Honorable John C. Coughenour

UNITED STATES DISTRICT COURT
WESTERN DISTRICT OF WASHINGTON
AT SEATTLE

WASHINGTON STATE REPUBLICAN PARTY, et al.,

Plaintiffs,

WASHINGTON DEMOCRATIC CENTRAL COMMITTEE, et al.,

Plaintiff Intervenors,

LIBERTARIAN PARTY OF WASHINGTON STATE, et al.,

Plaintiff Intervenors,

v.

STATE OF WASHINGTON, et al.,

Defendant Intervenors,

WASHINGTON STATE GRANGE,

Defendant Intervenors.

NO. CV05-0927-JCC

DECLARATION OF JOHN M. ORBELL IN SUPPORT OF PLAINTIFFS' MOTION FOR PARTIAL SUMMARY JUDGMENT

I, John M. Orbell, hereby declare under penalty of perjury as follows:

LIVENGOOD, FITZGERALD & ALSKOG
121 THIRD AVENUE
P.O. BOX 908
KIRKLAND, WASHINGTON 98083-0908
PHONE: (425) 822-9281 FAX: (425) 828-0908

1. I am over the age of eighteen years, and make this declaration of my own knowledge, except where indicated to the contrary.

2. I am Professor Emeritus of Political of Science at the University of Oregon. I have been a professor at the University of Oregon since 1967. A copy of my *Curriculum Vitae* is attached as **Exhibit 1**. Since 1988, I have been associated with the University's Institute of Cognitive and Decision Sciences, serving as its Director from 1998 through 2001. As Director, I was responsible for restructuring of the Institute as well as for its day to day operations. An important role of the Director is to encourage and support research projects undertaken by members. The Institute of Cognitive and Decision Sciences is a multi-disciplinary group of faculty (and student associates), drawing members from Political Science, English, Sociology, Psychology, Linguistics, Philosophy and Anthropology (and some other disciplines) to conduct social research, hopefully of a collaborative and interdisciplinary nature. The Institute's goal is to advance empirical and theoretical understanding of cognition, decision-making, culture and communications by drawing on the perspective of multiple disciplines. Social science experimentation is a regular activity of many institute members.

3. Since the middle 1970s, I have conducted a succession of laboratory experiments on human behavior and cognitive processes with a number of collaborators (including psychologist Robyn Dawes of Carnegie Mellon University with whom I worked between 1975 and 1993). Papers from these projects have been published in leading journals in several disciplines—including in the Political Science profession's leading journal, The *American Political Science Review* (6 experimental papers) as well as in Psychology's the *Journal of Personality and Social Psychology,* Sociology's *American Sociological Review* and Philosophy's

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P.O. BOX 908
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Ethics. Of approximately 60 articles on my vita, 30 are of a laboratory/experimental nature. My collaborations have all been equal-contribution interactions, with my collaborators and I either rotating authorship or simply using alphabetical sequencing—making me reluctant to claim "principal authorship" on any.

4. Beyond two National Science Foundation grants supporting computer simulations, these laboratory experiments have been funded by a succession of eleven grants from the National Science Foundation—the most recent experimental project (with political scientist Mikhail Myagkov) having been completed about six months ago. Earlier in my career I also conducted a number of research projects involving survey research, most notably in the late 'sixties and early 'seventies; papers from two of these projects were published in *The American Political Science Review*, as well as elsewhere; my graduate training at the University of North Carolina and at summer programs at the University of Michigan concerned exclusively such work. As part of my scholarly obligations, I have reviewed both survey and experimental work (as well as computer simulations) for all the leading journals in Political Science as well as in Psychology and Sociology having served on the Editorial Board of *The American Political Science Review*.

5. I have reviewed the report prepared by Professor Manweller. The version of the report I reviewed was identified as having been presented to the Annual meeting of the Western Political Science Association on April 1st, 2010. I also reviewed copies of Professor Donovan's two reports, consisting of 16 and 49 pages, respectively. These reports were provided to me by John J. White, Jr., counsel for the Republican Party in this action.

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121 THIRD AVENUE
P.O. BOX 908
KIRKLAND, WASHINGTON 98083-0908
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6. Much of Professor Donovan's critique revolves around what appears to me a misunderstanding of the role of representativeness in the design and conduct of social experiments as opposed to the design and conduct of social surveys—the latter appearing from his vita to be an methodology that he employs in his own work. (I point out that survey methods have a far longer history in Political Science than experimental methods, with the latter having started to appear in the leading journals not much before the 1975. This is not the case in Psychology, where experiments are a much preferred methodology.)

7. When conducting survey research with the object of generalizing one's findings to some particular population, it is obvious that randomness—and the assumption of representativeness that it can support—matters. Thus, for example, many electoral surveys are concerned with identifying the reasons why people vote the way they do, and the personal attributes of people who vote one way or another. Since some version of survey research is probably still the dominant tool employed by empirical researchers in Political Science, and since many empirical researchers in the discipline remain uninformed about experimental design, it is not surprising that 'representativeness' can be a concern for political scientists when they are evaluating empirical work beyond standard survey research.

8. The role of representativeness is, however, fundamentally different in experimental design. Standard experimental design seeks to identify the consequences of some experimental intervention (as for example, variously designed election ballots) by exposing different populations that, while not necessarily representative of any specific more general population, are defendable as being similar in all attributes relevant to their response to the experimental intervention, requiring therefore *random assignment to experimental conditions*. It

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121 THIRD AVENUE
P.O. BOX 908
KIRKLAND, WASHINGTON 98083-0908
PHONE: (425) 822-9281 FAX: (425) 828-0908

follows that any difference observed in the response of such randomly assigned experimental subjects to an experimental stimulus (again, election ballots) can be defended as 'statistically significant' or '*not* statistically significant' depending on standard tests for such things (e.g. ANOVA, chi square, etc).

9. In short, therefore: Representativeness is certainly important in survey work insofar as a researcher is concerned to generalize from his or her sample to some *particular wider population* or to the pattern of opinions or behaviors within that population. But it is important in experimental research insofar as a researcher is concerned to identify the consequences of exposing *people in general* to diverse experimental stimuli. In the present case, if there were any reason to suppose that the population of the State of Washington was likely to respond differently to ballot design than people in general—that, for example, Washingtonians were more or less easily confused—then there *might* be a reason for concern about representativeness of the subject pool. Since there is no reason to believe this is so, the argument about representativeness that concerns Donovan is moot. And, since it is clear that Manweller did assign his subjects to ballot type randomly, any concern about their reflecting the parameters of Washington's population is, from an experimental perspective which is at issue here, simply misplaced.

10. The only possible qualification one might claim here is that subjects from a particular pool *might respond to an experimental stimulus differently* from subjects drawn from some other pool. Imaginably, for example, subjects randomly assigned to experimental conditions might respond differently to the experimental stimuli than subjects from a different pool. But notice that arguing in this way requires a sophisticated theoretical reason for

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121 THIRD AVENUE
P.O. BOX 908
KIRKLAND, WASHINGTON 98083-0908
PHONE: (425) 822-9281 FAX: (425) 828-0908

supposing that would be the case—beyond that it is simply *imaginable* that could be so—and none is given here. In fact, I know of no criticism of any experimental study that has plausibly argued in such terms. That said, as I will develop below, Manweller's design *did* in fact involve replication across three different subject pools—pools that precisely address Donovan's concern about voter information about politics.

11. In the course of my experimental research in political science, I have never attempted to obtain a random (viz: representative) sample of a particular subject population, nor have any critics raised that issue with respect to my work or that of my collaborators. In fact, I know of no experimental work in any discipline (or, indeed, in Psychology which is a quintessentially experimental and laboratory discipline) that has attempted to ensure, via representative sampling, that its population is 'representative' of any particular population—such as, for example, the voting population in the State of Washington. To be as confident as possible that I have not missed any such instances, I have inquired widely among other experts, including one who is an unusually distinguished scientist with an extensive record of experimental studies and having received remarkable recent recognition, and have been confirmed in that impression.

12. Professor Donovan makes the following sweeping statement: "Scientific research - particularly research on social and behavioral phenomena - require [sic] that the researcher collect data from samples of a target population that are broadly representative of the target population. Without representative samples, scholars cannot make inferences about a larger population from their observations. The essential requirement of any sample is that it is representative of the population from which it is drawn." To repeat an earlier point, Donovan appears to be equating survey research with *all* modes of scientific or empirical research. *The logic of representativeness* in experimental

LIVENGOOD, FITZGERALD & ALSKOG
121 THIRD AVENUE
P.O. BOX 908
KIRKLAND, WASHINGTON 98083-0908
PHONE: (425) 822-9281 FAX: (425) 828-0908

research is *not* to describe the attributes of some target population but to identify the behavioral (or attitudinal, etc) consequences of some experimental intervention *in general* by exposing similar populations to the various experimental interventions. Thus, positive results from an appropriately conducted experiment, if they are to bear on the natural world, must be complemented by an argument that the effect observed in the laboratory would also be observed in the natural world—viz, that humans in a laboratory situation confronting a given experimental intervention would respond in essentially the same way in natural world circumstances. It is here, and only here, that Donovan's laborious documentation of the generally low levels of voter knowledge might, imaginably, be relevant.

13. But that low level of voter knowledge and understanding of politics is not a relevant criticism of Manweller's design. Acknowledging that voters differ in their levels of information (thus likely confusion), Manweller took care to differentiate three subject populations in terms of such information about politics ('new voters,' 'registered voters,' and 'active voters') and exposed subsets of each of these respective subject populations to three distinct experimental interventions (the three ballot designs). We would expect—from the common knowledge about voter sophistication that Donovan reviews—that new (thus younger) voters would be more confused about politics *in general*, but that is not the issue; it is whether they, and subjects exposed to those three interventions in the other two experimental populations, respond in a more or less confused manner to that three ballot designs.

14. In effect, therefore, Manweller has conducted three replications of his experimental design, one among each of three populations distinguished by different levels of political sophistication and knowledge. Should Manweller's data have confirmed that these populations did differ as expected in such sophistication and knowledge it would have comprised an unsurprising

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121 THIRD AVENUE
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KIRKLAND, WASHINGTON 98083-0908
PHONE: (425) 822-9281 FAX: (425) 828-0908

confirmation of Manweller's entering assumption about political knowledge but it would not have borne on the issue at hand—the potential for confusion in diverse ballot designs. Should his data have shown that confusion differed by ballot design *within* one or more of those three separate subject populations, the finding would have addressed directly his theoretical concern with ballot design—that being, of course, precisely what Manweller observed.

15. The incidence of the three "information types" within the natural population (in particular, presumably, the natural population of Washington state) does bear on the natural world importance of any such findings—that is to say, the extent to which the findings about ballot design might generalize to those natural populations. Most notably, had Manweller found an effect by ballot design in the least sophisticated population only, then Donovan's repetition of the standard findings about widespread voter ignorance *would* be relevant—showing, however, precisely that ballot design would matter greatly in any representative natural population where low levels of political knowledge and sophistication are known to exist. On the other hand, had Manweller found such an effect of ballot design across all three populations, the conclusion that ballot design matters would have been stronger still.

16. This is the important issue of 'external validity' that all experimentalists must confront, and Manweller goes a long way toward addressing that by introducing his experimental stimulus to the three populations differing in political knowledge—a real world parameter that Donovan (along with the rest of the Political Science world) recognizes is notably low across the whole population. Perplexingly in this context, Donovan does critique Manweller for not employing a 'control group', in particular, an experimental population in which subjects responded to a standard partisan ballot. It is, of course, normal for survey researchers interested in isolating the 'causal impact' of some individual attribute to 'control for' everything that might arguably have a causal

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121 THIRD AVENUE
P.O. BOX 908
KIRKLAND, WASHINGTON 98083-0908
PHONE: (425) 822-9281 FAX: (425) 828-0908

impact on both the dependent and independent variable—and to do so, usually, by employing some variant of multiple or partial regression. But this is nothing more than a survey researcher's attempt to capture the causal inferences that are available from well-designed experiments, and more *powerfully* available there than in survey research. I can see nothing to be gained by such 'controlling' in the present context, granted (1) that Manweller's use of three distinct populations and replication of the design within each is precisely a 'control' in Donovan's terms, and (2) that random assignment of subjects to experimental stimuli within the three populations effectively addresses the concern that survey researchers address with the exercise of statistical devices intended to isolate a causal effect of one survey-measured variable on another such variable.

17. I point out that 'random assignment' does not necessarily require use of, for example, a table of random numbers in assigning subjects to ballot conditions. It only requires that the method by which an experimenter assigns subjects to experimental condition (in Manweller's case, to those conditions within each of his three subject populations) is not plausibly related to the manner in which they might respond to particular experimental interventions. It is normal among laboratory researchers to assign subjects to experimental conditions in the order in which they sign up for an experiment or, perhaps, alphabetically by name. Notice that I am *not* speaking here of assignment to the different populations in which Manweller conducted his experiment; those assignments, obviously and sensibly, were made according to their political involvement.

18. Certainly, 'external validity' in any social experiment cannot be perfect; there are many emotional and other considerations that concern voters in 'real' elections that simply cannot be present in the laboratory—a problem, notice, that is no less difficult for survey researchers than for experimentalists. Nevertheless, Manweller has gone as far as is reasonably possible to present voters with the information circumstances that they would confront in the

LIVENGOOD, FITZGERALD & ALSKOG
121 THIRD AVENUE
P.O. BOX 908
KIRKLAND, WASHINGTON 98083-0908
PHONE: (425) 822-9281 FAX: (425) 828-0908

natural world use of various types of ballots—and information is, of course, critical to the issue of voter 'confusion.'

19. Similarly, 'internal validity' concerns whether the experimental situation does or does not provide subjects with information that is extraneous to the theoretical question at hand, but that might bear on their response to the experimental intervention. One major concern here is whether the experimenter—in instructions to subjects or otherwise without being aware of the fact—provides cues as to how subjects 'ought' to respond. I don't know any reason to suppose that internal validity of this order is a concern in Manweller's experiments. The objective is to measure the consequences of ballot design for subjects' ("voters'") understanding of the information that is available, and there is every reason from his description of his interaction with the subjects for accepting that Manweller limited himself to the absolute minimum of extraneous interaction in that context. The questions presented, reflected at WSDCC 00032 of the Manweller report, are expressly limited to the information contained in the sample ballot presented.

20. The criticisms expressed by Professor Donovan at pages 3 - 11 of the Report on Paper by Mathew Manweller are misplaced. The question tested by Professor Manweller's study is whether ballot design presented causes confusion regarding the relationship between candidates and political parties among those confronted with the ballot. It is not necessary to test whether some other ballot design—for example, a regular, fully partisan ballot—might cause more or less confusion in order to reach the conclusions expressed in Professor Manweller's report. I have addressed this issue above in the context of Donovan's concern about Manweller's

LIVENGOOD, FITZGERALD & ALSKOG
121 THIRD AVENUE
P.O. BOX 908
KIRKLAND, WASHINGTON 98083-0908
PHONE: (425) 822-9281 FAX: (425) 828-0908

alleged failure to introduce appropriate control variables—which, as I argue there, is also quite misplaced.

21. Two of the criticisms leveled by Professor Donovan do, perhaps, have some validity. Certainly, it would have been better for the number of observations, referred to as "n," to have been reflected in Tables 6 and 7. It is, however, a simple matter to amend those tables to include the number of observations. The size of the second sample, referred to in the Manweller reports as "registered voters," also appears to be somewhat small. However, it is certainly not so small that it deprives the experiment of validity. There have been numerous experiments conducted and results published where the "n" is no larger than in the Manweller experiment. The criticism at pages 23 – 29 rely on standards for public opinion research where the size of a 'representative sample' does bear on the confidence that can be placed on that sample as adequately reflecting the target population. The experiment is not testing attitudes or opinions, but the consequences of presentation of particular information.

22. But this is also beside the point. It is widely understood that the power of an observed experimental intervention (an 'effect') on some behavior or perception of interest is a function of both the difference size (in this case, the observed difference in 'confusion' between experimental populations) *and* the number of individuals in the experimental condition. It follows that statistically significant effects with relatively small numbers are *more* impressive than had numbers been much larger.

23. It might have helped avoid misunderstandings had Manweller summarized the experimental design in tabular format—making clear that there were, in effect, three replications

LIVENGOOD, FITZGERALD & ALSKOG
121 THIRD AVENUE
P.O. BOX 908
KIRKLAND, WASHINGTON 98083-0908
PHONE: (425) 822-9281 FAX: (425) 828-0908

of the same experiment by levels of information. But this is a small point, and the tables presenting Manweller's data ought to have made that amply clear.

24. I am not a statistician and would prefer not to comment in greater detail on Donovan's criticisms of Manweller's statistical methods—other than to say that they appear quite normal as employed by experimentalists not only in Political Science, but also in Psychology and Economics, as well as in the natural sciences.

25. In his discussion of terminology employed in questions to subjects (pp. 38-44) Professor Donovan gives a quite correct account of the problems that survey researchers have recognized with respect to the particular wording that they use in their questions; it has been known for sixty or more years that survey researchers can 'get' quite different responses from people they are interviewing by using different phraseology. This is not news. The relevant question is whether Manweller's phraseology falls into the same trap. I can see no way that Manweller could have phrased his questions more simply. In fact, adding further information or explanation would simply have introduced more opportunity for interviewer bias to have crept in and would, thus, have made measuring the effect of ballot design more problematic.

26. In fact, the analogy to "surveys to measure public attitudes about 'abortion [ ]'," just further illustrates Professor Donovan's confusion between the problems of survey research and those of laboratory research. There is nothing in Professor Donovan's discussion of the term "abortion" that suggests that survey respondents would not understand what an abortion is—only that respondents have conflicting attitudes regarding its propriety. Consistent with the kind of problem addressed by standard survey research methodology, Professor Donovan appears to be preoccupied with the well-understood issues involved with measuring attitudes (e.g. toward

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121 THIRD AVENUE
P.O. BOX 908
KIRKLAND, WASHINGTON 98083-0908
PHONE: (425) 822-9281 FAX: (425) 828-0908

abortion) not with differential responses to some political stimulus—something that would be difficult at best to address via survey methodology, but is appropriately and readily addressed by standard experimental methods (as Manweller's work illustrates).

27. In Section IX of his report, Professor Donovan asserts that the interpretation of voter error under the experiment is mistaken. He bases his criticism on his statements that political parties may endorse candidates who may appear on the Top Two ballot. Professor Manweller's report makes clear that the ballots used in the experiment do not list any real candidates. The instructions to participants in the experiment plainly state that the answers are to be based solely on the information presented in the sample ballot itself. I am at a loss to understand how he believes subjects could interpret this as in any way involving endorsement (or not) of real candidates in the natural political world.

28. Professor Donovan's suggestion that connections among real political candidates might change the effect of ballot design in the natural world is imaginably true (although I know no evidence that it is)—but it begs the theoretical question being addressed by Manweller's analysis which is simply and only the effect of ballot design on voter confusion. A wide variety of natural world conditions might, imaginably, influence any observed effect but that is a different issue altogether.

29. In reviewing my comments above I recognize that, in places, I have adopted something of a 'tutorial' tone, but I mean no offense to Donovan who is obviously quite skilled in his particular methodological domain. As I have indicated above, experimental methods are still very much a minority activity among empirical political scientists, and few departments offer such training in their graduate programs. For some years since the initial work appeared in

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121 THIRD AVENUE
P.O. BOX 908
KIRKLAND, WASHINGTON 98083-0908
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the 1970s it was necessary, when submitting such studies to political science journals, to spend much space explaining basic methodological issues such as those discussed above. Even now, Professor Donovan is by no means alone among political scientists in confusing the finer points of experimental methods with the problems encountered in survey research. As evidence that experimentalism is becoming more widely appreciated in the discipline, I need only point to the accomplishments of Elinor Ostrom—a recent President of the American Political Science Association as well as the Public Choice Society and an accomplished experimentalist—who received the last Nobel Prize (for economics) in significant part because of her pioneering laboratory work in and around the problem of behavior toward public goods.

30. I have neither requested nor received any compensation for my review of the reports referenced herein, or the preparation of this declaration.

Executed at EUGENE, Oregon on September 8, 2010.

John M. Orbell

LIVENGOOD, FITZGERALD & ALSKOG
121 THIRD AVENUE
P.O. BOX 908
KIRKLAND, WASHINGTON 98083-0908
PHONE: (425) 822-9281 FAX: (425) 828-0908

Monday, September 13, 2010

VITA

JOHN M. ORBELL

Emeritus Professor,
Political Science Department
Address: Institute of Cognitive and Decision Sciences
Straub Hall, University of Oregon, Eugene, OR 97403
Email: jorbell@uoregon.edu
Phone: 541 3460133
Fax: 541 3464914

PERSONAL DATA:

Born June 3, 1936, New Zealand
US and New Zealand citizen.
Married, two children

DEGREES AWARDED:

B.A. (History) -	University of Auckland, New Zealand, 1957
M.A. (History) -	University of Auckland, New Zealand, 1960
Ph.D. (Political Science) -	University of North Carolina, Chapel Hill, 1965. Dissertation dealing with the origins of the protest movement among black college students in the early 'sixties. Advisor: James W. Prothro

HONORS

Distinguished Professor of Political Science and Cognitive Science, College of Arts and Science, University of Oregon, 1997-

PREVIOUS POSITIONS:

1960-61	High school teacher in New Zealand
196l-1964	Teaching and research assistant, Department of Political Science, University of North Carolina at Chapel Hill
1964-1965	Instructor, Department of Political Science, The Ohio State University, Columbus, Ohio
1965-1967	Assistant Professor, Department of Political Science, The Ohio State University, Columbus, Ohio.
1967-1969	Assistant Professor, Department of Political Science, The University of Oregon, Eugene, Oregon
1969-1973 June-	Associate Professor, University of Oregon

Aug., 1972	Visiting lecturer, Department of Political Science, University of Canterbury, Christchurch, New Zealand
1973-	Professor, University of Oregon
May, 1973- Nov, 1974	Visiting lecturer, Department of Political Science, University of Canterbury
June- Aug., 1975	Visiting lecturer, Department of Political Science, University of Canterbury
1976-1979	Department Head, Department of Political Science, University of Oregon
1979-1983	Director, Institute for Social Science Research, University of Oregon
1983-1985	Associate Dean for Undergraduate Studies, College of Arts and Sciences, University of Oregon
1985-1986	Visiting Professor, Department of Social and Decision Sciences, Carnegie-Mellon University, Pittsburgh, PA 15213
1986-1987	Visiting Merrill Professor of Political Science, Political Science Department, Utah State University, Logan, Utah 84322.
Spring, 1993	Visiting Merrill Professor of Political Science, Political Science Department, Utah State University, Logan, Utah 84322.
1998-2001	Director, Institute of Cognitive and Decision Sciences, University of Oregon
Spring, 2003	Emeritus professor, University of Oregon

SCHOLARSHIP:

***In progress*:**

"The evolutionary roots of human sociality." With Tim Johnson & Mikhail Myagkov. An NSF-funded laboratory project developing an evolutionary explanation of our previously published findings with respect to the human propensity to form cooperative groups in the domain of losses more than in the domain of gains. In progress.

Modeling the human response to abrupt climate change, worldwide. An NSF-funded project (Cyber-Enabled Discovery and Innovation). Oregon PI; Project PI Oleg Smirnov, Stony Brook. With Douglas Kennett (archaeology) and Amy Lobben (geography) at Oregon, and Haipeng Xing (statistics) and Minghua Zeng (climate science) Stony Brook.

Publications:

"An Evolutionary Account of Suicide Attacks: The Kamikaze Case." Forthcoming in *Political Psychology*. With Tomonori Morikawa.

"The Selective Consequences of War." Forthcoming. With Holly Arrow, Oleg Smirnov and Douglas Kennett (2007). In Thompson, L. and K. Behfar (Eds.). Conflict in organizational groups: New directions in theory and practice. Evanston, IL: Northwestern University Press Board.

"Ancestral Warfare and the Evolutionary Origins of 'Heroism.'" November, 2007. With Oleg

Smirnov, Holly Arrow and Doug Kennett. in *Journal of Politics.* Pp. 927-940.

"Evolutionary Psychology and a More Satisfactory Model of Human Agency." 2007. In *Cooperation: A Powerful Force in Human Relations.* Edited by B.A. Sullivan, M. Snyder, and J.L. Sullivan. Blackwell. With James Hanley, Jason Hartwig and Tomonori Morikawa.

"Mindreading and Manipulation in an Ecology of Prisoner's Dilemma Games: Laboratory Experiments," *Journal of Bioeconomics,* (2006) 200:67-83. With Mikhail Myagkov.

"Science, Anti-Science, and Rational Choice." *The Political Economist; Newsletter of the section on political economy, American Political Science Association.* Fall, 2005.

"A Bias toward Sociality when Confronting Loss," 2005. *Politics and the Life Sciences.* 25: 13-19. With Tim Johnson and Mikhail Myagkov.

"Teaching Bioeconomics: A Political Scientist's Experience Teaching Evolutionary Psychology." *Journal of Bioeconomics* (2005) 7: 41-44.

"'Machiavellian' Intelligence and the Evolution of Cooperative Dispositions." 2004. Orbell, John, Tomonori Morikawa, Jason Hartwig, James Hanley and Nicholas Allen. *The American Political Science Review*, 98: 1. Pp. 1-16.

"'Social Poker: A Laboratory Test of Predictions from Club Theory." Scott Crosson, John Orbell and Holly Arrow. *Rationality and Society.* May, 2004: 16, No. 2. Pp. 225-248.

"Conflict, Interpersonal Assessment, and the Evolution of Cooperation; Simulation Results." 2003. In *Trust, Reciprocity, and Gains from Association; Interdisciplinary Lessons from Experimental Research*, edited by Elinor Ostrom and James Walker, Russell Sage Foundation. With James Hanley and Tomonori Morikawa. Pp. 170-206.

"Cognitive Requirements for Conflict of Interest Games; A Functional Analysis." With Tomonori Morikawa and James Hanley. *Politics and the Life Sciences*, March, 2002. 21:1, Pp. 3-12.

"The Evolution of Political Intelligence: Simulation Results." With Tomonori Morikawa and Nicholas Allen. *British Journal of Political Science*, 2002. 32: 613-639.

"Physical Attractiveness, Opportunity and Success in Everyday Exchange." May 1998. *American Journal of Sociology* 103: 1565-92. With Matthew Mulford, Catherine Shatto and Jean Stockard.

"Individual Experience and the Fragmentation of Societies." 1996. *American Sociological Review*: 61: 1018-1032. With Langche Zeng and Matthew Mulford.

"The Robustness of Cognitively Simple Judgment in Ecologies of Prisoner's Dilemma Games." 1996. *BioSystems; A Journal of Biological and Information Processing Sciences: 37.* With Audun Runde and Tom Morikawa, 81-97.

"Teaching *Evolution, Cooperation and Ethics.*" 1996. *Politics and the Life Sciences.* March. 121-124.

"The Benefit of Optional Play in Anonymous and One-Shot Prisoner's Dilemma Games." 1995. In K. Arrow, R. Mnookin, L. Ross, A. Tversky, R. Wilson (eds.), *Barriers to conflict Resolution*. Norton & Co., with Robyn Dawes, 62-85.

"The Advantage of Being Moderately Cooperative." 1995. *The American Political Science Review* 89: 601-611. September. With Tom Morikawa and Audun Runde.

"Cooperation under Laissez faire and Majority Decision Rules in Group-level Social Dilemmas," with Robyn Dawes. In David Schroeder (ed.), *Social Dilemmas; Perspectives on Individuals and Groups*, Praeger. 1995.

"Trust, Social Categories And Individuals: The Case Of Gender." 1994. *Motivation and Emotion 18:* 109-128. June. With Robyn Dawes and Peregrine Schwartz-Shea.

"Hamlet And The Psychology Of Rational Choice Under Uncertainty." 1993. *Rationality and Society.*127-140.

"Social Welfare, Cooperators' Advantage And The Option Of Not Playing The Game." 1993. *American Sociological Review 58:* 787-800. December. With Robyn Dawes.

"Simple Dilemmas, Complex Dilemmas, and Experimental Research. 1992. *Small Group Research*. February, 23: 4-25. With Peregrine Schwartz-Shea.

"Religion, Context and Cooperation with Strangers." 1992. *Rationality and Society*, July. With Marion Goldman, Matthew Mulford and Robyn Dawes.

"Covenants Without the Sword; the Role of Promising in Social Dilemma Circumstances." 1991. With Robyn Dawes. In Ken Koford and Jeffrey B. Miller, (eds.) *Social Norms and Economic Institutions,* Ann Arbor: University of Michigan Press. 1991.

"A 'Cognitive Miser' Theory Of Cooperators' Advantage." 1991. *The American Political Science Review*. June. With Robyn Dawes.

"Response to McLean." 1991. *American Political Science Review*. December. With Robyn Dawes.

"The Limits of Multilateral Promising." 1990. *Ethics*, vol. 100, April, 616-627. With Robyn Dawes and Alphons van de Kragt,

"The Experimental Study of Social Dilemma Behavior." 1990. *CogSciNews*, Spring, 3, #1, 4-6.

"Collective Rationality in Dilemma Situations for the Benefit of Us--not Me, or my Self-esteem." in Jane Mainsbridge (Ed.), 1990 *Beyond Self Interest*, Chicago: University of Chicago Press.

"Selfishness Examined: Cooperation in the Absence of Egoistic Incentives." 1989. *Behavioral and Brain Science*, December, 12, No. 4, 683-699. With Linnda Caporael, Robyn Dawes and Alphons van de Kragt. (A "target" paper with thirty open peer commentators).

"Thinking in Sociality; Authors' Response." 1989. *Behavioral and Brain Science*, December, 12, No. 4, 727-739. With Linnda Caporael, Robyn Dawes and Alphons van de Kragt. (An extended response to open peer commentary on "Selfishness examined...".)

"Not Me or Thee but We: The Importance of Group Identity in Eliciting Cooperation in Dilemma Situations: Experimental Manipulations." *Acta Psychologica*, 1988, 68, 83-97. With Robyn Dawes and Alphons van de Kragt.

Reprinted in William M. Goldstein and Robin M. Hogarth. (Eds.) *Research on Judgment and Decision Making*. Cambridge, UK: Cambridge University Press.

"Explaining Discussion-induced Cooperation." 1988. *Journal of Personality and Social Psychology*, 54, 811-819. With Robyn Dawes and Alphons van de Kragt.

"Sherlock Holmes as a Social Scientist," 1988. *The Political Science Teacher*, 1, Fall, 15-19. With Veronica Ward.

"Are People who Cooperate Rational Altruists?" 1988. *Public Choice*, 56, 233-248. With Alphons van de Kragt and Robyn Dawes

"Understanding and Cooperation in Social Dilemmas." 1988. *Public Choice*, vol 57, 275-280. With David Goetze.

"Doing Well and Doing Good as Ways of Resolving Social Dilemmas," in H. Wilke, D. Messick and C. Rutte, *Social Dilemmas*, Frankfurt/Main, Lang. GmbH. 1986. With Robyn M. Dawes and Alphons van de Kragt.

"Organizing Groups for Collective Action." 1986. *The American Political Science Review*, December. With Robyn M. Dawes, Randy Simmons and Alphons van de Kragt.

Reprinted in: Donald R. Kinder & Thomas Palfrey (eds.). *Experimental Foundations of Political Science*. Ann Arbor: University of Michigan Press, 1993.

"Response to Calvert and Wilson." 1985. *American Political Science Review*, September. With

Alphons van de Kragt and Robyn M. Dawes.

"Do Cooperators Exit More Readily Than Defectors?" 1984. *American Political Science Review*, March. With Peregrine Schwartz-Shea and Randy Simmons.

"Two Pedagogical Games." 1984. *News for Teachers of Political Scienc*e, Summer. With Steven Maser.

"The Minimal Contributing Set as a Solution to Public Goods Problems." 1983. *American Political Science Review*, March. With Robyn M. Dawes and Alphons van de Kragt.

"Cooperation in Social Dilemma Situations: Thinking About It Doesn't Help," in *Research in Experimental Economic*s, Volume 3, 1982. With Robyn M. Dawes

"Social Dilemmas," in *Progress in Applied Social Psychology, Volume I*, G.M. Stephenson and J.M. Davis, eds., 1981. With Robyn M. Dawes.

"The Governance of Rivers" 1979. *Western Political Quarterly*, September. With L.A. Wilson, II.

"The Uses of Expanded Majorities." 1978. *American Political Science Review*, December. With L.A. Wilson, II.

"Institutional Solutions to the N-Prisoners' Dilemma." 1978. *American Political Science Review*, June. With L.A. Wilson, II.

"A Proposal to Establish a Centre for the Study of New Zealand Society." 1974. *Australia and New Zealand Journal of Sociology*, October. With Geoffrey Fougere.

"Social Peace as a Collective Good." 1974. *British Journal of Political Science*, Volume 4. With Brent Rutherford.

"Can Leviathan Make Life of Man Less Solitary, Poore, Nasty, Brutish, and Short?" 1973. *British Journal of Political Science*, October. With Brent Rutherford.

"A Theory of Neighborhood Problem-Solving: Political Action vs. Residential Mobility." 1972. *American Political Science Review*, June. With Toro Uno.

"Intra-Party Conflict and the Decay of Ideology." 1973. *Journal of Politics*, May. With Geoffrey Fougere.

"Grass Roots Enthusiasm and the Primary Vote; McCarthy and Kennedy in Oregon" 1972. *Western Political Quarterly*, June. With Robyn M. Dawes and Nancy J. Collins.

"The Structure of Graduate Education in Departments of Political Science," 1972. *P.S.*, Winter.

With Alvin Mushkatel and Lawrence Pierce.

"An Information-flow Theory of Community Influence," 1970. *Journal of Politics,* May.

"The Impact of Metropolitan Residence on Social and Political Orientations," 1970 *Social Science Quarterly*. December.

"Racial Attitudes and the Metropolitan Context: A Structural Analysis," 1969. *The Public Opinion Quarterly*, Spring. With Kenneth Sherrill.

"Protest Participation Among Southern Negro College Students," 1967. *American Political Science Review*, June. Reprinted Bobs Merrill reprint series in the social sciences, 1969.

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