make judgements about how students at other universities would answer a series of questions. The subjects had 45 minutes to complete this part of the study and were paid $20 for their efforts.

Payment was in the form of twenty single-dollar bills. The subjects were asked to count the money and put it into one of six plastic bags marked with the letters A through F. These letters served as the subjects' identification for the duration of the study. The plastic bags containing the pay from this part of the study were placed on a table in the centre of a large room. Subjects were told that this would be their 'starting money' for the subsequent 'decision-making' study, and that it would be possible for them to gain or lose money from this $20 base, 'depending on what you and the others here choose'. This was all done to maximize the likelihood that the subjects would set their monetary status quo entering the prisoners' dilemma games at a positive $20, and would therefore perceive losses from that point as real.

Subjects were then seated in chairs marked with their identification letters around the periphery of the room. When everyone was settled, an experimenter read standardized instructions.[7] From the outset these emphasized that there was no deception in the study and that it was very important that everybody understood what was going on. The subjects were told to ask at any time about anything that was not clear to them, that their questions would be answered in full, and that the experiment would not go on until they were confident that they understood what was going on. The subjects were also told that their decisions would be strictly anonymous and that no other subject would ever know what they chose.

Study 1 consisted of forty-eight groups of six subjects (total $n = 288$).[8] A simple prisoners' dilemma matrix was explained, with particular attention drawn to the dominance of defection over co-operation.[9] The payoffs were unambiguously a

[7] A complete copy of the relevant instructions and decision forms for both studies are available upon request.

[8] The findings reported here were collected as part of a larger study on the effects of feedback on choice. Given the possibility of this feedback influencing the substance of subsequent decisions, only the decisions prior to feedback, or knowledge that feedback would occur, are reported here.

[9] At no time were the words 'co-operation' or 'defection' used. Instead, a co-operate choice was referred to as 'choosing *X*'; a defect choice was referred to as 'choosing *Y*'. It should also be noted that much of the discussion in the introduction referred to social dilemmas and the experiments used two-person prisoners' dilemmas; *n*-person social dilemmas and two-person prisoners' dilemmas are, obviously, structurally different. However, they have been shown to be formally equivalent granted they are uniform. For a discussion and proof of the equivalence of two and *n*-person social dilemmas, see Robyn Dawes, 'Formal Models of Dilemmas in Social Decision-Making', in Martin F. Kaplan and Steven Schwartz, eds, *Human Judgment and Decision Processes* (New York: Academic Press, 1975), pp. 88–231.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

	Co-operate	Defect
Co-operate	(–2,–2)	(–6,5)
Defect	(5,–6)	(–3,–3)

Fig. 1. Matrix of payoffs in dollars: Study 1

prisoners' dilemma and were the same for each decision. Each of the matrices had the formal structure of $t > c > d > s$; where, by convention, t = the payoff to a lone defector; c = the payoff to mutual co-operation; d = the payoff to mutual defection; and s = the payoff to lone co-operation – the sucker's payoff. See Figure 1 for the matrix of payoffs.

Some argue that co-operation in dilemma interactions results from subjects failing to understand the nature of their choices.[10] In order to minimize the chances that subjects did not understand the instructions, they completed a quiz. Answers were checked and any necessary explanations repeated until the experimenter was satisfied that everyone understood what was going on.[11] In spite of this, in order to avoid any possible contamination of the results by responses from people who did not understand the dilemma choices, all data from subjects who did not receive a perfect quiz score were dropped from the analysis.[12]

Each subject played prisoners' dilemma games, one at a time, with each other subject in the room. All subjects had to make their choices in a single game before anyone moved on to the next. Subjects knew only that they would be making one choice with respect to each of the other persons in the room but did not know the order of the play (their own or others') or who was making a decision with respect to whom on any given play. It would have been impossible for every pair of people to interact with each other simultaneously. By the end of the entire sequence, however, each subject had made a decision with respect to each other subject in the room.[13]

Along with their own co-operate/defect choices, the subjects were asked to indicate their expectation of what they thought the other person would do. Subjects recorded their co-operate/defect decisions, as well as their expectations, on forms affixed to clipboards. They were told to use the clipboards to shield their choices from the others. They were also told that when the experiment was completed they would be excused one at a time to a 'payoff room' where they would be given what they had earned and dismissed. After one subject had left the area, the next would be excused.

Finally, to assess their subjective evaluation across the four possible outcomes in the

[10] See a review of such critiques in John Ledyard, 'Public Goods: A Survey of Experimental Research', in J. Kagel and Alvin Roth, eds, *The Handbook of Experimental Economics* (Princeton, NJ: Princeton University Press, 1995), pp. 111–94.

[11] This was done without identifying which subject(s) had answered incorrectly. Care was taken not to use any descriptions of the game not included in the original instructions. Most of the clarification involved repeating the written instructions while physically pointing out, on an enlarged replication of the decision matrix, the consequences of combinations of choices. Effort was made not to use any different language to describe the game than had been used in the standardized instructions.

[12] All analyses reported were also carried out with data including those that demonstrated misunderstandings of the game. The coefficient values are available upon request but the pattern of results is equivalent.

[13] A description of the sequence of the pairing used is available from the author upon request.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

dilemmas, subjects were asked to complete a questionnaire after they had made their dilemma choices, but before they were told the amount of money they had gained or lost. Subjects were asked to rank, for each other individual they had played with, the desirability of the four possible outcomes. Specifically, they were asked, 'if you, unilaterally, could choose the outcome for you and the other person, what would be your first choice, your second choice … etc.' From the ordinal rankings of the four possible outcomes, composite 'rank types' were assigned for each subject based on the twenty-four possible combinations.[14]

Study 2

For the purposes of the work reported here, Study 1 and Study 2 differed only in the following ways. Study 2 consisted of sixteen groups of seven (as opposed to six) subjects (total $n = 112$). In addition, all decisions in Study 1 involved a single prisoners' dilemma matrix. Subjects in Study 2 made a dilemma choice with each of six other people on three different matrices. Each subject made two choices on each of the three matrixes. The individual monetary payoffs, though different than the matrix used in Study 1 in absolute terms, all followed the same formal dilemma structure ($t > c > d > s$). See Figure 2 for the monetary payoffs, by matrix type, for each decision on each round of Study 2.

	Matrix 1		Matrix 2		Matrix 3	
	Co-operate	Defect	Co-operate	Defect	Co-operate	Defect
Co-operate	(–2,–2)	(–7,5)	(–1,–1)	(–4,4)	(–1,–1)	(–2,2)
Defect	(5,–7)	(–4,–4)	(4,–4)	(–1,–1)	(2,–2)	(–1,–1)

Fig. 2. Matrices of dollar payoffs: Study 2

Study 2 also measured subjects' subjective evaluations of the dilemma outcomes using a different method. Instead of recording only the ordinal ranking of the outcomes, Study 2's outcome evaluation questionnaire was designed to allow the subject to indicate how much more they preferred one outcome over another in the particular case. Specifically, the subjects were asked to put a letter representing each outcome on an eleven-point scale, from most to least preferred.[15] When reporting cross-study comparisons with Study 1, this response form has been used to construct a simple ordinal rankings.

After the subjects had finished reporting their outcome aspirations in Study 2, they were asked a series of attitudinal questions. Two of these questions are relevant here. First, subjects were asked to indicate their preferences in a hypothetical dilemma played

[14] It is, of course, possible that the subjects' reported preferences over the outcomes were contaminated by having the subjects record them after their choices. Some may have simply recorded preferences consistent with their choices. If anything, however, this should bias the results towards the SEU model. As the findings section will indicate, confirmation does not occur. Alternatively, self-maximizers may have been unwilling to report themselves as such. This also should bias the findings towards the SEU model. If true self-maximizers report themselves as would-be co-operators then their co-operation can be explained by the SEU model. However, those people who are self-maximizers *and* are willing to report it, should be the very people *least* likely to be affected by expectations of co-operation. Finally, asking subjects to rank the outcomes before they acted would have chanced a contamination of their actual dilemma behaviour. Given the choice between risk of contamination of behaviour or of reported preferences, I chose the latter.

[15] The eleven-point scale corresponded to the response form used in the recording of expectations (also an eleven-point scale) and allowed for each point to represent a dollar difference (if the subjects were so inclined).

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

with people with a known history of co-operation and defection. Specifically, the questionnaire read,

Let us say that you are playing the game with some other person who is not in the experiment today. If you know that person has chosen *Y* about 80% of the time in the past, and has chosen *X* about 20% of the time, what would your preferences be?

The next question was identically worded, except that the person had chosen *X* 80 per cent and *Y* 20 per cent of the time.[16]

For the purposes of the research reported here, with the exception of the differences just reviewed, Study 1 and Study 2 were conducted in the same manner. Because of this, the findings from each can be treated as an opportunity to test the strength of the findings from the other. To anticipate our results, it may be useful to state here that the major findings replicate.

FINDINGS

Overall Co-operation Levels

There is a substantial incidence of co-operative behaviour, even without formal incentive compatibility, in both studies. Table 1 reports the proportion of co-operative choices, for each replication in both studies. The table also reports the overall proportion of co-operative choices across all replications. Subjects co-operated 30–50 per cent of the time.

Rank Types

A large proportion of subjects subjectively transformed the dilemmas they confronted into other kinds of interactions. For each subject in both studies, a composite 'rank type' was constructed from their ordinal ranking of each of the outcomes in a prisoners' dilemma. The most common rank type in all but one instance (see Play 3 in Study 1) corresponds to self-maximizers ($t > c > d > s$). However, not far behind in frequency

TABLE 1 *Proportion of Co-operative Choices*

	Study 1	Study 2
Play 1	0.500 (106/212)	0.424 (39/92)
Play 2	0.439 (93/212)	0.348 (32/92)
Play 3	0.453 (96/212)	0.326 (30/92)
Play 4	0.462 (98/212)	0.261 (24/92)
Play 5	0.373 (79/212)	0.402 (37/92)
Play 6	–	0.380 (35/92)
Total	0.445 (472/1,060)	0.357 (197/552)

Note: In Study 2, plays 1 and 2 are on matrix 1, plays 3 and 4 are on matrix 2, and plays 5 and 6 are on matrix 3.

[16] To avoid any possible order effects, the order of these questions was reversed in half of the questionnaires.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

are 'would-be co-operators' who ordered outcomes $c > t > d > s$. These subjects prefer mutual co-operation to free-riding, but would rather free-ride than lose money. Self-maximizers and would-be co-operators together account for 60.2 per cent and 77.2 per cent of all rank types in Study 1 and 2, respectively.

The next two most common rank types are 'equalizers' and 'group-maximizers'. Equalisers rank the outcomes $c > d > t$ or s. This is consistent with a preference for equal monetary outcomes – even equal losses – over all other outcomes. Group-maximizers rank the outcomes $c > t$ or $s > d$. This is consistent with a preference for maximizing the group's (in this case the dyad's) payoff. While equalizers and group-maximizers are of theoretical interest, the number of subjects ranking the outcomes in this order was so relatively small that statistically meaningful analysis of their dilemma choices was impossible. Therefore, all further analysis will examine only the choices of self-maximizers and would-be co-operators.[17]

Table 2 reports the proportion of self-maximizers and would-be co-operators across Study 1 and Study 2.

Co-operate vs. Defect Choices

The choice of co-operative vs. defect for would-be co-operators was affected by their expectations of defection in the manner predicted by the relaxed SEU model. As expectations of defection rise, the proportion of would-be co-operators defecting also rises. In contrast, self-maximizers have reported preferences that, for them, create a dominant incentive to defect and thus their expectations of others' behaviour should not make any difference to their own choices. This prediction is not supported. In both studies, would-be co-operators' *and* self-maximizers' expectations of others' defection are found to significantly affect their own co-operate vs. defect choices.

A logistic regression analysis was performed to examine the determinants of choice. Subjects' co-operate vs. defect responses are the dependent variable. Three independent variables are: (1) whether people are would-be co-operators or self-maximizers; (2) subjects' expectations of the other's defection; and (3) the interactive term between the first two independent variables. Tables 3 and 4 report the results of the analysis for Studies 1 and 2, respectively. To aid in the interpretation of the results, the log odds of defection are transformed into probabilities of defecting for each expectation level, and both rank types. Figure 3 reports the results of these transformations graphed to form predicted logarithmic lines modelling the relationship between a subject's expectations of defection and the probability of their own defection, for each rank type.

For subjects of both rank types, the higher the expectation of defection, the higher the probability of defecting. In Study 2, self-maximizers were significantly (at $\alpha = 0.05$) more likely to defect at any given level of expectation of defection. This difference, while apparent in Study 1, is not significant (at $\alpha = 0.05$). Finally, the lack of significance (at $\alpha = 0.05$) of the interactive term in both studies allows us to conclude that it is unlikely that there exists a difference in how each rank type's co-operate vs. defect choices are affected by any given level of expectation of the other's defection.[18] The relaxed SEU

[17] A complete table of all reported rank types is available upon request.

[18] Table 1 suggests that the order of play may have had an effect on defection rates. This is marginally the case in Study 1 ($\beta = 0.11$, $p = 0.08$) and is not the case in Study 2 ($\beta = 0.01$, $p = 0.93$). The inclusion of the order of play made no significant impacts on the β's of the other factors in either study.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

TABLE 2 *Proportions of Rank Types*

	Study 1		Study 2	
	Self-maximizers	Would-be co-operators	Self-maximizers	Would-be co-operators
Play 1	0.307 (65/212)	0.283 (60/212)	0.544 (50/92)	0.207 (19/92)
Play 2	0.311 (66/212)	0.288 (61/212)	0.490 (45/92)	0.261 (24/92)
Play 3	0.300 (63/212)	0.302 (64/212)	0.544 (50/92)	0.250 (23/92)
Play 4	0.311 (66/212)	0.292 (62/212)	0.511 (47/92)	0.294 (27/92)
Play 5	0.302 (64/212)	0.292 (62/212)	0.544 (50/92)	0.228 (21/92)
Play 6	–	–	0.500 (46/92)	0.261 (24/92)
Overall proportion	0.310 (324/1,060)	0.292 (309/1,060)	0.522 (288/552)	0.250 (138/552)

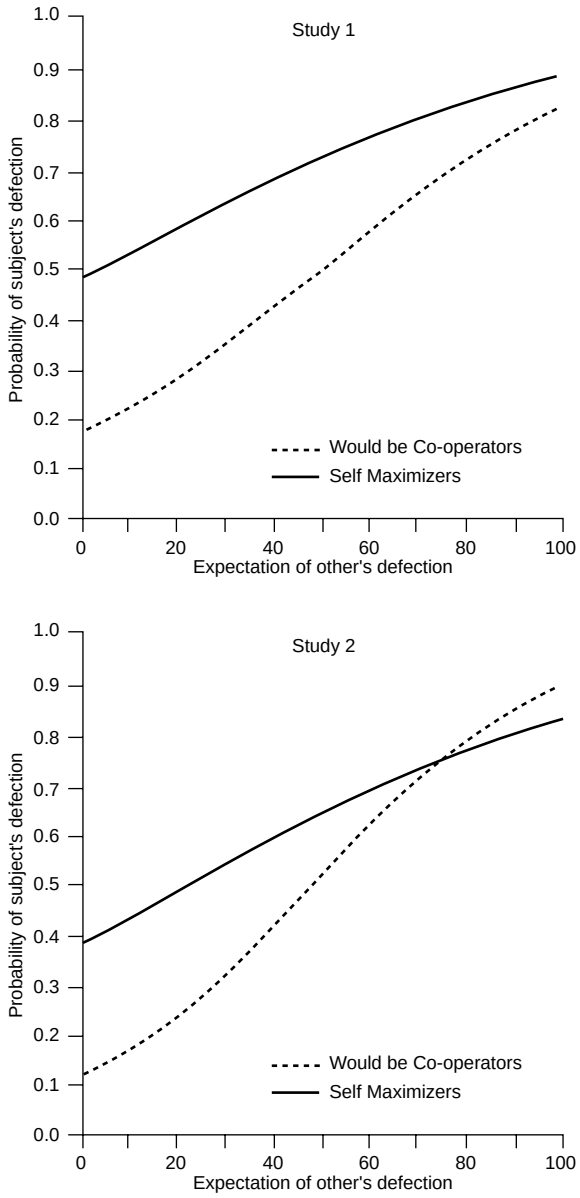


Fig. 3. Graphic representation of regression results

model predicts expectations to be insignificant when the interaction term is included. That is, expectation should affect choices iff the subjects' preferences do not create a dominant strategy. Yet, *both would-be co-operators and self-maximizers reacted to expectations of co-operation by becoming more likely to co-operate*.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

TABLE 3 *Logistic Regression Results: Study 1*

$$\textit{Probability (Defection)} = \frac{1}{1 + e^{-z}}$$

Where:

e = the natural logarithmic base = 2.7214

$z = \alpha + \beta_1 X_1 + \beta_2 X_2 + \beta_3 X_1 X_2$

Where:

X_1 = Would-be co-operator (1) or Self-maximizer (0)

X_2 = Expectation of defection (0, 10, 20, … 100)

	Play 1	Play 2	Play 3	Play 4	Play 5	Overall
Rank type	$\beta_1 = -0.98$ (0.25)	$\beta_1 = -2.09$ (0.03)	$\beta_1 = -0.36$ (0.71)	$\beta_1 = -2.59$ (0.01)	$\beta_1 = -1.19$ (0.27)	$\beta_1 = -1.48$ (<0.01)
Expectation of defection	$\beta_2 = 0.02$ (0.03)	$\beta_2 = 0.02$ (0.09)	$\beta_2 = 0.03$ (0.01)	$\beta_2 = 0.01$ (0.23)	$\beta_2 = 0.04$ (0.02)	$\beta_2 = 0.02$ (<0.01)
Interaction (rank type * Exp. of defection)	$\beta_3 = 0.003$ (0.83)	$\beta_3 = 0.02$ (0.26)	$\beta_3 = 0.007$ (0.69)	$\beta_3 = 0.04$ (0.07)	$\beta_3 = 0.001$ (0.96)	$\beta_3 = 0.01$ (0.15)

Note: The values within parentheses indicate the significance level of the coefficients using a two-tailed Wald statistic for 'Rank Type' and one-tailed for 'Expectation of Defection'. The overall regression's constant is -0.08.

TABLE 4 *Logistic Regression Results: Study 2*

$$Probability\ (Defection) = \frac{1}{1 + e^{-z}}$$

Where:

e = the natural logarithmic base = 2.7214

$z = \alpha + \beta_1 X_1 + \beta_2 X_2 + \beta_3 X_1 X_2$

Where:

X_1 = Would-be co-operator (1) or Self-maximizer (0)

X_2 = Expectation of defection (0, 10, 20, … 100)

	Matrix 1	Matrix 2	Matrix 3	Overall
Rank Type	$\beta_1 = -1.28$ (0.22)	$\beta_1 = -1.78$ (0.08)	$\beta_1 = -2.47$ (0.03)	$\beta_1 = -1.69$ (<0.01)
Expectation of Defection	$\beta_2 = 0.03$ (<0.01)	$\beta_2 = 0.04$ (<0.01)	$\beta_2 = 0.03$ (0.01)	$\beta_2 = 0.03$ (<0.01)
Interaction (Rank Type* Exp. of Defection)	$\beta_3 = 0.02$ (0.47)	$\beta_3 = 0.02$ (0.35)	$\beta_3 = 0.03$ (0.17)	$\beta_3 = 0.02$ (0.10)

Note: The values within parentheses indicate the significance level of the coefficients using a two-tailed Wald statistic for 'Rank Type' and one-tailed for 'Expectation of Defection'. The regression's constant = -0.43.

Expectations of Defection and Rank Types

The SEU model holds expectations of the other subject's behaviour and the subject's own outcome aspirations as fully independent quantities. If this is the case, there is no *a priori* reason to believe that different outcome rank types should be systematically associated with expectations of others' behaviour. This is not supported by the data. A comparison of the differences in subjects' expectations of defection shows that in every play, across both studies, would-be co-operators perceive others' defection to be less likely than do self-maximizers. In seven out of eleven possible cases (five plays in Study 1, and six plays in Study 2), *t*-tests demonstrate that these differences are significant (at $\alpha = 0.05$). See Table 5 for a summary of these results.

In addition, there is a significant negative correlation ($r = -0.098$, $p < 0.01$) across all subjects and plays in Study 2, between expectations of the other's defection and the difference between subjects' reported value of mutual co-operation and free riding (a positive score indicates a preference for mutual co-operation over free riding).[19] *The more a subject believed the other was going to defect the less valuable the outcome of mutual co-operation versus free-riding.*

This finding is particularly striking if we look at the mean differences in preferences over these two outcomes across three collapsed categories of expectations. Subjects were asked to mark the midpoint where there was a 50 per cent chance of defection and a 50 per cent chance of co-operation if they did not have any idea what the subject would choose. This provides a logical dividing line between those who expect co-operation and those who expect defection. For subjects who expect co-operation the mean difference

[19] Recall that only ordinal rankings were collected in Study 1 and therefore this analysis cannot be carried out on that data.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

TABLE 5 *Comparison of the Proportion of Each Rank Type's Average Expectation of Defection*

	Play					
Study 1	1	2	3	4	5	Total
Self-maximizers	0.54	0.53	0.56	0.51	0.54	0.535
Would-be co-operators	0.45	0.43	0.46	0.44	0.48	0.453
t scores	2.60	3.08	3.12	2.09	1.85	5.66
	(0.01)	(< 0.01)	(< 0.01)	(0.04)	(0.07)	(< 0.01)

	Matrix 1			Matrix 2			Matrix 3			
	Play			Play			Play			Pooled across all plays
Study 2	1	2	Total	1	2	Total	1	2	Total	
Self-maximizers	0.52	0.53	0.52	0.57	0.57	0.57	0.51	0.57	0.54	0.55
Would-be co-operators	0.48	0.51	0.50	0.50	0.50	0.50	0.47	0.47	0.46	0.49
t scores	1.15	0.40	1.08	1.98	2.01	2.82	1.21	3.49	3.19	4.07
	(0.26)	(0.69)	(0.28)	(0.05)	(0.05)	(< 0.01)	(0.23)	(< 0.01)	(< 0.01)	(< 0.01)

Note: The numbers in parentheses refer to the *p* values of each *t* score.

is 1.12; for those who report co-operation and defection as equally likely, the mean difference drops to 0.493; and for those who expect defection the difference drops still further to 0.232.

Finally, recall that subjects were asked to rank the outcomes of hypothetical others with known co-operate vs. defect histories. If expectations are independent of outcome aspirations, then differences in past behaviour should not affect subjects' ranking of the possible outcomes. This expectation is not supported. When subjects ranked dilemma outcomes while supposing they were playing with someone who had co-operated 80 per cent of the time in the past, 26 per cent of them ranked the outcomes as would-be co-operators, and 47 per cent as self-maximizers – about the same proportion of each ranking as was found when the subjects ranked the outcomes with respect to each other. However, when they ranked the outcomes while supposing that they were playing with someone who had defected 80 per cent of the time in the past, only about 8 per cent of them ranked the outcomes as would-be co-operators.

Notably, for this circumstance about 12 per cent of the subjects ranked mutual defection and their own free riding as the two most desirable outcomes – apparently ranking any outcome that resulted in the other's loss as more desirable than any that resulted in the other's gain. Less than 1 per cent of subjects ranked the outcomes in this way when they supposed that the other had co-operated 80 per cent of the time in the past. If we assume that the information about past play creates expectations about future play, then knowledge of the other's past behaviour clearly affected the evaluation of outcomes. In short: *changes in the expectations about others' behaviour resulted in changes in preferences over outcomes*.

Summary

Overall, the predictions from the SEU model are not supported by the data in two important ways. First, expectations of others' likely defection affect co-operate vs. defect choices regardless of rank type. Even self-maximizers who expect others to co-operate are likely to co-operate themselves. Secondly, there is evidence that expectation of others' likely behaviour can affect outcome evaluations.

DISCUSSION AND SPECULATION

Why would self-maximizers give up their golden opportunity to achieve what they say is their most desired outcome by co-operating when they expect co-operation? More generally, why should others' expected behaviour have any effect at all on self-maximizer's co-operate vs. defect choices? The answer may be found by asking a slightly different question – namely, what does it mean to say that one expects another to co-operate in a dilemma interaction? One possibility is that expectations tell us something not only about what people think others are likely to do, but also about their beliefs regarding the others' motives.

Expectations and Motives

Imagine two players, Judge and Target, are in a dilemma interaction. If Judge is certain that Target will defect then Judge must be convinced that Target is either (1) trying to free-ride, or (2) aspires to mutual co-operation, but is certain of Judge's defection and will defect to avoid being exploited. Thus, even if certain that Target will defect, we can infer nothing about Judge's beliefs about Target's motives. Ironically, only when Judge

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

is *completely certain* of Target's defection are we *completely uncertain* about Judge's conception of Target's motives.

If Judge expects Target will defect with a specific probability $p(d)$, where $0 < p < 1$, then we can make two inferences about the Judge's perception of the Target's motives. First, Judge must believe that there is at least some $(1 - p)$ probability that Target aspires to mutual co-operation (assuming pure altruism is not considered). Judge's estimate could be higher – i.e., Judge could be certain that Target aspires to mutual co-operation, but also believe that Target thinks that the Judge will co-operate with a probability of $(1 - p)$ – but it could never be lower. Secondly, Judge must believe that the probability that Target aspires to free ride is less than p.

The lower Judge's expectation of Target's likely defection, the lower the likelihood of Judge believing that Target is motivated to free ride and the higher the likelihood of Judge believing Target is motivated by an aspiration for mutual co-operation. Motive attribution could lead to changes in behaviour by (1) causing feelings of obligation associated with trust and/or generalized norms of reciprocity to be primed, or (2) changing in the utility of possible outcomes.

Trust, Obligation and Reciprocity

If Judge expects Target to co-operate, then this also tells us something about Judge's perception of Target's belief about Judge's likely behaviour. First, Judge must believe that: (1) Target expects Judge to co-operate (if Target expected defection, s/he would presumably defect to avoid exploitations); (2) Target believes that Judge aspires to mutual co-operation (why else would Target expect Judge to co-operate?); and (3) Target is sufficiently certain to willingly put him/herself at risk by co-operating with Judge.[20] In other words, Judge believes that Target is *trusting* him/her to co-operate – Target is accepting a risk of loss, based on expectations that Judge will forgo temptation for narrow personal gain.

If Judge aspires to mutual co-operation and has this optimistic expectation of Target, then choice is simple; Judge should co-operate. Now assume that Judge aspires to free-ride. In this case Target's expectation of Judge's likely co-operation should present Judge with a golden opportunity to free-ride, and under these circumstances (and all others for that matter) Judge should always defect. But, as the data show, this is not what happens.

This may be caused by Target's trust imposing some sort of feelings of obligation, on Judge's part, to co-operate. This felt obligation would presumably increase with the magnitude of Judge's expectation of Target's likely co-operation – the more certain Judge is that Target will co-operate, the more Judge believes that Target trusts, and therefore the more likely Judge will feel obliged to co-operate. Attitudinal data from the two studies are consistent with this explanation.

The subjects in Study 2 were asked a series of attitudinal questions at the end of the experiment. One of these questions asked, 'How much do you think that someone trusting you to do something obliges you to do that thing?' The subjects responded by placing a mark on an eleven-point scale between 'A great deal' (1) and 'Not at all' (11).[21] The average score was 2.81 for would-be co-operators, and 3.57 for self-maximizers. Thus, even though self-maximizers agree with the statement significantly less than would-be co-operators ($t = 2.28$, $p = 0.024$), they *both* recorded relatively strong

[20] The obvious exception is if Target were perceived to be a pure altruist.

[21] An eleven-point scale was used to keep response mode consistency with the reporting of expectations.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

agreement with the idea that someone trusting you to do something obliges you to do that thing.[22] But why should people feel obliged? Surely people so disposed in one-shot anonymous games are at a disadvantage compared to others not so disposed. This may not be the case.

Robert Frank, in *Passions within Reason*, argues that negative affective responses to 'cheating' (the equivalent of free-riding in a dilemma) could serve a person's self-interests.[23] He writes:

> Suppose that cheating is difficult, but not impossible to detect. If Smith gets away with it, he receives an immediate payoff. If he gets caught, however, he not only receives no current payoff, but he also acquires a bad reputation. His rational assessment tells him it is not a golden opportunity. He knows that people who refrain from cheating in these circumstances will develop good reputations and do better in the long run. But the prospect of the immediate payoff is tempting to him nonetheless. If Smith is emotionally predisposed to regard cheating as an unpleasurable act in and of itself – that is, if he has a conscience – he will be better able to resist the temptation to cheat.[24]

In a similar fashion, feeling obliged not to break someone's trust could serve one's long-term interests.[25]

Note that in order for a feeling of obligation and/or applications of general rules of reciprocity to further one's long-term self-interest, they must not be *strategically* adopted. Indeed, a strategic decision about whether to feel obliged or to adhere to certain manners would be identical to a co-operate vs. defect decision without these mechanisms. The data suggest that indiscriminate application extends to single-shot anonymous interactions.

Contingent Evaluation

The process by which feelings of obligation to honour another's trust lead to co-operative choices remain to be specified *within* an SEU model. One possible process would be an *ad hoc*, revealed preferences explanation: the fact that people co-operate mean that they prefer co-operation over free-riding. This still leaves us with the puzzle of self-reported self-maximizers co-operating. If they preferred mutual co-operation over free-riding, why not report it?

The second possibility is that expectations, reflecting motive attributions, affect the evaluation of outcomes directly. Matthew Rabin has recently proposed a sophisticated and influential general model of choice that incorporates one's attributions of others' motives into the evaluation of outcomes.[26] In Rabin's words, 'the same people who are

[22] Rank types were determined by subjects' reported ranking of dilemma outcome when playing with 'people in general'.

[23] Robert Frank, *Passions within Reason: The Strategic Role of the Emotions* (New York: W. W. Norton, 1988).

[24] Frank, *Passions within Reason*, p. 81.

[25] Some scholars have argued that non-self-maximizing behaviour observed in ultimatum and dictator games is a manifestation of general etiquette-based rules of reciprocity. See, for example, Colin Camerer and Richard Thaler, 'Ultimatums, Dictators and Manners', *Journal of Economic Perspectives*, 9 (1995), 209–19. Other scholars have argued that co-operation based on expectations of others' co-operation in single-shot dilemma interactions is the result of a decision heuristic based on either a 'matching' or 'control' heuristic. While both are consistent with expectations/choice correlations, neither explicitly address the link between expectations and aspirations. For a discussion of these heuristics, see Morris, Sim and Girotto, 'Distinguishing Sources of Cooperation in the One-Shot Prisoner's Dilemma'; Eldar Shafir and Amos Tversky, 'Thinking Through Uncertainty: Nonconsequential Reasoning and Choice', *Cognitive Psychology*, 24 (1992), 449–74.

[26] Matthew Rabin, 'Incorporating Fairness into Game Theory and Economics', *American Economic Review*, 83 (1993), 1281–302.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

altruistic to other altruistic people are also motivated to hurt those who hurt them.'[27] However, while Rabin's way of incorporating reciprocal altruism into game theory allows for things other than a specific payoff to influence utility of outcomes, it does not use a player's subjective probability judgements of another's likely behaviour.

Furthermore, Rabin is committed to defining a fairness equilibrium whereby predictions are based on the weights of different degrees of perceived fairness. In contrast, the less specific *contingent evaluation model* (CEM) directly incorporates subjective expectations into one-shot dilemma choices. That is, it leaves open the question as to what features lead to a fairness equilibrium and instead focuses on expectations. The difference between the CEM's and the SEU's explanation of co-operation in dilemma interactions will highlight the difference in the models.

If the SEU of co-operation (SEU(C)) is greater than the SEU of defection (SEU(D)), then one should rationally co-operate in dilemma interactions. The calculations are straightforward:

$$\text{SEU(C)} = pu^{cc} + (1 - p)u^{cd} \tag{1}$$

$$\text{SEU(D)} = pu^{dc} + (1 - p)u^{dd} \tag{2}$$

where p = probability of other's co-operation; $(1 - p)$ = probability of other's defection; u^{cc}, u^{cd}, u^{dc} and u^{dd} = the individual's utility for mutual co-operation, being suckered, free-riding and mutual defection, respectively. More generally, u^{st} = the utility of the payoff to row of choosing strategy s and column strategy t. Importantly, regardless of the value of p, u^{st} is assumed to remain fixed.

The relaxed SEU model is consistent with the findings of some non-trivial level of co-operation and a correlation between co-operative choices and expectations of co-operation. The model cannot, however, account for the effects of expectations on choice *even on those that proclaim to be self-maximizers*.

It is, of course, possible to have a high expectation that the other will defect, and still aspire to mutual co-operation, meeting the other's defection with one's own *only* to avoid exploitation. Alternatively, expectations of the other's behaviour, linked to perceptions of the other's likely motives, may change preferences regarding the possible outcomes. The data are consistent with the latter.

Assume that the expectation of co-operation is positively correlated with the value of mutual co-operation and negatively with the value of free-riding. In this case, the contingent expected utility of co-operation, i.e., CEU(C), would follow the same basic form of the SEU(C). However, the utility for strategy pair s,t would be computed differently. Specifically, for row player:

$$u^{st} = V^{st} + f(\hat{t}, n^{st}) \tag{3}$$

where v^{st} = the utility of the monetary payoff of row choosing strategy s, and column choosing strategy t, and $f(\hat{t}, n^{st})$ = some additional utility that is a function of row's expectation of column playing t ($\hat{t}$) and some other utility (n^{st} – exogenously defined) associated with outcome (s,t).

In this framework it makes little sense to ask people to report preferences over outcomes in a game defined only by payoffs. Preferences will partially depend on what one expects others to do. Then why do people seem to have no problem reporting game preferences based only on a matrix? One possible explanation is that they report their preferences using the central tendency of some internal distribution of expectations of

[27] Rabin, 'Incorporating Fairness into Game Theory and Economics', p. 1281.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

others. For example, one could quite reasonably answer a question of overall evaluation by indicating a preference for free-riding if one generally believed that others would defect. But, on any given instance, this self-described self-maximizer may well co-operate, assuming that they have a high enough expectation of co-operation from the other. Thus, the CEM can account for self-maximizers co-operating when they have high expectations of other's co-operation.

The Possible Fitness Implications of Contingent Values

Frank's argument concerning usefulness of being emotionally predisposed to find the act of cheating itself unpleasurable helps explain the possible fitness benefits to people employing a CEM of choice.[28] In iterated play a predisposition against cheating serves one's (in Frank's example, Smith's) interests by helping her resist the temptation to do something that would damage her reputation and thus long-term interests. Implicit in this explanation is the assumption that the other is not cheating Smith.

If we drop that assumption, then an emotion making it less likely for Smith to cheat (thus protecting herself from exploitation) would be to Smith's disadvantage. Not only would it be disadvantageous in the current interaction, but by increasing the probability of Smith acquiring a reputation as a gullible and exploitable partner, it would also hurt her long-term welfare. Just as a reputation for trustworthiness attracts others in search of mutual co-operation, so a reputation for *naïveté* would draw those seeking to free-ride.

Given that Smith may be interacting with either would-be co-operators or self-maximizers, it would be in her long-term interests to aspire to mutual co-operation – with all the attendant emotions and felt obligation – iff Smith's partners were not going to cheat her when given the opportunity. Otherwise, if interacting with self-maximizers, she would be better served to co-operate only when it was in both their best self-interests, traditionally defined – in other words, if she acted as a self-maximizer.

Jane Mansbridge, making a similar argument, sees altruistic motivations for co-operation (what she calls 'love') surviving by being 'nested' in self-interest.[29] This does not mean that the behaviour need be consciously self-interested. Instead, she argues that altruistic motivations could survive if certain conditions exist to provide 'an ecological niche in which that altruism can nest'.[30] If other-regarding feelings are selectively activated only by those perceived to be other-regarding themselves – a pattern of activation consistent with the data – these conditions could be met.

CONCLUSION

This Note demonstrates that, even for those who reported preferences to create a dominant strategy, expectations of others' behaviour affect choice in single-shot, anonymous dilemma interactions. Two explanations are offered: (1) feelings of obligation to respond to trust by being trustworthy over-ride and are somehow separate from ordinary SEU calculations, or (2) the utility of outcomes is contingent on expectations of others' behaviour. The experiment reported here is not a test of either of these explanations. Clearly, further work is needed to disentangle the two possible mechanisms. However, it is clear from the results that expectations have a more complicated effect on choice in strategic interactions than is traditionally believed.

[28] Frank, *Passions within Reason*.
[29] Jane Mansbridge, 'Rational Choice Gains by Losing', *Political Psychology*, 16 (1995), 137–55.
[30] Mansbridge, 'Rational Choice Gains by Losing', p. 142.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

Validating Party Policy Placements

IAN BUDGE*

Textual analyses of party and government programmes open up exciting possibilities for the investigation of policy and operationalization of theory. This Note focuses on the validity of the resulting estimates, particularly of the massive policy time-series assembled by the Manifesto Research Group (MRG) of the European Consortium.[1] These are important not only for the policy measurements they provide for fifty post-war democracies, but also from the point of view of validating other codings of texts, especially those deriving from computerized analyses. No other validating standard is available for any but a handful of post-war elections – certainly none other that so unambiguously measures policy preferences as opposed to actual party behaviour and which itself has been so well established by extensive use.

Some doubts have been expressed about the extent to which the one-position saliency codes typically used by the MRG can really measure the kind of policy spaces assumed by classical theories of party competition and coalition formation.[2] The Note addresses this point by

(a) showing that they do (Figures 1–4 below).
(b) Examining 'saliency and valency' assumptions to see if they provide a reasonable theoretical underpinning for such spaces

The conclusion has to be that the MRG data provide a solid basis for measuring party policy and can therefore be used substantively, to provide reliable estimates for party policy in post-war democracies; and methodologically, as a general standard for validating other measures of party and government policy.

TEXTUAL ANALYSES OF PARTY ELECTION PROGRAMMES

Parties, preferences and policies are at the heart of rational choice theories of democracy.[3] Measuring policy positions and movements is crucial to the development

* Department of Government, University of Essex. This Note draws on work conducted under a British Social Science Research Council Grant No. R000222289.

[1] Ian Budge, David Robertson and D. J. Hearl, eds., *Ideology, Strategy and Party Change*, Cambridge: Cambridge University Press, 1987). Data-collection continues under the direction of H-D. Klingemann and Andrea Volkens of the Wissenschaftzentrum Berlin under the title of the Comparative Manifestos Project (CMP) and now covers fifty countries.

[2] Kenneth Janda, Robert Harmel, Christine Edens and Patricia Goff, 'Changes in Party Identity: Evidence from Party Manifestos', *Party Politics*, 1 (1995), 171–96: Robert Harmel, Kenneth Janda and A. Tan, 'Substance versus Packaging: An Empirical Analysis of Parties' Issue Profiles' (paper presented to the American Political Science Convention, Chicago, 1995); Michael Laver, 'Party Policy and Cabinet Portfolios in the Netherlands 1994; Results from an Expert Survey', *Acta Politica*, 35 (1995), 3–28; Michael Laver and John Garry, 'Estimating Policy Positions from Party Manifestos' (paper presented at the ECPR Joint Sessions of Workshops, Warwick 1998 and Mannheim 1999).

[3] As illustrated in all the classic works: Kenneth Arrow, *Social Choice and Individual Values* (New York: Wiley, 1951); Anthony Downs, *An Economic Theory of Democracy* (New York: Harper, 1957); Abram de Swaan, *Coalition Theories and Cabinet Formations* (Amsterdam: Elsevier, 1973); Robert A. Axelrod, *Conflict of Interest* (Chicago: Markhan, 1970); Bernard Grofman, 'A Dynamic Model of Protocoalition Formation in Ideological *N*-Space', *Behavioural Science*, 27 (1982), 22–90.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

of such theories. This accounts for the growing interest in estimating party policy positions.[4] Most recently, such estimates have taken the form of computerised word-counts of party manifestos – an exciting development since it opens up the prospect of rapid and easy analysis of policy pronouncements in general, not just of party ones. This will facilitate more extended studies of strategic interaction between collective and individual actors in all sorts of political contexts.

Studying texts has the advantage that they are statements of policy made at a particular place and time by a specific person or organization. They therefore avoid the problems associated with expert judgements of party positions, which confuse preferences with the actual behaviour they are designed to explain, and are ambiguous about the time period involved, the criteria used to locate parties and what exactly they are locating.[5] The same might be said of electoral perceptions of where parties stand on policy.[6]

Given that non-textual measures have their limitations, the question arises of how text-based estimates – more acceptable in principle – are to be validated in detail? Word-counts can be made on very different bases and thus differ considerably in their results and the 'maps' they draw of policy movements by the same parties.[7] This creates the need for a generally acceptable, known standard against which counting procedures can be checked and if possible harmonized, before they are applied to previously unanalysed material. Only in this way can we accept these extended applications as trustworthy.

Party election programmes provide the best material on which to run such checks. This is partly because, as public documents issued for a mass audience, they are written in a very straightforward way. They aim at getting certain points over, clearly and simply. Repetition is their hallmark; making policy points involves highlighting them, repeating them in slightly varied form and coming back to them in a variety of contexts. Word counts should, therefore, work better here than on more complex arguments.

Election programmes also have a special standing as the only collective policy statement that parties as such ever make. No other source represents the combined views of the party as an organization. Constitutional provisions usually specify a series of formal processes through which the programme is composed and approved – preparation by the leadership, discussion at various levels of the organization and endorsement by a representative gathering of the party.[8] If one wants to study party policy, and not the policies advocated by internal factions or individuals inside the party, one has to study the manifesto, platform or election programme.

The main methodological advantage of such programmes is that they have *already* been comprehensively studied, coded and analysed in a statistical form. Their sentences[9]

[4] Frank Castles and Peter Mair, 'Left–Right Political Scales: Some Expert Judgements', *European Journal of Political Research*, 12 (1984), 83–8; Budge, Robertson and Hearl, eds, *Ideology, Strategy and Party Change*; Michael Laver and Ben Hunt, *Policy and Party Competition* (London: Routledge, 1992); John Huber and Ronald Inglehart, 'Expert Interpretations of Party Space and Party Locations in 42 Societies', *Party Politics*, 1 (1995), 73–111.

[5] Huber and Inglehart, 'Expert Interpretations'; Ian Budge, 'Expert Judgements of Party Policy: Uses and Limitations', *European Journal of Political Research*, 37 (2000), 103–13.

[6] Matthew Gabel and John Huber, 'Putting Parties in Their Place: Inferring Party Left–Right Ideological Positions from Party Manifestos' Data', *American Journal of Political Science*, 44 (2000), 94–103.

[7] Judith Bara, 'Comparing Computerized and Manual Maps of British and American Parties' Left–Right Movement' (unpublished paper, Department of Government, University of Essex, 1999).

[8] Budge, Robertson and Hearl, eds, *Ideology, Strategy and Party Change*, p. 18 and *passim*.

[9] Strictly, quasi-sentences, as units of text between semi-colons, colons and commas have been coded separately when they occur as strings within long 'periods' – a device that is commoner in some languages than others.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

have been counted manually into one or other of fifty-seven inductively-derived policy categories by a large team of scholars and assistants (the Manifesto Research Group) operating over the last twenty years (1979–99, and continuing).[10] Resulting datasets cover 634 political parties in fifty countries, from the first democratic election in the post-war period onwards.[11]

Clearly the MRG dataset is the only one which currently allows for comprehensive study of party and governmental policy positions. As textual analyses multiply, however, its main use may become that of validating other approaches. At the moment these are mostly computerized word counts (with some phrases thrown in).[12] While the unit of analysis of such counts (the word or phrase) differs from the (quasi) sentences of the MRG data, the basic idea of measuring the relative emphasis of the text on various policy areas is the same. If the counts are valid, therefore, they ought to produce broadly the same results – in particular similar spatial representations – as the earlier codings.[13]

The MRG data thus seem well placed to aid the development of policy-based measures and theories over the next decade. They have been challenged, however, for generally failing to assess whether a particular reference is positively or negatively viewed by a manifesto.[14] This criticism is directed at the basic MRG procedure of counting sentences into one-position policy categories, rather than into 'pro' and 'con' categories inside each issue area. Without explicit provision for party confrontation on different positions within each issue, the criticism runs, the coding simply measures the relative emphasis that parties put on the issue. Hence the MRG codes measure emphases on, or saliency of, policies rather than policy positions as such. They are consequently incapable of capturing the concerns of theorists such as Downs, in an *Economic Theory of Democracy*, or Axelrod, in *Conflict of Interest* with party policy positions and the direction of policy movements.

A first 'question arising' here is whether MRG-derived spaces do or do not look like the classical ones, and whether like them, they are capable of registering party positions and movements along specified dimensions. The following section juxtaposes the two with a view to seeing if any significant differences emerge between them.

We then investigate whether or not the policies which enter into party competition are actually 'valence' issues as registered by most of the MRG codes – where only one

[10] Originated by the Manifesto Research Group of the ECPR under the direction of Ian Budge and David Robertson, the collection has been taken over by the Wissenschaftzentrum Berlin from 1990. The WZB has devised a series of internal checks and controls along with instantaneous electronic contact between coder and supervisor, to ensure the reliability of codings. These are described in Andrea Volkens, 'The MRG since 1989, with Particular Reference to Quality Control' (paper presented to ECPR Joint Sessions, Mannheim, 1999). Datasets are publicly available from the Zentralarkiv Cologne and the British National Data Archive at the University of Essex. A book that will report all estimates of party, government and elector positions derived from the MRG data is under preparation by Ian Budge, Hans-Dieter Klingemann, Andrea Volkens, Judith Bara and Eric Tanenbaum, *Mapping Policy Preferences* (Oxford: Oxford University Press, 2001).

[11] Britain and the United States are also covered under a slightly different coding scheme from 1922 onwards. Government policy documents for ten countries are coded under the same scheme for 1945–83.

[12] More sophisticated computer approaches are possible using the MRG codes as 'seed text' for replicating their results on party election programmes and then broadening out (Leonard Ray, 'A Natural Sentences Approach to the Computer Coding of Party Manifestos' (paper presented to the ECPR Joint Sessions, Mannheim, 1999)). This involves the MRG set even more centrally in the development of computerized textual analyses and renders the question of its validity even more important for developments in this field.

[13] Comparing spatial representations, as in Figures 2–4 below, provides the best general estimates of validity, because they draw on the results of codings as a whole rather than simply comparing detailed coding decisions within the general framework.

[14] For citations see fn. 2 above.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

position can be adopted by parties without committing electoral suicide. That this is indeed the case has been powerfully argued by Stokes[15] and even more generally by Riker,[16] who sees it as extending to political rhetoric as a whole. If the only possibility for competing parties is to vary the emphasis they put on a shared policy position, then clearly to study party differences one needs to examine their strategic variations of emphasis on that position.

There are in fact good theoretical and empirical grounds for adopting the 'valence' assumption and its 'saliency' implications, so far as party competition is concerned. This should be a relief not just for users of the MRG data but for analysts using computerized word counts as well. Operationally, words like 'taxes' and 'welfare' have been found to designate only one policy position in the manifestos – 'cutting taxes' and 'extending welfare' respectively.[17] Counting the frequency of such words by computer is in fact to measure relative emphases on one policy position as the MRG has done with most of its manual codings. Such findings will be discussed in more detail in the section after next.

MEASURING POLICY SPACES

First, however, we want to see if the MRG data do produce good representations of the policy spaces envisaged by the classical theories. Such 'mappings' draw on all parts of the dataset, and so they constitute a more general check of its validity than comparisons of specific coding-decisions. As a standard of comparison for the MRG representations we have the advantage of having the actual 'pictures' of party policy spaces devised on theoretical grounds by analysts such as Downs and de Swaan.

Figure 1 presents Downs's well-known representation of two-party competition with

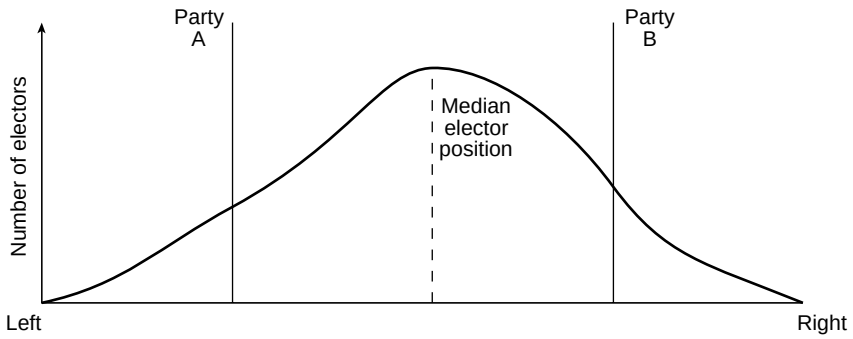


Fig. 1. Party policy space as envisaged theoretically by Downs

Note: The representation comes from Downs, *An Economic Theory of Democracy*, p. 118 (labelling added). The policy dimension is labelled 'Left–Right' in accordance with Downs (p. 132).

[15] Donald E. Stokes, 'Spatial Models of Party Competition', chap. 9 of Angus Campbell, Philip Converse, Warren E. Miller and Donald E. Stokes, *Elections and the Public Order* (New York: Wiley, 1966).

[16] William Riker, *Agenda Formation* (Ann Arbor: University of Michigan, 1993), pp. 81–126.

[17] A finding made many times in the MRG research publications, such as *Ideology, Strategy and Party Change*, pp. 50–1, and most recently confirmed in an independent word analysis of recent British and Irish manifestos undertaken by Laver and Garry in 'Estimating Policy Positions'.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

a unimodal distribution of electors along a policy continuum, and two parties placed on each side of the central mode. Downs originally presented the policy continuum as a distribution of preferences for greater or lesser government intervention, but later describes it as Left–Right in a more general sense.[18] The crucial point about a unimodal distribution of electoral preferences, in his view, was that vote-seeking parties would converge on the position of the middle or median voter, at the mode of the distribution, in order to get a majority of votes.

Whether or not this expectation is upheld in practice has been much disputed over the last forty years. There is no doubt, however, but that the picture of parties and electors presented in Figure 1 has dominated discussions of the question ever since.

The ability to reproduce this representation convincingly on the basis of actual data is thus a fairly crucial test for any measure of party policy. Figure 2 presents the picture given by the MRG data of the policy positions of the US parties in the presidential election of 1968 (Humphrey versus Nixon).[19]

The Left–Right scale produced on the basis of the data runs from a theoretical −100 (pure Left) to +100 (pure Right). How it is constructed is explained below (see Table 1).

For the moment, however, we should note that the estimate places the two parties very close together, near the position of the median voter. The median voter position is not based on the MRG platform codings which pertain to *party* positions. Rather, it is superimposed on the party space from an *independent* measure of 'public opinion liberalism' in the United States.[20] It is inserted here to bring the picture into full

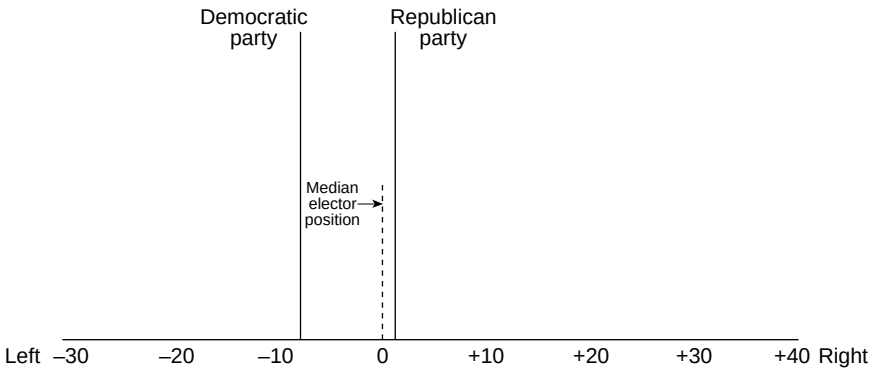


Fig. 2. Party policy space as measured by the MRG coding of party platforms for the US presidential election, 1968

Note: Party positions are estimated as described in the text. The median elector position has been superimposed on the MRG Left–Right scale on the basis of the 1968 score for 'public opinion liberalism' in Stimson, McKuen and Erikson, 'Dynamic Representation', p. 557.

[18] Downs, *An Economic Theory of Democracy*, pp. 118, 132.

[19] The United States is given as an example because two-party competition takes on a particularly pure form there and thus corresponds most closely of all countries to Downs's assumptions in his two-party model. The 1968 election is the more competitive of the two at the middle of our time-series and thus seems a reasonably 'typical' election example. The full time-series from 1956 to 1988 appears in Figure 3.

[20] James Stimson, Michael B. McKuen and Robert S. Erikson, 'Dynamic Representation', *American Political Science Review*, 89 (1995), 543–65, p. 557.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

conformity with the kind of space envisaged by Downs, to show that party positions can be related to electoral ones, within the Left–Right space created from MRG codings.

MRG codings also have the advantage of being able to represent party policy change over a series of elections as well as party positions in one election. Figure 3 shows how the MRG measures trace the movements of the American parties over the series of post-war American elections from 1956 to 1988 (when the public opinion series terminates). Public opinion change is superimposed in this figure, as in Figure 2, to show the usefulness of the representation for tracing party strategic movement relative to electoral positions, as envisaged by Downsian and other theories of party competition.[21]

The usefulness of Figure 3 lies in being able to check out dynamic theories of party movement, including those of Downs but also many alternative and modified Downsian formulations. The extent to which many analysts and theorists have found the representation plausible and workable helps of course to strengthen its face validity.

Figure 3 also lends itself to validation in a more direct way – against the historical record. If the Goldwater presidential candidacy of 1964 did not see a swing to the right on the part of the Republican party, followed by strong rightward tendencies under Reagan (1980, 1984), then we would suspect its plausibility. Similarly if the McGovern campaign of 1972 did not coincide with a strong leftwards shift by the Democratic party, we would also be suspicious. But they do. The paths of the parties mapped out in Figure 3 broadly correspond to the impressions of contemporary commentators and subsequent historians. Indeed, it is difficult to see how the representation could be much improved

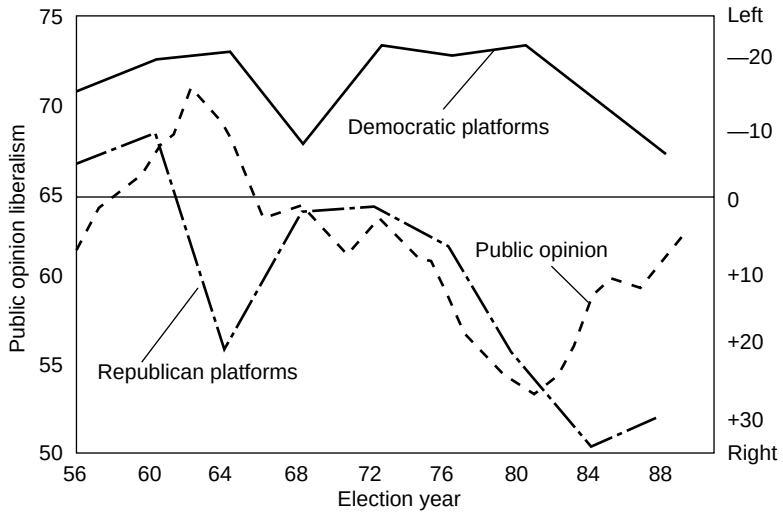


Fig. 3. Party policy space as measured by the MRG coding of party platforms for post-war presidential elections, 1956–88, showing positioning and direction of movement in relation to public opinion liberalism

[21] David Robertson, *A Theory of Party Competition* (London: Wiley, 1976); David Baron, 'A Spatial Bargaining Theory of Government Formation in Parliamentary Systems', *American Political Science Review*, 85 (1991), 137–64; Ian Budge, 'A New Spatial Theory of Party Competition', *British Journal of Political Science*, 24 (1994), 443–67; James Adams, 'A Theory of Spatial Competition with Biased Voters: Party Policies Viewed Temporally and Comparatively', pp. 121–58 of this issue of the *Journal*.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

upon to fit the historical record. Here is empirical evidence for the validity of the codings to add to their theoretical plausibility.

The Left–Right mappings for American parties can be reproduced for each of the fifty post-war party systems to which the MRG codings have been applied. In all cases they seem capable of operationalizing Downsian-type spaces for party competition. And in each they provide a plausible correspondence with the historical record.[22] The MRG policy data have thus been applied comprehensively to a whole series of actual cases. Critics of the codings need to confront these successful applications rather than ignore them in discussing the applicability of the estimates.

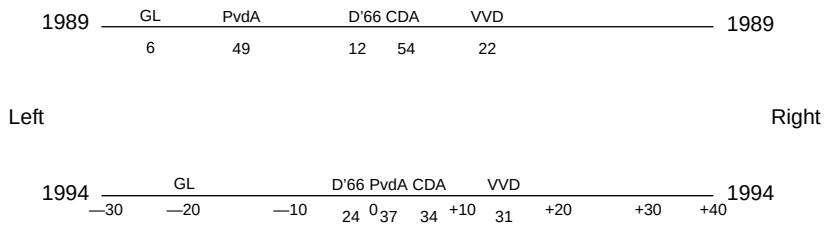


Fig. 4. Party policy space as measured by the MRG coding of Dutch party election programmes, 1989 and 1994

Note: Party positions are measured as described in the text in the same way as US parties. Figures under each party initial are the number of seats held in the legislature. Parties are: GL, Left-Ecological; PvdA, Labour; D'66, Progressive Liberal; CDA, Christian Democratic Appeal; VVD, Market Liberal.

The other classical policy spaces within which rational choice theories of parties have been set are those related to coalition formation. Sometimes these have been multidimensional[23] – again these have been convincingly operationalised by the MRG data.[24] More usually, however, such spaces have been conceived as one-dimensional Left–Right continua similar to the space of the party competition pictured above.[25] The plausibility of the MRG data in the coalition context is illustrated by Figure 4, which shows changes in Dutch party positions between 1989 and 1994. This is of interest in the light of the critical government formation of 1994, when the secular Labour and Liberal parties succeeded in ousting the Christians from office for the first time in seventy-five years – an earthquake in Dutch politics. The actual coalition change is prefigured by the shift of the Labour party to a central policy position, from which it could more easily form an alliance both with the Progressive Liberals (D'66) and the free-market Liberals (VVD).

Again, a policy space emerges from the MRG codings which fits both theoretical concerns and historical experience. Similar spaces can be created on this basis for all the coalition systems over time, and used both to operationalize and test theories with highly credible results.

[22] Hans-Dieter Klingemann, R. I. Hofferbert, Ian Budge *et al.*, *Parties, Policies and Democracy* (Boulder, Colo.: Westview, 1994); Laver and Budge, eds, *Party Policy and Government Coalitions, passim*.

[23] Grofman, 'A Dynamic Model'; Norman Schofield. 'Political Competition and Multiparty Coalition Governments', *European Journal of Political Research*, 23 (1993), 1–35.

[24] Laver and Budge, eds, *Party Policy and Government Coalitions, passim.*

[25] As by de Swaan, *Coalition Theories*; Axelrod, *Conflict of Interest*.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

TABLE 1 *Creation of an Additive Left–Right Scale from Codings of Manifesto Sentences*

Right emphases: Sum of %s for		Left emphases: Sum of %s for
Military: positive		Decolonization
Freedom, human rights		Military: negative
Constitutionalism: positive		Peace
Effective authority		Internationalism: positive
Free enterprise		Democracy
Economic incentives		Regulate capitalism
Protectionism: negative	minus	Economic planning
Economic orthodoxy		Protectionism: positive
Social services limitation		Controlled economy
National way of life: positive		Nationalization
Traditional morality: positive		Social services: expansion
Law and order		Education: expansion
Social harmony		Labour groups: positive

Other uses of MRG data – relating party to social change[26] or tracking and anticipating policy outcomes[27] – have also fitted with theoretical and historical explanations, strengthening the validity of the data. The results are sufficiently well known, however, not to over-stress the point here. In all their applications the MRG data have produced empirically plausible and theoretically consistent results. This puts the onus on critics to prove any case to the contrary with comparable evidence.

MRG CODINGS AND PROCEDURES

None the less, even if made in the abstract, such criticisms still have to be confronted. The assertion that the MRG codings are incapable by their nature of measuring party policy positions and tracing the direction of policy movement has been squarely met in the preceding section. The codings do measure movements in party positions in plausible and acceptable ways. Are there other reasons to distrust the results? These certainly do not appear in the 'pictures' and actual measurements they produce. What about the nature of the assumptions used to get there? It is here that critics concentrate on the MRG's 'valency and saliency' assumptions. We discuss these here, starting with the procedures based upon them which we have used to produce Figures 2–4.

As indicated above, the MRG coding counts sentences into fifty-seven policy categories (see Table 2). It then standardizes the distribution for different document lengths by percentaging each category count over the total number of sentences. The resulting category percentages then constitute the scores used in further analysis of the programmes. To create the Left–Right scales used as dimensions in Figures 2–4, certain categories were identified theoretically as belonging to the Right (free enterprise, for example) and certain to the Left ('economic planning'). Exploratory factor analyses were carried out to see if the selected categories 'hung together' – which they did – and to investigate whether any others belonged with them on the same dimension.

[26] Geoffrey Evans, Anthony Heath and Clive Payne, 'Class: Labour as a Catch-All Party', chap. 5 of Pippa Norris and Geoffrey Evans, eds, *Critical Elections* (London: Sage, 1999).

[27] Klingemann *et al.*, *Parties, Policies and Democracy*, *passim*; Michael McDonald, Ian Budge and R. I. Hofferbert, 'Party Mandate Theory and Time Series', *Electoral Studies*, 18 (1999), 587–96.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

TABLE 2 *The MRG Coding of Election Manifestos 1945–99*

101	Foreign special relationships: positive
102	Foreign special relationships: negative
103	Decolonization
104	Military: positive
105	Military: negative
106	Peace
107	Internationalism: positive
108	European Community: positive
109	Internationalism: negative
110	European Community: negative
201	Freedom and domestic human rights
202	Democracy
203	Constitutionalism: positive
204	Constitutionalism: negative
301	Decentralization
302	Centralization
303	Government efficiency
304	Government corruption
305	Government effectiveness and authority
401	Free enterprise
402	Incentives
403	Regulation of capitalism
404	Economic planning
405	Corporatism
406	Protectionism: positive
407	Protectionism: negative
408	Economic goals
409	Keynesian demand management
410	Productivity
411	Technology and infrastructure
412	Controlled economy
413	Nationalization
414	Economic orthodoxy
415	Marxist analysis
416	Anti-growth economy
501	Environmental protection
502	Arts, sports, leisure, media
503	Social justice
504	Social services expansion
505	Social services limitation
506	Education expansion
507	Education limitation
601	National way of life: positive
602	National way of life: negative
603	Traditional morality: positive
604	Traditional morality: negative
605	Law and order
606	National effort and social harmony
607	Multiculturalism: positive
608	Multiculturalism: negative
701	Labour groups: positive
702	Labour groups: negative
703	Agriculture
704	Middle class and professional groups
705	Minority groups
706	Non-economic demographic groups

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

Percentage scores for 'Right' and 'Left' categories were added up separately, and the total 'Left' score subtracted from the total 'Right' one[28] to give the final locations in Figures 2–4. The procedure is summarized in Table 1.

It is clear from Table 1 that some of the contrasting Left and Right scores are for directly opposed positions (pro-military and anti-military, for example). Others, however, find no direct contrast on the other side: pro-peace on the Left is not contrasted with anti-peace on the Right. Nevertheless, this is where the criticism is made. By contrasting mentions of peace on one side with a lack of mentions on the other side, the measure is recording 'saliency' rather than 'position' and this is 'improper'.[29]

In order to pursue this argument we have to go back to the original MRG codings and see why they take the form that they do. As they stand (Table 2) the codings provide fifty-seven categories into which sentences can be counted and percentaged. The aim is to count all sentences in the text whether they seem to have a direct policy content or not (thus the 'count' for each category includes vague historical generalizations about a problem or simple references to its importance). The justification for doing this is that party programmes are carefully considered and finely honed documents, so no sentence appears in them without a purpose.

Including all such references to an issue-area does make it difficult to tie most sentences down to specific pro- and con- positions, however. Long digressions on the growth of unemployment are presumably saying it is a bad thing and the party would do something to counter it. Is any party going to say explicitly that it is in favour of unemployment? Immediately the question is put it seems unlikely. A party might, however, say very little about unemployment and expatiate greatly on the evils of inflation, implying that all other considerations should be subordinated to fighting this problem.

These tricks of party rhetoric are no doubt familiar to every reader. They do not leave much room for parties to line up for or against each issue. What party wants to appeal for votes by extolling either unemployment or inflation – or supporting war against peace? The seminal finding of the pioneering analysis of party manifestos made by David Robertson was that parties do not directly oppose each other on an issue by issue basis.[30] They rarely took specific policy stands at all or mentioned any other party or its positions. Indeed, their programmes assume that there is only one tenable position on each issue and devote their energy to emphasizing the policy areas on which their credibility on that position is strong enough to pick up votes. This is a discovery which has been repeated in subsequent analyses of party rhetoric[31] and computer word counts of manifestos.[32]

[28] Note that the actual percentages are used, giving a simple numerical total. An alternative approach would be to factor-analyse all categories to give scores on a Left–Right dimension, which could then be used to locate parties, as is done using MRG data by Gabel and Huber, 'Putting Parties in their Place'. The objection to factor scores, however, is that they are influenced by all the data put in, thus making, for example, British Labour's location in 1997 depend in part on where Italian neo-fascists were in 1948! Percentage scoring of course is uninfluenced by the positions and movements of other parties.

[29] See fn. 2 above.

[30] David Robertson, *A Theory of Party Competition* (London: Wiley, 1976).

[31] Riker, *Agenda Formation*, pp. 81–126.

[32] Judith Bara 'Computerised and Manual "Public Opinion" Codings of British Manifestos and American Party Platforms': Laver and Garry, 'Estimating Policy Positions from Party Manifestos' (both papers were presented to the ECPR Joint Sessions of Workshops, Mannheim, 1999). Peculiarly, although Laver and Garry criticize saliency assumptions and the derived MRG codings at the beginning of their paper, they are driven to using precisely these assumptions in their actual word-count where, for example, a mention of taxes signifies only one position – cutting them.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

TABLE 3 *Theoretical Assumptions of the MRG Approach to Measuring Party Policy*

1. Party strategists see electors as overwhelmingly favouring one course of action on most issues. Hence all programmes endorse that position, with only minor exceptions.
2. Strategists also think that electors see one party as more likely to carry through this course of action than the other(s).
3. Hence each party has a set of issues which 'belong' to it, in the sense that their centrality in an election will increase their vote.
4. In its election programme therefore, it emphasizes its 'own' issues so as to increase their salience to electors and emphasizes 'rival' issues less or not at all.
5. Policy differences between parties thus consist of contrasting emphases on different policy areas (thus, one party often mentions taxes, another benefits). Sets of policy emphases which go together can be added numerically and contrasted with sets of opposing emphases to form a unified directional index such as Right versus Left (cf. Table 1).

It is mistaken therefore to characterize either Robertson's original coding, or the MRG coding so far as it is derived from Robertson's, as totally saliency-based and blind to questions of party position. Rather they are indeed positional in nature, but only one-positional so far as most issue-areas are concerned. This is because the texts themselves are one-positional. Coding-categories are inductively derived – basically formed by grouping related sentences in the text – and so they reflect the textual practice of only endorsing the 'obvious' position on each issue – against unemployment, inflation and high taxes, for extending services, etc. Hence the codings directly reflect *party* assumptions that there is only one tenable position on each issue.

However, we do not need to rely simply on induction to justify the MRG approach. A perfectly plausible theory of party competition underlies the strategists' programmatic presentations. Its constituent assumptions are picked out in Table 3. Given the limitations of party informational and processing capacities, strategists write programmes in response to perceived majority endorsements of obvious courses of action on each issue (cutting taxes, extending welfare, etc.). Parties seeking votes do not buck majority opinion. They thus do not oppose perceived popular preferences and hence mostly end up endorsing the same position. Examining 'pro' or 'con' stands on each issue thus gives little mileage in measuring party differences as parties are all generally 'pro' or 'con' on a specific issue. (For internal checks and empirical evidence for this assumption see below.)

Dynamism is given to party competition and the construction of programmes by strategists' belief that electors see one party as more able to carry through their preferred preference in a particular policy area than others. Again this seems plausible – to cut taxes one would prefer market Liberals to Socialists, and vice versa for extending welfare. To win votes, therefore, strategists do not argue about (agreed) policy positions but emphasize the importance of those issues where the party is most trusted by electors. This is what enables us to contrast the left and right stances of parties and to trace movement in them, in terms of the index proposed above (Figures 2–4; Table 1).

The 'valency and saliency' theory of Table 3 is supported empirically by:

(a) Classical qualititative studies of party competition (Stokes and the predominance of valence issues, Riker and the 'dominance principle' of party rhetoric). Such studies demonstrate that parties, rather than continuing to endorse a minority-

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

supported position against a majority-supported one, shift to emphasizing other issues in line with saliency ideas.[33]

(b) Computerized word counts which show that different policy themes are selectively emphasized by different parties.[34]

(c) Results deriving from internal checks in the MRG codings themselves. The checks were included because some members of the original group were sceptical of precisely the 'valency and saliency' ideas codified in Table 3. They could not really credit, just like latter-day critics, that party competition did not consist in direct confrontation between pro and con positions on each issue. To settle this dispute all issues where such confrontations seemed likely had pro and con positions assigned to them in the coding. This feature undermined the pure saliency nature of the MRG framework. But it did allow for a continuing empirical check on the validity of the saliency assumptions in Table 3, which turned out to be strongly supported. *Even where key issues were coded into opposing positions, they yet turned out to be valence issues, with the overwhelming number of references going to one of the possible positions*.[35]

While this result supports the theory underpinning MRG procedures, it means that the coding itself is not in principle a pure saliency one. In practical terms, however, it works like one simply because one or other of the pro–con positions on individual issues are so overwhelmingly dominant. Few parties will, for example, propose limiting education: thus references to education are almost all for expansion.

A confrontationist might still say that as some pro and con positions are already encapsulated in the coding, why can they not be put in for all categories? That would satisfy both saliency and confrontational approaches and could be used to settle the differences between them on a comprehensive basis.

There are three reasons why this cannot be done:

(a) Consistency and continuity. Introducing pro and con categories does not simply mean subdividing existing categories but revising the whole coding scheme and re-applying it to all the documents – an enormous, costly and probably impossible task at this point. It cannot be undertaken simply to meet an abstract consideration, unsupported by empirical evidence, that anything is actually *wrong* with MRG measurements (cf. Figures 1–4).

(b) Unreliability. The multiplication of small, thinly populated categories makes for random error and statistical noise[36] in the coding.[37]

(c) Theoretical plausibility. Having shown that the valency and saliency theory actually

[33] Stokes, 'Spatial Models', pp. 170–6: Riker, *Agenda Formation*, pp. 104–5. Even analyses of the specific policy pledges made in manifestos conclude that they rarely specify different courses of action in the same issue-areas (Robert Thomson, *The Party Mandate: Election Pledges and Government Actions in the Netherlands 1986–1998* (Amsterdam: ICS, 1999), p. 219). Rival campaigns under direct democracy also tend to emphasize different themes rather than coming out for and against the same policy, cf. Mads Qvortrup, 'Constitutional Implications of the Use of the Referendum' (doctoral dissertation, University of Oxford, 1999), chap. 2.

[34] The strongest finding comes from Laver and Garry, 'Estimating Policy Positions from Party Manifestos' (Warwick version), p. 12.

[35] Budge, Robertson and Hearl, eds., *Ideology, Strategy and Party Change*, Table 3.1 and *passim*.

[36] Laver and Budge, eds, *Party Policy and Government Coalitions*, pp. 23–4.

[37] Such 'noise' does occur between existing categories such as 'anti-military' and 'peace', which can be avoided by associating them in indices for 'Left–Right' (Table 1). This is not an argument for creating even greater 'noise', however.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

works, it would be odd if we then weakened its explanatory power by diluting it with further confrontationalist assumptions within the MRG codings.[38]

MRG CODES AS A GENERAL STANDARD FOR PARTY POLICY ESTIMATES

We clearly need a validated standard measure in the field of textual analysis to be sure that computerized approaches produce trustworthy results. The comprehensiveness of the MRG measures, the plausibility of their estimates (Figures 1–4), external and internal validation of assumptions and procedures – all designate them as the central point of reference in party policy estimation. Indeed, if these data were ruled out, there is no standard that could replace them, and we would have to rely on *ad hoc* comparisons of very disparate estimates (electoral perceptions and 'expert' judgements, for example), which are in any case available only for limited time-points.

Using the MRG data as a central standard means, where there are disagreements with other measures, that the MRG estimates would break the tie. It is quite likely of course that better and more authoritative standards may be developed in the future. But they should be shown to be so through the same detailed evaluations already applied to the MRG data, not simply asserted to be better. Otherwise we risk methodological anarchism, where every investigator has his or her own measure and there is no way of evaluating the substantive conclusions properly.

The question of authority is particularly relevant to computerized textual analysis. Clearly, this offers the possibility of rapid, reliable coding with vast savings in cost and a consequent extension of quantitative research. However, we need to be able to test the computer estimates to know that they are valid and useful. A comparison with MRG codings and Left–Right mappings of the documents is facilitated through the 'valency and saliency' assumptions underlying both the manual and the computer codings. However, only when the results of the latter match the former can we proceed to use them with any confidence. Validating computerized coding in this way is a first necessity before proceeding to substitute it for manual procedures.

Of course, even an authoritative general coding scheme for party policy positions does not necessarily function well for more specialized concerns within the policy field. Debating whether a *general* coding of election programmes is best based on saliency assumptions is not the same thing as assuming that it can serve all purposes equally. Specialized investigations may well need their own specialized codings. For overall comparability, however, we need a general cross-time, cross-national coding of all election programmes, focused on party policy positions, but able to serve as an acceptable surrogate for other investigations when re-coding cannot be done. The central question is, therefore, what kind of general scheme best serves these purposes? In terms of the review undertaken here, this is clearly the MRG one.

These examples also help demonstrate what a standard is not. It is not a final, unsurpassable procedure set in stone. We have already demonstrated that the present coding has defects, to which others could be added. However, this is no reason for abandoning it for, or substituting it by, more dubious alternatives. Indeed, not only is

[38] A more flexible and reliable computerized approach, basing itself on the MRG categories but then characterizing them in terms of positive or negative qualifiers, might overcome these problems. The expectation would still be that only positive or negative referents, but not both, would dominate all parties' stands on each category, and that they would again be checked empirically. The results would constitute another validity check on the procedures and theory outlined in Tables 1–3, which current evidence suggests they would meet (cf. Ray, 'A Natural Sentences Approach to the Computer Coding of Party Manifestos'.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press

the MRG scheme the best general measure of party policy positions we currently have, but it is likely to remain so until computerized text-processing is fully validated.

When computerized coding is validated against the MRG scheme, it may gradually substitute for it (though probably the two should run in parallel for a considerable period to make more extensive validation possible). As they base themselves on the same saliency assumptions about parties the computerized estimates should be entirely comparable with the MRG results. However, the new coding should, prospectively, vastly improve the range and flexibility of existing categories.

Setting a standard is thus very far from reifying existing measures. But it is to lay down a systematic way of developing new ones. The considerations laid out above show that the point of departure has to be the MRG data. Once this is accepted we can get on with the job of improving on them.

https://doi.org/10.1017/S0007123401230087 Published online by Cambridge University Press