

THE POLITICAL ECONOMIST

Newsletter of the Section on Political Economy, American Political Science Association

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Feature Essay Political Economy and Experiments

Rick K. Wilson, Rice University with R. Mark Isaac, Florida State University

“Experiments are quaint methodological gimmicks that tell us a lot about college students. Real social science involves populations, samples of those populations and inference from those samples.” This seems to be the reaction by many social scientists (including some of my colleagues). I share some of that sentiment, but obviously I do not wholeheartedly agree. I have been a practicing experimentalist for almost 25 years and I continue to use the tool.

Where I agree with many social scientists is that an experiment is a methodological tool (I prefer this to “gimmick”). As with any other tool, it is one part of a toolbox. As with any other tool, it should be used appropriately and not indiscriminately. I shouldn’t call myself an experimentalist anymore so than someone calling herself a regressionist. Tools should be used appropriately.

What follows are some general thoughts about experiments. In this discussion I have been greatly helped by Mark Isaac and by the editors of the newsletter. Mark and I both grew up in the tradition of experimental economics. He has edited this piece and has rightly pointed out where I’ve been wrongheaded or stubborn. I’ve tried to make changes about the former, but, predictably, have stuck with the latter. In a couple of places I have included Mark’s rejoinder to what I’ve written. Neither Mark nor the editors should be thought responsible for any intemperate comments that I have written.

Causality.

So what are experiments for and how can they inform us about political economy? Experiments do one thing well:

they establish causal claims. Among the difficult problems that social scientists face is establishing causality. While we often talk as if we know the cause of something, it is difficult to establish. We might want to know whether imposing economic sanctions on another country causes a change in that regime’s behavior; we might want to know whether changing a particular voting mechanism causes a change in proportional representation; or we might want to know whether the size of the group affects cooperation in a common pool resource. While it is (relatively) easy to observe correlations between dependent and independent variables, being confident of the cause is more difficult. This is where experiments excel.

Pinpointing a cause typically means being able to manipulate the purported cause and ruling out other possible explanations for a change in the dependent variable. While this may seem an easy task, it is not. Isolating a cause (and being able to test it) is often daunting. Moreover, ruling out all possible alternative explanations for the observed change in the dependent variable may be impossible. After all, humans are very good about inventing explanations and seeing patterns even when they do not exist.

Internal and External Validity.

Laboratory experiments have an entire repertoire of dos and don’ts in order to isolate cause and effect. Most of the concerns involve threats to internal validity that can call into question claims about cause. Internal validity refers to ensuring that inferences about what caused the outcome are in fact due to the

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A Letter From the *Editors*

One of the long-running debates in *The Political Economist* centers on the definition of political economy: does the field represent a substantive concern with the interaction between politics and markets or is political economy an approach to the study of politics that employs the theories and tools of economics? From the perspective of the political economy section, this question has taken on a more practical urgency: while the section enjoys one of the larger memberships in APSA, attendance at panels is relatively sparse, resulting in a loss of slots and resources. Some have suggested that the amorphous nature of political economy can explain this puzzle.

In her inaugural "Message from the Chair," former section president Lin Ostrom challenged all of us to consider not just what political economy *is*, but also what political economy *could be* in the coming generations. During our tenure as co-editors of the newsletter, we sought to answer Lin's call. We reached out to scholars across a variety of research areas and asked them to assess the state of knowledge in their areas of specialization and to project how future research could and should evolve. When possible, we published two features essays on a given topic, often pairing an economist with a political scientist. We were consistently surprised at how the two essays complemented each other.

After two and a half years and eight newsletters, we are still not sure exactly what constitutes political economy. Nevertheless, the provocative essays of our contributors and enthusiastic feedback of our section members put us in a position to draw some conclusions about what makes political economy not just a coherent sub-field, but one that represents the most interesting and vibrant research frontiers in political science.

First, the community of political economists shares a concern with developing theoretical explanations that rest upon well defined microfoundations. Attention to the incentives and beliefs

of individual actors is the basis of political economy theories that link cause and effect. Regardless of the choice of empirical tools, political economists agree that theoretical explanation is the key goal of social science.

Second, political economists give priority to the substance of politics and markets to motivate their work. The interaction between politics and markets presents puzzles and problems that excite attention. The link between substance and research, especially in the initial stage of the research cycle, is a core strength of political economy.

Third, political economists are curious and adaptable. By definition, the sub-field is multidisciplinary. Because political economists have already had to master literatures from two disciplines, we are open to new ideas, approaches, and tools. We recognize and incorporate new and interesting frontiers into the research agenda, even beyond economics: bio-politics, experimental research, normative bases of behavior—the list goes on. We embrace these new directions in the quest to develop better theory and sharper empirical evaluation.

To be sure, one finds these qualities in other areas of political science. But we believe that they enjoy more pervasive acceptance in political economy than in other sub-fields. It is difficult to find a group of scholars that shares a cohesive set of social scientific values and intellectual curiosity more than the political economy community. These qualities have made political economy the most influential sub-field in political science over the past 20 years.

We cannot take these qualities for granted. We see two related challenges on the horizon. Ironically, both stem from the success of political economy in bringing an increased emphasis on social scientific rigor and technical expertise to the discipline. The first challenge concerns the types of research questions that political economists ask. Over the last 20 years, political economy has

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consistently asked innovative, fresh questions that challenged accepted orthodoxies or illuminated interesting puzzles. Political economists became leaders in political science because they were able to “think outside the box.” But as political economists emphasized rigorous evaluation of causal mechanisms, one consequence has been that our research questions have, almost by necessity, become narrower and more specialized. While rigor and careful analysis are positive developments that contribute to the accumulation of knowledge, we cannot lose sight of the importance of asking the right research questions, of pursuing compelling research agendas even if they don’t lend themselves to fundamental analysis or fit with the latest methodological technique.

Economics as a discipline faced similar pressures in the 1950s and 1960s. At that time, economics began to evolve in a manner that rewarded mathematical precision and technical skill. While that trend contributed to a discipline with a coherent focus, it also limited research activity to a set of rather narrow concerns. A scholar like Steven Levitt (*Freakonomics*) has gained fame and fortune, in part, by daring to ask “innovative” and “non-obvious” research questions – questions that seem intuitive and fundamental to someone outside the limited confines of mainstream economics.

The sub-field of political economy benefited from the narrowness of economics – many interesting and compelling questions were left for us to answer. But as we strive for sharper theoretical explanations and stronger empirical evaluations, we cannot lose sight of the importance of searching for novel puzzles and asking inventive, original research questions. Political economists must continue to address issues central to our discipline and the world as a whole. The future of the field will be defined more by the questions we pose, rather than the answers we

provide.

The second, related challenge is continue to foster intellectual exchange across political economy. Historically, the community of political economy scholars has been actively engaged with each other, making connections within the sub-field and across disciplines. As research has become more specialized, however, it has become more difficult to participate in the intellectual arbitrage that is one of the hallmarks of political economy. If we don’t speak to each other across our specializations, however, we lose opportunities to ask innovative questions, to identify new puzzles, to see old questions with fresh eyes, or to bring new ideas to bear. The breadth of our field is not a weakness; it is a fundamental strength. Our common social scientific values and language permits communication across different specializations. We must find ways to enhance that valuable communication. We must demonstrate to our students how political economy (broadly-defined) creates synergies and opportunities for intellectual growth. Section activities can also create a sense of community and an easy way for scholars in one area to learn more about what’s going on in another area. Bridging the gap between sub-fields within political economy will be always be a challenge, but one with immense rewards.

Editing *The Political Economist* has given us an opportunity to address these challenges, if only in a rudimentary and preliminary way. We have learned much from the process. We are excited to hand over the newsletter to Matt Gabel (Washington University) and Randy Calvert (Washington University). They will bring a fresh perspective to the issues and concerns facing political economy.

As we end our tenure as co-editors, we would be remiss if we did not acknowledge the people who made the newsletter a success over the past few years. First, we thank our contributors – always provocative, always patient, and often punctual. Ultimately, it was their

ideas and the generosity to share them that made the newsletter fun to read. Second, we thank the section leadership for their institutional support of the newsletter, especially Lin Ostrom. Her enthusiasm and encouragement for the newsletter was contagious. Third, we want to give a big shout out to Amanda Harris, who produces each newsletter. Her tolerance for our disorganization was amazing. Finally, we want to thank all those who told us how much they enjoyed the newsletter. Knowing that you appreciated the newsletter made the task worthwhile.

In this issue of *The Political Economist*, we survey research on “Experiments in Political Economy.” Rather than following our standard format of publishing separate essays by a political scientist and an economist on the status of research in this area, our feature contributor, Rick Wilson (Chair, Department of Political Science, Rice University), collaborated with R. Mark Isaac (Department of Economics, Florida State University) on a single essay. Despite minor disagreement – see the provocative and enlightening discussion of the problem of deception in experiments – this collaboration speaks to the remarkable consistency across the social sciences in experimental political economy.

We are very pleased to end our tenure as editors on this interdisciplinary high note. We hope that other areas of political economy will soon reach a similar consistency of theory, method, and research design.

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experimental manipulation (and nothing else). Many threats to internal validity are esoteric (e.g., “compensatory rivalry”) and some should be avoided for ethical reasons (“subject mortality”). However, they should all be taken into account. Good experimental design is an entire subject of its own and I would encourage the interested reader to look at some of the classics in the area – Cook and Campbell (1979) is a good start.

The wonderful thing about using an experimental design is that it makes the researcher the Grand Poobah of the experimental environment. This is one of the few instances in which political economists can implement their vision of the world. Subjects coming into the laboratory are at the mercy of the experimenter in that they must operate within the rules dictated by the experiment. But this means that the experimenter has to take the blame for an experiment gone awry. When the experimenter loses control over his or her creation, then internal validity is lost.

A researcher can rectify most problems with experiments by using random assignment for subjects and treatments. Obviously it is difficult to anticipate every alternative explanation when designing an experiment. However, it is possible to minimize the effect of unanticipated relationships that might explain the observed outcome (for example, females being more cooperative in non-competitive situations). Random assignment of subjects and treatments can neutralize such relationships. In turn the research can focus on the primary causal relationship of concern in the study.

For all this emphasis on internal validity, what about external validity? External validity is the degree to which the conclusions from a study would hold for other people in other places and at other times. How can we make inferences about populations if we do not worry about the characteristics of our subjects? After all, most subjects are simply college students. They are bounded by age and

they are relatively homogeneous. I tend to ignore questions of external validity, mainly because I think that ensuring internal validity is so important. As I noted at the outset, experiments aim to answer questions about cause. I see an experiment as simply one tool that helps answer complicated questions. Theory guides us as to where to look. Experiments enable us to test causal links in a theory. Archival work helps us answer historical trends. Survey instruments, with appropriate sampling, help us make inferences about a population. All of these (and other tools) can help us answer a difficult question with many logical linkages. No single tool provides a clear answer to all aspects of a complicated political mechanism.

This being said, experiments should be used for what they’re good for: testing explicit causal claims. If my theory imputes a cause and that cause does not depend on population characteristics that I can control in the laboratory, then why bother with external validity? Only once the cause is clearly established, does it make sense to go beyond the laboratory. Even then, experiments are unlikely to be necessary to answer broader questions of correlation or conditional dependencies. This is where other tools excel. Of course, as I note, going outside the laboratory is not crucial unless there is something about the population that makes a difference.

Theory Testing.

Experiments also are good for theory testing and theory development. Usually our theories are esoteric; they are stylized representations of the world that pinpoint very specific relationships among variables. Because of their highly abstract character, many theories (particularly formal models) appear untestable. But those theories posit explicit relationships that should hold up in controlled environments. Experiments are an excellent way in which to give a theory its “best shot.” My sense is that

if a theory performs poorly in an environment that provides the best chance for succeeding, then the theory will not do very well in explaining events in a natural setting. If it performs well, then there is reason to tease out additional implications that are capable of being put to test by other means.

At the same time I do not see experiments as handmaidens to theory. Experiments can be used to design and test new mechanisms. In economics different auction mechanisms have been thoroughly explored in the realm of experiments and then applied to natural settings. In turn the design of new mechanisms has spurred new theoretical developments.

Finally, experimental results often provoke new theory. Theorists in economics, when challenged with anomalous experimental results, have responded with new theoretical models. The persistence of fairness/altruism in a simple experiment like the “dictator game” has given rise to a number of formal models of inequality aversion.¹ These models, in turn, have spurred new experiments and resulted in a valuable interchange between theory and experiment. Many would claim that these experiments have opened up a new field of “behavioral game theory” [Camerer (2003)].

What have we learned?

What have we learned from experiments? We have learned a great deal about the ways in which institutions affect behavior. I’ll touch on voting mechanisms. While this might seem an obvious arena for political scientists to already know a great deal, this was not necessarily the case. The work in social choice in the 1960s and 1970s was abstract, very theoretical and usually empirically unsupported. The models made very precise predictions about outcomes, but testing among competing models was difficult because the preferences of actors could not be

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measured very precisely. This did not stop theorists, but it made it hard for empiricists to find much to admire. The complaint, often lodged, was that the models were simply puzzle-solving exercises and they did not mean much for the things that political scientists studied.

Several clever economists and political scientists decided that the laboratory might be an excellent place in which to begin testing the ways in which institutional design affects outcomes. To do so, those scholars worked to fix the preferences of subjects. One insight (dragged in from the nascent field of experimental economics) was the concept of “induced” value. Here the experimenter uses money to motivate subjects. This is achieved by making clear the link between actions in the experiment and outcomes (payments to the subject). In the case of voting mechanisms, the ways in which votes are aggregated and which outcomes are then selected was all that mattered to the subject. Subjects were paid for the outcome selected. In truth this was no different than what psychologists working with pigeons or rats had discovered early on – it was important to motivate the subject, it was important that subjects preferred more of the reward medium than less and it was important to avoid satiation. Unlike laboratory animals, students had one other feature that was important – the potential amount earned had to exceed the opportunity costs for doing something other than showing up at the lab (sleeping, eating, or, gasp, studying).

By mapping payoffs to various outcomes, it became easy for scholars to study the ways in which institutions mattered. The earliest findings confirmed that when a unique equilibrium exists (the Core), subjects settle on it. Of course, that was the rare case. When there was no equilibrium, the resulting empirical outcomes were patterned. This in turn gave rise to a variety of new theoretical solution concepts – each attempting to

explain seemingly anomalous results. Other experimental findings illustrated the power of agenda setting, the power of blocking coalitions and veto points. The net effect of these experiments was to make it clear that naïve subjects who were not cued into their task exhibited collective behavior that looked very much like what abstract theoretical models predicted.² I would argue that these experimental findings made it easier for subsequent non-experimental (but empirical) work to use many of these models to move the discipline forward.

There are many other domains in which we know a great deal. A useful place to start for reviewing work of interest to political economists is Kagel and Roth (1995). While a little dated (volume 2 is promised on the shelves sometime in 2008), it provides an overview of a large number of experiments ranging from public goods to mechanism design. Camerer (2003) has a nice discussion of anomalous findings from experiments conducted in economics that might be of interest to political economists. McDermott (2002) offers an overview of particular interest to political scientists. The essays appearing (soon) in Webster and Sell (2007) address all the disciplines in the social sciences. In short, there is a wealth of intriguing work using experiments that is answering fundamental questions about cause and directing theory development.

A Sampling of Questions.

When I teach about experiments or when I get an email from someone wanting to start running an experiment, I often get similar questions. I note a number of these questions below. My responses will not please everyone, but the editors gave me this soapbox. So, I’ll use it.

Should I pay subjects? Usually, I’m not someone who insists that all subjects be paid for their performance in experiments. The point to induced value is to control for the preferences of subjects. If the experiment does not

require fixing preferences (motivating subjects) then there is no reason to insist on payment for performance. This doesn’t mean using subjects without compensating them. Their time is valuable and ought to be taken into account. But to insist that subjects be paid for all tasks is going overboard. If I’m interested in having a subject evaluate whether a person in a photograph is happy or sad, I’ll pay them for their time. However, I have no reason to want to peg their evaluation to a particular form of pay.

Should I use deception? No! But what constitutes deception? Deception has a long tradition in experiments. It involves misleading a subject about some aspect of the research so that the subject does not guess the experimenter’s intent and conform to the behavior expected by the experimenter. Deception may sometimes be necessary – after all you do not want subjects to understand that they are being tested about turnout in an election, because they may well conform to that behavior (see the very nice paper by Levine and Palfrey (2007) in which they use no deception and successfully mask their hypotheses). In economics all deception is forbidden. Reviewers are quite adamant on the point and a paper with any deception will be rejected. The arguments against deception are twofold. First, using deception will ruin the experimenter’s reputation. If subjects know that an experimenter has used deception in the past then they will be skeptical of whatever the experimenter does. They will not believe the instructions and they will try to second guess the “real” aim of the experiment. In short the experiment will have too much noise to be of value. Second, a public goods argument is made. The view is that experimental economists have pristine laboratories (no deception) and that this is a public good. If soiled by deception, then everyone who runs an experiment in that laboratory is under a

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cloud of suspicion.

I find the position on deception given by most experimental economists to be one of religious belief and moral certitude, rather than a reasoned position. Subjects coming into a lab are already suspicious of what the experimenter is going to do. Most of our experiments are seemingly abstract and leave plenty of room for doubt to creep into the mind of a subject about what we're doing. Instead, my own position has to do with the reason we pay subjects (see below). We should never deceive subjects about the link between their actions, the actions of others and their payment. If we do, then we've lost control over induced value for preferences. If a subject does not believe that their actions are mapped into promised payments, then it is impossible to draw inferences about their preferences. Beyond this, deception can be useful, especially if it is easy for the subject to guess the experimenter's hypothesis or if it is crucial for ensuring internal validity. Despite this statement deception should be avoided whenever possible. A good experimenter can always find ways to avoid deception and should strive to do so. I will not summarily recommend that a paper be rejected if the experimenter can justify the use of deception and the subject's link between actions and payments is not broken.

Mark Isaac's comment:

I'm an economist and I'm one of those deception hardliners that Rick talks about. I often have contact with subjects after they are in our experiments (many later enroll in my experimental economics class), and I am impressed by the fact that they almost always take as a given the "what they saw was what they got" nature of our experiments. I teach public goods problems in the context of borrowed bicycles and coffee co-ops in my principles of economics class, so I don't see any reason why I should not believe that a subject pool expectations problem from deception experiments is not at least hypothetically a public goods issue for experimentalists.

Actually, I see three levels of information control that could occur in an experiment, and I think that they should be considered individually. For purposes of arranging them for discussion, let's let level 1 be the most intrusive and level 3 being the least intrusive. I would put in level 1 treatments that subjects would, in the vernacular, consider lying, i.e. telling them something demonstrably false about how their profits are determined, the use of a confederate in an experiment for the deliberate purpose of lowering subject earnings, and so forth. My goal is never to do this, and I am extremely hostile to such behavior when refereeing papers, grant proposals, etc.. Category 3, on the other hand, consists of simply not telling the subjects everything that is known to the experimenter; when such withholding is crucial to the control of the experiment, when it does not pass into the condition of misrepresentation, and when it poses no psychological risk to the subjects. For example, we are under no obligation to tell one subject what another subject has earned. Legally, we may actually be prohibited from doing so. Likewise, if our theoretical model requires that a subject not know other subject's values, but know that they are drawn from a particular distribution, then that is the information condition that we must create. On things such as random draws, however, I am typically prepared to discuss with a subject after the experiment how those draws were constructed, and, if technologically possible, demonstrate that they came from the identified distribution. I'm not sure that I have had more than one or two people ever make such an inquiry.

The in-between issues of category 2, because they are boundary conditions, are more complex and more problematic. They give me pause when I see them either up close or in referees reports. These are the type of decisions that need to be carefully considered in terms of consequences and alternatives. I would put in this middle category such things as using vague language which, although not technically misleading, might be reasonably be expected to be misinterpreted by subjects. Another example would be the following. A standard recruitment language is to tell subjects that we are interested only in their decisions in the context of abstract models of economic (or political) institutions. But, some experimental researchers are now in fact interested in variations in behavior as the

experimental situation intersects with potentially sensitive demographic variables of the participants. We might want to consider whether the recruiting language in such cases needs to be modified so it is not misleading.

Should I use "real people" rather than students? A recent change in experiments involves moving the laboratory from the safety of the University (and its ready supply of undergraduate students) into the world outside the University (and using "normal" subjects). I have run numerous experiments on non-student populations well outside my usual laboratory. For example, I have run experiments on villagers in Yakutia (Siberia) and on Katrina evacuees arriving by bus at the Houston Astrodome complex. As a rule I would not recommend leaving the control (and comfort) of the laboratory. However, there are times in which it is worthwhile to move outside the University. As always, it depends on the question being asked. If there is no reason to expect that students will be any different than non-students, then why abandon the laboratory? After all, if the model predicts that a particular mechanism causes the effect and that mechanism can be instantiated in the laboratory, why bother with a sample of "normals?" Students should suffice, unless there is something systematically different about their cognitive abilities or about their demographic makeup.

When running subjects in Siberia, I was interested in enduring ethnic conflict. I also wanted a distribution across age in order to control for different degrees of experience with the transition from the Soviet period. In short, I had good theoretical reasons to expect that variation in my subjects would be an important part of my story (and indeed this turned out to be true). When I conducted experiments with Katrina evacuees I was interested in the impact of a traumatic event on individuals and how they were adjusting given their

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experiences. Students in my usual laboratory could not possibly share the same experiences.

Running experiments on subjects outside the laboratory is costly and time consuming. Don't do it unless there are good reasons. As I have already argued, seeking to satisfy both internal and external validity in an experiment usually results in neither being done well. External validity is hardly justification for wanting to run an experiment outside the laboratory. Too many things can (and will) go wrong when on the road. The question ought to dictate which kind of populations to use.

I have a conference paper due in a month and I want to run an experiment. Should I? No! If you've never designed and run an experiment, now is not the time to start. Like any technique, it is something for which you will need time. Experiments require substantial time for planning, pre-testing and re-testing. 80 percent of the work is with design. Conducting the experiment is relatively simple, once the protocol is designed. Of course there are other issues such as Institutional Review Board clearance, recruiting subjects and finding money to pay subjects, etc. It'll take much longer than you expect. My co-author and I have been planning an experiment for the last three years – we've run many pilot experiments and we're still not happy with the design. Of course, we're both distracted by too many other projects. No matter what you think, planning, designing and running an experiment will take much longer than you think – just like everything else in life.

What would Charlie do? Mark Isaac quite rightly pointed out to me that it was strange to see a piece on experiments in political economy without mentioning Charlie Plott.

Mark Isaac's comment:

Some political scientists may be confused about how the frame of these discussions flips from experimental economics to experimental

political science. There are several reasons for this, but the biggest one is probably named Charles Plott. Some readers may forget that in the 1970s, at Caltech, Charlie had parallel research programs in both political science and economics, using essentially the same experimental paradigm in each. This contributed not only to Charlie's (still flourishing) career in both areas, it also influenced a generation of colleagues, co-authors, and graduate students. The cross fertilization he pioneered has, not surprisingly, become a part of the vernacular of experimental economics and political science.

I completely agree with Mark. Charlie Plott sparked my interest in experiments. I can clearly remember reading one of Charlie's working papers from Cal Tech in 1980. I burst into my dissertation advisor's office (Lin Ostrom) and announced that I wanted to run an experiment in my dissertation based on what I had read. Lin thought that might be an interesting direction for me to take and sent me off to read more systematically. I read everything I could find by Charlie. Early in my career he graciously gave me plenty of time and advice. Even now I ask him for advice (and when I don't, I ask myself what he would do).

One of the great joys in hanging out with people who run experiments is that they are helpful to the nth degree. It is a great community. Charlie is one such exemplar. There are many others.

Should I run an experiment? I think that by now it is clear that you should run an experiment when you have the right question in mind. That question should focus on establishing cause. Despite what I have written, do not be dissuaded from running an experiment. They are fun to do and can provide great insight.

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¹ The dictator game is an experiment between two players. One is endowed with a sum of money and the other player is passive. The first player is allowed to send any amount to the second player (and keep whatever remains). The second player has no role to play, except to receive whatever is passed. Typically the play is between anonymous individuals. Students tend to send 20 percent or less to their counterpart. Adults, in a number of environments, tend to split the money evenly (see for example, Bahry and Wilson (2004) or Whitt and Wilson (forthcoming)). For a sampling of work on inequality aversion, see Bolton and Ockenfels (2000) and Fehr and Schmidt (1999).

² For an overview of this literature, see Wilson (2007).

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APPLICATION DEADLINE: FEBRUARY 28, 2007

UCLA will host the sixth annual Summer Institute on EITM: Empirical Implications of Theoretical Models this summer, 24 June through 21 July 2007. Funded by the National Science Foundation (NSF), this program seeks to leverage the complementarity between formal models and empirical methods. EITM is training a new generation of scholars to integrate theoretical models more closely, effectively, and productively with empirical evaluation of those models. The Summer Institutes are highly interactive training programs for advanced graduate students and junior faculty. They are led by teams of scholars from across the discipline who are working at the forefront of such empirical-theoretical integration.

We welcome applications from advanced graduate students who have passed all qualifying exams, preferably with a completed dissertation prospectus or plan but not yet at the writing-up stage. Graduate students will benefit most from the program if they are committed to using both theoretical models and empirical data in their dissertations. They should have some training in both formal methodology and quantitative analysis, and advanced training in at least one of these areas. We also welcome applications from junior faculty looking to improve their defended dissertation in a direction that incorporates EITM, or who are embarking on an EITM-style post-dissertation project. We will base admission decisions substantially on the quality and potential of research proposed in the application. Please note that the above describes our ideal applicants; interested students or junior faculty should not refrain from applying due to some perceived shortfall on any of these dimensions. We intend to accept about 25 participants. Applicants will be notified by email by March 31.

A complete application consists of the following four components:

- (1) **Curriculum Vita** with name and contact information, current location and position. If you are a student, the CV should indicate your current status in graduate school (year in program, whether you've passed qualifying exams, whether you've defended a dissertation proposal).
- (2) Description of your **EITM research proposal** (5-10 pages). We will base admission substantially on the quality and potential of this proposal — particularly its integration of theoretical modeling and empirical testing.
- (3) Brief (1-2 page) **statement of interest** and purpose in applying for the summer program.
- (4) **Two letters of recommendation** sent as email attachments to eitm@ucla.edu. Please ask your letter writers to place your name and "EITM" in the e-mail message's subject heading and to email the letters directly to us.

Please submit application materials as **PDF attachments** or **MS-WORD** via e-mail to eitm@ucla.edu.

Applicants will be notified of our acceptance decisions (by e-mail) by March 31.

Financial support

There are no fees or tuition. Dormitory lodging, meals and domestic travel expenses will be provided.

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EITM SUMMER INSTITUTE 2007

Content of the 2007 EITM Summer Institute:

EITM Summer Institutes organize themselves into 3 week-long modules of substantive and methodological focus. This year's fourth week will be split between a mini-module on experimental methods and participant presentations. This year's EITM VI program and faculty (as so-far committed) are:

WEEK ONE (June 25-June 29): Institutions and Institutional Analysis

Lead Lecturers: John Aldrich (Duke) & Arthur Lupia (Michigan)

This unit explores Empirical Implications of Institutional Models. It traces the origins, successful development, and potentially problematic aspects of the New Institutionalism literature, combining lectures and innovative class activities to understand modern studies of the causes and consequences of institutional choices. Activities use examples of bureaucratic performance, voter competence, Congressional organization, election laws, separation of powers, coalition bargaining, jury decision-making, political development, etc. The week also addresses (a) some constructive debates on the appropriateness to political contexts of the modern proliferation of equilibrium concepts and statistical-estimation procedures, (b) how incomplete information affects institutional efficacy, and (c) innovative data-collection methods.

WEEK TWO (July 2 - July 6): Models of Bargaining and Coercion

Lead Lecturers: Jeffrey Lewis (UCLA) & Kenneth Schultz (Stanford)

Many important political interactions involve bargaining in the shadow of costly conflict and outside the context of fixed institutional rules. Prominent examples include bargaining between states in international crises (e.g., Morrow 1989; Bueno de Mesquita and Lalman 1992; Powell 2000), between a government and a rebel group within a state (Fearon 2004), or between parties involved in political transitions (e.g., Acemoglu and Robinson 2001). In such settings, the failure to arrive at an agreement can result in war, rebellion, the imposition of sanctions, or other forms of political conflict. As a result, these interactions generally involve implicit or explicit threats, as actors attempt to manipulate the risks of conflict to their advantage and/or to signal their resolve to fight for their goals (e.g., Schelling 1960; Fearon 1995). In addition, because bargains arrived at in these contexts are generally not enforceable by outside parties such as courts, the outcomes must be self-enforcing, underpinned by credible commitments on all sides to live by the deal (Fearon 1995, 2004; Powell 2004).

Our aim during this week is to familiarize students with this class of models, their applications in international and comparative politics, and the issues that arise in testing their empirical implications. On the theoretical side, we will focus on models of bargaining and games of incomplete information, with particular attention to issues of signaling (e.g., Fearon 1997; Sartori 2001) and credible commitment (e.g., Powell 2004). On the empirical side, we will discuss different strategies for testing the empirical implications of these models, such as comparative-static analysis (e.g., Fearon 1994; Schultz 1999) and fully structural models (Signorino 1999; Lewis and Schultz 2003).

WEEK THREE (July 9 - July 13): Complexity: Computational Models and Social Networks

Lead Lecturers: Scott de Marchi (Duke) & James Fowler (UCSD)

This week will provide a practical and hands-on introduction to using computational methods, focusing on how they relate to closed-form analytical models and empirical tests. As a way of grounding the key topics in computational modeling, we will cover social network theory and the techniques used to analyze politically-relevant networks (with a substantive focus on problems such as Congressional co-sponsorships and judicial citations). A key feature of this treatment will be to demonstrate how one connects the analysis of social networks with specific hypotheses and tests on observed data. Finally, we will also provide one additional substantive unit based on the interests of guest faculty. In previous years, this has included computational models of elections, international conflict, and bargaining.

WEEK FOUR (July 16 - July 20): Testing the Implications of Theoretical Models Using Laboratory Experimental Games.

Guest Lecturer: Daniel Posner (UCLA)

In this mini-module, we will explore how laboratory-based experimental games can be used for the purpose of generating observable implications of theoretical models and subjecting them to empirical tests. The presentations will focus on the design and findings of Habyarimana, Humphreys, Posner and Weinstein's 2006 experimental study of ethnic diversity and public goods provision in Kampala, Uganda. The issues to be discussed include: why did the authors choose the experimental games that they did, and how did these games help them test competing hypothesis for why diversity undermines public goods provision? How did the authors design and implement the experiments? What kinds of data did the games generate and how were these data analyzed? At least one of the sessions will be held in UCLA's CASSEL lab, where students will get hands-on experience playing and analyzing experimental games.

EITM VI 2007 is hosted by the UCLA Department of Political Science and led by Kathleen Bawn.

THE POLITICAL ECONOMIST

INTERNATIONAL POLITICAL ECONOMY SOCIETY: CALL FOR PAPER PROPOSALS 2007

The International Political Economy Society (IPES) is now accepting paper proposals for its second annual meeting to be held November 9-10, 2007 at Stanford University. Proposals can only be submitted at <http://weblamp.princeton.edu/~pcglobal/ipes/2007proposals.php>. The abstract for each proposed paper should be sufficiently detailed to allow the program committee to assess its suitability for the conference, but should not exceed 250 words. The deadline for proposals is February 15, 2007. The preliminary program will be announced and participants notified by early May 2007. Please forward this announcement to interested colleagues and students.

The inaugural meeting of the IPES was an enormous success. The program, papers, and presentations are available online at <http://www.princeton.edu/~pcglobal/conferences/IPES/finalprogram.html>.

Reviewing the 2006 program provides a guide to the general types of papers the program committee is most likely to accept. Papers appropriate for the conference include some international component (i.e., either the independent or dependent variable must be "international" in some meaningful sense), and either focus on the politics of economic phenomena (e.g., globalization), an economic policy (e.g., monetary policy), or use economic methods to analyze political interactions.

Rationale for the Society

The IPES provides an annual forum for scholars of international political economy to present their best new work in progress to an informed and critical scholarly audience. The annual conference is centered on a small number of carefully selected papers. We are particularly interested in recruiting participants and papers from outside the field of political science. For more information, please visit our website at <http://polisci.ucsd.edu/ipes/>.

Expenses for the Conference

The IPES does not have any permanent institutional support. The 2007 meeting will be hosted by the Division of International, Comparative and Area Studies at Stanford University, which has generously agreed to underwrite some costs for the conference.

This year, we will be charging a registration fee of \$50 for faculty and \$25 for graduate students attending the conference. This fee will be waived for faculty and students of the host institution (all participants will be asked to pre-register).

The Division of International, Comparative, and Area Studies will provide breakfast and lunch on Friday and Saturday of the conference. The Center for Globalization and Governance at Princeton University, host of the 2006 conference, will also host a dinner for all participants on Friday evening. Please plan to attend these meals as they are an important opportunity for networking within the conference.

All participants are expected to pay their own travel and hotel expenses. We understand that travel funds are limited for everyone. We hope that the high quality of the meeting and discussions will make this a worthwhile expense. The Division of International, Comparative, and Area Studies at Stanford has contributed limited monies to help defray hotel expenses for those attendees who do not have travel funds of their own. Priority in distributing these funds will be given to graduate students and junior faculty presenting papers at the meeting. If you wish to be considered for these travel funds, please indicate this at the end of your proposal abstract.

Information about registering for the conference and logistics (i.e., hotel reservations) will be forthcoming in May 2006 with the preliminary program and posted at our website <http://polisci.ucsd.edu/ipes/>.

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THE POLITICAL ECONOMIST

UPCOMING AWARDS ANNOUNCEMENT APSA Political Economy Awards Committees, 2007-2008

William H. Riker Award: The William H. Riker Award is given for the best book on Political Economy. One copy of each book should be sent to each committee member by **March 1, 2007**.

Award Committee Chair

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Mancur Olson Award: The Mancur Olson Award is given for the best dissertation completed and accepted in the previous two years. One copy of each dissertation should be sent to each committee member by **March 1, 2007**.

Award Committee Chair

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Michael Wallerstein Award: The Michael Wallerstein Award is given for the best published article in Political Economy at the APSA meeting. One copy of a nominated article should be sent to each member of the committee by **March 1, 2007**.

Award Committee Chair

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