What Mechanism Design Theorists Had to Say About Laboratory Experimentation in the Mid-1980s[†]

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Abstract

Thanks to the recent studies of the history and philosophy of experimental economics, it is well known that around the early 1980s, experimental economists made a case for the legitimacy of their laboratory work by emphasizing that it was a nice and indispensable complement to mechanism design theorists' mathematical study of institutions. The present paper examines what mechanism design theorists thought of laboratory experimentation, or whether they were willing to form a coalition with experimental economists circa the mid-1980s. By exploring several dimensions of the relationship between mechanism design theory and experimental economics, the present paper shows that a close rapport had been established by the early 1980s between the representative members of the two camps, and also that mechanism design theorists were among the strongest supporters of laboratory experimentation in the economics profession in the mid-1980s.

Recently some light has been shed on the relationship between mechanism design theory and experimental economics (Starmer 1999; Lee 2004; Santos 2010).[1] As a result, it is well known that around the early 1980s, experimental economists made a case for the legitimacy of their laboratory pursuit by highlighting that it was a nice and indispensable complement to mechanism design theorists' mathematical study of mechanisms (Plott 1979; Smith 1980a, 1982; Wilde 1981). Of course, legitimation of a scientific activity, such as laboratory experimentation, cannot be achieved exclusively by its practitioners in a solipsistic manner; rather it requires a collective, community-level action involving not only its practitioners, but other members of the relevant community. One may well, therefore, wonder what mechanism design theorists thought of laboratory experimentation, or whether they were willing to form a coalition with experimental economists in the late-1970s through the first half of the 1980s. This issue deserves attention because mechanism design theorists, as high theorists in the highest echelons of the economics profession were in such a position as to exert heavy influence upon the rest of the profession and the new generation of economists (Leijonhufvud 1973; Kreps 1990, chap. 18; Fudenberg and Tirole 1991, chap. 7; Milgrom and Roberts 1992; Mas-Collel et al. 1995, chap. 23).

The objective of the present paper is to provide a reasonable construal of the relationship between mechanism design theory and experimental economics. In particular, it aims to assess whether the relationship in question was reciprocal, or there was only one-way route running from experimental economics to mechanism design theory. To state the conclusion first, representative mechanism design theorists thought of the relationship as mutually beneficial, and

[1] The present paper does not aspire for comprehensive treatment of the entire gamut of experimental economics. Instead, the focus is concentrated upon the version of experimental economics developed by Vernon Smith, Charles Plott and their collaborators.

hence were willing to forge alliances with experimental economists. In detail, the present paper concludes that a close rapport had been solidly established by the early 1980s between central figures of experimental economics and main contributors to mechanism design theory, and that the latter were among the strongest supporters of laboratory experimentation in the economics profession circa the mid-1980s.

The steps towards this conclusion the present paper takes are as follows: Section1 is devoted to depicting the emergence of the network linking together the later-to-be pioneers of experimental economics and those of mechanism design theory over the period of the mid-1950s through the 1970s. In section 2, attention shifts towards the research outputs mechanism design theorists produced during the period of the early 1970s through the mid-1980s, with a view to examining whether mechanism design theorists then found laboratory experiments relevant and meaningful to their research practice. One sees in that section that the answer is in the affirmative. Also in section 2, it is shown that mechanism design theorists and experimental economists share the same conceptual framework by means of which to structure their research activity. Sections 3 is intended to offer a plausible answer to the question of what mechanism design theorists had to say about laboratory experimentation in the mid-1980s. It is therein ascertained that representative mechanism design theorists then explicitly endorsed the legitimacy, importance and social usefulness of laboratory experimentation, thereby lending strong support to experimental economics. And section 4 concludes.

1. Emergence of the Network Linking Together Mechanism Design Theory and Experimental Economics

In order to see how the scholarly network encompassing central players of mechanism design theory and those of experimental economics came into being, one may well first zero in on the US Midwest in the first half of the 1950s. Through the serendipity of academic McCarthyism in that region initiating a chain reaction leading up to an academic entrepreneurial activity at Purdue University, protagonists of our story came to share the same intellectual atmosphere over a decade from the mid-1950s. During the second half of the 1960s, they began rapidly dispersing, later to form central forces underlying the development of mechanism design theory and experimental economics.

1.1. Emergence of the Purdue Economics Program

In the early 1950s, the McCarthyist Red Scare struck hard at the College of Commerce and Business Administration at the University of Illinois (see Solberg and Tomilson 1997). In its aftermath, the economists identified as 'Keynesian' (for instance, Leonid Hurwicz, Don Patinkin and Franco Modigliani) left for new places. Among them was Emanual T. Weiler, who left Illinois for Purdue, which hired him to head its economics department.

At Purdue, the Department of Economics was separated from the Department of History, Economics and Government in 1953 when Weiler arrived there. He was expected to create a new economics department oriented towards 'scientific' research, and he was happy to do so. Being sure that the economics profession would soon take a new direction, Weiler made a vigorous attempt to recruit a series of young economists (see Davis 1998, 7), with the result that Edward Ames, Lance Davis, George Horwich, Jonathan Hughes, James Quirk and Rubin Saposnik came to Purdue in the 1950s. Also, he was successful in hiring a future Nobel laureate, Vernon Smith,[2] and Stanley Reiter,[3] the latter of whom Hurwicz was mentoring (Smith 2008, 230).

Not only was Weiler successful in hiring a group of young economists “who would never have come to Purdue, had theory or history been in fashion at the time they got their degree” (Davis 1998, 7), but he also successfully maintained a good connection he had established in Illinois. Hurwicz, Modigliani and Jacob Marschak made frequent visits to Purdue to participate in the seminars held there. As a result, Purdue produced a vibrant intellectual atmosphere in the second half of the 1950s, and managed to attract good graduate students. An interesting snapshot of the intellectual environment at Purdue in the early 1960s provided by Morton Kamien (then an

[2] Smith received a formal undergraduate education in physics and electrical engineering at the California Institute of Technology (Caltech) in the second half of the 1940s. After receiving his MA in economics from the University of Kansas in 1952, he moved to Harvard with the intention to work with Wassily Leontief. In 1955, he received his PhD from Harvard, and became assistant professor at Purdue.

[3] Reiter initially studied economics at Queens College, from which he graduated in 1947. From 1948 to 1950, he worked for the Cowles Commission for Research in Economics. After receiving his MA from University of Chicago in 1950, he moved to West Coast. At Stanford University, he got involved in a research project sponsored by the Office of Naval Research in 1950 through 1953, and worked as research associate for the Applied Mathematics and Statistics Laboratory in 1953 through 1954. Reiter worked with David Blackwell and Kenneth Arrow at Stanford (Golosinski 2007). He came to Purdue as an assistant professor in 1954, and subsequently received his PhD from University of Chicago in 1955.

economics graduate student) is worth quoting at some length to get a vivid picture of what was going on there:

> That winter [the winter of 1960] I attended a Purdue Quantitative Institute Seminar for the first time. ... Abba Lerner, Franco Modigliani, Jerome Rothenberg, Hirofumi Uzawa, Jacob Marschak, John Chipman, and Martin Beckmann, were there. The conference didn't begin until after a short man with a long, old coat and a very full briefcase arrived. I heard someone say, "Leo [Hurwicz] is here, we can start now." … I believe Jim Quirk and Rubin Saposnik presented their paper on stochastic dominance at that time, and there was talk about Uzawa having found a more direct method of proving one of their results on his flight out from California.
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> Each speaker had about an hour and a half to present his paper. Leo asked a lot of "clarifying" questions. … Jacob Marschak, with a few very simple, straightforward remarks, would clarify what was going on. The speaker would resume, and soon Leo had another clarifying question and the process would repeat itself.
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> … There was also Vernon Smith, with a motorcycle jacket and blond hair combed back in a D.A. style that reminded me of New York, but the accent wasn't right. The lesson [of the Seminar] was that not all the problems of economics had been solved and that even the great men didn't always agree. (1981, 367)

Within this intellectual milieu, several later-to-be distinguished economic theorists received their graduate training at Purdue, such as Hugo Sonnenschein (PhD in 1964), Nancy L. Schwartz (PhD in 1964), Morton Kamien (PhD in 1964), Thomas Muench (PhD in 1965), and “one of our [its] star graduate students” (Smith 2008, 223; see also Davis 1998, 8), John O. Ledyard (PhD in 1967).

In 1963, Smith (1992, 275) first offered his experimental economics workshop seminar at Purdue. Until 1967 when he left for Brown University, his workshop seminar had been offered once a year. It is far from easy to measure how seriously his graduate students at that time took his seminars; yet, there is no question that those graduate students at Purdue in the 1960s were some of the first economists ever exposed to Smith’s laboratory endeavors.

And it is worth mentioning that Hurwicz played a substantive role in invigorating the ‘spirit’ at Purdue, although he was not an official faculty member there. The Weiler-Hurwicz connection established in the 1940s in Illinois was instrumental in having Hurwicz try his new ideas at Purdue in the 1950s and 1960s. Under his influence, some of its faculty members and graduate students got seriously interested in the issue of mechanism design. For instance, Edward Ames, who once remarked that Hurwicz had been “all along, an honorary member of the department” (Ames 1981, 358), became very interested in Hurwicz’s approach after realizing that the Soviet-type economic systems, which was then Ames’ research interest, could be studied within Hurwiciz’s conceptual framework (358). And according to him, the “new theory of economic systems”—what is nowadays called mechanism design theory—“began to emerge in the early 1960s, notably in the Purdue seminars (referred to locally as ‘the Strange Ones’)” (358).

Needless to say, the Purdue economics program had another route along which the idea of mechanism design could spread, Stanley Reiter. In fact, prior to concentrating on the development of mechanism design theory in collaboration with Hurwicz, Reiter made a significant contribution to the birth of cliometrics at Purdue. In the mid-1950s, he began collaborating with Purdue economic historians, Lance Davis and Jonathan Hughes (see Hughes 1981, 361-362) to produce a series of early, landmark papers in cliometrics (see Hughes and Reiter 1958; Davis, Hughes and Reiter 1960, 544). Jay Wiley (1982), a colleague of Reiter's in the 1950s through the 1960s at Purdue, described the intellectual environment at Purdue as follows:

> We became extremely well known in economic history. The expression 'Cliometrics' … was coined by one of the Purdue Staff [Reiter] and was widely known among economists as referring to the development of quantitative economic history that started out at Purdue University. Under the leadership of Lance Davis and Jonathan Hughes, there were regular, several-day-long colloquiums held here with distinguished economic historians from all over the country. Later on, these were replaced by similar sets of meetings for researchers in econometrics and mathematical economics headed by Stanley Reiter of Purdue and Leon Hurwitz [sic] of the University of Minnesota. (4)

As one can see from the final part of this quote, Reiter and Hurwicz were squarely located at the center of the economics research being conducted at Purdue in the 1960s. Indeed, Reiter was considered by his colleagues at Purdue a "far-out guy" (Ames 1981, 360) and "still water that runs deep" (Smith 2008, 230). Of course, he exerted huge influence upon his students through, among other things, his 'Socratic' teaching of the fundamentals of economic theory (see Ledayrd 1995, xi; Smith 2008, 230).

Around the mid-1960s, the Purdue economics program was indeed filled with a distinctively modern flavor. And Charles Plott (another, later-to-be pioneer of experimental economics)—who the Purdue economics faculty viewed as “likely to do important original work, not potboiler stuff’ (Smith 2008, 224)—made an appearance on the scene of the Purdue economics program in 1965.[4] Over the period of 1965 through the first half of the 1970s, Smith and Plott spent together numerous hours informally discussing, among other things, Smith’s experiments on their bass fishing trips; that not only had Plott consider entering a new territory of research, but helped Smith maintain his interest in laboratory experimentation (275; Smith 1992, 277), as is widely known in the community of experimental economists as folklore. So, the second half of the 1960s was the time when one of the most important intellectual partnerships in the history of experimental economics was formed.

1.2. Collapse of the Purdue Economics Program and Its Unintended Consequences

Be that as it may, it was also during this period of time that the Purdue economics program (almost) completely collapsed. Young, research-oriented faculty members began leaving Purdue around the mid-1960s for certain reasons (see Davis 1998, 7-10). Most of all, they became famous enough to get nice offers from many different ‘good’ places.

1.2.1. Migration to Caltech

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[4] Plott received his BS in production management and his MS in economics from Oklahoma State University, respectively in 1961 and 1964. He was granted a PhD in economics from University of Virginia in 1965.

So, in 1968, Davis left for the Division of the Humanities and Social Sciences at Caltech. He was instrumental in having Quirk and Plott join in 1971 his Division at Caltech (Davis 1998, 13), which was then “anything but a first-rate place” (11) in social sciences. Soon after their arrival, Davis and his two, erstwhile Purdue colleagues—collectively called “the Purdue Mafia” (Oliver 1988/1990, 30, 31)—began attempting to steer the Division’s social sciences into much “more technical and more formal” (Davis 1998, 16) direction, with the help of Roger Noll and David Grether who arrived at Caltech in the second half of the 1960s and in 1971 respectively. To this end, they hired John Ferejohn, Morris Fiorina, Michael Levine and W. David Montgomery in 1972; Robert Forsythe and Forrest Nelson in 1975; Louis Wilde in 1976.

In 1972 through 1974, Plott vigorously pursued laboratory studies at Caltech. In that period, his “first four experimental papers” (Plott 2001, x) was produced in collaboration with his Caltech colleagues (see Levine and Plott 1977; Fiorina and Plott 1978; Plott and Levine 1978), and also with Smith (Plott and Smith 1978) who was visiting Caltech in 1973-1974 as a Sherman Fairchild Distinguished Scholar. What is worth noting here is that in that period, Plott’s main “research objective was essentially the same as today’s mechanism [design] theory, only the tools were more closely related to axiomatic social choice theory than to non-cooperative game theory” (Plott 2001, xii). It goes without saying that Plott and his collaborators’ experiments in 1972-1974 were part of their studies of mechanism design or institutional design.

“Slight changes in procedures and organization,” said Plott as early as 1976, “make enormous changes in the outcome” (Staff 1976, 6). It was widely known at Caltech circa the mid-1970s that he was building “an experimental methodology for examining the impact of subtle changes in rules, procedures, and modes of organizations” (6) in collaboration with his colleagues. Indeed, around the mid-1970s, “Charlie, Mo Fiorina—later Roger Noll and John

Ferjohn—and others [at Caltech] were in the process of creating experimental public choice" (Smith 2008, 278), in which comparative institutional analysis and institutional design held a central position. In the second half of the 1970s, Plott (1979) did not hesitate to highlight the importance of what he called "institutional engineering" (139)—building new, "synthetic" (139) institutions with a view to accomplishing certain pre-specified goals. As one can see from the description of the Division's research activities by its then acting chairperson, Rodman Paul, this applied research tightly linked to the theme of mechanism design garnered widespread attention at the Division:

> A major milestone in social science is the establishment of the Program for the Study of Enterprise and Public Policy. The theme of the new program is to develop the conceptual foundations for a new science of institutional design. Research projects in the program will apply the methods of economic theory, analytical political science, and group experiments … to the problem of designing more effective decisionmaking procedures and organizations, with particular emphasis on institutions at the interface between the private sector and government. The program will become the focal point of applied research in social science at Caltech. Primary responsibility for developing the program and acquiring financial support for it is shared by Professors Charles Plott, who is the director of the program, and Roger G. Noll. (California Institute of Technology 1979, 68)

That institutional engineering and its close cousin, comparative institutional analysis, became a central research topic for Smith as well is manifest in their coauthored paper (Plott and

Smith 1978), an outcome of their collaboration at Caltech.[5] In 1967, Smith left Purdue for Brown University, and the following year, left Brown for the University of Massachusetts at Amherst (UMASS-Amherst). Due to the ‘turmoil’ at the UMASS-Amherst (see Lee 2004, 241-243), he drifted around the next four years. In any case, when Smith arrived at Caltech in 1973 as a Sherman Fairchild Distinguished Scholar, he had already been very familiar with mechanism design theory and other studies on institutions.[6] And while at Caltech, he and Plott “discovered together a new significance for institutions” (Smith 1992, 277).

1.2.2. Migration to Arizona

In 1974, having a joint appointment at Caltech and the University of Southern California, Smith (2008) set out to conduct an array of experiments “that would lead to a series of papers testing

[5] At the outset of their paper, they cited Hurwicz’s (1973) Richard Ely Lecture, and stressed that in the laboratory, one could perform empirical studies on the mechanisms proposed by mechanism design theorists (Plott and Smith 1978, 133).

[6] One may well conjecture that at Purdue, he must have been exposed to Hurwicz’s and Reiter’s ideas about mechanism design theory. Aside from this conjecture, we have hard evidence showing that he was well aware about the contemporary discussion of institutions in economics. In the academic year 1969-1970, Smith and his UMASS-Amherst colleague, Arthur Wright, provided together a seminar course in economic theory. In that course, they carefully reviewed the papers by Ronald Coase, Harold Demsetz, Leonid Hurwicz, Thomas Marschak, Otto Davis and Andrew Whinston, and also provided a careful analysis of Hurwicz’s (1960) classic paper in mechanism design theory. See Lee (2004, 248) for the details.

the incentive properties of various public good mechanisms” (279); the next year when he moved to Arizona, we was ready to report the outcomes of his laboratory studies on public goods provision mechanisms.

Smith’s papers on public goods provision mechanisms published in the second half of the 1970s through the first half of the 1980s (Smith 1977a, 1979, 1980b; Coursey and Smith 1984) are all the more important because one can clearly see therein the link between mechanism design theory and Smith’s laboratory study. As acknowledged in Smith (1979), it was Reiter who first introduced Smith to “the path-breaking work of Groves and Ledyard (1974) before the first draft of their work was available for distribution” (119). And Theodore Groves and John Ledyard—who were then at Northwestern along with Reiter—helped Smith better understand their mechanism and conduct experiments on it and other mechanisms (119). In March of 1976, in the course of presenting his paper (Smith 1977b)—which was “derived from an ongoing laboratory experimental study of incentive compatible mechanisms for group choice of a public good” (97)—Smith unreservedly and enthusiastically spelled out his ‘vision’ for his audience (including Gordon Tullock) as follows:

> I’m proposing it [the laboratory experimental study of incentive compatible mechanisms for public goods provision] perfectly seriously (i.e., as a subject for serious study). And remember, I said this was just a start. Gordon [Tullock], use your ingenuity! Take the problem that you’re talking about, your objection to this, and see if you can fix this up and make it work. The point is
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> *(Tullock interrupts and is overridden)*

> that the public good problem is not insoluble. I think the mechanisms that we're talking about open up the possibility of an entirely new way of thinking about these problems.
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> *(Tullock agrees)*
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> And I have no idea what that's going to lead to in 20 or 30 years. It may turn out in retrospect that this kind of stuff is really not any good at all in terms of handling some problems but it is too early to make that judgment. The heritage that comes down from 1955 is that the public good problem is insoluble and so everybody just throws up his hands and says, 'Well you know you have to have some kind of imposed solutions when you have public goods.' I think now we see that this is not necessarily correct and, I think, when economists and others begin to think more in terms of the design of mechanisms and the design of process it could very well make a difference in the shape of future societies. (Smith in Auster and Sears 1977, 267)

As is widely known, it was in 1975, when he found a new institutional home at the University of Arizona, through 1985 that Smith successfully established his version of experimental economics, and built his own network of laboratory researchers—see Smith (2008, chap. 13) for the details. Within this period of time, "test bedding became an integral part of a much larger program in economic system design" (296); namely, the use of laboratory experiments with a view to examining the workings of new, synthetic institutions (or mechanisms) proposed to accomplish certain normative goals or to solve certain policy-related problems became a distinctive feature of his (and Plott's) version of experimental economics in 1975 through 1985. During this period of time, he pushed, with zeal and devotion, the frontier of laboratory knowledge concerning how to build such mechanisms as to be conducive to the improvement of resource allocation.

All in all, it is fair to say that in the late-1960s through the early 1980s, two erstwhile colleagues at Purdue, Plott and Smith, successfully managed to create two of the most important centers of experimental economics, with institutional design or economic system design taken as their core research topic. By the way, what was Reiter, whom Smith used to call the 'still water that runs deep,' doing during the same period of time?

1.2.3. Migration to Northwestern

In 1966, prior to the departure of Smith and Plott, a pioneer of cliometrics, Hughes, left Purdue for Northwestern University. The next year, he was delighted to see Reiter join Northwestern. Reiter was then jointly appointed to departments of economics, mathematics, and the business school (Sedlak and Williamson 1983, 135; Reiter 2003, 449), soon to find himself spending most of time renovating its business school and economics department (see Reiter 2003, 449).

Since the late 1950s, the business school education in the US had been under severe criticism. Some of its critics (Gordon and Howell 1959; Pierson 1959) claimed students at the business school had to be so educated that they could have a good command of analytical tools like those developed in operations research and game theory. The criticism of this sort, in addition to the financial constraint at Northwestern, had a deep impact upon the business school of Northwestern (Golosinski 2008, 122), so much so that in 1966 when it became quite evident that "graduate business education was the emerging trend" (127), the business school of Northwestern decided to remove its undergraduate education entirely, and instead to concentrate solely upon the graduate education (131). The Managerial Economics and Decision Sciences (MEDS) Department was created within the business school, and Reiter was appointed as its chair (132) in 1968, a year after Reiter's arrival at Northwestern.[7]

In an effort to build a new faculty for a new program, Reiter made contact with his erstwhile colleagues and students at Purdue as well as other mathematically-oriented economists, and some of them responded in a positive fashion. In 1968, David Baron came to Northwestern; the next year, Mark Walker, who would receive his PhD from Purdue in 1970, joined the MEDS. Three Purdue PhDs who had been teaching at Carnegie Mellon University (Kamien, Schwartz and Ledyard) moved to Northwestern in 1970 "after considerable coaxing by John Hughes and Stan Reiter" (Kamien 1998, xviii). The following year, the Center for Mathematical Studies in Economics and Management Science (CMS-EMS), "the wonderful brainchild of Stanley Reiter" (Ledyard 2003, 83), was created at Northwestern; Kamien was substituted for Reiter as chair of the MEDS; Reiter was appointed as director of the CMS-EMS, the position he held until his retirement in 2007. Throughout the 1970s, with the strong support from the central administration of Northwestern (Golosinski 2008, 143), Reiter, Kamien and Schwartz kept recruiting young mathematical economists whom Kamien rightly portrayed as "'very theoretical people'" (Kamien in Golosinski 2008, 143). Donald John Roberts came to Northwestern in 1971; Theodore Groves and Mark Satterthwaite in 1972; Sonnenschein in 1973; Ehud Kalai in 1975; Roger Myerson in 1976; Bengt Holmstrom, Paul Milgrom and Robert Weber in 1979.

[7] And the following year, the business school of Northwestern was renamed the Graduate School of Management "to reflect its exclusive graduate orientation" (Golosinski 2008, 132).

In the 1970s through early 1980s, the MEDS was a leading hothouse of mechanism design theory (Bowmaker 2012, 338), in which tools and concepts developed in social choice theory and game theory were routinely employed, and in which a variety of resource allocation processes (such as auctions) were mathematically analyzed and compared with one another. In fact, mechanism design theory was "not mainstream economics then" (321); in the post-1980s period, however, "the mainstream moved to us in large part as a result of the research we did," said Milgrom (321) who was at the MEDS from 1979 to 1983. As a result, the theoretically-oriented economists affiliated with the MEDS and the CMS-EMS at Northwestern in the 1970s through the early 1980s became "a virtual who's who of mechanism design, auctions, and applied game theory" (321), and one cannot but agree that the MEDS "was a very special place and became responsible for much of the influence of game theory on economics" (321).[8] Of course, it was Reiter—"'the intellectual godfather of the research culture at the Kellogg School'"[9] (Robert Magee in Golosinski 2008, 240) who continued his collaboration with Hurwicz (see Hurwicz and Reiter 2006)—that played a foundational role in the creation of this remarkable place.

2. Conceptual Dimensions of the Relationship between Mechanism Design Theory and Experimental Economics

[8] See the CMS-EME discussion papers in the 1970s and the first half of the 1980s, available at http://www.kellogg.northwestern.edu/research/math/discussion/index.htm (last accessed on Oct. 2, 2013).

[9] The Graduate School of Management at Northwestern was renamed Kellogg School of Management in 1979 (Golosinski 2008, 189-195).

The previous section has shown how the Purdue economics program emerged in the 1950s, later in the second half of the 1960s to disintegrate into two centers of experimental economics and one center of mechanism design theory. Although there is no question that those three centers had in common that institutional design was a central theme of research, it is not clear whether mechanism design theorists (at Northwestern) were indeed taking the laboratory study seriously in the 1970s and the first half of the 1980s. Since the main focus of the present paper is concentrated upon the question of whether they were willing to strike alliances with experimental economists circa the mid-1980s, it is obligatory to check over their research outputs to see whether they indeed heeded laboratory experiments, before moving on to a discussion of the underlying conceptual linkage between the two camps.

2.1. Mechanism Design Theorists at Northwestern Paying Attention to Laboratory Experiments

In the 1960s, Smith's interest in laboratory experiments and the issues relevant thereto resulted in a series of publications (see Smith 1991, part I); circa the early 1970s, however, he was making a pause partly because of the indifference of the economics profession towards his laboratory work (see Plott 2001, xi). Be that as it may, among the very few economists showing interest in Smith's laboratory work, were his erstwhile students at Purdue, Kamien and Schwartz. In the course of their discussion of a bidding model of preference revelation for public goods, Kamien and Schwartz (1970, 21) paid attention to Smith's (1966, 1967) papers—one theoretical and the other experimental—on bidding behavior. Another bidding model was being developed by their colleague at Northwestern, Baron, in an attempt to shed light on incentive contracts. And he was also appreciating Smith's theoretical and experimental work (Baron 1972, 385).

As mentioned in section 1, Kamien accompanied Ledyard on his move to Northwestern in 1970. It was also Kamien who first helped Ledyard become familiar with the issues stemming from public goods (Ledyard 2003, 83). At Northwestern, Ledyard collaborated with Groves, to produce the Groves-Ledyard mechanism for pubic goods provision (Groves 2003, 65). The CMS-EMS discussion papers # 119 (dated December 1, 1974) and # 144 (dated March 1976) are previous versions of their 1977 paper (Groves and Ledyard 1977). One cannot find any indication of interest in laboratory experimentation in their discussion paper # 119, on the one hand; on the other, in their discussion paper # 144, one can. They therein mentioned "the logical next step in the theoretical development of the mechanism would be to formulate an explicit adjustment process" (45), subsequently pointing out that Smith performed laboratory experiments upon such an adjustment process and his experiments produced certain nice results (45, see also Groves and Ledyard 1977, 783). The list of Northwestern economists having shown interest in Smith's laboratory studies on public goods provision mechanisms does not stop here. So did Kalai and Robert Rosenthal (CMS-EMS discussion paper # 215 dated April 1976; see also their 1978), and Roberts (CMS-EMS discussion paper # 264 dated January 6, 1977; see also his 1979a, b).

On June 27-July 1, 1977, the first European Summer Workshop of the Econometric Society was held in Paris "to bring together specialists in social choice theory and economists working on incentive problems" (Laffont 1979, vii). In this workshop, Muench and Walker presented their paper on the free rider problem. According to them, economists hardly came to grips with "the *actual* behavior that people would follow" (Muench and Walker 1979, 83, emphasis in original) when faced with specific rules of the game, with the result that its aggregate social outcomes could rarely be pinpointed. Muench and Walker claimed laboratory experimentation could serve the purpose of filling up this lacuna, without forgetting to indicate who was then attempting to do so:

> This is an area in which the science of economics must develop an experimental component, and in which theoretical and experimental work must complement one another. Theorists must determine just what differences in behavior can lead to differing consequences, and experimentalists must determine what kind of behavior people can be expected to follow in various classes of economic situations. Experimental work on these kinds of questions is being pioneered by, for example, Vernon Smith …; we hope that he will be followed by other economists. (83)

In 1979, *Review of Economic Studies* published ten papers that had been presented at its symposium on incentive-compatible mechanisms. Among them was Groves' (1979a), and he therein made an interesting remark on the relationship between the Nash equilibrium concept and laboratory experimentation (Groves 1979a, 239; see also his 1979b, 42). Since it reappeared in a more detailed form in Groves (1982), one may well focus on the latter only.

In the 1970s, the use of the dominant strategy equilibrium concept resulted in a proliferation of impossibility results in the incentive compatibility literature, thereby arousing interest in alternative, weaker equilibrium concepts. Against this background, the Nash equilibrium appeared on the central stage of mechanism design theory, to prove useful in producing some possibility results (Postlewaite 1985, 222; Groves 1982, 6-7). Yet, mechanism design theorists were well aware that it would produce a "severe conceptual problem" (7) for their work. Incomplete information being the rule, not the exception in multi-agent interactions, a reasonable criticism of the Nash equilibrium was such that "no player can compute, in one step, even his[/her] own Nash equilibrium strategy" (7) in most situations.

Mechanism design theorists suggested, according to Groves (1982, 7-8), two approaches to handle this conceptual problem. First, some opined that a Nash equilibrium could be viewed as an equilibrium of certain iterative procedures. In this view, every agent is assumed to announce his/her act (message) in an iteration, and to decide whether or not to revise his/hers in the next one. Of utmost importance are, hence, the following two questions: Does the iterative process in question reach any equilibrium at all? Should an equilibrium be reached, does it coincide with a Nash equilibrium? As of 1980, theoretical investigations had not yet shed much light on these two questions; by contrast, laboratory studies had produced some noteworthy results. "Although the theoretical results are sparse," Groves (1982, 8) said, "a variety of experimental results of Smith and others, cf. Smith (1979), suggest that a (simple) Nash equilibrium is often the outcome of such iterative procedures."

In the late 1970s and the early 1980s, laboratory experimentation had considerable appeal for mechanism design theorists because it looked as if experimentalists could conduct, on behalf of them, a reality check on their inchoate, theoretical ideas. Of course, it was just one of the reasons why they found laboratory experimentation appealing during that period of time. As one can see below, another reason was that it seemed as if experimental economists were ready to provide them with certain empirical regularities, to the explication of which to apply their modeling techniques.

Most mechanism design theorists circa the early 1980s were in agreement, according to Groves (1982), that another way to handle the conceptual problem with the Nash equilibrium was simply "to reject the (simple) Nash equilibrium concept in favor of the Bayesian [Nash] equilibrium solution concept for games of incomplete information" (8). In the 1980s, John Harsanyi's (1967/1968) formulation of Bayesian games of incomplete information emerged as a predominant format within which to tackle the mechanism design problem. And among those taking Harsanyi's Bayesian framework very seriously, was Satterthwaite (2003, 370) at the MEDS. In February 1983, he had his coauthored paper with Thomas Gresik listed as the CMS-EMS discussion paper # 551.[10] The objective of their paper was to offer, within the Bayesian framework, a theoretical model capable of making sense of the empirical "[e]vidence from controlled experiments with double auctions that Smith (1982, Proposition 5) … summarized" (1)—an empirical regularity that "five or six traders on each side of the market is enough to generate essentially competitive, *ex post* efficient outcomes" (1). Namely, an empirical regularity established in the laboratory was taken by a well-known mechanism design theorist and his collaborator at Northwestern as an important *explanandum* for their theoretical inquiry.

This section has so far provided a series of illustrations, which indicate that mechanism design theorists (at Northwestern) paid considerable attention to laboratory experiments in the 1970s through the first half of the 1980s. Of course, these illustrations are far from exhaustive, and the illustrations of this sort, however thorough they are, cannot substitute for a discussion of

[10] This working paper titled "The Number of Traders Required to Make a Market Competitive: The Beginnings of a Theory" has once been considered one of the best examples demonstrating potential of the so-called 'revelation principle' within Harsanyi's framework (Wilson 1987, 36).

the conceptual, common denominator shared by the two camps. The rest of section 2 is devoted to that end.

2.2. Mechanism Design Theorists Sharing the Same Conceptual Framework with Experimental Economists

A detailed portrayal has been presented elsewhere (Lee 2006) of the unifying conceptual framework that underlies mechanism design theorists' mathematical studies. Therefore, what is provided below is just a brief, informal discussion about it, which is followed by a description of the 'representations' that experimental economists make use of to structure their laboratory studies.

2.2.1. Mechanism Design Theorists' Conceptual Framework

In mechanism design theorists' conceptual framework, an 'environment' denotes a collection of what are to be taken as given. Usually, preferences, technology and initial endowments are considered its elements. In contrast, a 'mechanism' or 'institution' refers to a set of what are to be built with a view to achieving certain normative goals. The mechanism consists of (a) the strategies available to each agent, (b) the rules governing the interactions among the agents, and (c) the outcome-determining rules given the set of strategies chosen by the agents. The mechanism is, accordingly, a game-form in Allan Gibbard's (1973) sense—namely, "a game with no individual utilities yet attached to the possible outcomes" (588-9)—or rules of the game.

In the early 1970s, mechanism design theorists began tackling the issue of incentive compatibility by making use of non-cooperative game theory. In the incentive-compatibility literature, agents are assumed to behave strategically where uncertainty prevails, and their behavior is considered a “descriptive phenomenon” (Groves and Ledyard 1987, 57) concerning which models should be developed. The Bayesian Nash equilibrium concept has risen to predominance in this literature, and each agent, taking full cognizance of the mechanism, is presumed to behave in the Bayesian Nash manner. Bayesian Nash equilibrium strategies are translated into the final outcomes by the outcome-determining rules, and mechanism designers examine whether the predetermined normative goals are accomplished. Or, they compare the performances of different mechanisms to figure out which ones are better at fulfilling the desiderata.

2.2.2. Experimental Economists' ‘Representations’

Once attention is directed to the similarity between how mechanism design theorists conceptualize their objects of theoretical inquiry and how experimental economists represent those of laboratory study, one can come to grips with the tight relationship between the two camps. Smith and his collaborators have once emphasized that the “world” could be brought into the laboratory only after being filtered, processed and tamed by certain “*representations*” (Smith, et al. 1991, 197, emphasis in original). Then, what are their representations like?

Their representations are best captured in Smith’s (1982) “microeconomic system theory” (924), in which the microeconomic system consists of an environment and an institution (924-

6).[11] His microeconomic system theory specifies the environment (the institution) in a manner analogous to the way mechanism design theorists' conceptual framework characterizes it, so much so that the components of the environment (the institution) in the former are the same as those in the latter. Indeed, the elements of the environment in Smith's microeconomic system theory are "agent values, costs, resources, knowledge" (Smith 1989, 157, Figure 1); those of the institutions in it are what characterize rules of the game, such as "language of market, rules of communication and of contract, extensive form structure" (157, Figure 1).

What experimental economists do with their representation and human subjects in the laboratory can be explained as follows: First, they strive for control over the environment and the institution (Smith 1982, 930). Second, they "observe and measure the message responses of agents" (930) to test and improve on mechanism design theorists' behavioral models. Third, they measure and compare the aggregate outcomes resulting from the agent behavior within different mechanisms or institutions (930); in other words, they engage in institutions design and comparative institutional analysis.

To sum up, experimental economists use the same way to represent their objects of inquiry that mechanism design theorists do. The environment and the institution are the things to be controlled by experimental economists in their laboratory. Concerning the strategic behavior of agents, whose modes of interactions are specified beforehand by the mechanism, mechanism design theorists provide models mainly by relying on game theoretic tools and concepts. In order to examine the validity of mechanism design theorists' models of agent behavior, experimental economists collect and record within their laboratory the details of experimental subjects'

[11] See Lee (2004, 197-199) for an argument that the way Plott represents his objects of laboratory study resembles Smith's very closely.

behavior. They are interested not only in how agents (subjects) behave but in the aggregate outcomes mechanisms (institutions) turn out, in the similar fashion that mechanism design theorists not only offer models of agent behavior, but also conduct normative analyses upon the performances of different mechanisms.

In the late 1970s through the 1980s, Smith, Plott and their collaborators were keenly interested in studying mechanisms (institutions), and well aware that their laboratory endeavors were a nice complement to mechanism design theorists' contributions (see Wilde 1981, 137-8; Smith 1982, 923; 1989, 156). There is no question that during the same period of time, they were in a position to feel willing, eager and happy to strike alliances with mechanism design theorists. Now a question arises: Were mechanism design theorists also willing to form a coalition with them?

3. Mechanism Design Theorists Giving Support to Laboratory Experimentation

There were professional ties running through the economists at the three centers (see section 1). Mechanism design theorists (at Northwestern) were paying attention to laboratory experiments being conducted in close relationship with their work in the 1970s through the first half of the 1980s, and furthermore, they shared with experimental economists the same conceptual scheme (see section 2).

Despite all this, we have not yet seen for ourselves what the leading scholars in mechanism design theory had to say *explicitly* about the importance of laboratory experimentation and its relationship with their intellectual pursuit, except some fragmentary remarks they made parenthetically in passing. This section is intended to illuminate the position they took up on the legitimacy and social-usefulness of laboratory experimentation, when placed in the arena where the 'pecking order' for research funding was at stake. Involved in a project devoted to coming up with a priority list for research funding in the mid-1980s, they stated unreservedly their opinion about laboratory experimentation. In what follows, a very brief explanation is offered regarding how mechanism design theorists got involved in this project. Subsequently, the focus is concentrated upon what they had to say about laboratory experimentation.

3.1. Mechanism Design Theorists Getting Involved in a Project for Setting Priorities for Research Funding

Soon after its inauguration in January 1981, the Reagan administration caused panic among social and behavioral scientists, especially economists, by proposing a revision of the fiscal year 1982 budget. Indeed, a "vigorous onslaught" (Smelser 1989, 2) was made exclusively upon social and behavioral sciences (SBS). According to its proposal, the National Science Foundation (NSF) budget for the Division of Social and Economic Science was to be slashed by 75%, that for the psychology and linguistics programs by 67%, and that for the anthropology program by 39% (Miller 1987, 375-6). More specifically, due to the revision, the budget of the NSF economics program would shrink from $14 million to $3.7 million in fiscal year 1982 (Silk 1981, D2).

Many economists were, understandably, so upset about the proposed budget cuts that they "rallied from the right, left and center to try to forestall these sharp reductions in research support" (D2). So did other social and behavioral scientists, with the result that the Consortium of Social Science Associations (COSSA) was rapidly converted into a powerful lobbying entity. What is more, the Committee on Basic Research in the Behavioral and Social Sciences (Committee)—which had been officially launched early in 1980 by the National Research Council, an operating branch of the National Academy of Sciences (Smelser 1989, 1; Adams et al. 1982, v)—did its best to bring about the (re)emergence in the Washington establishment of the recognition that basic research in SBS was worth supporting with the taxpayers' money. Over the first half of the 1980s, the activities of COSSA and the studies by the Committee (Adams et al. 1982; Smelser and Gerstein 1986) were instrumental in producing the desired effect to some limited extent.

At the same time, however, a dominant view emerged that scarcity of federal funds made it necessary that social and behavioral scientists themselves should come up with a priority list for research funding (Smelser 1989, 3; Larsen 1992, 189-194). An agreement was shortly reached that the Committee should be charged with this daunting project, dubbed "A Decade Outlook on Research Opportunities in the Behavioral and Social Sciences" (McCartney 1984, 442). After a lot of preparatory work, the Committee managed to select 31, top-priority topics for SBS, and to form the same number of working groups. Early in 1985, each working group was given a task "to prepare [concerning the topic assigned] an amply documented essay of some 20 pages, outlining major research accomplishments as well as future research promise, and to come up with a number of recommendations for kinds of research support required to facilitate maximum advances in research in the coming decade or so" (Smelser 1989, 5). By the end of the summer of 1985, all the working groups had finished submitting to the Committee their reports (Gerstein et al. 1988, xii-xiii; Smelser 1989, 3-5).[12]

Among those 31 top-priority topics were six topics directly relevant to economics, and a number of elite members of the mainstream of the economics profession participated in this priority-setting project (see Luce et al. 1989, chaps. 10-15). Called on to give advice upon where the future of economics lay in the spring of 1985, those elite members knew very well that they were expected to make a strong and accurate case for the importance and social usefulness of economics in general and their specific areas of research in particular.[13]

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[12] The Committee finally put an end to its priority-setting project in 1988 when its special report (Gerstein et al. 1988)—written up on the basis of the working groups' reports—saw the light of day. The following year, the working groups' reports were published as a companion volume (Luce et al. 1989).

[13] For instance, Oliver Williamson (chairperson of the Market Efficiency working group) explained their task to his working group members in the following way: "As I understand it, we should adopt something of an advocacy posture. Support for Social Science Research in general and Economics in particular has come under a lot of criticism. The reasons for this are various. I don't think that we should try to address them. Rather, what we need to do is make the case that there are enormously important theoretical, empirical, and public policy problems out there for which economic analysis is absolutely critical. Economics both has been and will continue to be the queen of the Social Sciences. Applied micro has been and prospectively will be an even more important part of this effort" (his letter to the members of the Market Efficiency working group, titled "Overview," dated April 4, 1985, [ff: National Academy SS Committee Market Efficiency,

Directly relevant for the purpose of the present paper is the report submitted by the Markets and Organizations (MO) working group (Luce et al. 1989, chap. 13). The chairperson and many of the members of the MO working group were hall-of-famers of mechanism design theory. Stanley Reiter was chairperson, and he had such luminaries as Kenneth Arrow, Jerry Green, Theodore Groves, Andrew Postlewaite and Roy Radner in his working group.[14] Lance Davis (a Purdue Mafioso at Caltech in the 1970s) and Karl Shell (editor of *Journal of Economic Theory*) took part in Reiter's working group as well; the liaison assigned from the Committee to the MO working group was a pioneer of mechanism design theory, Leonid Hurwicz. The other three members were all sociologists (Paul Dimaggio, Mark Granovetter and Michael Hannan).

Some portions of the MO working group's report reflected the research interests of those three sociologists; however, its overall contents bore the imprint of the ambitions and concerns of Reiter and other mechanism design theorists. It comes as no surprise, therefore, that in the report, heavy emphasis was put on the importance of the issues mechanism design theorists had wrestled with. What is significant for the purpose of the present paper is the fact that it also stressed the legitimacy and importance of laboratory experimentation.

3.2. Mechanism Design Theorists' Explicit Endorsement of the Legitimacy of Laboratory Experimentation in Their 1985 Report

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Box 34], Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University).

[14] For Arrow's contribution to mechanism design theory, see Arrow and Hurwicz (1977) and Arrow (1979). See Groves, Radner and Reiter (1987) to ascertain that Green, Postlewaite and Radner were all close to Hurwicz and also to mechanism design theorists at Northwestern.

Reiter and his working group members began their detailed discussion of experimental economics by endorsing its significance. "One of the important developments in economics over the past two decades or so is," they said, "the emergence of controlled experimentation" (Luce et al. 1989, 303). Being interested in the laboratory studies on various resource allocation mechanisms, they did not forget to cite Smith's (1982) comprehensive, methodological discussion of the version of experimental economics developed by Smith, Plott and their collaborators. As acknowledged by Smith (1991) himself, he attempted in his 1982 paper at "the bridge-building … between experimental economics and the Reiter-Hurwicz—sometimes called the Northwestern—view of economic theory" (162) on the one hand; on the other, Reiter, along with the MO working group members, responded to Smith by making plain that they were appreciating experimental economists' laboratory studies.

Subsequently, Reiter and his working group members moved on to clarifying the linkages between mechanism design theory and laboratory experimentation:

> Experimental work mirrors theory. Experimental methods require specification of the detailed structure of the processes operating in the market or organization under study. The models of mechanism [design] theory … appear to be particularly well suited to the needs of experimental research, because these models contain explicit formulations of essential processes. This connection between mechanism [design] theory and experimentation has already led to fruitful interaction in which theoretical investigations provide models for experimenters and experimental results provide guidance for new theoretical formulations. (303-4)

As one can see in the quote above, Reiter and the MO working group members noted that the conceptual bridge between mechanism design theory and laboratory experimentation had already been firmly built. What is more, they had no problem mentioning that a mutually beneficial, stimulating relationship had been established between the two camps. They did not stop there. As if they felt the need to support their statement about the reciprocal interaction with a concrete example, they inserted a snapshot of the interaction between mechanism design theorists and experimental economists:

> An illustration of this sort of interaction is given by a recent experiment involving a mechanism (the Walker mechanism) for allocating a public good. This mechanism has been shown to yield certain optimal allocations (Lindahl allocations) at its Nash equilibria, but it does not guarantee individual feasibility, that is, individuals can experience bankruptcy (negative payoffs) as a result of the actions of others in the experiment (Walker, 1981). This possibility is not acceptable in the experimental setting. The experimenter introduced a variant mechanism to prevent bankruptcy, but this opened the possibility that the strategic structure of the game was distorted. Stimulated by this situation, a graduate student at Minnesota[15] came up with a modification of the Walker

[15] A draft of the MO working group's report with a title, "Draft Report: Working Group on Markets & Organizations" (dated July 8, 1985, [ff: Reiter's Report-July 18, Box 80], Leonid Hurwicz Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University) composed by Reiter did not include the phrase, "at Minnesota." After receiving Reiter's draft on July 18, 1985, Hurwicz, a prominent theorist at the University of Minnesota, inserted the phrase,

> mechanism that preserves its essential strategic structure but guarantees individual feasibility. There has also been deeper analysis of the variant mechanism by another theorist. What might appear to be a mere theoretical nicety turned out to be a crucial point for experimental investigation; in turn, the experimental need gave rise to improvements of theory. (304)

At this juncture, it is worthwhile to stress an obvious fact that this portrayal of the dynamic, synergistic interaction was drawn up by a group of scholars including no experimentalist. Had it not been for their endorsement of the legitimacy and importance of laboratory experimentation, mechanism design theorists would not have had interaction with experimentalists, and Reiter and his working group members could not have provided an illustration of this sort. More important, they would not have felt the need to stress the cross-fertilization in the 'political' document they were composing.

In the ensuing part of their discussion of laboratory experimentation and its relationship with mechanism design theory, Reiter and his working group members highlighted three, distinct aspects. First, they noted that laboratory experiments had provided positive evidence concerning the extant literature of mechanism design theory. For instance, they drew attention to the fact that not a few laboratory experiments had produced the data giving support to Nash equilibrium models (304).

"at Minnesota" in between "a graduate student" and "came up with" on page 16 of Reiter's draft. The future Nobel laureate was proud that the interaction in question was (also) taking place at his institutional home.

Second, they did not forget to draw attention to negative evidence offered by laboratory experiments. Using the well-known winner's curse as an example, they pointed out that laboratory studies had "opened up new issues for theory as the limits of applicability of certain theoretical models [were] found" (304). Rightly diagnosing their example as "a challenge to theorists and to experimentalists" (305), they admitted that persistence of the anomalous cases would oblige mechanism design theorists to adopt different behavioral assumptions, or to come up with new mechanisms, which would later make experimental economists busy.

Third, it was emphasized that policy studies had already benefited from laboratory experiments (305-306). According to Reiter and the MO working group members, experimental economists put to a series of laboratory tests, one of the representative 'paper' mechanisms for the provision of public goods (Groves-Ledyard mechanism), and the positive results in those tests encouraged them to conduct laboratory experiments on other mechanisms tightly linked to it (see, e.g., Smith 1979). And, as Reiter and his working group members noted, experimental economists took a further step by applying their laboratory method to policy studies. For instance, they participated in the projects intended to help the National Aeronautics and Space Administration (NASA) find solutions to certain resource allocation problems (see Ledyard 1987; Banks, Ledyard and Porter 1989). Concerning the role laboratory experiments played in the NASA project, Reiter and the MO working group members made illuminating remarks:

> Initial experiments indicated that the process … was nevertheless not sufficiently reliable to use … The experiment provided an inexpensive means of testing the process. A field test could have cost hundreds of thousands of dollars in mistakes and delays. Experimental debugging of new ideas is much less expensive. (305)

These remarks are all the more importance because they were a clear indication of mechanism design theorists endorsing the main points experimental economists made during the period of the late 1970s through the early 1980s in their methodological papers (Plott 1979; Smith 1980a, 1982; Wilde 1981), which could be summarized as follows: (a) In most empirical studies of mechanisms proposed by mechanism design theorists, economists can hardly avail themselves of naturally occurring circumstances because those mechanisms are just a brain child of mechanism design theorists. (b) Two promising alternatives for the empirical study in question are the field (social) experiment and the laboratory experiment. (c) Although they are complementary, the field experiment is impractical in many cases due to costs, ethical and legal barriers, among other things. (d) Or, it is wise to perform field experiments *only* upon the mechanisms that have previously shown satisfactory performances in preliminary laboratory tests. (e) Hence, there exists much room for the laboratory experiment, especially in the context of mechanism design.

To sum up, called on to state their belief with regard to the types of economics research worth supporting with public money in the mid-1980s, prominent mechanism design theorists endorsed enthusiastically the legitimacy and social usefulness of experimental economics tightly linked to their theoretical pursuit.[16] Mechanism design theorists were indeed willing and eager to strike alliances with experimental economists in the mid-1980s.

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[16] One might assert that mechanism design theorists' endorsement in question could have been just an appearance hiding their opportunistic motivation to make their project look much more 'scientific'. There is at least one reason why we should be skeptical about this interpretation: the prevalent skepticism towards economists' laboratory studies.

Among the five or six separate laboratory research activities being pursued by economists in the first half of the 1980s (see Roth 1987) was the laboratory study of animal choice behavior by Raymond Battalio, John Kagel and their collaborators. In 1981, their animal study was awarded the Golden Fleece Award—see Miller (1987, 373-4) for the relationship between this award and SBS—as one can see from the manuscript of the press release (dated October 13, 1981) prepared at the Office of Senator William Proxmire, available at http://content.wisconsinhistory.org/cdm/compoundobject/collection/proxmire/id/443/show/297/rec/3 (last accessed on November 21, 2013). The publication of one of Plott's early laboratory-based policy studies (Hong and Plott 1982), which had been completed by 1976, "was delayed by the Department of Transportation for fear that it would be ridiculed with the Golden Fleece Award" (Plott 2001, xxvii), and "the paper was rejected by many journals before final publication" (xxvii). Indeed in the late 1970s and the early 1980s, many scholars in SBS including economists openly expressed their skepticism of the laboratory studies being contemporaneously performed by Smith, Plott and their collaborators (see Chamberlin 1979; Cross 1980; Stafford 1980).

It was not a very safe strategy for Reiter and the MO working group members to make a case that what they were doing was a real science by striking opportunistic alliances with experimental economists. Given that since the late 1960s, Reiter had been deeply involved in building the MEDS at Northwestern, and that in the first half of the 1980s, Hurwicz was a member of the Committee, one cannot reasonable claim that they were novices in 'politics.' Therefore, one may well not believe that their endorsement of laboratory experimentation was an opportunistic one. It seems more reasonable to conclude that they indeed thought Smith, Plott and their collaborators were doing something interesting and important.

4. Concluding Remarks

Protagonists of our story came to be professionally linked with one another in the period of the mid-1950s through the 1970s. Believing in the efficacy of neither the naturally-occurring market nor the government, they were commonly interested in building new institutions (mechanisms) for better collective decision-making and resource allocation circa the late-1970s. They thought of theoretical and laboratory studies of mechanisms as complementary, and considered the coalition between mechanism design theory and experimental economics to be mutually beneficial. And in the mid-1980s when some of the representative mechanism design theorists were involved in a priority-setting project for research funding, they openly stated that they were happy to form alliances with experimental economists. Hence, one may well conclude that mechanism design theorists were giving strong support to experimental economics in the mid-1980s.

Of course, what the present paper has shown so far does *not* warrant the conclusion that the coalition between mechanism design theorists and experimental economists was *the* main force behind laboratory experimentation getting incorporated into the toolbox of mainstream economics in the 1980s through the early 1990s (Friedman and Sunder 1994, 131; Roth 1995, 21). One may, however, entertain a historical hypothesis that mechanism design theory and experimental economics were part and parcel of a larger movement in which the significance of designing new mechanisms or institutions with a view to making improvement upon the preexisting social states was taken for granted. An in-depth examination of this hypothesis, which is beyond the scope of the present paper, will shed much light on the history of laboratory experimentation in the economics profession at the very least.

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