

DISSENT IN ECONOMICS:  
Making Radical Political Economics and  
Post Keynesian Economics, 1960-1980

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## ABSTRACT

The history of dissent in economics has thus far been subject to scant interest. The existing scholarship, authored by dissenters probing their own past, has failed to address the crucial questions of how dissent emerged and rooted itself.

This study is about two dissenting communities, Radical Political Economics and Post Keynesian Economics. I review the circumstances that led to their emergence in the late 1960s and early 1970s. I draw from the histories of religious and scientific dissent to explore the making of the dissenters' challenge to the economics orthodoxy. Notably, I use the concept of boundary work to analyse the debates between dissenters and mainstream.

The history of Radical Political Economics begins with the founding in 1968 of the Union for Radical Political Economics. Onto this Union converged a generation of young radicalised academics that sought to unite their political interests and their scholarly pursuits. After a period devoted to the design of a "paradigm of conflict," radicals turned to outreach work with popular movements. The new commitment brought divisive political identities into their Union that barred any agreement on a programme to transform economics.

Post Keynesian Economics emerged in the aftermath of debates on capital theory between Cambridge left Keynesians and neoclassical economists. With the conviction that the debates signalled the emergence of a new theory in economics, American dissenters decided to ally with the Cambridge critics. The content of the alliance was redefined many times in the 1970s by a succession of spokespersons for the group. Of this period resulted a weakly bound community joined by a sense of shared ancestry.

The two case studies reveal the diverse resources and allies that dissenters mustered for their battle with the economics orthodoxy. They show how the dissenters' challenge shaped the boundaries of their communities and the content of their identity.

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This history is about American economics of three decades past and an ocean of distance (and time) can be a difficult obstacle to surmount. Fortunately, I had the kind support of Frederic S. Lee, with whom I shared archival material, and Geoffrey C. Harcourt, who opened his personal correspondence to my research. I also thank Ruth Indeck, Laurie Nisonoff and Donald Katzner for sharing their recollections and personal files. The librarians and archivists at the Rare Book, Manuscript, and Special Collections Library of Duke University, were helpful and patient beyond the call of duty. It was thanks to all of them that I learnt the unlikely excitement of prospecting an archive.

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## ABBREVIATIONS

- AAUP** American Association of University Professors
- AEA** American Economic Association
- AER** American Economic Review
- CSWEP** Committee on the Status of Women in the Economics Profession
- EEP** Economics Education Project
- ERAP** Economic Research and Action Project
- D&S** Dollars & Sense
- JEL** Journal of Economic Literature
- JPKE** Journal of Post Keynesian Economics
- MR** Monthly Review
- NAM** New American Movement
- Newsletter** Newsletter of the Union for Radical Political Economics
- PEA** Political Education and Action
- PEAC** Political Education and Action Coordinator
- PKE** Post Keynesian Economics
- PL** Progressive Labor
- PPEB** Popular Political Education Editorial Board
- QJE** Quarterly Journal of Economics
- RES** Review of Economic Studies
- RPE** Radical Political Economics
- RRPE** Review of Radical Political Economics
- RU** Revolutionary Union
- RYM** Revolutionary Youth Movement
- SDS** Students for Democratic Society
- SNCC** Student Non-Violent Coordinating Committee
- URPE** Union for Radical Political Economics

## Part I

# LITERATURES ON DISSENT

# 1

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## Dissent in science

### A historical and methodological introduction

#### 1.1 Introduction: the troubling 1970s

It is well recognised in the history of the American social sciences that the late 1960s and the 1970s were a period of crisis and major transformation of these disciplines. The decade arrived after the social sciences' "highest point of self-confidence and of intellectual popular authority in the United States and around the world."<sup>1</sup> The causes of the crisis and its impact are now being examined as some of the historical record begins to settle.<sup>2</sup>

One particular social science, economics, appears in these narratives as an anomaly. The received view compares economics to other social sciences to conclude that

The political shifts that battered the other social sciences served to benefit economics. In the United States, the left radicalism of the 1960s had little influence in economics, while the conservative and libertarian politics of the following decades rewarded rational choice theory and the generally antistatist neoclassical mainstream of the discipline.<sup>3</sup>

This assessment agrees with recent reviews of the past by leading economists.<sup>4</sup>

While these economists may concede the existence of 1970s controversies between

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<sup>1</sup>Ross (2003), p. 229.

<sup>2</sup>A notable exchange on the identity debates of the academic professions can be found in the Winter 1997 issue of *Daedalus*. In the volume prominent literary critics, philosophers, political scientists and economists were asked to provide overviews of the developments in their respective fields. Katznelson, commenting on the volume's contributions, noted how little was said about the sixties' upheaval ((1997), p. 311).

<sup>3</sup>Ross (2003), p. 236.

<sup>4</sup>Solow (1997), Kreps (1997).

“saltwater” Keynesians and “freshwater” Monetarists, they downplay their significance.<sup>5</sup> Instead the stress is placed on continuity in post-World War II economics, characterized by accumulating achievements and devoid of intellectual revolutions.

However, the anomalous status of economics in the history of the social sciences may have been overplayed. Few historians of economics will deny that the late 1960s and the 1970s were a period of decreased self-confidence and public clout for economics, and that this once consensual academic field was in that period partitioned into factious competition. For A. W. Coats, an attentive observer of the trends in the American economic profession, the crisis of economics was not perceived as less acute than that faced by the other social sciences, but more so. Coats argues that “the trauma seems to have been especially acute in economics, a subject in the forefront of public affairs which has often been described as the “hardest” of the “soft” sciences.”<sup>6</sup>

The crisis does figure in some histories of economics. For instance, Michael Bernstein’s history of economists’ involvement in national policy making discussed it in connection with the bad fortunes of the economy. He noted that,

Not the least of the consequences of the national economy’s unfortunate performance throughout the 1970s, and the attendant fracturing of the broad based analytical consensus that had characterized the predominant part of the American economics profession for decades, was the havoc wrought on policy discussion itself. Gone were both the self-confident agreement about the principles and the interpersonal comity that more often than not went with it.<sup>7</sup>

One of the crucial features of the crisis was thus the emergence of “a veritable babel of tongues, a variety of groups, schools and splinter movements, and almost as many standpoints from which to assess the current state of the art”.<sup>8</sup> The transformation was manifest at the level of graduate programs; hence “a sharpened differentiation in the intellectual orientations of the various departments became a more significant

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<sup>5</sup>Kreps (1997), p. 80-82. These controversies have been exhaustively discussed, for a recent overview see Leeson (2000).

<sup>6</sup>Coats (1992a), p. 407.

<sup>7</sup>Bernstein (2001), p. 168. Bernstein discusses the crisis of economics on pages 148-184.

<sup>8</sup>Coats (1992a), p. 411.

part of their respective identities.”<sup>9</sup> The “babel of tongues” has not regressed into silence. The persistence in economics of various groups of dissenters and orthodoxies is one of the legacies of the 1960s and 1970s.

## 1.2 History of dissent in economics

As Roger Backhouse has argued, it was only in the post World War II period that organized dissenting groups emerged in the economics profession, following the rise of an orthodoxy.<sup>10</sup> Under the heading of dissenters, Backhouse listed “Post Keynesians, institutionalists, evolutionary economists, Radicals, Marxists, Feminists, and other such groups”.<sup>11</sup> The author put forward a sociological explanation for the groups’ emergence along three necessary conditions: “(a) a dominant orthodoxy against which ‘rebellion’ can take place; (b) an environment in which dissent can be institutionalised; (c) the motivation to identify as a heterodox group.”<sup>12</sup> Backhouse also highlighted the socio-political context as seconding the dissenting groups’ emergence: “the intellectual environment, the changed economic environment and the challenge of the Vietnam War, the expansion of higher education in many developed countries, and even the changing economics and technology of publishing.”<sup>13</sup>

In the history of economics, interest in the study of dissent is a recent development. The character of existing work on the subject can be gauged from the essays contained in *Economics and its Discontents* and *A Biographical Dictionary of Dissenting Economists*.<sup>14</sup> Both books examine dissent from the perspective of the dissenting author by collecting either first-person accounts of a life’s work or through biographies written by disciples of the dissenters.<sup>15</sup> Dissent is broadly defined as

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<sup>9</sup>Barber (1996), p. 25.

<sup>10</sup>It is now well established in the history of economics, that there is a discontinuity between the economics profession pre-World War II, pluralistic in approach, and post-WW II economics dominated by a brand of neoclassical economics, see Morgan and Rutherford (1998).

<sup>11</sup>Backhouse (2004), p. 265.

<sup>12</sup>Backhouse (2000), p. 150.

<sup>13</sup>Ibid., p. 151.

<sup>14</sup>Holt and Pressman (1998); Arestis and Sawyer (2000).

<sup>15</sup>Further examples of this style of biographical or autobiographical histories are Harcourt (1993) and Kregel (1989).

disagreement with an orthodoxy prevailing at the author's lifetime,<sup>16</sup> "Dissent is a synonym for disagreement",<sup>17</sup> but the particulars may differ widely, for instance: a rejection of mathematics and of professional neutrality for the sake of philosophical arguments;<sup>18</sup> an ideological opposition to capitalism;<sup>19</sup> or opposition to progressive politics.<sup>20</sup> As these studies remain disconnected they provide us not so much with a *history of dissent* as with *histories of dissents*. Even within the fold of a single author, dissent may be multiple, as in the statement that: "Lowe's dissents are complex".<sup>21</sup> The multiple definitions of dissent have led some authors to attempt to clarify what the referents for such terms as "dissent" and "heterodoxy," traditionally used interchangeably, ought to be. For instance, Backhouse has proposed that heterodoxy should be defined as a subset of dissent. For him, dissent is prolonged controversy, while heterodoxy is the organized form of dissent that expounds a complete rejection of the received views of the discipline. This leaves open the possibility of classifying orthodox writers as dissenters attempting to change particular ideas in economics instead of engaging in their wholesale rejection.<sup>22</sup>

More important than how one ought to use the term dissent is how it has been used. In the history of economics the term dissent has served primarily as a rhetorical device for the writing of life histories. Historians of economics, often in the dual role of historian and economist, evoke tales of lone heroes battling the intolerant forces of orthodoxy. In this fold all authors become eligible to the rank of dissenter. It is said that "Adam Smith, Thomas Robert Malthus and David Ricardo would have been considered dissenting economists during their time as they tried to understand the broad new features of capitalism as it emerged during the late eighteenth century.

Likewise, the marginalists were dissenters when they questioned the classical

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<sup>16</sup>Even the historical context may be dismissed to validate the dissenting label, in the case of John R. Commons, "Whatever the status of Commons during his life, and whatever his own interpretation of the relationship between his work and orthodox economics, today the contributions of Commons appear as a literature of dissent – dissent against certain tendencies in economic thought which, since his death, have become thoroughly ingrained in economics." (Biddle and Samuels (1998), p. 41).

<sup>17</sup>Latzko (1998), p. 226.

<sup>18</sup>Boettke (1998).

<sup>19</sup>King (1998).

<sup>20</sup>Emmett (1998).

<sup>21</sup>Forstater (1998), p. 183.

<sup>22</sup>Backhouse (2004), p. 265 - 266.



perspective.”<sup>23</sup> The list of dissenters further includes James Buchanan, Friedrich Hayek, Frank Knight, J. M. Keynes<sup>24</sup> and Milton Friedman, adding the comment that: “Many insiders are successful dissidents. In fact, being a successful dissident is one path to becoming an insider.”<sup>25</sup> According to these authors, to dissent is to join this list of notables. Dissent thus becomes foremost a badge of distinction, devoid of historical content.<sup>26</sup> It is not surprising that this literature has failed to register the emergence of dissent in the 1960s and 1970s as an interesting historical problem.

My focus is on the late 1960s and 1970s crisis of economics and in particular on the emergence of dissenting groups amidst the crisis. I treat the history of the discipline of economics as a social historical formation.<sup>27</sup> To set up my research question and methodology, in this chapter I discuss two other literatures on dissent: the social history of religious dissent, and the social studies of science approach to dissent. I conclude by providing an outline of this thesis’ argument.

### 1.3 History of religious dissent

The term dissent was first used to denote English and Scottish religious non-conformism.<sup>28</sup> The most authoritative author on the study of seventeenth century religious dissent in its multiple connections with politics, literature, science and class was the historian Christopher Hill. His most important text on the subject was *The World Turned Upside Down*<sup>29</sup> which constitutes my entry point to the history of religious dissent. My purpose in surveying this work is to showcase how one can approach intellectual dissent from a socio-historical perspective.

England in the 1640s saw the explosive spread of radical ideas, and the formation of groups known as Levellers, Diggers, Ranters and Quaker sects. Hill argues that radical ideas were not novel, in fact there is evidence for religious dissention and

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<sup>23</sup>Holt and Pressman (1998), p. xi.

<sup>24</sup>Boetke (1998); Moss (1998); Emmett (1998); Chick (1998).

<sup>25</sup>Colander (1998), p. 56.

<sup>26</sup>Rider (1998), p. 166; Clark (1998), p. 267.

<sup>27</sup>Porter (2003), p. 5.

<sup>28</sup>The Oxford English Dictionary dates the first uses of the term to the late sixteenth and middle seventeenth century.

<sup>29</sup>Hill (1991).

heresy from at least the Reformation period and even earlier in Continental Europe. Despite the overlap between the sixteenth century Lollards and Familists and later sectaries in their geographical location, class origins, and theological deviance, radical religious ideas only became a social force in the mid-seventeenth century. The source of the transformation was the breakdown of the bonds of loyalty and dependence that held feudal society together.<sup>30</sup> A class of “masterless men” was being created out of the enclosure movement and early capitalist development. The list included cottagers and squatters on common lands, the itinerant trading population, rogues, vagabonds and beggars, and the working poor of London.<sup>31</sup> Freed from the hierarchical hold of agrarian society, these men were responsive to radical ideas that questioned the theological justification sustaining the English monarchy and clergy.

The English revolution (1640s) and the Commonwealth (1650s) owed much to the “masterless men” for they staffed its paid army, the New Model Army. The army was the centre for debate which radicalized many of the sectaries that came to prominence in the following decades.<sup>32</sup> The New Model Army became the first agency of radical politics when the Levellers attempted to take control of the army’s ruling council.<sup>33</sup> The radicals’ utter defeat, with the “Agitators” being arrested or killed, was a harsh lesson about the workings of the material world, one that informed later actions and beliefs of the sectaries.<sup>34</sup>

Thus in Hill’s account the formation of a class of “masterless men” was not a sufficient condition for the emergence of radical ideas and sects. It was only with the politicizing experiences of the New Model Army and the relaxing of religious control brought by the Republic that radical ideas began to be expressed. However, these were still sects and idiosyncratic preachers and not “the churches” we now identify with non-conformism. Self-conscious and organized dissent was a creation of the Restoration.<sup>35</sup> After 1660 the Church of England sought to reassert its monopoly

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<sup>30</sup>Hill (1991), p. 39.

<sup>31</sup>Ibid., pp. 40-44.

<sup>32</sup>The crucial role played by the New Model Army is a point Hill makes repeatedly in his writings, in Hill (1991), pp. 70-71; and in his biography of John Bunyan, Hill (1989), pp. 47-60.

<sup>33</sup>Hill (1991), p. 67.

<sup>34</sup>Ibid., p. 72.

<sup>35</sup>Hill (1989), p. 337.

position but non-conformists had become too numerous and too powerful. The Restoration context meant that “tighter organization and discipline were forced on the congregations in order to survive. There were struggles within and between sects over conditions for church membership. Time and energy were consumed in discussing the conduct befitting a church member, in visiting and correcting backsliders, etc.”<sup>36</sup> The disconnected sectaries were creating an identity and coalescing into national churches. “Excluded from politics and the universities, they nevertheless gained an identity as sects, and a greater identity as dissent.”<sup>37</sup> Following a few intervening decades of repression and deadlock, the Toleration Act of 1689 conceded the right of non-conformists to worship.

Hill’s work is insightful in its treatment of the relationship between social change, radical ideas, and radical institutions. These are elements that my own research on dissenting economics will address. Interestingly, for Hill there was no deterministic link between any of these. To become a radical was as much a product of class origins as it was of social experience. Radical ideas and radical institutions, in this case post-Reformation non-conformist churches, were pragmatic accomplishments drawing on the social experiences of its actors.

In Hill’s social history, religious dissention was fused to politics in two senses. Dissention was a political achievement made out of victories and defeats during the Revolution and the Commonwealth. When radicals manoeuvred in alliances with Cromwell and between themselves in the New Model Army, radical communities and ideas took definite shape. With the Restoration radicals made the defensive move of redesigning their sects into churches, which were achievable only by creating a shared identity for what were once separate and distinct sectaries.

But religious dissention was also political in another sense, in that theology in the seventeenth century had a clear bearing on the organization of society. The heretical views of equating the Church of England with the Antichrist,<sup>38</sup> the belief that law had been abolished by the non-conformist gospel or the view that physicians were out of

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<sup>36</sup>Ibid., p. 342.

<sup>37</sup>Ibid., p. 346.

<sup>38</sup>Hill (1990), p. 157.

step with the wisdom of God all carried a clear political rationale.<sup>39</sup> At the core of the seventeenth century dissention was the opposition to the monopoly of the Church and of the professions (lawyers and physicians), the pillars of authority in English society.

Hill stresses the politics of religious dissent. We are pressed to abandon the prejudice of treating theology (and dissent) as an intellectual subject separable from the totality of social practices. Contemporaneously (and independently) to Hill's research, the fields of the history and sociology of science also came to adopt a broad socio-historical perspective on the history of ideas. This literature, and the place that the study of dissent occupies in it, form the subject of the following section.

## 1.4 History and sociology of dissent in science

### 1.4.1 The uses of dissent

The last 30 years has brought a major redefinition to the fields of the history and sociology of science. The change comprised a departure from Mertonian sociology that spoke of science as an insulated field of knowledge production.<sup>40</sup> It was also a rejection of the Whiggish history of science that celebrated genius and progress while ironing out discontinuity and uncertainty. It furthermore instituted scepticism towards justificationist philosophies of science.<sup>41</sup> Under the new understanding, science was the unprivileged product of social practices, a social formation bound to the society that engendered it. A crucial resource for the reshaping of our understanding of science has been the study of dissent, "deviant science," or "rejected knowledge." Episodes of dissent with ensuing controversy, conflict and resolution, have been invaluable entry points to question some of the myths surrounding science.

Histories of dissent had traditionally been driven as studies of the dissenters' "ideological biases, social influences, or 'irrational commitments' which led to departure from truth and reason".<sup>42</sup> In this mould, history reasserted science's sense

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<sup>39</sup>Hill (1996), p. 329; Hill (1974), p. 159.

<sup>40</sup>Restivo (1995), p. 99.

<sup>41</sup>Rouse (1999).

<sup>42</sup>Wallis (1979), p. 5.

of self-worth by reaffirming the veracity of later knowledge and by explaining the faults of the dissenter. This is what has been often called in the science studies literature a “participant’s account,” as it uncritically writes into the past the meanings of its author’s contemporary scientific culture. To overcome the biases of participant’s histories the new approach to science studies adopts a relativist stance, becoming “agnostic” to the truth status of claims, and attempting to place competing claims on the same footing. The new setting entails writing the past as the social negotiation of some claims into truth and scientific knowledge, and others into dissent and rejected knowledge.

H.M. Collins and T.J. Pinch’s study of the scientific certification of paranormal phenomena is a revealing instance of how dissent may be brought to inform our understanding of science in the making. The authors outline their goals as: to “analyze the tactics used by parapsychologists in their efforts to gain scientific recognition for their discipline and their findings, and the tactics used by orthodox scientists to deny them this stamp of legitimacy.”<sup>43</sup> Collins and Pinch distinguished between two sorts of conflict, implicit and explicit, and two forums of debate, the constitutive and the contingent. The “implicit conflict” is one of silence, when a theory is rejected by being ignored, while “explicit conflict” is represented in controversies. The “constitutive forum” was Collins and Pinch’s label for scholarly mediums, i.e. journals, conferences, but also inclusive of the practices of theorizing and experimenting. The “contingent forum” comprises of those actions that philosophical orthodoxy would suggest do not impinge on the constitution of knowledge. The “contingent forum” may therefore include “the content of popular and semi-popular journals, discussion and gossip, fund raising and publicity seeking, the setting up and joining of professional organizations, the corralling of student followers”.<sup>44</sup>

The case studied by the authors, between parapsychologists and the scientific orthodoxy, was one of “explicit conflict.” Collins and Pinch were not interested in

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<sup>43</sup>Collins and Pinch (1979), p. 237.

<sup>44</sup>Ibid., pp. 239-240.

reassessing the debate, in judging the reality of paranormal phenomena. Their primary concern was to trace the movements of the social actors. In this sense they found:

“full frontal” breakdown of the norms [of the constitutive and contingent forums]. In the constitutive forum we have found prejudice and other particularistic bases of belief openly endorsed. In the contingent forum, we have found items intended as substantive contributions to the constitution of scientific knowledge, rather than commentary and exposition.<sup>45</sup>

These boundary crossings underline the orthodox tactic that parapsychology should not be treated in the same way as science, even if it gained access to the “constitutive forum.” In this tactic the orthodox scientists opposed the parapsychologists’ attempts to metamorphosise themselves into scientists and fact makers, and thus to secure science’s “stamp of legitimacy.”

Collins and Pinch’s distinction between “constitutive” and “contingent” forums is a useful one. In a recent historical study of the dissenting ideas of Fred Hoyle, a renowned Cambridge physicist and science populariser, Jane Gregory took up this distinction to show how scientific actors may selectively and strategically travel between forums. As Gregory noted: “Hoyle might, in some contexts, have seen a distinction between his science and his fiction, he also, when it served his purposes, made explicit links between the two, breaching that boundary to capitalise the authority of the one and on the scope and reach of the other”.<sup>46</sup> This analysis challenged the notion that popularisation is a by-product of academic practice. Hoyle’s life-from-space theory was first developed in the 1950s and 60s in the public domain, in newspapers and novels, and was later brought into academic forums for development and scientific certification. Once expelled in the late 1980s from scientific discourse, the theory went on to survive in public media.<sup>47</sup>

Both the study of parapsychology and of Hoyle’s cosmological biology focused on the communication of science. They addressed the manoeuvring between public and

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<sup>45</sup>Ibid., p. 262.

<sup>46</sup>Gregory (2003), p. 39.

<sup>47</sup>Ibid., pp. 39-40.

academic spaces in scientific controversies. They reveal in episodes of dissent how the resources of science, its forums, may be used strategically by different actors.

As a further example of the study of dissent in science, I wish to briefly discuss the work of Steven Shapin on phrenology. Shapin wished to question the conception that scientific knowledge “is generated, and acquires its objective character, by processes of disinterested contemplation”.<sup>48</sup> His aim was to reveal the play of social interests in a rather esoteric episode of controversy, one over cerebral anatomy and the validity of phrenology c.1800-c.1830 in Edinburgh. Phrenology was dissenting science, a doctrine proposing a programme of social change and institutional reform, and favoured by an audience of high working-class and petty-bourgeois groups.<sup>49</sup> Phrenologists believed that the brain was the physical embodiment of the mind and that it was made up of separate organs responsible for distinct mental faculties. The power of a particular brain organ and mental faculty was linked and determined by the size of the organ, which was measurable by craniological examination, associating lumps in the skull to the organs. Since mental faculties were broadly considered to include behaviour and dispositions (for instance, criminal behaviour), phrenologists hoped to establish a naturalistic basis for social reform. Once “the innate constitution of an individual mind was scientifically diagnosed” the potential for improvement in individual behaviour could be envisaged.<sup>50</sup>

Shapin’s analysis of the debate revealed that the crucial object of the anatomic debate was to displace (on the side of phrenologists) or to preserve (on the side of the establishment) academic authority. Phrenologists were in want “to secure a naturalistic basis for one’s policies, and, thereby, credibility in society”.<sup>51</sup> This was the motivation to “bet” (Shapin’s term) on a perceptual account that others would hopefully corroborate to phrenology’s advantage. But observation was also subordinated to the legitimating tasks at hand, i.e. it had to display a brain with no common origin of nerves and fibres, with multiple segmented organs as proposed by

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<sup>48</sup>Shapin (1979), p. 139.

<sup>49</sup>Ibid., p. 145.

<sup>50</sup>Idem.

<sup>51</sup>Ibid., p. 168.

phrenology. The academic establishment had to preserve established anatomical knowledge from the phrenologist revision, defending “the existing model of the brain as a reliable account of reality, and at the same time to expose the new, deviant account as deficient”.<sup>52</sup> It was in the establishment’s interest to show no differentiation in the human brain.

The central theme of Shapin’s account of phrenology and the staple of what is known as the Strong Program of the Sociology of Science is the role of social interests in the development of science. In this account, observation and the accumulation of naturalistic knowledge is subordinated to the play of interests. Science is thus not a disjoint social field that follows rules of its own, in methodological or intellectual containment. Science is studied from a wider vantage point, that of societal conflict and interest.

Sociologists and historians of science have abandoned the conventional explanatory-set of scientific controversies punctuated by belief in the arbitration of the scientific method and that “scientists knew best.”<sup>53</sup> The new explanatory agenda is predominantly an “outsider’s account” looking to wider society as the key for interpreting scientific disputes, and in opposition to the “participant or insider accounts” of the past. Where there was once a divide between science and society, the new narratives of science highlight the social embeddedness of scientific practice.

#### 1.4.2 History of science as political history

Among the exemplar works in the history and sociology of science is Steven Shapin and Simon Schaffer’s study of the contest between Robert Boyle and Thomas Hobbes over Boyle’s air-pump experiments, *Leviathan and the Air-Pump*.<sup>54</sup> A discussion of this work further clarifies what is entailed by an “outsider’s” approach to the study of dissent.

Shapin and Schaffer’s immediate focus was on the historical circumstances in which experiment arose as the privileged means to generate natural knowledge. For

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<sup>52</sup>Ibid., p. 169.

<sup>53</sup>Wallis (1979), p. 5.

<sup>54</sup>Shapin and Schaffer (1985).



this they looked at Boyle's researches in pneumatics and his controversies with Hobbes. The authors argued that previous historical treatments of the controversy between Boyle and Hobbes had been coloured by our contemporary scientific culture, biased towards Boyle's side (the victor) in the controversy.<sup>55</sup> For a member of our present experimental communities, Boyle's arguments appear as self-evident while Hobbes's as irremediably flawed. The experimental practices, that Shapin and Schaffer wished to discuss, were thus not regarded as problematic and in need of explanation. The authors suggested that to correct this cultural and historiographical bias the historian must perform a "Herculean" effort to distance himself from Boyle's position. To give Hobbes a fair hearing and thus reveal the contingent nature of experimental practices, one must attempt an outsider's account of the controversies.

In the mid-seventeenth century, natural philosophy was in a state of disarray and "scandalous dissention. Nowhere was scandal more visible than in the handling of the Torricellian phenomenon and related effects."<sup>56</sup> Boyle addressed the resolution of the Torricellian phenomenon through the construction of a mechanical device, an air-pump, whose workings would potentially lead to the evacuation of air from a recipient, and the creation of a vacuum within which different experiments were to be conducted. In 1660 Boyle published *New Experiments Physico-Mechanical*, comprising of forty-three trials made with the pneumatic engine.<sup>57</sup> The *New Experiments* were however part of a wider agenda, namely the creation of the Royal Society and the experimental project that this organization stood for.

The purpose of experiments was to create matters of fact. There was a crucial boundary between the experimental matter of fact and its causes and explanation. Matters of fact were discovered not invented, they were not of man's making but made by nature. The empiricists thus dramatised their lack of preconceived expectations and investment in the outcome of experiments.<sup>58</sup> "Experimental

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<sup>55</sup>Ibid., p. 4.

<sup>56</sup>Ibid., p. 80. The authors note on p. 41: "The 'noble experiment' of Evangelista Torricelli was first performed in 1644. A tube of mercury, sealed at one end, was filled and then inverted in a dish of the same substance. The resultant "Torricellian space" left at the top became a celebrated phenomenon and problem for natural philosophers."

<sup>57</sup>Ibid., p. 40.

<sup>58</sup>Ibid, pp. 51, 67-68.

practices were to rule out of court those problems that bred dispute and divisiveness among philosophers, and they were to substitute those questions that could generate matters of fact upon which philosophers might agree.”<sup>59</sup>

Thomas Hobbes, a prominent figure in both civil and natural philosophy, saw Boyle’s experiments and the Royal Society’s experimental philosophy in a different light. For Hobbes the scandals of natural philosophy could only be overcome by identifying and expelling from it “*absurd* metaphysical language.”<sup>60</sup> Shapin and Schaffer took Hobbes’s famous civil philosophy tract, the *Leviathan*, as a work of natural philosophy to argue that:

In this book Hobbes took away vacuum on definitional, historical, and, ultimately, on political grounds. The vacuism Hobbes attacked was not merely absurd and wrong, as it was in his physical texts; it was *dangerous*. Speech of a vacuum was associated with cultural resources that had been illegitimately used to subvert proper authority in the state.<sup>61</sup>

He objected to the notion of incorporeal substance, “To Hobbes such talk of incorporeal substance was at once an absurdity of language-use, an impossibility in right philosophy, and one of the key ideological resources used in priestcraft.”<sup>62</sup> His commentary on Boyle’s *New Experiments* rejected the vacuum explanations and provided alternative interpretations of the results predicated on plenist philosophy.

Hobbes’s objections amounted to a rejection of the experimentalist program. Philosophy for Hobbes was the practice of demonstrating how effects followed from causes or of inferring causes from effects. The experimental programme failed to satisfy this definition. He systematically refused to credit experimentalists’ claims that one could establish a procedural boundary between observing the positive regularities produced by experiment (facts) and identifying the physical cause that accounts for them (theories).<sup>63</sup> Against these views, Hobbes’s model of philosophical practice was geometry, “In Hobbes’s dialogues, it is method, not matters of fact, that

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<sup>59</sup>Ibid., p. 46.

<sup>60</sup>Ibid., p. 84, emphasis in original.

<sup>61</sup>Ibid., p. 91, emphasis in original.

<sup>62</sup>Ibid., p. 92.

<sup>63</sup>Ibid., p. 111.

puts men right and that mobilises consensus. When empirical evidence, whether from observation or from experiment, is given a role in the dialogues, it serves to *illustrate* the conclusions reached by method, and not to determine belief.”<sup>64</sup>

For Boyle and his supporters, Hobbes’s influence was to be feared, he had a close friendship with the King, he was a leading exponent of mechanical philosophy, and he commanded a score of loyal followers. The supporters of the experimentalist project attacked Hobbes as a dogmatist. They argued that “Dogmatism inclined men to become “imperious,” to be unshakable in their convictions, and to be “impatient of contradiction.” It produced egotism and individualism, which is a “Temper of mind, of all others the most pernicious”.” In contrast, the experimentalists “were “modest, humble, and friendly”; they were tolerant of differing opinions and worked collectively towards attainable and solid goals.”<sup>65</sup>

To connect scientific debates to wider socio-political conflict is one of the striking characteristics of “outsider’s accounts” in the history of science, and the crucial insight I draw from Shapin and Schaffer’s study. They note that the controversy developed in the early years of the English Restoration at the end of the twenty years of Protectorate and Civil War. In this setting it was fundamental to

outline ways in which the sects could be controlled and sectarian knowledge contested. This was a powerful constraint, since it was widely argued that knowledge itself was a source of sectarian conflict. So the proponent of any successful mode of pacific knowledge must both deny its tendency to promote dissension and also deny the basis of the sects’ own forms of belief.<sup>66</sup>

Hobbes was a religious dissenter and despite his friendship with the King, he was persecuted by the Church of England for his beliefs.<sup>67</sup> For him, as outlined in the *Leviathan*, the King was the only source of law and assent, never the Church which alongside the professions represented sectarian interests. Hobbes argued that experiment shared the characteristics of priestcraft, and that Boyle made

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<sup>64</sup>Ibid., p. 145, emphasis in original.

<sup>65</sup>Ibid., pp. 138-139.

<sup>66</sup>Ibid., p. 285.

<sup>67</sup>Ibid., pp. 293-295.

experimenters into a new kind of clergy.<sup>68</sup> Hobbes's reliance on the philosophical method was a means to deny the multiplication of sectarian interests, experimentalists included.

The experimentalists presented their community as an

*ideal society* where dispute could occur safely and where subversive errors were quickly corrected. Their ideal society was distinguished by the source of authority the experimenters recommended. (...) No isolated powerful individual authority should impose belief. The potency of knowledge came from nature, not from privileged persons. Matters of fact were made when the community freely displayed its joint assent.<sup>69</sup>

Dispute was not just tolerated, it was necessary for establishing matters of fact. The experimenters were teachers on how to safely dispute. The equivalent of a civil war could be staged with no harmful effect. They proclaimed: "There we behold an unusual sight to the *English Nation*, that men of disagreeing parties, and ways of life, have forgotten to hate, and have met in the unanimous advancement of the same *Works*."<sup>70</sup> There was a community that aimed at peace and had found out the methods for effectively generating and maintaining consensus; a community without arbitrary authority that had learnt to order itself.

Shapin and Schaffer argued that the history of science can be treated as political history. They provided three senses by which this claim may be sustained:

First, scientific practitioners have created, selected, and maintained a polity within which they operate and make their intellectual product; second, the intellectual product made within that polity has become an element in political activity in the state; third, there is a conditional relationship between the nature of the polity occupied by scientific intellectuals and the nature of the wider polity.<sup>71</sup>

Shapin and Schaffer masterfully fulfil their program, since they are able to show how the problem of knowledge was political, predicated in laying down rules and

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<sup>68</sup>Ibid., p. 310.

<sup>69</sup>Ibid., p. 298.

<sup>70</sup>Cited in *ibid.* p. 306.

<sup>71</sup>Ibid., p. 332.

conventions of relations between men in the intellectual polity; that knowledge so created became an element in political action in the wider polity (for instance, by the Church's endorsement of the air-pump experiments); and finally that the contest among alternative forms of life and their intellectual products depended upon the "political success of the various candidates in insinuating themselves into the activities of other institutions and other interest groups. He who has the most, and the most powerful, allies wins."<sup>72</sup>

The consideration of power, and of scientific practice as political practice, has pervaded much of the recent work in science studies. Following Bruno Latour, constructing knowledge is akin to building an empire, *a la* Machiavelli, where one must ask: "With whom can I collaborate? Whom should I write off? How can I make this one faithful? Is this other one reliable? Is this one a credible spokesperson?"<sup>73</sup> For Latour as for actor-network theory (or sociology of translation) advocated by John Law and Michel Callon, allies are not just people but also things.<sup>74</sup> Things, such as scallops, are actors enrolled into empire-networks in attempts to discipline them into collaboration, to interest them.<sup>75</sup>

I use the term political and ally in the course of this research in meanings more akin to Shapin and Schaffer's than to the semiotics of Latour. Not least because of the nature of the controversies I discuss. It is useful to draw a distinction between laboratory studies and the doctrinal debate characteristic of controversies in economics between dissenters and mainstream. The latter, in so far as they target the polity of a discipline and its relationship to the state and the public, fall on the more conventional definition of politics. It is to better characterise and conceptualise how dissenting controversies relate themselves to the wider polity that I turn to the notion of boundary work.

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<sup>72</sup>Ibid., p. 342.

<sup>73</sup>Latour (1987), p. 125.

<sup>74</sup>Callon, Law and Rip (1986).

<sup>75</sup>Callon (1999).

### 1.4.3 Boundary work and the topography of science

I have suggested that the study of episodes of dissent is a privileged entry point to the contingent nature of scientific practices. I have also argued that the study of dissent allows for an outsider's approach to the history of science, taking seriously the political character of scientific activity. In this section I bring together these elements in a methodological frame for my study, through a discussion of Thomas F. Gieryn's concept of boundary work and of his proposed topographic metaphor.

The concept of boundary work has been advanced in science studies to describe the activity by which "scientists, would-be scientists, science critics, journalists, bureaucrats, lawyers, and other interested parties accomplish the demarcation of science from non-science."<sup>76</sup> In Gieryn's first discussion of the subject, he identified boundary work as a "rhetorical style" native to the ideology of science. His primary goal was to compare strain theories of ideology developed from Talcott Parsons's seminal work and interest theories associated with Karl Marx.<sup>77</sup> Gieryn's second thoughts on the subject abandoned discussion of boundary work as a rhetorical style to survey it as a theme in the work of sociologists of professions, social world theorists and historians of social classifications. It is Gieryn's latest contribution on the subject that is of greatest interest for this thesis. In his 1999 book, Gieryn developed a methodology for the study of scientific controversies exemplified in case studies of boundary work.

For Gieryn there are two positions on the demarcation of science from non-science. The essentialist position argues "for the possibility and analytic desirability of identifying unique, necessary, and invariant qualities that set science apart from other cultural practices and products, and that explain its singular achievements (valid and reliable claims about the external world)".<sup>78</sup> In opposition to this view the constructivists argue "that no demarcation principles work universally and that the separation of science from other knowledge-producing activities is

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<sup>76</sup>Gieryn (1995), p. 394.

<sup>77</sup>Gieryn (1983), p. 782.

<sup>78</sup>Gieryn (1995), p. 393.

instead a contextually contingent and interests-driven pragmatic accomplishment drawing selectively on inconsistent and ambiguous attributes.”<sup>79</sup>

Recent work in the sociology of science has raised doubts over the possibilities of defining a *universal* demarcation. Taking Karl Popper’s falsificationism as an exemplar of a universal demarcation principle,<sup>80</sup> Gieryn notes that under Popper’s suggestion scientists are said to work out theoretical generalisations into statements that are falsifiable, i.e. that may be confronted with potentially contradictory evidence and thus refuted. A theory would be deemed unscientific under three instances: if it were unfalsifiable, if practitioners made no effort to refute the falsifiable claims, or if once falsified a theory was not rejected.<sup>81</sup> Gieryn argues that, as a practical accomplishment of observation and experiment, falsification is unlikely to occur. Evidence in science is always permeated by interpretative ambiguities that are only ultimately resolved in social negotiation.<sup>82</sup> In support of this claim, Gieryn alludes to the work of H. M. Collins and his concept of “experimenter’s regress.” To refer to a case studied by Collins, one begins by posing the question: what is the correct outcome produced by a gravity wave detector? To calibrate an experimental setting, to know that it is working properly is a source of major difficulties. The correct outcome “depends upon whether there are gravity waves hitting the Earth in detectable fluxes. To find this out we must build a good gravity wave detector and have a look. But we don’t know if we have built a good gravity wave detector until we have tried it and obtained the correct outcome! But we don’t know what the correct outcome is until. . . and so on ad infinitum.”<sup>83</sup> We only know if an experiment is being truthful if we know what the truth is, experiments do not bear transparently on theory choice. As Gieryn argues, Collins’s work shifts the focus of our understanding of evidence in science, the question no longer laying on whether results

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<sup>79</sup>Idem.

<sup>80</sup>Gieryn argues that Robert K. Merton and Thomas S. Kuhn’s sociological and historical accounts of science are further instances of essentialism. Gieryn’s discussion highlights that both authors hold universal definitions of science: Merton presenting science as a field of knowledge production that once established is autonomous from political and social forces/influences; Kuhn presenting science as paradigmatic consensus (ibid., pp. 398-404).

<sup>81</sup>Ibid., p. 396.

<sup>82</sup>Ibid., p. 397.

<sup>83</sup>Collins (1992), p. 84.

can or cannot be replicated but how and why some replications are deemed authentic and authoritative while others are not. Evidence is arrived at only after a process of social negotiation.<sup>84</sup>

The absence of a *universal* demarcation warrants a new approach to the problem of science's definition. There are always not one but many definitions for science, and these are subject to contention. The battle over the definition of science is not an extraordinary activity, but a rather frequent feature of scientific practice. As Pierre Bourdieu has noted:

In the struggle in which every agent must engage in order to force recognition of the value of his products and his own authority as a legitimate producer, what is at stake is in fact the power to impose the definition of science (i.e., the delimitation of the field of the problems, methods, and theories that may be regarded as scientific) best suited to his specific interests (i.e., the definition most likely to enable him to occupy the dominant position in full legitimacy) by attributing the highest position in the hierarchy of scientific values to the scientific capacities which he personally or institutionally possesses.<sup>85</sup>

According to Gieryn, one must stand outside the contested work of defining what is in and out of science, and watch as boundary work is played out in society. "The task of demarcating science from non-science is reassigned from analysts to people in society, and sociological study focuses on episodes of "boundary work".<sup>86</sup> This is therefore an outsider's approach.

It follows from this critique that science ought to be examined as a continuous labour of controversies and negotiations. Science must be first characterised by its emptiness, as a *space*, "one that acquires its authority precisely from and through episodic negotiations of its flexible and contextually contingent borders and territories. Science is a kind of spatial "marker" for cognitive authority, empty *until*

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<sup>84</sup>Gieryn (1995), p. 398. There are other difficulties which impinge on essentialist projects of demarcation. Alongside the "experimenter's regress" one ought also to note the role that tacit knowledge and learning by doing plays in scientific networks. Collins and Evans (2002) have recently argued for a redirection of science and technology studies towards studies of expertise and experience grounded on this crucial aspect.

<sup>85</sup>Bourdieu (1999), p. 34.

<sup>86</sup>Gieryn (1995), p. 405.



its insides get filled and its borders drawn amidst context-bound negotiations over who and what is “scientific”.<sup>87</sup>

To talk of science as a space of contingent boundaries is to appeal to a topographic metaphor.<sup>88</sup> Following this metaphor, maps

do to nongeographical referents what they do to the earth. Boundaries differentiate this thing from that; borders create spaces with occupants homogeneous and generalized in some respect (though they may vary in other ways). Arrangements of spaces define logical relations among sets of things: nested, overlapping, adjacent, separated. (...) Most important, just as maps of earthly patches get drawn to keep travellers from getting lost, so maps of other worlds – culture, for example – are drawn or talked to help us find our way around.<sup>89</sup>

The practice of demarcating science from non-science is the practice of drawing such a cultural map, one that enlists a host of alliances that invest science with definition and authority. The mapping of alliances operates through “rhetorical games of inclusion and exclusion in which antagonistic parties do their best to justify their cultural map for audiences whose support, power, or influence they seek to enrol.”<sup>90</sup> Similarly to Shapin and Schaffer’s history of the air-pump controversy, which Gieryn translated as a case study for his topographic frame,<sup>91</sup> or to Hill’s account of radical ideas in the seventeenth century, because mapping culture is also mapping alliances, one is drawn into a history of science as political history. To create a social/cultural space, one engages in the politics of enlist allies, and excluding enemies. The language of map-making proposed by Gieryn is useful to frame history of science as political history.

In adopting the topographic metaphor as a methodology to study scientific controversies, I wish to note some of its advantages. An examination of one of Gieryn’s case studies may be useful to illustrate the insights brought by the topographic metaphor. I have chosen his discussion of the 1836 competition for the

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<sup>87</sup>Idem., emphasis in original.

<sup>88</sup>Ibid., p. 416, notes that this metaphor pervades the work of anthropologist Clifford Geertz, as “culturescape”.

<sup>89</sup>Gieryn (1999), p. 7.

<sup>90</sup>Gieryn (1995), p. 406.

<sup>91</sup>Ibid., pp. 424-429.

chair of logic and metaphysics at Edinburgh University. The contest was between George Combe and William Hamilton. Combe was born to a family of Scottish brewers, after a humble beginning working as a clerk, he was able to fund his law studies in Edinburgh and established his own legal practice. He was the spokesperson of the phrenology theory, founder of the Phrenological Society and editor of the *Phrenological Journal*. Hamilton was educated in medicine, while studying at Oxford he changed his interests to law and later practiced as an advocate in Edinburgh. However Hamilton was uncomfortable with his career as a lawyer and remained an active scholar, via his articles to the *Edinburgh Review* he gained a place amongst the Edinburgh literary elite.<sup>92</sup>

The resources for the historian to identify the maps of the two authors were the testimonials collated by the candidates in their bid for the Chair of Logic and Metaphysics at Edinburgh University. Gieryn extracted four pairs of cultural referents or in his terms “spaces”: expert knowledge/lay knowledge; mental philosophy/physiological anatomy; pure science/useful applications and science/religion; and he discussed how they could be represented in conflicting cultural maps.<sup>93</sup>

The Hamilton testimonials argued in support of the scholar’s expertise. They conceded that Hamilton’s work was comprehensible only to a handful of scholars, and that this stood as proof of the depth of his contributions. The authors of the testimonials, few in number, stressed their own credentials as scholars and their authoritative assessments of the candidate. The Combe testimonials, in contrast, compiled into a two-volume book published for the general public, comprised of short statements from scholars but also from professionals. The choice for many over few testimonials reflected phrenology’s conviction on popular participation. Phrenology spoke to the streets and to the young.<sup>94</sup> Although development of phrenological theory remained the property of a few, fact creation was open to public participation.

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<sup>92</sup>Gieryn (1999), pp. 134-135. Gieryn in this case study owes a significant debt to the work done by Steven Shapin on Edinburgh Phrenology, see p. 20 of this thesis.

<sup>93</sup>Ibid., p. 149.

<sup>94</sup>Ibid., p. 153.

A second aspect of contention was the brain/mind relationship. Hamilton saw “mental faculties [as] processes of the mind at work, not physical states in the brain”,<sup>95</sup> moral philosophy was separate from anatomy. The tools of the moral philosopher were reflection and reasoning, not the examination of skulls. For Combe, mind and brain were one, and therefore so were mental philosophy and physiological anatomy. Inductive work was not only possible but indispensable to answer the questions posed by mental science. “Combe’s testimonials present phrenology as a complete science (because it brought mind and brain within a single compass), a true science (because it was founded on observations in nature), and a new science (destined to sweep Scottish tradition into the dustbin).”<sup>96</sup>

Thirdly, unlike Hamilton’s science that stood as separate and uninterested in application, in the Combe testimonials science is said to serve a practical purpose. Not only did phrenology call for people to participate in fact creation, it was also aimed at giving something back. It fed an understanding of individuals’ talents and limitations, and with this personal knowledge it promised personal development.

A final subject where Hamilton and Combe diverged was the relationship between science and religion. Phrenology impinged on the subject of religion as it re-conceptualised notions of free will and determinism and thus moral responsibility. It was potentially a science of morality, however, it refused to present itself as a challenger to religion, arguing that it was instead its assistant. In the testimonials supporting Combe’s bid for the Edinburgh chair, phrenology was presented settling matters of nature as a preamble to addressing matters of the divine. Hamilton’s testimonials make no allusion to religion. In Hamilton’s cultural map science and religion are too far apart to be discussed together, “they occupied well-bounded, amply distanced cultural spaces”.<sup>97</sup>

#### Combe’s map

extend[ed] the frontiers of science outwards into once autonomous regions of politics, religion, metaphysics, and common sense, in order to get other

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<sup>95</sup>Ibid., p. 157.

<sup>96</sup>Ibid., p. 160.

<sup>97</sup>Ibid., p. 178.

audiences interested in his project. The old kind of science proffered by William Hamilton and the *Edinburgh Review* was so confined, so circumscribed, that huge chunks of Edinburgh society (and beyond) had no reason to be interested in it and no hope of actually entering its halls. Combe's map stretched out a vast terrain for science, capacious enough to hold the masses of people now given a reason to learn about *this* science and even to become part of it.<sup>98</sup>

In contrast:

Hamilton's testimonials in effect reproduced brick-and-mortar Edinburgh, preserving in cultural form geographic distances between the institutions of church and politics, preserving the space between scientific disciplines at the university, preserving the separation of the Royal Society from the Canongate slums. Science keeps its place, in the university and in the Royal Society, the preserve of a learned few.<sup>99</sup>

Gieryn ascribes the result of the contest, Hamilton's victory, to the political times. The 1820s in Edinburgh had been years of political unrest with the Whig party facing the Conservatives, during which both Hamilton and Combe sided with the Whigs and their liberal reform. But in the 1830s with political power just secured, the Whig party wished to halt further political turmoil. Hamilton was thus able to enlist the party's support in addition to that of the Church and the University status quo. The Church fervently refused to have "their religion "thrown into the character of a science." God needed no additional evidence from facts."<sup>100</sup> Gieryn concludes:

Combe simply could not sell phrenology among those who had circumscribed science so narrowly, from their chairs at the university or from the *Edinburgh Review* - it took a different science to attract enthusiasts from elsewhere, a science pushing out into cultural domains (politics, religion, common sense) where it had not gone before, enrolling allies along the way. These allies, in their testimonials, then redrew for the town council a map of Edinburgh's culturescape that must have been unsettling enough to make a vote against Combe easily justified.<sup>101</sup>

<sup>98</sup>Ibid., pp. 130-131, emphasis in original.

<sup>99</sup>Ibid., p. 147.

<sup>100</sup>Ibid., p. 176.

<sup>101</sup>Ibid., pp. 148-9.

This case-study exemplifies how the topographic metaphor may be instrumental for the study of epistemic disputes. In Gieryn's study of the Combe-Hamilton contest, the maps join the rhetorical (phrenologists' attempt at convincing the church that it could benefit from their science), the symbolic (the rank of chair of metaphysics and logic as a site of authority), and the epistemic (anatomy and philosophical reflection) in the construction of allies and enemies in a particular historical context (Whig Edinburgh).

Unlike laboratory controversies, those centred on a theoretical assertion or experimental result, these debates delve into the doctrinal, on the identity of science and its disciplines. Disputes between dissenters and mainstream in economics are also contests of this kind, where scientific credibility is under challenge. Where both sides of the dispute contend for scientific credibility, we are faced with distinct cultural maps as providers of cognitive authority and meaning.

As Gieryn's research attests, what lies in such cultural maps cannot be divined. There is no ahistorical master map from which all emerge, made of recurring cultural spaces. For instance, there is no reason to expect that the expert/lay knowledge distinction plays a role in all controversies. The maps are constructed from the statements of authors distinguishing themselves from other authors, from their rhetoric of difference, and thus given by the historical case. Such precepts yield an outsider's account of scientific controversy.

## 1.5 The making of dissent in economics

I am forewarned by work in the social studies of science to reject essentialist definitions of dissent, as one ought to reject essentialist definitions of science and non-science. Dissent is a socio-political construction contingent to its historical context. With this outlook I see the present work as following the challenge set by Theodore M. Porter to historians of economics, to undertake what he terms cultural history:

It refuses to take our own theories and standards of knowledge as self-evident, but asks where they came from, how they were embodied in institutions, and what meanings they have evoked to practitioners. Above all, cultural history cannot take the boundaries of a discipline, such as economics, as fixed or impermeable, but strives to situate it on a wider terrain as part of a broad historical dynamic.<sup>102</sup>

Attention to the problem of the shifting boundaries of economics as a historical subject is not new. For instance, Mary Furner's *Advocacy and Objectivity*<sup>103</sup> studied the emergence and crystallisation of professional values that came to define the American economics community. Furner chose "academic freedom cases" as a unit of analysis. By studying and comparing two separate "waves" of freedom cases, in the 1880s and the 1890s, she was able to describe the evolution of professional values, and to show the emergence of a consensus on how a social scientist should behave. In the process, what may have been acceptable for an economist in the early 1880s (for example publicly defending labour unions) became unacceptable in the late 1890s. Dissent is not a static concept, as Furner has convincingly shown with her notion of "permissible dissent."<sup>104</sup> Even in the course of two decades, neither "dissent" nor "permissible" were static concepts. However, and in stark contrast to Furner's work, I have argued that most of the treatment of dissent in the history of economics has been opportunistic. This scholarship which I reviewed early in this chapter, has continuously sought to reinterpret the boundaries of science and dissent in accord to its presentist agenda. Using the distinction made in science studies and discussed in this introduction, such work on the history of dissenting economics is not concerned with studying how dissent is defined in particular historical contexts, *studying* boundary work; but rather seeks to create new, operative definitions for present day concerns, *doing* boundary work.

The subject of this thesis is the construction of dissent from circa 1960 to 1980 in North American economics. I approach dissent and its controversies as instances of

<sup>102</sup>Porter (2004), pp. 165-66.

<sup>103</sup>Furner (1975).

<sup>104</sup>Permissible dissent is any action that may put an economist in a dangerous position with respect to the 'outside world' (of non-economists) but for which he will most likely receive the support of his peers, were he to be threatened by this outside world.

boundary work, which following Gieryn may be interpreted as mapmaking in the “culturespace.” My research question is thus how did dissenters in USA economics in the 1960s and 1970s define their challenge, their cultural maps of economic science? My focus is on self-definition, on the dissenters’ making of their identity. This theme runs in the literature of religious and scientific dissent I have reviewed in this chapter. The prominence given to strategic or political elements in these studies scientific controversy highlights the dissenter’s role as an agent in her own history.

For my exploration into the making of dissent in 1960s and 70s American economics, I undertake a study of two groups that formed in this period, Radical Political Economics and Post Keynesian Economics. I will explore these two cases separately in two distinct parts to the thesis (Part II for Radical Economics, Part III for Post Keynesian Economics).<sup>105</sup> Each part begins with an introduction to the dissenting group, discussing historical scholarship on their emergence and identity, and is concluded with a chapter surveying my findings on the process of dissenters’ self-definition.

My literature survey (chapter 2) on the history of the Radical Political Economics community locates its emergence in the late 1960s and in connection with the student radical movement. My review of this literature also reveals some gaping omissions on the tales of emergence, silences that are characteristic of participant accounts of history. The first of two substantive chapters on Radical Political Economics (chapter 3) explores the origins of radical economics in the American New Left. I show that radical economics was until 1971 a programme to be defined under the auspices of the Union for Radical Political Economics (URPE). I interpret the definition of radical economics as a challenge to the credibility of the economics profession, where political commitment was made to take a central role in knowledge making. In the following chapter (chapter 4) I explore controversies amongst the radicals over their participation in changing American society. In the 1970s, radicals

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<sup>105</sup>The labelling of the groups is one of the historical subjects discussed in the thesis. In the text I capitalise both “Radical Political Economics”, “Post Keynesian Economics” and “Post Keynesians”, but I do not capitalise “radical economists”, as I have found these to be the most commonly used spellings in the relevant literature.

at URPE became engaged in outreach work with political movements and these efforts forced a period of re-definition for their dissent. Prominent among the materials I use for my account of radical history are a series of interviews.<sup>106</sup> The interviews provided me with an overall grasp of what the valuable documents were, as a road map to the radical literature and events, and they underpinned my interpretation of the radical texts.<sup>107</sup> Secondly, the interviews allowed me to tap into facts not recorded in the documents, notably personal and organisational relationships that typically leave no written trail.<sup>108</sup> My narrative of radical economics concludes in the late 1970s, I draw some conclusions (chapter 5) over the role of URPE in the history of radical economics and I extend my discussion on the process of making radical economics. It is based on the discussion of earlier chapters, that I show how radical identity changed from 1968 to 1978 and how the radicals' challenge to mainstream economics amounted to a new cultural map. The controversy between radicals and mainstream is cast as a case of boundary work.

As I discuss historical scholarship on the emergence of Post Keynesian Economics (chapter 6) I identify an overwhelming body of literature reflecting on the intellectual origins of the group. The role that self-history plays in the debates between Post Keynesians is a starting point for my examination of their emergence. I've located their origins to the early 1970s (chapter 7) in connection with an influential interpretation of the capital theory debates between Cambridge Keynesians and neoclassical economists. In my account, the Post Keynesian community was formed by efforts of American dissenters to tie their research to what they saw as a revolutionary new theory emergent in the Cambridge work. I retell how in the mid-1970s Post Keynesians challenged the mainstream and formed into a self-conscious grouping. Focussing on a later and wider period extending into the mid-1980s (chapter 8) I discuss how different spokespersons within the recently created Post Keynesian camp, proposed conflicting strategies and identities to further the expansion of the group and its challenge to the profession. These later

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<sup>106</sup>See Appendix A on the interview's methodology, p. 285.

<sup>107</sup>Seldon and Pappworth (1983), p. 43.

<sup>108</sup>Ibid., p. 39.



controversies often focused on the labelling of the group and I use debates over the label and its definition as an entry point to trace internal strife in Post Keynesian Economics. My research in the history of Post Keynesian Economics is informed primarily by archive research. I have found that the archival record in many respects contradicts the current understanding of Post Keynesian history. I conclude (chapter 9) by relating my findings to other contributions in the history of Post Keynesian Economics. I also discuss how the controversies between Post Keynesians may be analyzed in the light of the concept of boundary work and outline what changes these brought to Post Keynesian identity.

In the concluding chapter 10 I reflect on the origins of the 1960s and 1970s crisis in economics. I compare the boundary work of radicals and Post Keynesians and its impact on the communities' identity. Finally I discuss how the economics profession managed the challenge of the dissenters and how this response has brought about the isolation of dissenters from the core of the discipline.

## Part II

# RADICAL POLITICAL ECONOMICS

# Introduction to Radical Political Economics

## 2.1 Participant histories of Radical Political Economics

Dissenting economics communities emerged in the 1960s and 1970s. However, we know little about the process of their constitution. How were dissenting identities constructed so as to join discontents into a challenge to the economics profession? Nor do we know much about how the dissenting communities developed in their first years of existence. The goal of this chapter is to survey the historical literature on the first dissenting community that I research in this thesis, Radical Political Economics (RPE). Herein, I discuss accounts of radical economics' emergence and outline how my own research may be related to their insights.

Most of the contributions to the history of Radical Political Economics can be found in introductory books, surveys or collections of essays written by radical economists. Authored by members of the radical community, these are not detailed historical enquiries. Instead, most provide only a brief background setting before tackling in greater detail contemporary concerns such as discussing new horizons for radical scholarship. It follows that, the presentist concerns of these accounts often impose upon their treatment of the past the consensual meanings of today. They should therefore be classed as participant accounts. This literature warrants a critical reading that isolates what is asserted from what is well sustained by evidence.

## 2.2 The “left academic”

It is unanimously acknowledged that Radical Political Economics was part of the 1960s American surge of academic radicalism. In a volume surveying the growth of radical thought during the 1970s, Herbert Gintis wrote on the economics counterpart to this “cultural revolution.” His primary focus was to outline radicals’ views on theory, contrasting these to the economics mainstream. With regard to the why of RPE’s emergence, he wrote that it “was a direct response to the civil rights, antiwar, and feminist movements which shattered the post-World War II “consensus”.”<sup>1</sup> Gintis’s answer is in line with the accepted explanation for the broad radicalisation of American academia, an explanation applied to all disciplines, according to which radical thought was a response to the social movements of the 1960s.<sup>2</sup>

Similarly, Samuel Bowles and Richard Edwards have argued that radical economics emerged as “a critique of the forms of domination confronted by workers, women, and people of colour.”<sup>3</sup> The left politics of American society was said to inform the radicals’ work in the design of research agendas, that “much of the research agenda of radical political economy has derived from involvement and commitment to these movements”,<sup>4</sup> or even by shaping the content of radical theories. As Victor Lippit has argued, radical economists “perceive the existence of a relationship between scholarship and class interests, and understand that a given social structure tends to support its dominant class interests as much through the intellectual environment it sustains as through other means.” Radical Political Economics is said to be scholarship committed to the disadvantaged or oppressed.<sup>5</sup>

While radical economists see their history beginning in the 1960s, others have pointed to a longer frame of reference. Notably, Frederic S. Lee has stressed the importance of considering the repression of American left-wing economics in the 1940s and 1950s. According to Lee, repression brought about by McCarthyism was

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<sup>1</sup>Gintis (1984), p. 53.

<sup>2</sup>Ollman and Vernoff (1984) contains studies on other disciplines.

<sup>3</sup>Bowles and Edwards (1990), p. 4.

<sup>4</sup>Idem.

<sup>5</sup>Lippit (1996), p. 5.

coupled with the conservative ethos of American society that was pro-business and anti-government. At the level of the Universities, increased mathematisation and a system of loyalty oaths contributed to the exclusion of heterodox discourses. Marxism was said to have survived briefly in Communist Party Schools, which legislative action in the late 1940s soon forced into extinction. In the midst of repression, two publications resisted and contributed to extending interest in Marxian economics, the scholarly *Science and Society* and the magazine *Monthly Review*. For Lee, these publications bridge the repressed 1950s Marxism and late 1960s radical economics.<sup>6</sup>

While all authors concur on the importance of the 1960s social movements for the emergence of radical economics, they offer dissonant accounts on their role. For Bowles, Edwards, Gintis or Lippit, all prominent members of the radical community, the “movement” raised intellectual concerns that only the development of radical economics could satisfy. Because they tell us that only RPE could provide the answers demanded by the oppressed, it follows that the oppressed demanded the creation and have provided continued justification for the radicals’ existence. Lee takes a different view: the social movements permitted radical economics to break the cage cast by the mainstream,<sup>7</sup> and were secondary to the formation of the group. For the author radical economics pre-dated the 1960s, laying latent in 1950s’ Marxism. Lee in his history did not make much of radicals’ activism for the formation of the group. He narrated a succession of meetings and institution-building efforts starting in 1965 with the joining of radicals across the U.S.A., but social movements seem to have played no role in these developments.<sup>8</sup>

Despite the recurring claims that radical economics is fused to political commitment and was born out of this connection, remarkably little exists on how this relationship has played itself out historically. How have these social movements communicated their concerns to radical economists? What kind of political work have radical economists been involved in? How have radicals’ political commitments

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<sup>6</sup>Lee (2004a), pp. 179-180, 182-183.

<sup>7</sup>Although militantly a heterodox economist, Lee is not a radical and is best described as a Post Keynesian.

<sup>8</sup>Lee (2004a), pp. 187, 189.

changed over time? What demands have these commitments placed on their academic work? These questions are never posed. For members of the group, for radical economists authoring their own history, that left politics was part of their work is self-evident and requires no scrutiny.

The radicals' vagueness in detailing the content and workings of their political commitments is evident in reading Susan Fleck's history of the Union for Radical Political Economics (URPE). The author interpreted the history of the Union as subservient to political concerns. URPE was created in 1968 and she notes that its "core purpose was and is to be an alternative professional organization for left political economists and an intellectual home for academics, policy-makers, and activists who are interested in participating in a left intellectual debate on theoretical and policy issues".<sup>9</sup> Fleck highlighted how commitment to the "movement" was part of the stated purposes of URPE. Of the three objectives presented in the Union's first prospectus, the second referred to developing "new course and research areas which reflect the urgencies of the day" and the third that "political economics should be sensitive to the needs of the social movements of our day".<sup>10</sup> Fleck asserts that the creation of caucuses within URPE was the product of activist demands. A Women Caucus was formed in 1971, a Gay, Lesbian and Bisexual Caucus formed in the early 1980s and a Third World Caucus founded in the late 1980s. These were created to voice the adherents' concerns inside URPE, reshaping its research agendas and debates, to lobby for new emerging political concerns.<sup>11</sup> Fleck's history of URPE publicizes the radicals' left politics but is elusive on details and evidence. While she mentions the importance of "activist demands" for instance in the formation of the caucuses, we are not told in what activist activities radicals were involved, and in what way the caucuses were designed and functioned to support these activist concerns.

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<sup>9</sup>Fleck (2003), p. 24). Fleck first wrote her account as an introduction to URPE to be posted on the organization's website (Fleck in [www.urpe.org](http://www.urpe.org)), since then she has published several versions of her account but these do not vary substantially (Fleck (1998, 2003)).

<sup>10</sup>Fleck (2003), pp. 24-25. For the sake of completeness the first objective of URPE was "to promote a new interdisciplinary approach to political economy which includes also relevant themes from political science, sociology and social psychology" (Ibid., p. 24.).

<sup>11</sup>Ibid., pp. 25-26.

The recurring references to political commitment as informing radicals' work ought to be contrasted with the more contentious subject of into which economics tradition RPE should be classed. This classification problem seems connected with the radical economists' uneasy relationship to Marxian economics and its canon. Radical economists often note some intellectual debt to Marxist scholarship in shaping the field's early development, namely "The work of three American Marxists – Paul Baran, Paul Sweezy and Harry Magdoff – as well as German-born but American-educated Andre Gunder Frank and the Belgium Marxist Ernest Mandel played an important early role in the formation of ideas that became known as radical political economy".<sup>12</sup> But most radical economists are reluctant in taking on the label of Marxists, and when kinship between radical economics and Marxism is established, it is accompanied by many disclaimers.<sup>13</sup> There is substantial criticism of Marxist scholarship, the underlying claim being that: "much of received doctrine in European Marxism was merely the result of the dominance of Soviet-style preconceptions relevant to an earlier era and not necessarily applicable to advanced capitalism in general nor to the United States in particular."<sup>14</sup> The social reality of American capitalism is seen as demanding an abandonment of "traditional Marxism."<sup>15</sup>

Radical economists critique "traditional Marxism" for sharing some of the shortcomings of neoclassical economics. They argue that old Marxism holds a restrictive view of the economy, limited to the factory or the market which are portrayed as a mere loci for an economicist rationale. In contrast, Radical Political Economics studies the appropriation of nature, a broad system of political and cultural relations of which schools, families and communities are also part. Radical economists also critique the view that the individual precedes and is independent of the economy. Their argument is that the economy produces goods, services and fundamentally also individuals.<sup>16</sup> As a result of the contested overlap between radical

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<sup>12</sup>Barone (2004), p. 3.

<sup>13</sup>Significantly, and in opposition to those closer to Marxism, some radicals argue that institutionalism should be seen as the defining intellectual influence in radical economics, with a deliberate focus given to institutions and the social environment as shaping economic activity (Lippit (1996), p. 7).

<sup>14</sup>Gintis (1984), p. 55.

<sup>15</sup>Also referred to as "official", "fundamentalist" or "orthodox" Marxism, see Sherman (1987).

<sup>16</sup>Gintis (1984), pp. 55-56.

economics and Marxism, histories of Marxian economics are mute to Radical Political Economics.<sup>17</sup>

The majority of radical authors argue that a plurality of traditions converge in radical economics:

First, there is the classical tradition, with emphasis on David Ricardo and the neo-Ricardian tradition, especially in the work of Piero Sraffa. Second, there is the Marxist tradition, taken as a set of powerful hypotheses, but not as an eternal truth. Third, there is the left Keynesian tradition and the post-Keynesian writers, such as Joan Robinson. Fourth, the radical paradigm has been strongly affected by the institutionalist economics of Thorstein Veblen. All of these traditions have merged to some extent in modern radical political economy, in different proportions in different writers<sup>18</sup>

The suggestion is that radical political economics ought to be characterised as a diverse whole that includes Marxist analysis, institutional, left Keynesian and social economics. But there is no genealogy of authors that radical economists may consensually accept as their intellectual forefathers; and each radical economist claims his own influences.

For most dissenters intellectual diversity is a weakness in want of explanation. According to Lee it was the 1950s repression preventing the teaching of Marxism, that produced in some radical economists' work an uneasy mix of neoclassical economics tools and Marxism. Most of the radical economists had only been trained in neoclassical economics and knew very little of Marxist economic theory. Equally important in Lee's explanation of RPE's diversity, was that it mirrored the political culture of the 1960s:

many college activists in the New Left movement were also interested in a plurality of non-Marxian ideas, such as existentialism and anarchism. At the same time, they restricted their interest in Marx to his concept of man and alienation and adopted an American radicalism that railed against large corporations, Wall Street, and the exploitative nature of free enterprise and the status quo. As a result, they preferred to call

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<sup>17</sup>Howard and King (1991); Eatwell, Milgate and Newman (1990).

<sup>18</sup>Sherman (1987), p. 4.



themselves radicals or leftists as opposed to Marxist or socialist (which were felt to be narrower and less inclusive).<sup>19</sup>

While Lee found the origins of diversity in the radicals' education, others have suggested it has been a more recent development. They argue that "radical economics was led to differentiation "as theoretical sophistication grew".<sup>20</sup> There is reassurance in this explanation of diversity, as it is ascribed to theoretical progress. Overall, and as in the case of the radicals' commitment to left politics, it seems the origins and history of radical economics' intellectual diversity have been only marginally explored.

Distinct from the participant accounts I have reviewed so far, was Paul Attewell's *Radical Political Economy since the Sixties*.<sup>21</sup> Attewell was not a radical economist writing the history of his community, instead he studied the subject from the vantage point of the sociology of knowledge. His approach was markedly analytical. It progressed in two stages: beginning with the definition of a stylised radical agent and an environment within which this agent must act; followed by an argument on how the characteristics of radical economics arose from the interplay of his radical agency and environment.

Attewell began by constructing what he called "a *homunculus*, a hypothetical left academic" that was characterised by three fundamental goals distinguishing him from "apolitical colleagues." The first characteristic was the left academic's adherence to Marxism, from which followed efforts to preserve it as a scientific paradigm. Secondly, the left academic was said to hold a belief in social improvement, which prompted his moral and evaluatory analyses of social phenomena. Finally, the left academic was said to analyse emerging social events; Attewell defined him as a commentator on contemporary affairs.<sup>22</sup>

In Attewell's setting, the left academic was a player with defined goals facing "obstacles" inherent to the academic environment. He was described as acting in the midst of "tensions" and "paradoxes" operating both internally and externally to

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<sup>19</sup>Lee (2004a), p. 188.

<sup>20</sup>Flaherty (1987), p. 36.

<sup>21</sup>Attewell (1984).

<sup>22</sup>Ibid., pp. 17-20.

theory. Outside the field of theory, the agent was faced with inter-paradigmatic and group hostility - the academic environment, being structured into exclusive solidarities, would be inherently hostile to the left-wing academic. The primary tension inside theory was set in the language of Thomas Kuhn's paradigms, a shock between maintaining the Marxian paradigm in the face of new phenomena and the goal of addressing these in morally charged judgment.<sup>23</sup>

Attewell deduced from the described axiomatics, defining agency and the environment in which it acts, some of the general characteristics of Radical Political Economics. He singled out as in need of explanation radical economics' empirical character, its stress on periodisation and its departure from Marxism. The strong empirical character of American radicals' work, which Attewell compared with the abstract theoretical work of British socialists, was explained with reference to the academic environment that the left-wing American faced. According to Attewell, shows of technical competence shielded the left academic's work from the hostile establishment.

The radicals' emphasis on periodisation was another artefact that Attewell's analysis accounted for, and it arose from the left academic's goal of preserving the Marxist paradigm from criticism (what Attewell labels a tension inside theory). By breaking capitalism into different periods, Marx's relevance could be asserted for earlier stages of capitalism while radical economics' contributions were shown to address later ones, namely the new stage of American monopolist capitalism.<sup>24</sup> Radical economists were thus able to present new perspectives on the economy without discarding Marx and Marxism as flawed.

Attewell further explained radical economists' departure from traditional Marxism in terms of the left-wing academic's goal of securing a moral bite for his work. In explaining changes in theories of imperialism, Attewell argued that the choice of a nationalist definition of wealth: "wealth belongs to the natural owner by right of residence"; instead of the traditional Marxist conception of wealth as a

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<sup>23</sup>Attewell implicitly assumes that new phenomena invariably contradict the Marxian paradigm. Ibid., pp. 21-36.

<sup>24</sup>Ibid., p. 31.

product of labour, catered to the moral metaphor of imperialism as theft.<sup>25</sup>

Despite Attewell's insightful composition of general characteristics of radical economics from an idealised depiction of the radical academic, the study suffered from several shortcomings. Given my focus on the emergence of dissent it is problematic that Attewell's sociological frame was never justified and was imposed a priori. We are not told how the "left academic," the base for his idealisation, came to be. Instead the author must rest his claim on a "*hypothetical* left academic."<sup>26</sup> As noted in a review of Attewell's book, these failings arise from a lack of historical detail.<sup>27</sup> The author approached radical theory through his ideal academic and never related it to the real (historical) academics that developed it. Attewell also studied the characteristics of particular branches of radical economics, under the headings of imperialism, dependency and crisis theories, "the labour process and the firm" and "theories of economic segmentation and poverty". But these accounts of sub-fields of radical theory read as testing grounds for the conceptual framework, the sub-fields of radical economics appear as largely disconnected, beyond their role as further raw material for Attewell's sociological exploration. Perhaps for these reasons Attewell's book was ignored by radical economists and was reviewed only in sociology journals.<sup>28</sup>

It is interesting to note that Attewell revisits many of the themes found in radical economists' brief historical reflections. The author's concept of moral drive as a distinctive characteristic of the left academic echoed the stress given elsewhere to activism. The author repeated the argument held by radicals that it was political engagement that directed their research. The theme that radical economics is the product of a mix of intellectual traditions was also noticeable in Attewell's account when he considered non-Marxist radical work, for instance the radical institutionalist dual labour market theory.<sup>29</sup> Attewell largely accepted as unproblematic what I have called participant accounts of the history of radical economics, in effect they seem to be the unacknowledged source for Attewell's characterization of the left academic.

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<sup>25</sup>Ibid., p. 34.

<sup>26</sup>My emphasis.

<sup>27</sup>Riddell (1986), p. 435.

<sup>28</sup>Riddell (1986), Banuri (1986).

<sup>29</sup>Attewell (1984), pp. 44-53.

### 2.3 Externalizing the history of Radical Political Economics

The primary aim of my research is to explain the emergence of dissent in 1960s and 1970s American economics. For this endeavour existing scholarship on the history of dissenting groups provides guidance, but one that must be carefully scrutinised. We are offered participant accounts, informally written by radical economists and lacking in historical evidence. Their authority is that of self-examination and of first-person testimony. These histories are likely to present myth as history and to impose on the past the definitions that the radical community now holds as unquestionable, and which I wish to research as contingent and constructed. The only major book-length research in the history of radical economics, although not written by a radical economist, accepted the radicals' contemporary self-definition and rationally reconstructed their theory development on that basis. Attewell manifestly replaced the history of radical economics with an analytical exploration.

Even if participant accounts are unsuited to provide clues as to the history of the construction of radical economics, they nonetheless effectively highlight what lies most prominent in radical identity. Two themes stand out from my reading of this literature: radicals' closeness to the political left-wing, in particular, the social movements of the 1960s; and Radical Political Economics' intellectual diversity.

Radical Political Economics is at times deemed Marxist, Institutional, Keynesian, or all of the above. The diversity it exhibits is taken by commentators as inevitable, something either inherent to the progress of ideas, or an outcome of the radicals' education. In either case, we are never shown how intellectual diversity developed. It is quite remarkable that we know so little of the content of the radicals' early debates, and virtually all references to controversies between radicals are post-1980.<sup>30</sup> While Lee offers a thorough chronology of meetings between radicals up

<sup>30</sup>For instance, see Norton (1992) for a survey of radical crisis theory and Ruccio (1992) on radical work on debt and development. Understandably, more current surveys make even less note of radicals' work in the 1970s and focus exclusively on the 1980s and 1990s, see Barone (2004).

to URPE's creation in 1968, details on the content of the radical meetings are thin.<sup>31</sup> The literature leaves us uncertain about the intellectual landscape of radical economics prior to 1980.

No subject is more important for radicals than their politics. It is in politics that radicals locate their emergence, namely in their connection to the 1960s social movements. Yet, the details of this relationship are never elaborated. For instance, although Lee references the early involvement of Michigan radical economists with the student activist Radical Education Project in the mid-1960s, we are not told how it shaped radical economics.<sup>32</sup> At the level of URPE history, little evidence is given of activist demands shaping the organisation's everyday activities, one of its stated aims. In what ways did activist demands inform the radicals' work? How did activism prompt radicals to organize a Union with its own journal, newsletter and annual meetings, as Fleck has suggested? These questions form a suitable starting point for my inquiry. I must begin by revealing what the relationships were, if any, between the sixties social movements and emerging radical economics. This is the subject of the following chapter.

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<sup>31</sup>Lee (2004a), p. 187-190.

<sup>32</sup>Lee (2004a), p. 187.

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## A political divide

### The radical challenge to economics

#### 3.1 Students, radicals and the economics profession

Radical economics emerged in the U.S.A. in 1968 with the creation of the Union for Radical Political Economics (URPE). While in agreement with the historical literature that traced the origins of radical economics to the 1960s,<sup>1</sup> the present account is distinct from earlier works by placing radical economics' emergence as the outcome of the creation of URPE. It was the success of URPE as a union of discontent young economists that urged the enunciation of a new kind of economics.

I describe the process of making radical economics in the interplay of three histories: the history of student radicalism, the history of URPE and the history of the debates between radicals and the mainstream. I begin by reviewing the history of student radicalism separately, taking as my sources the secondary literature on the 1960s and the New Left. The histories of URPE and of the radicals' controversies with the mainstream nest into the high point of student protest, from 1966 to 1970. I deal with the two histories together to show how URPE's mission became the creation of a radical economics to challenge the economics profession. My discussion shows how the radicals' controversy with the mainstream was related to the growing tensions and conflicts that then pervaded American society. I further look at how the dispute between radicals and mainstream economists quietened in the period of decline in student unrest, post-1971, examining some notable cases of hiring and firing of radical economists.

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<sup>1</sup>As shown in the previous chapter.

### 3.2 A three-period history of student radicalism

The background to the history of radical economics' emergence is the student protest movement of the late 1960s. Although the history of Radical Political Economics proper begins only in 1966, the later period in the history of this movement, it is important to reach further back to capture the intellectual development of student radicalism. The intellectual origins of the New Left help explain some of the paradoxes in the history of radical economics.

I introduce a three-period history of the student left.<sup>2</sup> It will allow me to make three points. The first was that sixties radicalism had its intellectual origins tied to American liberalism. Secondly, the protest over the Vietnam War transformed student radicals into a national political force while deepening their criticism of American society. By 1968, self-confident of their political clout, radicals began calling for revolution. Thirdly, as the 1960s generation of radicals graduated from University and as the Vietnam conflict began to cool into a victory for the Vietcong, the momentum of student unrest was broken and waned leaving the campuses devoid of radical activity.

#### 3.2.1 Parting ways with liberalism (1961-1965)

The radical student movement in the U.S.A. was from its inception labelled "New Left" in a reference to the contemporary British experience.<sup>3</sup> The adoption of the label may seem paradoxical. For unlike Britain, where 1950s and 1960s young radicals were breaking with a still influential Marxist Left, in the United States there was no active left wing. The 1950s McCarthyism had effectively decimated left of centre politics.<sup>4</sup> The intellectual and organizational origins of the American New Left were

<sup>2</sup>My periodisation of the history of the New Left owes much to Kirkpatrick Sale's history of Students for Democratic Society (Sale (1974)). The author noted four periods in SDS history: 1960-62 – reorganization; 1962-65 – reform; 1965-68 – resistance; 1968-1970 – revolution. Sale's scholarship is sometimes criticised for being overly brief in analysis, but it remains invaluable as a source of primary materials on SDS since the author poured into his account much of the organization's office archives.

<sup>3</sup>Buhle (1998), p. 545.

<sup>4</sup>On the repression of the left, see Cauter (1978), pp. 161-223; on the resulting destruction of the Communist Party-USA, see Steinberg (1984), pp. 261-292.

not the Old Marxist Left.<sup>5</sup> The “New” in the New Left label was posed in opposition to American liberalism.

American student radicalism had its intellectual and institutional origins in liberalism. The early 1960s radicals were born into well-educated, liberal-minded families.<sup>6</sup> They “were frequently the democratically bred, intelligent children of highly educated parents. . . . They believed in the intrinsic value of education and came to college with the expectation that in it reposed value and virtue”.<sup>7</sup> The early radicals were also overwhelmingly students of the social sciences (sociology and history, but also a large number from economics). Involvement in radical activism by students from other subjects came only later in the 1960s and was never as widespread.<sup>8</sup>

The major student New Left organisation, Students for Democratic Society (SDS), grew from the liberal political camp, created at Ann Arbor, University of Michigan, in 1960.<sup>9</sup> In one of their earliest activities, beginning in 1960, members of SDS travelled in their summer breaks to the American South and participated in the “freedom rides” and voter registration campaigns, opposing racial discrimination.<sup>10</sup> Students’ reports from the South reflected their belief in liberal values but also voiced a sense of bankruptcy towards its practice. As Kirkpatrick Sale commented in his history of SDS, “the sham and the shabbiness of the liberal tradition in which they all had grown up was slowing coming to be felt”.<sup>11</sup>

The civil rights campaigns led SDSers to reassess their politics. They coined their alternative to liberalism, “radicalism.” The statement of the new political philosophy

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<sup>5</sup>Isserman (1987).

<sup>6</sup>Lipset and Altbach (1969), pp. 216, 128. Some would also add the adjective “affluent,” for instance Sherkat and Blocker (1994).

<sup>7</sup>Horowitz (1986), p. 13. The literature on student activism and radicalism is overwhelming; Keniston (1973) provided an annotated biography of over 300 sociological and psychological studies done in late 1960s and early 1970s.

<sup>8</sup>In the early years of SDS, up to its expansion in 1965, the centers of activity were the large public and private universities of the American Northeast. State universities particularly in the American Mid-West became increasingly important later on. The history of the radical movement in these public institutions is in many respects distinct from that of the private ones, on this subject see Heineman (1993).

<sup>9</sup>The students led by Al Haber renamed and reorganized the student branch of the League for Industrial Democracy (SLID), a liberal society devoted to cultural and educational activities. Robert Allen (Al) Haber was a sociology graduate student, son of Michigan economics department’s member Bill Haber, once an active member of the League for Industrial Democracy. (Horowitz (1986), p. 17).

<sup>10</sup>Sitkoff (1981), pp. 97-126.

<sup>11</sup>Sale (1974), p. 17.



was enshrined in the *Port Huron Statement* (1962). This manifesto drew on the values of humanism, individualism and community, to call for “participatory democracy.”<sup>12</sup> The SDS leadership in 1963-64, bolstered by their civil rights experiences, was planning to apply the bottom-up strategies of the Student Nonviolent Coordinating Committee (SNCC)<sup>13</sup> to the American North, to fight poverty and political alienation. In the summer of 1964, SDSers began “The Economic Research and Action Project” (ERAP). Radicals moved to local communities where they lived and organised with the goal of leading the poor to political action. ERAP was abandoned in 1965 (although two projects at Newark and Chicago continued into 1967). Radicals found it difficult to reach the poor and to spur them into self-organisation and sustained political work. In contrast to their initial grander prospects, they had achieved only small scale lobbying in improving the bureaucracy – helping local people claim benefits, talking to landlords, etc...<sup>14</sup>

Thus, the beginnings of student radicalism were made outside the Universities, first participating in the civil rights campaigns, later in the ERAP projects in the urban North. Radicalism was designed as the politics of popular participation. It was the voluntarist ideal of collectively curing the ills of American society.

### 3.2.2 Vietnam War and revolution (1965-1970)

Radicals only began to focus their political activity in the Universities when the Vietnam War took centre-stage. This happened in 1965, following the February Presidential decision to bomb North Vietnam and pour thousands of troops into the conflict. The first of many “teach-ins” in American Universities on the subject of Vietnam was held at the University of Michigan on the 24<sup>th</sup> of March 1965.<sup>15</sup> In a few days, teach-ins spread across the colleges. The most famous took place at Berkeley

<sup>12</sup>Ibid., p. 50.

<sup>13</sup>The Student Nonviolent Coordinating Committee (SNCC) was created in 1960 in the early years of the sit-in movement (Sitkoff (1981), pp. 91-4). It was an independent organization run by young black youth, and was responsible for some of the bravest activities of the civil rights campaign, from the “freedom rides” to “voter registration.” Later in the decade it was home to the first calls for Black Power (Carson (1981)).

<sup>14</sup>Sale (1974), pp. 143-144.

<sup>15</sup>The “teach-in” was agreed with the University administration to replace the earlier call for a mass strike instigated by faculty (Lipset and Altbach (1969), p. 203).

with an attendance of 35,000 and lasting for 36 hours. In April 1965 SDS organized its first demonstration against war in Washington, it drew about 25,000 people and launched SDS to national recognition. In weeks SDS membership multiplied from 50 chapters in December 1964, it had 95 in May and by December 1965 there were 124.

President L. B. Johnson in order to increase troop numbers in Vietnam in 1966 reformed the selective service system which decided who should be sent to fight overseas. The new system refined the student exemption, with only the top half of the class (ranked by grades) being given exemption from military service. The student radicals responded with the new tactics of sit-ins and burn-ins (burning draft cards) causing greater disruption to campus life. Significantly, soon after, the National Mobilisation Committee was created as an umbrella organization for anti-Vietnam War groups. In the Spring of 1967 it successfully organized two major rallies in New York and in San Francisco.

In the teach-ins, sit-ins, burn-ins, the “in” was typically the campuses, and the Vietnam conflict primed radicals to a critique of the University. The complicity of University administrations with war-related agencies was revealed throughout the year of 1967.<sup>16</sup> The criticism of the Universities led radicals to experimenting with “Free Universities,” an institution that could be free from the constraints of the academic establishment and would address issues that would otherwise be taboo, prominently the war. The first Free Universities in the fall of 1965 were at Berkeley, Florida, New York, and Chicago, by 1966 there were 10 and by 1970 they were said to number 500.

The year of 1968 needs to be singled out in the history of the student movement. It was a watershed year for campus militancy and the politics of the student movement.<sup>17</sup> From 1968 “revolution” became the catchword, the ultimate goal of radical activity. Developments at the national political scene were surely one of the causes for this change. In late January, at the Vietnamese Tet holiday, the army of North Vietnam launched a massive attack on U.S. forces in the South, taking for a

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<sup>16</sup>The most explosive case involved the University of Wisconsin and Dow Chemicals (manufacturers of infamous Agent Orange) recruiting in campus in October 15, 1967 (Sale (1974), pp. 369-374).

<sup>17</sup>This is recognized across the literature on the period, for instance Morgan (1991), Isserman and Kazin (2004), Gitlin (1987), but the argument is more forcefully made in Unger and Unger (1988).

few weeks many of the U.S.A. held cities. The aftermath of the “Tet Offensive” was to discredit President L. B. Johnson’s foreign policy, already strained by three years of growing anti-war sentiment. A poll in March showed that either Republican candidate Richard Nixon or Democrat hopeful Robert Kennedy would beat the President in the coming elections.<sup>18</sup>

Alongside Vietnam and the uncertainties of electoral politics, civil rights protests continued to flare. Martin Luther King, Jr.’s efforts had moved from the problems of southern race segregation to those of poverty. King was in 1968, advocating a “democratic form of socialism” while calls for Black Power and Black Nationalism were becoming more and more prevalent, from Oakland, Chicago and New York. In April King was assassinated and in response protests and riots erupted in 120 cities.<sup>19</sup>

In June Robert Kennedy was killed. With Kennedy’s death it was expected that the 1968 Democratic Party Convention in Chicago would choose pro-war Hubert Humphrey (Vice-President to Johnson) as the Democratic presidential candidate. At the occasion of the convention a protest was organised by the umbrella body of anti-war groups, the National Mobilisation Committee.<sup>20</sup> The protesters were met with disproportionate force by the Chicago authorities. Over 12,000 city policeman, 6,000 national guardsmen and 7,500 regular army troops were mobilized. The aftermath of the street battle was 200 injured and 500 arrests.<sup>21</sup>

The events caught the media’s attention:

The main feature of the August 29 NBC Morning News was a poetic montage of the previous day’s convention “highlights.” Back and forth NBC cut from the convention hall celebrations that followed Hubert Humphrey’s first-ballot victory to footage of protesters being beaten, clubbed, maced, and gassed by hordes of clearly enraged, out-of-control policemen. They showed this while the conventioners cheered and paraded and sang. They showed the “kids” being knocked to the ground,

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<sup>18</sup>Isserman and Kazin (2004), p. 232.

<sup>19</sup>Ibid., pp. 233, 235; Morgan (1991), p. 55.

<sup>20</sup>Prominent in the protests were the Yippies or Youth International Party, a group of anti-war protesters that politicised the counter-cultural figure of the hippie. Their protests were characterized by carnivalesque techniques of burlesque, clowning and ridicule. In Chicago, they nominated a live pig, called “Pigasus”, as their presidential candidate (Urgo (1998), pp. 918-19).

<sup>21</sup>Isserman and Kazin (2004), p. 241.

kicked in the face while delegates whooped and hollered with joy. NBC cut back and forth, back and forth, from the terror in the streets to the festivities that followed Hubert Humphrey's victory.<sup>22</sup>

The Democratic convention not only applauded Chicago Mayor Richard J. Daley's repression of the protests but elected Hubert Humphrey, the establishment figure, as presidential nominee. For the radicals that went to Chicago who were already disillusioned with electoral politics, the events signified the need for harder tactics to fight the government and its policies, "more and more people were concluding that simple parades and orderly rallies were no longer enough."<sup>23</sup>

Chicago was a major event,<sup>24</sup> one that would have crucial repercussions for the history of radical economics, as I will show later. In December of 1968 the Chicago Study Team of the National Commission on the Causes and Prevention of Violence published a report on the August police response. The Walker report, as it became known, attached the label of "police riot" to the Chicago events, attributing the responsibility for the violence to spontaneous acts of individual policemen. Late in 1968, recently elected President Richard Nixon's attorney general, John Mitchell, "charged Rennie Davis, Tom Hayden, David Dellinger, Bobby Seale, Abbie Hoffman, Jerry Rubin, and . . . National Mobilisation Committee marshals Lee Weiner and John Froines for conspiring to cause a riot, a charge that carried a penalty of up to five years in jail. Their trial would become the cause celebre of 1969."<sup>25</sup>

Around the world student unrest was on the rise. There were massive protests in West Germany, Italy, and famously in Paris in May, where students and mass strikes drove the country to a halt.<sup>26</sup> The opposition to the war in Vietnam may have made student radicals into a national political player, but it was the growing conflictual temper of American society<sup>27</sup> and the examples of student power elsewhere in the

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<sup>22</sup>Farber (1988), p. 251.

<sup>23</sup>Ibid., p. xv.

<sup>24</sup>The events were captured by Norman Mailer's pen in *Miami and the Siege of Chicago*.

<sup>25</sup>Farber (1988), p. 205. The trial began in September of 1969 and concluded in February of 1970 with the defendants being found guilty. But in November 1972 the Court of Appeal reversed all convictions on the grounds of misconduct by the Judge and the Jurors. For a first-person account of the trial see Hayden (1970).

<sup>26</sup>Katsifacas (1987), Fraser (1988).

<sup>27</sup>What Isserman and Kazin (2004) aptly called a "civil war."

world that convinced radicals that revolution was at hand. The events at Columbia University in April 1968 testified to this new mood. Over the issue of constructing a gym on land then occupied by Harlem's poor residents, students took over University buildings. They held their ground against the administration for seven days with support from a student strike, and were only ousted when police violently removed them at the Columbia University administration's request.<sup>28</sup>

Over the late 1960s the leadership of SDS broke into warring factions.<sup>29</sup> The leadership and older radicals at SDS redirected their efforts away from the campuses and into revolutionary politics. Yet, the bulk of the SDS activists followed none of the revolutionary factions, they set up independent local groups or joined national organizations such as those against the war in Vietnam.<sup>30</sup> Despite the SDS splits, student radicalism intensified in 1969-70. The main drive for continued student activism was the anti-war movement. "In 1967, 35 percent of students identified themselves as "doves"; by 1969 the proportion grew to 69 percent".<sup>31</sup> In July 1969, antiwar protesters proposed a "moratorium on business as usual," choosing October 15 1969 as a day for mass strike, national demonstrations and door-to-door canvassing, picketing and, leafleting.<sup>32</sup> The initiative was meant to be repeated every month while increasing the days of action by a day per month. Vietnam Moratorium committees were created in campuses and cities throughout the country to prepare for the October mass action. The resulting protest was a success, "it was a display without historical precedent, the largest expression of public dissent seen in this century"<sup>33</sup> drawing millions of Americans to the streets in all major cities, and to

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<sup>28</sup>On the Columbia events see the account by reporters to the *Columbia Spectator* collected in Avorn (1969).

<sup>29</sup>Beginning in June 1968 the SDS leadership called for the construction of a radical *revolutionary* movement and the organization was soon split over the tactics to be adopted. From the program "Towards a Revolutionary Youth Movement" came the RYM group (Elbaum (2002), p. 70) which in 1969 expelled Progressive Labour (PL) (Elbaum (1998), p. 472). PL however kept the SDS name. There were at the close of 1969 two SDS, one based in Chicago and one (PL's) based in Boston. Finally, the Chicago group split into the Weathermen and RYM II. The Weathermen would soon go underground and be responsible for 12 bombings in the following years, dissolving in 1976 (Chepesiuk (1995), p. 224). RYM II was short lived and dissolved in 1971.

<sup>30</sup>Halstead (1978), p. 467.

<sup>31</sup>Horowitz (1986), p. 22.

<sup>32</sup>Halstead (1978), p. 473.

<sup>33</sup>Life Magazine cited in Halstead (1978), p. 488.

absenteeism in high schools, junior high and elementary schools. The following month's moratorium days, in November, were accompanied by a national demonstration in Washington D.C., the biggest up to that date.<sup>34</sup>

In terms of the number of institutions affected and students involved, the high point of student activism was 1970. On April 30<sup>th</sup> President Nixon announced that American and South Vietnamese ground forces were moving against North Vietnamese sanctuaries in Cambodia. For the anti-war protesters it represented another escalation in the American imperialist war in Southeast Asia. Overnight there were demonstrations in several campuses and calls for nationwide demonstrations. Protests turned to upheaval when on May 4<sup>th</sup> at the Kent State University campus in Ohio, a student protest was targeted by National Guardsmen's bullets. Four students were killed and nine were injured. On May 14<sup>th</sup> police at Jackson State College in Mississippi fired at and killed two black students and wounded 12 more.<sup>35</sup> "The strike movement, initiated to protest the American military invasion of Cambodia, immediately increased: 4,350,000 students at 1,350 universities and colleges participated in demonstrations against the shootings and escalation".<sup>36</sup>

From 1968 to 1970 campuses were at fever pitch, with occupations, strikes, sit-ins, teach-ins, destruction of property, all increasing in intensity and with no sign of abating. The protests put student-faculty-administration relations under strain. Although the professoriate was never as critical to student protest as the general public, it was sternly opposed to the hard tactics of post-1968. In 1969, seventy-seven percent of the faculty felt that students disrupting the functioning of a college should be expelled or suspended, although only a quarter thought that demonstrations had no place on campuses.<sup>37</sup> Therefore what is significant in this period is not only the increasing calls for "revolution" with its new weaponry of protest but also the growing stress this placed on the universities.

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<sup>34</sup>The idea of continuing the moratorium was abandoned after the November success, but its organizational structure remained in place and would prove to be crucial for the continuation of the anti-war protests.

<sup>35</sup>Peterson and Bilorusky (1970), p. 1.

<sup>36</sup>Heineman (1992), p. 201.

<sup>37</sup>Ladd and Lipset (1975), pp. 34-35.

### 3.2.3 Demise of campus activism (1971-1974)

The protests increased in number and participants during 1969 and 1970, but the increase in numbers concealed a change in the nature of the major movement spurring student protest, the antiwar campaign. Not unlike the SDS splits, in the Summer of 1970 the National Mobilisation Committee, the antiwar campaign's united front, dissolved in a crisis of infighting.<sup>38</sup> The Vietnam Moratorium Committee became the dominant organization of the 1969-1970 antiwar drive, its "goal was to galvanise and express a broad, moderate, majority position against the war"<sup>39</sup> and the media began to endear the liberal wing of the antiwar movement. The new student participants in the antiwar campaign were "not radicals, not hippies, neither rebels nor revolutionaries, they were nonetheless prepared to take up a cause that both SDS and the McCarthy campaign had let slip away".<sup>40</sup> Notably, after May 1970, college militancy followed in proportion to the Vietnam draft,<sup>41</sup> which steadily decreased under Nixon's strategy of withdrawing soldiers while intensifying the bombing of North Vietnam.<sup>42</sup>

Dissent over the war became in 1971 a majority position in America, a poll in mid-April showed that sixty percent of Americans favoured withdrawal even if it meant the fall of the South Vietnamese government, and fifty-eight percent found America's War in Vietnam to be morally wrong.<sup>43</sup> The largest antiwar demonstration in American history, brought in April 24 1971 half a million people to the streets of Washington, D.C..<sup>44</sup> At the forefront of the April 1971 demonstration were the Vietnam Veterans Against the War (VVAW). Days before the demonstration,

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<sup>38</sup>The National Mobilisation Committee broke into two separate organizations, the single-issue National Peace Action Coalition and the multi-issue National Coalition Against War, Racism and Repression (Hopkins (1992), p. 72).

<sup>39</sup>Zaroulis and Sullivan (1984), p. 257.

<sup>40</sup>Ibid., p. 258.

<sup>41</sup>Elbaum (2002), p. 36. One should also note that the Nixon administration in the aftermath of the 1970s protests moved to create a new set of guidelines that schools had to abide to retain their tax exempt status, which put pressure on University administrations to clamp down on student protest, while also asking the FBI to intervene undercover in campuses (Halstead (1978), p. 580).

<sup>42</sup>By 1972 the US troops presence was down to 24,000 from a high of over 500,000 in 1969 (Ruane (2000), p. 127).

<sup>43</sup>Halstead (1978), p. 600.

<sup>44</sup>Hunt (1999), p. 116.

veterans camped in Washington's Mall and from there sent "squads" to lobby Congress, enact guerrilla theatre, and to talk to people on the streets.<sup>45</sup> Gauged by the media's reaction, the Veterans participation struck an emotional cord in the American public. A Pennsylvania newspaper compared in terms of effects on public opinion, Washington's massive demonstration with the veterans' protest: "It is doubted that this great outpouring of conscientious men and women will influence the thinking of the nation even nearly as much as the appearance of 900 warriors for peace in their battle attire."<sup>46</sup> Bringing further strain to the war effort were the desertions and resistance by soldiers at home and in Vietnam.<sup>47</sup> In 1971 the veteran and the soldier had replaced the radical student as the face of resistance to the war.

After a presidential campaign where the antiwar movement, liberal and moderate, coalesced around the anti-war Democratic Party bid of McGovern, Nixon signed in January 27 1973, the Paris Accords with the North Vietnamese. Though the Paris Accords inscribed a full withdrawal of American forces, the war continued with the US backing of the South Vietnamese regime. Hostilities effectively only ended when in 1975 North Vietnam decisively invaded the South. Without a clear military presence only the left-wing, outside the campuses and disconnected from the larger and moderate anti-war movement, protested against the continued aggression.

By mid-1970s the campuses were changing. The seventies student was a world apart from the sixties one.<sup>48</sup> Reflecting on the changes operating over the student population in the course of the decade, personnel and administrators at 586 colleges were asked in 1978 to describe how students had changed since 1969-70. A majority of the sample found students to be more "career-oriented," "better groomed" and more "concerned with self," while less "radical" and less "activist."<sup>49</sup> The perceived change was corroborated by a decreasing number of student protests and by the

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<sup>45</sup>Halstead (1978), p. 605.

<sup>46</sup>Cited in Hunt (1999), p. 116.

<sup>47</sup>From 1966 to 1971 desertions and AWOL rates increased nearly 400 percent in the army, three times higher than during the Korean war. "For every one hundred soldiers in 1971, seventeen went AWOL and seven deserted, the highest rates in army history. . . . In underground parlance, the armed forces were becoming the "armed farces"" (Anderson (1992), p. 111).

<sup>48</sup>Altbach and Cohen (1990).

<sup>49</sup>Levine (1980), pp. 7-8.



changing nature of students' campus organizations, i.e. with a decline of ideological politics on campus (be it left or right in orientation). The proportion of students participating in demonstrations had also fallen, from twenty-eight percent in 1969 to nineteen percent by 1976.<sup>50</sup> Even more significantly as evidence of the disappearance of radicalism from the campuses, the emphasis of student protest had changed from external issues such as the Vietnam War or civil rights, to internal campus issues, namely fees, financial aid or college facilities.<sup>51</sup> There were also changes in tactics: "a decline in use of tactics familiar from the sixties – building takeovers, strikes, demonstrations, and the destruction of property. What has taken its place are litigation and tactics ranging from lobbying and use of grievance procedures for educating the public and fellow students via seminars and research reports".<sup>52</sup> The inquests of the mid-to-late 1970s bore evidence of a quieting of campuses and a transformation of its protest culture away from radical subjects and tactics.

### 3.3 The challenge of Radical Political Economics

The seminal group of radical economists was composed by Michigan SDSers. Their early political development as radical students of economics fitting neatly into the history of radicalism I have reviewed. Where the two stories began to diverge was c. 1968 when the radicals took on a professional career and abandoned their student status. It was with the advent of this change that the Union for Radical Political Economics (URPE) was founded. From 1968 with the creation of the Union, the history of the economics radicals ran parallel to the history of student radicalism but the relationship between the two is no longer obvious and merits closer scrutiny.

URPE was originally a modest enterprise. But by 1969 radicals began to envisage a more ambitious future for their Union, as they fashioned a challenge to the economics profession. Radicals engaged in the design of a paradigm to compete against the mainstream and ultimately to displace it. It is revealing how this process

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<sup>50</sup>Ibid., p. 39.

<sup>51</sup>Exceptions were the campaigns against South Africa's apartheid in the research-oriented universities of the Northeast and Pacific Coast (Levine (1980), p. 42).

<sup>52</sup>Ibid, pp. 42-43.

interacted with the growing tensions inside campuses in the early 1970s when the student protest reached unprecedented intensity. The mainstream was made to respond by the nature of the radicals' attack, since it was made publicly and scandalously. The mainstream's response is another subject of this section. Ultimately I want to conclude by comparing the two strategies, the radical challenge, and the mainstream response, and see how they match with the social context in which they were raised.

### 3.3.1 Origins of the Union for Radical Political Economics (1965-1968)

Michigan economics students were active in SDS from its inception. They participated in national activities, notably in the 1964-65 ERAP projects, but also in local politics such as student government, and in the anti-war protests beginning in 1964.<sup>53</sup> It was around the anti-war protest that the history of radical economics began. President Johnson in 1966 revised the selective service system, Universities were expected to calculate rankings for male students and make these available to the military so that the lower half of ranked students be sent to fight in Vietnam. In 1966 many of the radical students of economics at Michigan were doing graduate research (they were the older generation of SDSers) and were employed as teaching assistants for undergraduate courses.<sup>54</sup> They sided with SDS demands that the University oppose the new selective system. They proposed that the administration refuse to compute the male ranking, so that the information required by the draft would be unavailable. The radical group pledged to withhold grades if "our judgment would serve to send people to be killed and kill in Vietnam."<sup>55</sup> The University authorities reacted with the threat that if grades were not produced by the teaching assistants, failings would be awarded to all students and rankings calculated on that basis. On the eve of the final exams the group of teaching assistants decided to hand-in the grades.<sup>56</sup>

<sup>53</sup>Interview with Michael Zweig, Barry Bluestone, 2003; *Harvard Law Review* (1969).

<sup>54</sup>The Michigan group included John Weeks, Howard Wachtel, Sander Kelman, Barry Bluestone, Michael Zweig and Eric Chester (not all teaching assistants).

<sup>55</sup>Interview with Michael Zweig, 2003.

<sup>56</sup>Brazer (1982), pp. 248-9.

It is important to note that the conflict did not represent disaffection with the economics faculty. In the 1966 episode, some faculty members came to the radicals' defence, in particular Daniel R. Fusfeld.<sup>57</sup> In Michigan there was no hostility to the radicals' actions and their questioning of the *status quo*. Some faculty members like Dan Suits and Fusfeld tolerated it and at times encouraged it, since for them it represented a revival of an inquiring attitude towards society crushed by McCartyism.<sup>58</sup> This pattern was not unique to Michigan, in fact University faculty were generally more critical of the war than the general population. A majority of faculty advocated in 1966, a disengagement from Vietnam and fifteen percent took the radical position of immediate withdrawal.<sup>59</sup>

The Michigan radicals that had been joined together in 1966 to fight the selective service system began to act cooperatively within the economics department. In 1967, they petitioned for changes in the curriculum and for greater student involvement in departmental affairs.<sup>60</sup> They were mobilising for greater student participation in the administration of the faculty and for the teaching of subjects that matched their political concerns, such as the economics of war and imperialism.

When in 1967 the "Free University Movement" reached Michigan the radicals organized a seminar on Modern Political Economy. In these meetings they drew a plan of future activity: "exploring new issues, ideas and approaches to economic problems and related social issues"; of the new issues "Imperialism and Foreign Economic Policy of the United States", "National Planning in the United States", "Contemporary political economy" and "Centralization versus Decentralization in Economic and Political Affairs" were the main headings that prompted questions for

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<sup>57</sup>Daniel Ronald Fusfeld was born in 1922 in Washington D.C., USA. He graduated from George Washington University in economics and has a PhD from Columbia University (1950). He taught at Hofstra College (1947-56), Michigan State University (1956-60) and from 1960 until his retirement in 1984 at the University of Michigan. He authored two successful books, one in the history of economics, *The Age of the Economist* (1966) running into seven editions; and a textbook, *Economics* (1972) with five editions. His major research work was *The Political Economy of the Urban Ghetto* (1984).

<sup>58</sup>Some older Michigan faculty would in fact join the younger radicals in their interest for radical scholarship and participating in radical activities, Fusfeld and Locke Anderson created a course on Marxian and Neo-Marxian Economics in 1970 - Economics 558 (Brazer (1982), p. 251).

<sup>59</sup>Armor *et al* (1967), p. 172.

<sup>60</sup>Interview with Michael Zweig, 2003. Brazer (1982), p. 251.

further research.<sup>61</sup> Although Frederic S. Lee's history of the emergence of radical economics seems to suggest that the 1967 meetings were occasion to debate a new approach to economics,<sup>62</sup> no such positive program was put forward. The young radicals were merely listing the subjects they deemed the academic profession had failed to address.

By 1967 the fostering of bonds between radicals along professional lines was well established within SDS. In the late 1960s, some of SDS's seminal and more intellectually oriented members had become marginalised from the student movement, from its progressively more violent tactics, and appeal to the irrational.<sup>63</sup> The so-called "old-guard" of SDS looked for means to remain politically active in a new environment, no longer as students but as professionals and academics. The New University Conference was created in 1967 with that goal in mind and amongst its members were some of the future founders of URPE.<sup>64</sup> The New University Conference program was to:

1. Organize local chapters across the nation to help overcome the isolation and impotence now afflicting campus-based radicals [and] to: define their political roles on and off campus; engage in mutual support and self-criticism concerning teaching and intellectual activity; create centers for radical initiative on the campus.
2. Encourage the formation of radical caucuses within professional disciplines and associations.
3. Organize so that we may eventually be prepared to defend campus radicals against politically motivated harassment and firings.
4. Aid in establishing a new magazine of analysis and research for the movement.
5. Form alliances with student activists seeking to expose and dislocate university collaboration in war research and social manipulation, and join with black and white radicals who are demanding that the

<sup>61</sup>Wachtel and Vanderslice (1973), pp. 14-15.

<sup>62</sup>Lee (2004a), p. 187.

<sup>63</sup>On this connection no one more vocal than Harvard's Todd Gitlin, see Gitlin (1987)(Personal Communication John McDermott, 12 September 2005).

<sup>64</sup>Lee suggests that the link between radical economics and SDS was the Michigan run Radical Education Project beginning in 1965 (Lee (2004a), pp. 184, 187). However, over my interview work I have been able to corroborate this claim (interview with Barry Bluestone, Michael Zweig, 2003).

universities become responsible to the needs of the black communities which surround them and from which they now seek protection, not insight.<sup>65</sup>

As in the history of student radicalism, 1968 was a crucial year for the history of radical economics. Economics students were amongst the youth at Chicago in August 1968, and they were convinced by the year's events that new forms of action were warranted.<sup>66</sup> It was with the backdrop of a year of increasing confrontation that a group of a dozen students of economics met in Ann Arbor in September 1968 on returning from the Chicago Convention. In near conclusion of their degrees or about to take their first academic jobs, they met with the goal of creating some institutional arrangement that would allow them to keep in contact and continue work on the economics of their radical subjects.

The group meeting in Ann Arbor in September 1968 decided that an organization of radical economists should be formed. They were reacting to the much feared prospect of their dispersion: "If we remain scattered throughout the country it will be difficult to avoid the "creeping socialization" of the university or government office. Money for research and jobs is plentiful for those activities that support the status quo. Our task as an organization, is to provide a strong counter-weight to the pressures of society."<sup>67</sup> There was, in the decision to create a Union, a departure from the organizational plans voiced at the February 1967 Free University meetings. Then, opinions had been divided between pursuing "the traditional academic roles of teaching and research" or "dedicate ourselves to political organizing?"<sup>68</sup> and their proposed tactic had been to "find an amenable economics department and locate ourselves in it".<sup>69</sup> By late 1968 these economics radicals were firmly set on an academic career and saw as unlikely the prospect of finding a single department to base themselves.

The label given to their organization, Union for Radical Political Economics, was

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<sup>65</sup>Sale (1974), pp. 412-3.

<sup>66</sup>Interview with Laurie Nisonoff, 2003.

<sup>67</sup>URPE Secretariat (1969), p. 174.

<sup>68</sup>Wachtel and Vanderslice (1973), p. 16.

<sup>69</sup>Ibid., p. 15.

the subject of some reflection. “Union” was preferred over “Association” since it reflected the basic objective of the group, to “unite” like-minded radicals. It was felt that the Union should be “political.” The apolitical character of the economics profession was at the core of radicals’ critique in 1968, they argued that the profession was politically disengaged and silent over the urgent social concerns of the time. “Political Economics” was clearly preferred over “political economy.” Political economy recalled David Ricardo and Karl Marx and an older sort of economics, also at this time, no radical felt strongly identified with Marxism and that label might be open to this mistaken interpretation. It was further felt that it should be a union “for radical economics” and not a union “of radical economists,” since from its inception it was desired that URPE welcome activist oriented individuals with an interest in economics.<sup>70</sup>

The label, Radical Political Economics, as the above comments make clear, was carefully carved out to be distinct from political economy of the Marxist or neoclassical type. But there wasn’t at the time any assurance that such a proposal would attract a following. URPE’s future was uncertain. Was it to be a mere liaison group? Or was it to function as an independent professional organization? URPE’s fortune was clarified later in 1968 and once again in close relation to the August events in Chicago.

The Chicago “police riot” divided the nation, on one side those supportive of the police response, on the other denunciation.<sup>71</sup> The same divisions were evident in the economics profession. The American Economic Association (AEA) had planned to hold its annual meeting in Chicago in December 1968, and President-elect Kenneth Boulding<sup>72</sup> was faced with demands by many AEA members to move the meetings away from the city as a symbolical gesture of condemnation for the police brutality.

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<sup>70</sup>Interview with Barry Bluestone; Arthur MacEwan, 2003.

<sup>71</sup>Even within the Democratic Party, Farber (1988), p. 205.

<sup>72</sup>Boulding was a renowned pacifist (religiously a Quaker) and was director at the University of Michigan of the Center for Conflict Resolution. “My own first reaction after watching the police action in Chicago on television,’ he [Boulding] confesses, ‘was to mail my presidential address rather than go to Chicago.’ On further reflection, however, Boulding decided that a meeting in Chicago would provide an ideal forum for the economics profession to discuss the problems that infect U.S. society and seem to be impelling it increasingly toward violent conflicts.” (*Business Week* (1969), p. 80).

Wrestling with the financial consequences of a change of venue, Boulding decided to keep the meetings in Chicago but added “that the decision to hold the meeting in Chicago in no way constitutes an endorsement, explicit or implicit, of last summer’s actions by the city of Chicago, its officials, or the demonstrators.”<sup>73</sup>

The AEA meetings besides hosting a number of scholarly sessions, played (and play to this day) a crucial role as a market for jobs where graduate students and hiring universities met. In December 1968, at the same time as the official Chicago meeting of the AEA, an alternative gathering took place in Philadelphia. The meeting called the Grey Market, was organized by Professor Lawrence R. Klein and Donald W. Katzner from the University of Pennsylvania<sup>74</sup>: “we are offering an alternative that simultaneously permits economists to boycott Chicago without disrupting the traditional market for new placements.”<sup>75</sup> To assure a job market outside Chicago was the prime concern of the Grey Market.

The Grey Market was held at the Sheraton Hotel in Philadelphia from the 19<sup>th</sup> to the 21<sup>st</sup> of December. Besides the job market there were also two sessions on Friday 20<sup>th</sup> with papers by graduate students. There were about 400 persons in attendance and over 100 institutions registered with about 250 jobs on offer and 357 jobs applicants listed.<sup>76</sup> The meeting was deemed a success by the organizers: “Last December’s Grey Market turned out to be so successful that we are considering a suggestion to the American Economic Association that the annual slave market for economists be separated from the traditional convention.”<sup>77</sup>

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<sup>73</sup>However the voices of discontentment did lead the Executive Committee of the AEA to poll its members concerning the 1972 meeting of the Association, also planned for Chicago. Of the pool 3498 voted for a change in location, 2911 voted to hold the meeting in Chicago (*American Economic Review* (1969a)).

It should also be noted that the AEA held its annual convention under the auspices of the Allied Social Science Associations, and while it refused to change venue, other smaller organizations of economists did not follow its decision. Notably, the Econometric society held its annual meeting in Evanston, Illinois, just outside Chicago.

<sup>74</sup>My account of the Grey Market is based on materials from Professor Katzner’s personal archives which he kindly made available to me.

<sup>75</sup>“An alternative to Chicago”, November 12, 1968, by Donald W. Katzner and Lawrence R. Klein. Donald W. Katzner personal files.

<sup>76</sup>The universities that joined included University of Pennsylvania, University of Michigan, Harvard University, M.I.T. and Stanford University.

<sup>77</sup>Letter from Lawrence R. Klein and Donald W. Katzner to “Participants in the Grey Market”, January 15, 1969. Donald W. Katzner personal files.

URPE joined the Grey Market and held its own conference parallel to the meeting.<sup>78</sup> This was made possible through contacts established in joint radical activities (at the already noted seminars of the Free University) and by similar professional interests. One informant recalls that the contact to secure URPE's place at the Grey Market was made through Ben Harrison and Norm Glickman, both graduate students working at the University of Pennsylvania on labour economics.<sup>79</sup>

The significance of the Grey Market for URPE was that it provided a unique opportunity for recruiting members.<sup>80</sup> This was a protest meeting against the officials of the economics profession, seen as pandering to the oppressive political establishment. The meeting's participants were likely to be receptive to a Union for radicals in economics and in fact many keenly joined URPE. Within three months of its creation URPE had over 300 paying members and a mailing list with over 800 names.<sup>81</sup> Early in 1969 and from Ann Arbor, Michigan, URPE began publishing the *Newsletter of the Union for Radical Political Economics* and the *Review of Radical Political Economics*, the former as a means to communicate to the membership the Union's activities and internal debates, the latter as an outlet for Radical Political Economics research.

### 3.3.2 Defining Radical Political Economics (1969-1971)

The radicals' first statements on their motives for creating a Union for Radical Political Economics and on the future of their efforts were contained in the 1968 Grey Market's conference papers.<sup>82</sup> In late 1968 the common root of the radicals' critique of the profession was not the inadequacies of economic theory, but the irrelevancy of the subjects addressed by economists. They made note of the profession's silences:

<sup>78</sup>A small contingent was sent to the official AEA Chicago meetings to advertise the recently created Union (Michelson (1969)). Interview with Michael Zweig, 2003.

<sup>79</sup>Interview with Barry Bluestone, 2003.

<sup>80</sup>According to Lee (2004a), p. 189, a conference was held at MIT in November 1968, prior to the Philadelphia meetings, which had already been a success in recruiting members for URPE.

<sup>81</sup>Bluestone (1969), p. 5.

<sup>82</sup>The publication was supervised by Barry Bluestone and Howard Wachtel, co-directors of the URPE Secretariat. Other members of the Secretariat participated in the editing and compilation: Barry Herman, Paul Gingrich, Craig Morgan, Lloyd Atkinson, Mary Huff, Larry Sawers and Virginia Morgan. It contained papers by Daniel R. Fusfeld, Victor Perlo, Michael Zweig, Stephan Michelson, Charles K. Wilber, James H. Weaver, Leroy P. Jones, Tom Standish and Rick Wolff.



“How frequent are articles which deal with the economics of racism, poverty in the American economy, international imperialism, or the real economics of defense?”<sup>83</sup> For radicals this signified that “modern economics deceives itself when it insists it is value-free.”<sup>84</sup> It is striking that radicals were willing to accept the tools of conventional economics, they clarified that: “This is not to say that we deny the value of some of the tools and concepts of modern economics, but is said to question the importance of the problems to which these props are applied and the uncritical way in which the neat tools of economics are inappropriately used.”<sup>85</sup>

In 1968 at the Grey Market conference there were some dissonant voices, extending the critique of the profession to an attack on the whole of economic theory. Michael Zweig’s “New Left Economics”<sup>86</sup> began by noting that the critique of economics had as its starting point a critique of contemporary American society as racist and imperialist.<sup>87</sup> It followed that standard economics was to be judged either useless for the construction of a decent society or, what was worse, supportive of the *status quo*.

Zweig established a correspondence between the apparatus of economic theorising and its political content. He argued that the fault was in marginalism:

Marginalist analysis can be pernicious as well as irrelevant. The spirit of marginalism is one of small adjustments on the periphery of some large aggregate whose fundamental and overall character is not an issue. (This spirit is particularly well suited to the bureaucratic mind.) But the larger questions are almost never asked. The spirit of marginalism is ill suited to radical questioning of the precepts of economic and social arrangements, and it is equally ill suited to deep, revolutionary change.<sup>88</sup>

<sup>83</sup>URPE Secretariat (1969), p. 173.

<sup>84</sup>Idem.

<sup>85</sup>Idem.

<sup>86</sup>Michael Zweig was born in Detroit in 1942. He studied economics at the University of Michigan (BA and PhD in 1967). He joined the faculty of the State University of New York at Stony Brook in 1967 and has taught there since. In 1999 he founded the Center for the Study of Working Class Life, of which he is Director. His major works are *Religion and Economic Justice* (1991), *The Working Class Majority: America’s Best Kept Secret* (2000).

<sup>87</sup>Zweig (1971b), p. 67. The paper had first appeared in the published Philadelphia 1968 conference papers, and was probably the most cited article on radicals’ early critique of the mainstream. It was one of the few contemporary texts included in Mermelstein (1970) which surveyed the history of radical critiques of economics. I cite from its reprint in July 1971 in the *Review of Radical Political Economics*.

<sup>88</sup>Ibid, p. 68.

Zweig challenged economics' alleged neutrality, he argued that "standard economics violates neutrality by militating against asking and answering certain radical questions. Economics is not, and cannot reasonably be expected to be, neutral."<sup>89</sup> Although the subject of what questions economics posed was central to Zweig's critique, he went further than most by arguing that questions were not being posed because of the theory that underlined economists' research. For him, it was at the level of its theory that the economics profession's faults lay.

The significance of Zweig's "New Left Critique of Economics" can be gauged by having occasioned the first article of the *Review of Radical Political Economics*, authored by John Weeks.<sup>90</sup> Week's article was a response to Zweig stating what was probably the dominant view among radicals in 1968-69. He argued that it was economists themselves who were the source of conservatism and not the tools or theories of economic science. He argued that,

the failure of marginal analysis is that it is used by those who through "explaining" how the economy works, seek to justify things as they are. ... It is the politics of economists, not the techniques of economists that makes this so. It is not because marginal analysis is reactionary that economics supports the status quo, it is because economists are reactionary and choose and use their questions, tools, and governments accordingly.<sup>91</sup>

Weeks found that the profession gained from the existing social and economic order, for him "the practitioners of standard economics are part of an economic and social elite, and therefore can no more be expected to develop and encourage radical analysis than businessmen as a group can be expected to advocate workers' control of production"; "they draw their wealth and influence from these social institutions";<sup>92</sup> and thus have a vested interest in maintaining them.

Both Weeks and Zweig were trying to reason what they saw as the economics profession unwillingness to address the pressing questions of the time, particularly

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<sup>89</sup>Ibid, p. 73.

<sup>90</sup>Weeks (1971)

<sup>91</sup>Weeks (1971), p. 75, emphasis in original.

<sup>92</sup>Ibid., pp. 75-76.

those in connection with war, imperialism and poverty. They offered contrasting answers to the problem, one focusing on the content of theory, the other on economists' commitment to the *status quo* in their status as intellectual elite. Radicals' critique of economics was thus, in 1969, not consensually established.

The focus of the radicals' activities in URPE's first years of activity, from 1969 to 1971, was the creation of radical courses and the organization of conferences where they could discuss their research. The outcome of these activities was a change in the relationship between radicals and the mainstream of the profession. Alongside which, the original call for a change in the questions of economics was replaced by talk of an emerging radical approach to economics.

Contemporaneous with the high point of student protest, as I have noted previously, 1969 and 1970, and bolstered by the creation of URPE, the first radical courses in economics were set up. The most noteworthy of these courses was created at Harvard University.<sup>93</sup> It followed earlier experiments with radical courses at Harvard. In the fall of 1968, students had designed the course Social Relations 148 "Social Change in America," and in the spring of 1969, Social Relations 149 "Radical Perspectives on Social Change." The faculty dismissed the courses, "Soc Rel 148-9 is a disgrace to scholarship", one professor told the *New York Times*.<sup>94</sup> The social relations course broke new ground for similar initiatives to follow from other departments. Starting in the spring of 1969 "Social Sciences 125 – The Capitalist System: Conflict and Power" was offered on Harvard's General Education program. The course was run by twelve graduate students and young economics faculty (Arthur MacEwan officially as the course proprietor, Samuel Bowles, Herbert Gintis, and Thomas Weisskopf) sharing both the lecturing and the section meetings. The junior faculty radicals had demanded that the course receive economics credits; though there was some support from the tenured faculty, the majority opposed their proposal. The course was a useful focal point for radical work, for which they had no training, and

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<sup>93</sup>There were reports of other courses at Michigan and New School in NY (*Newsletter* (1970) and URPE Collective – New School (1971)).

<sup>94</sup>Nossiter (1969).

for which there was no blueprint.<sup>95</sup> The course served to draw more students into the radical project. According to an informant's recollection there were about 150 students taking the course, with around twelve MIT undergraduate and graduate students, although not all from economics.<sup>96</sup>

Significantly, beyond their curricular content, radical courses were also a stage for new pedagogies. The desire for social transformation and liberation was brought to bear on the relationships between teacher and students,<sup>97</sup> radicals noted that: "It is necessary for our own liberation as well as for the student's welfare to break down authoritarian relations in the classroom."<sup>98</sup> Grading was seen as serving the interests of the *status quo*: "The principal external consumers of the information contained in the grades are employers and graduate schools, who need to identify the students they most prefer and the ones they least prefer. Grades provide employers and graduate schools with a costless means of ranking students for their own purposes"<sup>99</sup>.

Alongside the emerging radical courses, the other major initiative by radicals during this period was the organization of regional conferences on radical subjects. The first was a "Middle Atlantic Conference" that took place in Washington, prompted by President Nixon's inauguration on January 18<sup>th</sup> 1969.<sup>100</sup> The morning sessions were devoted to critiquing Nixon's economic program and discussing alternative economic development strategies. The afternoon sessions dealt with reformulations of economic theory. Interestingly, the final session of the meeting was concerned with the role of political economists. Opinions were divided between those that thought that the focus should be on research (developing a new approach to economics in research oriented institutions) and those that argued that efforts should be concentrated on teaching (radicalising students in teaching oriented

<sup>95</sup>A book of original and reprinted texts from Soc 125 was the immediate result of this effort, Edwards, Reich and Weisskopf (1972).

<sup>96</sup>Interview with Laurie Nisonoff, 2003.

<sup>97</sup>The Radicals' focus on education led them to a study of its economics, for instance Bowles (1971) and notably Bowles and Gintis (1976).

<sup>98</sup>Edwards and MacEwan (1970), p. 361.

<sup>99</sup>Ibid., p. 362. As an alternative to grades the Harvard Soc 125 staff would write evaluations of students' work and meet with them to discuss their progress.

<sup>100</sup>Lee notes over six conferences that year, held at American University, MIT, University of California at Berkeley, University of Michigan and Oberlin College ((2004a), p. 190).

institutions).<sup>101</sup>

In 1969 there were two URPE chapters in the Boston area, one at Harvard and the other encompassing MIT and the other campuses. The two chapters ran a conference at the MIT Student Centre on the 1<sup>st</sup> and 2<sup>nd</sup> of November, entitled the “New England Regional Conference”. The major theme of the conference was “the construction of an alternative paradigm.”<sup>102</sup> The main speaker was Paul M. Sweezy,<sup>103</sup> and he suggested that Marxism could provide the necessary theoretical alternative to orthodox economics. He contrasted Marxist theories of imperialism with the mainstream’s inability to explain the growing gap between rich and poor. Sweezy also emphasized the need to think beyond a critique of orthodox economics: “We must all pass through a stage of ridding ourselves of the brainwashing we have received in graduate schools, but new theoretical research must free itself from the framework of traditional economics to construct the convincing alternative which is essential to the success of any radical movement.”<sup>104</sup> The other session of the conference that was reported in some detail in URPE’s *Newsletter* was devoted to imperialism, chaired by Arthur MacEwan with Harry Magdoff, Michael Tanzer, Stephen Hymer, Stephen Resnick and David Schecter.<sup>105</sup>

In the summer of 1969, from August 24<sup>th</sup> to 31<sup>st</sup> in Charlevoix, Michigan, began

<sup>101</sup>Weaver (1969a), p. 4. In the following years radicals would take up jobs in liberal arts colleges or non-research oriented institutions where they would have greater freedom to design courses and work closely with students even if at the cost of doing research (interview with Arthur MacEwan, 2003).

<sup>102</sup>*Newsletter* (1969a), p. 1.

<sup>103</sup>Paul Marlor Sweezy (1910-2004) studied economics at Harvard (BA in 1932, PhD in 1937). He taught at Harvard from 1938 to 1942. In this period he wrote two famous articles, “Expectations and the Scope of Economics” in the *Review of Economic Studies* (June, 1938) and “Demand Under Conditions of Oligopoly” in the *Journal of Political Economy* (August, 1939). On his return from military service in the Second World War, Sweezy abandoned his academic career. He founded with Leo Huberman in 1949 the *Monthly Review – An Independent Socialist Magazine*. He was famously imprisoned in the late 1950s for refusing to collaborate with the McCarthy “witch-hunts.” His major works were *The Theory of Capitalist Development: Principles of Marxian Political Economy* (1942); *Monopoly Capital: An Essay on the American Economic and Social Order* (with Paul Baran, 1966) and *On the Transition to Socialism* (with Charles Bettelheim, 1971).

<sup>104</sup>*Newsletter* (1969a), p. 2. The paper was of some significance, it was published in the *Monthly Review* in January 1970 and in the 1970 spring issue of the *RRPE*, finally it was republished in the very important July 1971 *RRPE* issue, Sweezy (1971).

<sup>105</sup>The presence in the “New England” conference of the two editors of the *Monthly Review* (*MR*), Paul Sweezy and Harry Magdoff, did not indicate an endorsement of the *MR* brand of Marxism. There was deep admiration for *MR*’s combativeness in particular during the academic repression of the late 1950s. However, there were no close intellectual ties between *MR* and the radicals. Although some radicals published in the *MR*, there was never extensive collaboration or explicit endorsement of the *MR*-line (interview with Arthur MacEwan, Richard Wolf, 2003).

an URPE tradition, its Summer Conference.<sup>106</sup> The decision to meet in the summer had been made in December 1968 at the Grey Market. The Summer Conference was devised as the great annual event of the organization, the occasion for all members to meet and discuss economic theory and political practice. On the intellectual level the conference was not a success with new members feeling that they had not been adequately introduced to radical economics,<sup>107</sup> but on the personal and social levels it fulfilled the expectations. URPEers brought their families to the conference and some of the time was spent in parties and other recreational activities, such as playing music. Radicals still recall the early Summer Conferences with great fondness.<sup>108</sup> The Summer Conference shows that URPEers were bound not only by similar intellectual interests but also by ties of friendship and cultural affinity, bonds of identity that predated any clearly drawn intellectual program.<sup>109</sup>

Out of this flurry of activity it is apparent that talk of an alternative approach to economics began to take hold of the radical group. Radicals were moving from a critique of economics's defective choice of subjects, ignoring the real social concerns of the age, to a critique of the content of economic theory. At the 1969 AEA meetings, the Harvard radicals introduced their Soc 125 course as the beginning of a new approach to economics.<sup>110</sup> The Harvard radicals dismissed the traditional curriculum: "Our effort to develop a new curriculum is motivated by the conviction that the orthodox approach to economics cannot deal with the important problems of modern society. . . . The marginalist approach is useful only if, accepting the basic institutions of capitalism, one is primarily concerned with its administration."<sup>111</sup> At the close of 1969 radicals were identifying structural faults in economics and stating the need for an alternative.

<sup>106</sup>The URPE summer conferences are held to this day, always at a location that permits a mix of recreation, theoretical and political debate. Their role in the history of radical economics was significant primarily for the later years of the group, which I will discuss in the next chapter.

<sup>107</sup>Hinckley (1969).

<sup>108</sup>Interview with Jim Crotty; Laurie Nisonoff, 2003.

<sup>109</sup>Music was a crucial element in the sixties culture of protest, in particular in the folk genre, Denisoff (1970).

<sup>110</sup>Edwards and MacEwan (1970). It was followed by a discussion with Paul M. Sweezy, Phillip Saunders, and John R. Coleman. In a similar introductory mode Zweig (1972) presented a principles course in economics integrating radical and conventional analysis.

<sup>111</sup>Edwards and MacEwan (1970), p. 352.

In a July 1971 issue of the *RRPE*, entitled “On Radical Paradigms in Economics,” the design of an alternative approach gave way to the design of an alternative paradigm. The introductory paper of the volume by Gerald E. Peabody surveyed Thomas S. Kuhn’s work on the history and philosophy of science. In Kuhn’s conceptual framework the history of scientific disciplines was a history of paradigms, model ways of solving problems. Peabody noted that: “Each paradigm in succession allows an account of a wider range of natural phenomena or an account of previously recognized phenomena with greater precision.”<sup>112</sup> The goal of the special issue was to “call for such a revolution in the paradigm that provides the world view for current economic thought”.<sup>113</sup> While an “alternative approach” need not displace the orthodox approach, when radicals named their critique an “alternative paradigm”, they began to envisage the displacing of orthodox economic theory.

The special issue of the *RRPE* explored the definition of an alternative (radical) paradigm for economics. The major elements of the debate were economics’ input to a progressive change in American society and the interplay between power (in society, in the profession) and ideas. It called for radicals to show the weaknesses of standard economics and provide a solution to transcend its shortcomings. This was very far from URPE’s seminal 1968 prospectus that aimed its critique at the profession’s silence on the problems of American society but still accepted the tools of economics.

In the *RRPE* special issue the radical paradigm was named a “paradigm of conflict”:

A paradigm of conflict asserts that for each conflict there is a grouping of the members of society into a small number of classes. The class position of an individual is determined by some objectively verifiable relation to the issue of conflict, although the individual need not himself be aware, or conscious, of his class status. A society is characterized by the conflicts it contains, and consequently by the class structures associated with these conflicts<sup>114</sup>

While acknowledging that Marxism was the best known elaboration of this paradigm,

<sup>112</sup>Peabody (1971), p. 1.

<sup>113</sup>Ibid, p. 1.

<sup>114</sup>Zweig (1971a), pp. 48-49.

Zweig argued it was not the sole one:

Radical political economy based on a paradigm of conflict need not be Marxist in its analysis of the source of conflict. For Marxists the conflict is ultimately rooted in a labor theory of value. Such a theory of value gives rise to concepts of exploitation, surplus value, and other substantial and irreconcilable (under capitalism) characteristics of antagonistic class relations fundamental to Marx. Those who reject the labor theory of value see other roots of conflict endemic to capitalism.<sup>115</sup>

At the forefront of the radicals' criticism, marginalism was replaced by the concepts of "harmony" and "equilibrium" to describe bourgeois economics' structuring principles.<sup>116</sup> Zweig portrayed economics as informing people's views of the world, as an ideology, that precluded people's revolutionary action. He argued that: "To bourgeois economists, such non-marginal, systemic attacks are irrational and uncomprehensible in economic terms. They have no intellectual or formal analytic tools to deal with such behavior."<sup>117</sup>

The interplay of "knowledge" and "life" was crucial for the radicals' new critique. For radicals "knowing" had become associated to the aims of prediction and control, that in economics had become known as "positive economics" (Milton Friedman is referred to as the "archetypical social scientist"<sup>118</sup>):

The kind of society that comes to be viewed as good, perhaps even as natural or real, is an orderly and predictable and controllable society. Getting to work on time every day, functioning smoothly as part of an administrative structure, showing proper respect for expertise – all become types of behavior that are necessary to be a successful part of society. The way to live is to be machine-like. Ergo, the best society would be like a machine.<sup>119</sup>

<sup>115</sup>Ibid., p. 50.

<sup>116</sup>The term "bourgeois" took the place of the more commonly used terms of "conventional," "standard," "orthodox," "contemporary," denoted Zweig's belief that "'conventional' economics is rooted in bourgeois ideology, and deserves an adjective which reflects that fact. One point of this paper is the demonstration of these roots and a challenge to the notion that economics is value-free" (Zweig (1971a), p. 43).

<sup>117</sup>Zweig (1971a), p. 45.

<sup>118</sup>Behr et al (1971), p. 18.

<sup>119</sup>Ibid., p. 22.



Detachment was not to be desirable, to be objective was seen as doing violence to the researcher, since

In choosing to be a scientist and to participate in this interaction as observer, the would-be scientist must decide whether she can accept the bounds to be placed on her experience, spending most of her working life in the role of objective, rational woman, alienated by the scientific mode of inquiry from being able to experience the observed, the Out-There, in more meaningful ways.<sup>120</sup>

Radicals called for empathy with society and its ills. Society should be experienced subjectively if it was to be experienced at all.

To join “life” and to create a new kind of knowledge the radical paradigm also inscribed a radical ethics prescribing “living a radical political economics.”<sup>121</sup> In this ethics for radical economists, the connection with students was highlighted: “Students in colleges and universities today are the mind-workers, the human capital, the technocrats, the “new working class” of tomorrow. Our capacity to create a radical consciousness in large numbers of students is essential to the future success of revolutionary change in this country.”<sup>122</sup> Radical economists should assist the intellectual and political development of students. Equally important was a plan to reshape the University, to free it from oppression and commit it to liberation:

all authoritarian institutions must be eliminated from the classroom, starting with grades. As long as the teacher retains the power to give grades, there can be no equality in relating to students. All decision-making in the classroom must be put on a democratic basis, and we must look into ourselves and work to eliminate our own authoritarian and elitist attitudes.<sup>123</sup>

They proposed to limit lectures and written materials and to privilege direct contact with students. These proposals reflected work already done by radicals over the previous two years, namely with the radical courses coupled with attempts at reforming student assessments.

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<sup>120</sup>Ibid., p. 25.

<sup>121</sup>Ibid., p. 39.

<sup>122</sup>Ibid., p. 35.

<sup>123</sup>Ibid., p. 36.

The radical ethics warned against the confinement of radicals to the campuses. Radicals should seek to link themselves to political groups, to engage in organizing in their communities, “to find out what the basic problems are with which they are dealing, and then organize and participate in research projects designed to provide information and analysis needed in the community organizing campaign”.<sup>124</sup> In their radical ethics, the political economists should not seek to control the movement by appeal to their expertise. Instead the ultimate goal was to learn how to serve the movement. They argued that “Unless we are involved in a political movement engaged in the struggle for social change, and therefore know the needs of that movement, see strategies succeed and fail, see analyses proved correct and incorrect, we are almost certain to be irrelevant to struggles going on.”<sup>125</sup>

Radicals rejected objectivity. Their notion of objective was the objectification of subjects in accord with the interests of the *status quo*, necessary for maintaining a “machine-like” social system. To recover subjectivity, “life” alongside “knowledge”, the radical had to break with the established norms of academe. The power structure of the classroom had to be abolished, and the radical had to move closer, as a participant-observer, to political movements. Radical economics was thus defined as knowledge with the purpose “of being destructive, non conservative, of the existing social order”<sup>126</sup> of assisting revolution.<sup>127</sup>

Much had changed from the 1968 conference papers to the 1971 special issue of the *RRPE*. Radical Political Economics was presented in 1971 as a paradigm, and consequently as an alternative to the prevailing economic theory. Parallel to this development was the belief that the behaviour of the profession was structurally bound with its paradigm, they were one and the same, scientific framework and politics overlapping. Even those that earlier held a dissociation between the politics of economics (the theory) and the politics of economists (the profession), came to the view that conventional economics should be discarded for a radical alternative. Weeks

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<sup>124</sup>Ibid., p. 36.

<sup>125</sup>Ibid., p. 37.

<sup>126</sup>Ibid., p. 30.

<sup>127</sup>Zweig (1971c), p. 84.

in 1972 recanted his earlier views:

He [Zweig] is absolutely correct in saying that my analysis took appallingly little account of the role of capitalist institutions in conditioning our thoughts, as well as controlling our lives. The adequate understanding of these institutions is something which my formal training insured I would not learn, and while I feel I have liberated my thinking a bit, I have no pretensions to adequate understanding. It is clear that Mike has gone much further than I, and I have much to learn from him and others.<sup>128</sup>

### 3.3.3 Responses to the radical challenge

Radicals were fashioning themselves as challengers of the *status quo*. This is evident in the growing talk of a “new approach to economics” and later of an “alternative paradigm.” A divide was deepening between the young radical academics and the profession’s elders. It was fed by changes in the faculty’s attitudes toward student protest circa 1968-69, and by radicals enthusiasm in face of growing unrest. The campus unrest transformed faculty sympathy to disgust,<sup>129</sup> while convincing radicals of the possibility of change to academia and larger society. Feeling the support of the campuses in uproar, the URPE radicals began to think about confronting their profession, notably at the AEA annual meetings.

Radicals argued that the AEA should be challenged for its silence over the war,<sup>130</sup> the profession’s racism and sexism, and the elitist way in which the job market was organized. In the fall of 1969, following the November New England Conference, URPE’s *Newsletter* carried an item entitled “Confronting the AEA.” It began provocatively: “Should URPE demand ten million dollars in reparations from the AEA for the brainwashing its members have received at the hands of the

<sup>128</sup>Weeks (1972), p. 121.

<sup>129</sup>Caute (1988), p. 334. For instance, at Harvard University, conflict escalated in April 1969 when students seized the University Hall and were ultimately expelled by police intervention, this had until then been a peaceful campus. The conflict was traumatic in deepening the divide between radicals and conservatives in the faculty (Eichel *et al.* (1970)).

<sup>130</sup>At the 1967 AEA business meeting unidentified members proposed that the Association poll its members concerning the war and take a position on it. The Executive Committee was charged to consider the proposal and concluded the following year that the AEA should “reaffirm strongly the wisdom of the founders of the Association in . . . prohibiting the Association as such from taking an official stand or committing its members to any position on economic questions” (*American Economic Review* (1969b), p. 575).

profession?”<sup>131</sup> URPE planned to hold parallel sessions to the AEA meetings and added: “All members of URPE should come to New York prepared to pose searching questions to the official speakers and discussants; a real counter-presence depends upon the actions of all. Come to the URPE suite throughout the convention to keep informed of what is happening.”<sup>132</sup>

A year before URPE had been announced at the Philadelphia Grey Market meeting, boycotting the official AEA Chicago meeting. In 1969 at the AEA’s Business meeting: “a group of approximately twenty-five members of “a group of radical economists” filed into the meeting room. Their spokesman, Harvard’s Arthur MacEwan, demanded, on behalf of the group, that he be permitted to present a statement to the meeting.”<sup>133</sup> The statement read:

Economists in the United States work as a group and work contrary to the interests of the masses of people. The affluence and the power of the economists derive from their support of the elite, the elite which controls the institutional structure and the sources of power that perpetrate and reproduce the oppression of millions – the economists are the sycophants of inequality, alienation, destruction of environment, imperialism, racism, and the subjugation of women.

(...) economists do not merely praise the system; they also supply the tools – indeed, they are the tools – instrumental to the elite’s attainment of its unjust ends. They show how to manipulate people so that the system’s hinges are smoothly oiled.

(...) the A.E.A. plays directly destructive roles in our society. It serves to insure the perpetuation of professionalism, elitism, and petty irrelevance. It serves to inhibit the development of new ideas, ideas which are reflective of social reality.

Our conflict with the A.E.A. is not simply an intellectual debate. The A.E.A. cannot lessen our condemnation by their willingness to partake in debate, or by their willingness to provide a room to radical economists at this meeting. Our conflict is a basic conflict of interests. The economists have chosen to serve the *status quo*. We have chosen to fight it.<sup>134</sup>

<sup>131</sup> *Newsletter* (1969b), p. 1.

<sup>132</sup> *Ibid.*, p. 2.

<sup>133</sup> *American Economic Review* (1970), p. 487.

<sup>134</sup> *Ibid.*, pp. 488-9.

Thus, in scandal, the radicals announced their challenge to the profession.

The protest occasioned interest in the radicals and their economics. In less than a year the *Journal of Economic Literature* had commissioned and published a survey on radical economics. The text authored by Martin Bronfenbrenner, was impressively competent in its depiction of radicals' work. Bronfenbrenner drew upon URPE publications and captured many nuances that were lost to later commentators. For instance, he distinguished URPE economists from the *Monthly Review* group,<sup>135</sup> and he noted radicals' concentration in the field of economics of education.<sup>136</sup> Understandably, because radical economics in 1969-70 was in a process of definition, Bronfenbrenner did not have much to survey and he often ran into tangents in the history and ideology of radicalism and socialism, with little bearing on the immediate subject matter. The survey was not a critique of radical economics. There were criticisms but these were levied on particular authors and papers.<sup>137</sup> At times, Bronfenbrenner was also unashamedly complimentary: "Like too few of the rest of us, radical economists take their teaching seriously."<sup>138</sup>

Bronfenbrenner's conclusions in 1970 reflected the hallmark of later responses to radical economics. Firstly, that "Radical economics should be recognized as a legitimate field of concentration in the study and practice of economics."<sup>139</sup> Secondly, that the profession "require, and should continue to require, exposure of radical economists to orthodoxy".<sup>140</sup> The author expected that "the rise (or revival) of radical economics in America has far to go before it recedes" and he argued that this growth should occur within the profession and without antagonisms.

Radicals from 1969 kept a presence at the AEA meetings.<sup>141</sup> They were present in 1970<sup>142</sup> but what is worthy of note is their participation in the 1971 meetings. The

<sup>135</sup>Bronfenbrenner (1970), p. 758.

<sup>136</sup>Ibid., p. 755.

<sup>137</sup>Ibid., pp. 763-764.

<sup>138</sup>Ibid., p. 757. Bronfenbrenner was perhaps unique in being simultaneously a member of URPE and of the right-liberal Mount Pelerin Society.

<sup>139</sup>Ibid., p. 765.

<sup>140</sup>Idem.

<sup>141</sup>In 1969 radicals organized a session on the "Economics of Imperialism" with papers by Richard D. Wolff, Theotónio dos Santos, Harry Magdoff, and with discussants Stephen Hymer, Victor Perlo and Arthur MacEwan. The session was published in the meetings' Papers and Proceedings.

<sup>142</sup>Notably Gintis (1971) and Gurley (1971), of which more will be said later.

President-elect of the AEA for 1971 was John Kenneth Galbraith, a noted critic of mainstream economic theory.<sup>143</sup> Galbraith's programme for the meetings showcased radicals' work. There were sessions on the "Military-Industrial Complex," "Some contradictions of Capitalism," papers on "taxation of the rich," radical education and the political economy of women. The 1971 Richard T. Ely lecture was delivered by the critic of American economics, Joan Violet Robinson<sup>144</sup> and Galbraith gave a special luncheon in honour of Gunnar Myrdal, an author renowned for his analysis of race relations in America and the role of ideology in economic theory. At the business meeting under Galbraith's chairmanship, a group of women (including several URPE radicals)<sup>145</sup> motioned the AEA with the accusation of making economics "a man's field" and called for greater equality in training and hiring of women economists. It was proposed that a Committee on the Status of Women in the Economics Profession (CSWEP) be created to investigate the extent of gender discrimination. With Galbraith's support the motion was passed and the committee set up.<sup>146</sup> In Galbraith's Presidential Address, delivered in 1972,<sup>147</sup> he gave pride of place to the radicals, "the main problem with orthodox economic ideas – which Galbraith asserted were under attack from a 'new and notably articulate generation of economists' – is the separation of power from its subject."<sup>148</sup> "In a press conference, [Galbraith] encouraged so-called non-establishment economists to 'get their ideas met by making their demands painful'."<sup>149</sup>

It is clear that Galbraith was sympathetic to the radical cause, and he should not

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<sup>143</sup>John Kenneth Galbraith was born in 1908 in Iona Station, Canada. He received a B.Sc. from the Ontario Agricultural College (1931) and a Ph.D. in economics from University of California at Berkeley (1934). He taught at Berkeley and Princeton before moving to Harvard where he became a Professor in 1949, retiring in 1975. Galbraith was editor of *Fortune* magazine from 1943 to 1948 and published in popular periodicals such as *The New York Review of Books*, *The New Yorker* and *The New York Times Magazine*. He was an active participant in Democratic Party politics, adviser to President J.F. Kennedy and Ambassador to India from 1961 to 1963. Galbraith was a prolific and popular writer, from his many books, *The Affluent Society* (1958) and *The New Industrial State* (1967) stand out as the most influential.

<sup>144</sup>In a curious intersection between the histories of the two dissenting groups, Robinson's lecture had a profound impact for the history of Post Keynesian Economics.

<sup>145</sup>Interview with Laurie Nisonoff, 2003.

<sup>146</sup>In the same meeting Galbraith opposed a motion condemning the US war in Indochina, as a violation the Association's Charter (*American Economic Review* (1972)).

<sup>147</sup>Galbraith (1973).

<sup>148</sup>Jones (1972).

<sup>149</sup>Harbron (1972).

be taken as representative of the profession at large.<sup>150</sup> Therefore, the radicals' prominence at the 1971 meetings should not be taken as the profession's endorsement of radicals and their critique. Rather, the significance of the 1971 meeting was that Galbraith offered the radicals a platform that ensured the remainder of the profession would hear their challenge, take it seriously and respond.

Interestingly, the most explicit response to the economics of the radical left was authored by a visiting Swedish economist, Assar Lindbeck. In his 1969-70 visit to the US, Lindbeck had been impressed by the economic arguments of the New Left students. In a book entitled *The Political Economy of the New Left: An outsider's view*, he looked to outline the content of New Left economics and to show its shortcomings from the neutral position of a non-American. Lindbeck was not clear about his sources, he seldom made references, but in his introduction he noted three compilations of New Left texts. Only one of these contained any texts by URPErs, notably Zweig's "New Left critique of Economics", it is thus not surprising that Lindbeck only partially debated radical economics. That the book should not be seen as a response to radical economics was noted by Paul A. Samuelson in the book's foreword and by at least one of the book's reviewers.<sup>151</sup> Nonetheless in a section titled "New Left's Critique of 'Traditional' Economics" Lindbeck's comments do touch some of the radicals' (Zweig's) critical claims. He correctly noted the radicals' critique of marginalism and the profession's alleged neglect of the "interaction between economic and political factors."<sup>152</sup> Lindbeck in a style characteristic of his "outsider stance" both accepted and rejected the criticism. He adjudicated that not enough work had been done on important social ills (prominently racism, poverty and war) the source of the New Left's concerns, though Lindbeck then added that economists were beginning to address these subjects.<sup>153</sup> He also denied the radicals' claims by arguing that much of economics research was already relevant to address

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<sup>150</sup>In fact some AEA members were angered by his handling of the 1971 meetings, Arthur Okun bolted: "It was the worst thing I ever saw." (Collier (1973)).

<sup>151</sup>Samuelson (1971), p. xvi; Heilbroner (1972), ft. 1.

<sup>152</sup>Lindbeck (1971), p. 17.

<sup>153</sup>Ibid., pp. 23, 25.

those problems but was merely not being adequately translated to the public.<sup>154</sup>

For Lindbeck, the radicals' critique was grounded on two misunderstandings. Firstly, they failed to recognise that "economists are economists only and not also at the same time sociologists, political scientists, psychologists, philosophers, and so on (or social reformers or even revolutionaries)."<sup>155</sup> Secondly that radicals misunderstood the distinction between positive and normative economics, Lindbeck illustrated:

It is, of course, possible to study the effects on prices and quantities in the oil industry of a tax on gasoline, regardless of our feelings about the tax or about the oil industry, for that matter – an example of positive economics. The only subjective element in *positive* economics is, in principle, the choice of topic<sup>156</sup>

According to Lindbeck, within the boundaries of positive economics, separate from normative economics and other social sciences, economists could produce objective, apolitical work.

It is striking that Lindbeck's response to the radical critique echoed similar efforts by other authors. Typically the stage for these reflections were the sessions at the AEA annual convention. One instance of debate that I wish to single out occurred in a session, "The state of economics: the behavioral and social sciences survey", devoted to a report on the economics profession.<sup>157</sup> The debate over the report is worthy of note because it directly addressed radical economics as an approach intended to reform the practice of economics, as an alternative to conventional economics.

The radical commentator was John G. Gurley.<sup>158</sup> Gurley's main objection to the

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<sup>154</sup>Ibid., p. 23.

<sup>155</sup>Ibid., p. 22.

<sup>156</sup>Ibid., p. 26.

<sup>157</sup>It was part of a series reviewing the various behavioural and social scientific disciplines and providing advice to administrators on the needs and opportunities for their development, aiming for a new policy for science. The reports were organized under the auspices of the National Academy of Sciences and the Social Science Research Council (Riecken (1971), p. 43). The report is of some historical significance since it bore the ambitious plan of expanding doctoral programs in economics, a projected trebling of doctorates in ten years (Barber (1996), p. 23).

<sup>158</sup>Gurley is an interesting case of an established economist, just ending his term as editor of the *American Economic Review*, who converted to the radical cause. John G. Gurley was born in 1920. He studied economics at Stanford (BA in 1943, PhD in 1951). He was fellow at Brookings (1954-61) and taught at the University of Maryland (1953-61), in 1961 he joined Stanford University where



report was that it had ignored radical economics. Gurley claimed that the report reproduced mainstream economics' distorted conception of reality, "short on social relevance, precisely because its ruling paradigm – its conceptions of the world – excludes power, conflict, and disruptive change within a historical setting – that is, because it excludes a large part of reality."<sup>159</sup> For Gurley the political economist, unlike the conventional one, "studies economic problems within the historical context of ruler-subject relations, . . . he actively takes the side of the poor and the powerless, and he generally sees the system of capitalism as their oppressor."<sup>160</sup> Gurley's argument is close to the "paradigm of conflict" onto which radical economists converged by 1971.

Gurley concluded his attack by arguing that conventional economics' irrelevancy was carried into the report, "instead of worrying so much about national data systems, research money from foundations and the government, computer facilities, and research techniques, pay more attention to the economic implications of the fact that the United States is heavily involved as the dominant power in a hierarchical international capitalist system."<sup>161</sup>

One of Gurley's discussants, Robert L. Heilbroner was sympathetic to what he interpreted as the radicals' main goals: "to widen and deepen the range of what is called economic analysis."<sup>162</sup> Yet, despite Heilbroner's stated sympathy towards radical economics, he argued that the definition of the goals of economics was not a task for economists: "here, fortunately, the professional competence of the economist comes to an end, and he can do no more than take his place within the polity, to urge whatever goals – equality, freedom, growth, ecological balance and whatever – he seeks for society."<sup>163</sup> For Heilbroner, an economist abandoned his field of professional

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he remained until his retirement. Gurley was editor of the *American Economic Review* from 1962 to 1968. In the earlier part of his career Gurley wrote extensively on monetary theory, *Money in a theory of finance* (with Edward S. Shaw, 1960). After joining the radical group, he turned to research on comparative economic systems and the economic history of China, worthy of note is his book *Challengers to Capitalism: Marx, Lenin and Mao* (1975).

<sup>159</sup>Gurley (1971), p. 54.

<sup>160</sup>Ibid., p. 55.

<sup>161</sup>Ibid., p. 62.

<sup>162</sup>Heilbroner (1971), p. 66.

<sup>163</sup>Ibid., p. 67.

competence once he turned to advocacy. He raised other criticisms, namely that radicals failed from “over-generalization,” he questioned: “Is it capitalism alone – as the radical economists seem to maintain – that is responsible for the ills of racism, alienation, exploitation, bureaucratic indifference, etc.?”<sup>164</sup> Heilbroner noted the radicals’ lack of scientific precision, when spreading “a humanist-sounding gloss over areas of . . . ignorance,”<sup>165</sup> heading his list of radicals’ imprecise terms was “the people.”

Another of Gurley’s discussants, Robert M. Solow,<sup>166</sup> was more strongly worded against radical economics. In Solow’s view the omission of radical economics from the report would only be at fault if “the body of radical political economics is so large and so important that it demands mention. . . Radical economics may conceivably be the wave of the future, but I do not think it is the wave of the present.”<sup>167</sup> Solow went on to criticise radical economics: “I think that radical economics as it is practiced contains more cant, not less cant; more role-playing, not less role-playing; less facing of the facts, not more facing of the facts, than conventional economics.”<sup>168</sup>

The author took issue with the radicals’ claim that they had an alternative paradigm: “It is more a matter of posture and rhetoric than of scientific framework at all”, he continued: “the function of a scientific paradigm is to provide a framework for ‘normal science.’ But there is little evidence that radical political economics is capable of generating a line of normal science, or even that it wants to.”<sup>169</sup> Solow listed the kind of questions that might be seen as part of the normal science activity of the radical economics paradigm, but which he argued radicals had failed to address. For instance, Solow singled out a statement by Gurley where he asserted that shares in national income to workers and property owners were determined by the relative power of the two groups, and then questioned: “Am I to presume from

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<sup>164</sup>Ibid., p. 66.

<sup>165</sup>Idem.

<sup>166</sup>Robert Solow was born in Brooklyn in 1924. He earned a PhD from Harvard University in 1951. Solow taught at the Massachusetts Institute of Technology (MIT) since 1950. In 1987 he received the Nobel Memorial Prize in Economics.

<sup>167</sup>Solow (1971), p. 63.

<sup>168</sup>Ibid., p. 63.

<sup>169</sup>Ibid., p. 64.

this that there are studies of time series that show that short-run fluctuations in distributive shares reflect short-run fluctuations in the distribution of power in society?"<sup>170</sup> Solow dismissed the radicals' claim of the existence of an alternative Kuhnian paradigm by pointing to the absence of any "normal science" activity, the puzzle solving Kuhn identified as the habitual activity of scientists.<sup>171</sup>

Solow stressed the need for quantitative investigation ("calculations") and he found that radicals were disdainful of this sort of enquiry, he added with irony: "And when and if any of them [calculations that would prove the radicals' assertions] is done, you know who will do it – some poor damned graduate student in some conventional department, supervised by some conventional professor of conventional economics."<sup>172</sup> For Solow, scholarship was the interplay of "knowledge of technique and acquaintance with data"<sup>173</sup> which he found absent from the work of the radicals.

In the early 1970s Solow took up the role of paladin for conventional economics. In another article entitled "Science and ideology in economics," he responded to some of the radicals' criticisms, "the questions [that] confront any teaching economist these days who talks to his students and reads the handwriting on the wall".<sup>174</sup> Solow accepted that new urgent subjects had emerged in the late 1960s, but he asked for patience towards economic science:

It takes time for middle-aged men to change their research interests and their teaching, and it may take even longer for them to drum up any interesting and useful things to say. The theoretical analysis may be difficult, and statistical data are rarely available about something that has just now reared up in public consciousness.<sup>175</sup>

Solow also accepted that economists avoided the "dangerous" questions, those that

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<sup>170</sup>Idem.

<sup>171</sup>Solow's claim was not unreasonable, as my review of the literature on the radical paradigm attests, radicals preferred to present their challenge in treatises describing the distinctive features of their paradigm over producing exemplars of it.

<sup>172</sup>Idem.

<sup>173</sup>Ibid., p. 65.

<sup>174</sup>Solow (1970), p. 94. The article was initially intended as a response to Heilbroner (1970), an article in the same issue meant to reflect the radicals' critique. However, Solow is responding to more than Heilbroner's narrower interpretation of the radical critique. It is likely that Heilbroner and Solow were chosen to be Gurley's discussants because of their prior reflections in *Public Interest*.

<sup>175</sup>Solow (1970), pp. 95-96.

may challenge the system. But he found two good reasons for this: most such subjects were outside economists' competence, and some questions were prohibitively difficult to answer:

A study of "the size and distribution of the benefits of the war economy by socioeconomic grouping" scares me more by its impossible difficulty than by its possible subversiveness. I doubt that the data are available to do such a study with the rigor and precision that the profession now demands.<sup>176</sup>

Solow argued for the avoidance of the difficult subjects as a virtue. The profession's demanding standards could not accept such "vague and unanswerable" explorations.

Solow defended the profession's standards of objectivity, a frequent target for radicals and students. Solow accepted that despite professional standards some ideology crept into economists' work,<sup>177</sup> but also that the taint of ideology could be avoided if economists chose answerable questions. Characteristically, Solow did not reject the criticism; he sought to show how the problem could be contained:

It is a little hard to see how ideology sneaks into an attempt to discover how purchases of frozen orange juice respond to changes in price (even a socialist planning board might want to know that), or – to take something more specifically capitalist – how the plant and equipment spending of corporations is related to their sales and profits, interest rates, stock prices, taxes and other things.<sup>178</sup>

It was possible "to make social science as nearly value-free as it is possible to be"<sup>179</sup> by fostering professional criticism. Radicals were thus invited to participate in the professional community by respecting its rules, the profession's "standards of rigor, precision, and reliance on systematic observation interpreted by theory". They should abandon their "looser kind of discourse in which propositions are not supposed to be tested or testable, and it is never clear what kind of observation would definitely mark a hypothesis as false."<sup>180</sup>

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<sup>176</sup>Ibid., p. 98.

<sup>177</sup>Ibid., p. 99.

<sup>178</sup>Ibid., p. 100.

<sup>179</sup>Ibid., p. 101.

<sup>180</sup>Ibid., p. 102..

There were further exchanges between radicals and conventional economists at the AEA meetings, but these did not yield such wholesale assessments as the ones just reported. One such instance, occurred at the 1971 AEA meetings in a session on "Taxation of the Poor and the Rich." The primary piece of the session was by radical David M. Gordon<sup>181</sup> and in it he critiqued traditional tax incidence theory.<sup>182</sup> Gordon began by dismissing the possibility of major changes in the distribution of income under capitalism, but nonetheless he designed a program for the elimination of inequality.<sup>183</sup> The goal was to produce changes through time as measured by Gini coefficients, and he further calculated the needed yearly change in the coefficient for the elimination of inequality over a period of 25 years.

Commenting on Gordon's proposal were Robert J. Lampman and Henry J. Aaron. Their assessments were akin. It was felt that Gordon's paper was marred "by a tendency, common, I regret to say, in work of radical economists, to substitute exegesis for analysis."<sup>184</sup> The critics found radical economics lacking both in its analysis of the role of taxation in the capitalist system, but also and principally in its design of policy. They felt Gordon had not done enough work on the means to achieve his policy goal, which made the whole proposal objectionable:

I cannot agree with his implication that the goal should be pursued without regard to its cost. I gather that radical economics identify elimination of inequality as a prime goal. But, at the risk of being terribly irrelevant, I suggest that it is reasonable to ask what headlong pursuit of that goal may cost or gain us in terms of other goals. Further, I would like to know what policies would have the greatest cost-effectiveness.<sup>185</sup>

Gordon's critics were calling for more detailed calculations of the radical proposals.

<sup>181</sup>David M. Gordon (1944-1996) was born into a family of economists, his father a famous macroeconomist and once President of the AEA, his mother well-known for her contributions in economics of employment and welfare. He received his training at Harvard University (BA in 1965, PhD in 1971). He was a researcher at the NBER until 1973 when he joined the Graduate Faculty at the New School for Social Research. His most famous works are *Segmented Labor, Divided Workers: The Historical Transformation of Labor in the United States* (with Richard Edwards and Michael Reich, 1982), *Beyond the Waste Land: A Democratic Alternative to Economic Decline* (with Samuel Bowles and Thomas E. Weisskopf, 1983) and *After the Waste Land: A Democratic Economics for the Year 2000* (with Samuel Bowles and Thomas E. Weisskopf, 1991).

<sup>182</sup>Gordon (1972), p. 322.

<sup>183</sup>Ibid., p. 325.

<sup>184</sup>Aaron (1972), p. 333.

<sup>185</sup>Lampman (1972), p. 322.

They rejected the grand systemic questions posed by the radicals over what they saw as the empirics of precise, quantitative details.

The self-appointed spokespersons for the “conventional theory” that radicals had attacked responded to the criticism by first denying the existence of competing paradigms, and secondly by reaffirming economic science’s objectivity in its independence from ideology. In their response the relevance of economic science lay in its patient and precise “calculations,” in its modest and contained questioning. Both for the radical critics as for those defending traditional economics this was a debate over the boundaries of economic science, of its place in reference to such entities as politics and objectivity.

### **3.4 The resolution of the radical challenge**

Allies are indispensable players in scientific disputes. As Radicals distanced themselves from the remainder of the profession, making the profession into the enemy, they adopted the “movement” as their ally. Primary to this liaison with the political left movements were the students. Through curriculum and grading reforms, through outspoken advocacy for social revolution in the classrooms, the radicals sought to enlist the support of their activist students. This was not a surprising choice of ally, the students since 1969 had assisted the radicals’ setting up of the first radical courses. The student protests had never been as widespread as in May 1970, when the radicals were writing their challenge into paradigmatic form.

The mainstream on the other hand was primarily accommodating and reassuring. The mainstream sought to contain public controversy and to impose the conventional mode of debate in the profession, formally immune to political partisanship. This defence of economics appealed to those who required reassurance on the profession’s objectivity credentials.

The focus of this section is to show how decisive the allies were in settling the dispute. After 1971 there was a rapid decline of student protest. It shows that despite the radicals’ failure to supplant the mainstream of the profession, which may be

deemed a mark of defeat, the record was puzzlingly mixed, and the radicals were not without their victories.

### 3.4.1 Rejecting the radicals

Early in the 1970s there were signs that radical economics was successfully effecting change in the economics profession. Paul A. Samuelson, one of the most prominent figures of post World War II economics and author of the textbook that dominated the teaching of economics for over four decades, felt in the early 1970s that he should change his portrayal of Marx. Arjo Klammer noted in his comparative study of the editions of Samuelson's *Economics*, that in the 7<sup>th</sup> edition of 1967, Marx was mentioned briefly in a short historical summary in the introduction:

Thus almost as halfway point [in the history of economics], there appeared the massive critique of capitalism by Karl Marx. . . . A billion people, one-third of the world's population, blindly regard *Das Kapital* as economic gospel. And yet, without disciplined study of economic science, how can anyone form a reasoned opinion about the merits or lack of merits in the classical, traditional economics?<sup>186</sup>

In the 9<sup>th</sup> edition of 1973, a chapter on the history of thought had been added ("Winds of Change: Evolution of Economic Doctrines") where greater attention was devoted to Marxism.<sup>187</sup> And in a rewritten introduction, a new statement on Marx was added:

It is a scandal that, until recently, economics majors in economics [sic] were taught nothing of Karl Marx except that he was an unsound fellow. . . . In this edition I have tried to treat Karl Marx as neither God nor Devil – but as a secular scholar whom half the world's population deem important. The rudiments of mature Marxism, as well as the insights of the resurrected young Marx, are newly discussed in this revision.<sup>188</sup>

The change in Samuelson's *Economics* did not go unnoticed by contemporaries. They were surprised by Samuelson's new assessment of Marxism: "Marxism may be too

<sup>186</sup>Cited in Klammer (1990), p. 150.

<sup>187</sup>Ibid., p. 143.

<sup>188</sup>Cited in Klammer (1990), p. 150.

valuable to leave to the Marxists. It provides a critical prism through which mainstream economists can – to their own benefit – pass their analyses for audit.”<sup>189</sup> It was remarked that Samuelson had broadened the scope of economic inquiry to “questions that border upon such related disciplines as sociology, anthropology, political science, psychology and history.”<sup>190</sup>

Samuelson’s revised textbook bore the imprint of the radical critique. The radical critique was also reflected in the syllabus of the “economics principles” courses. For instance at Columbia University in addition to Samuelson’s textbook, teachers were reported to have used David Mermelstein’s collection of radical texts, with writings from such diverse authors as Paul Sweezy, Paul Baran, Ernest Mandel, Herbert Marcuse, Karl Marx, Galbraith and Oscar Wilde.<sup>191</sup>

It would be contentious to argue that these developments represented a major transformation (radicalisation) of the profession; in fact some saw it as mere “seasoning” of a larger banquet:

None of the senior faculty at Columbia objected to the introduction of radicalism into the economics curriculum. “We’re very glad to have some unorthodox teaching around,” said Prof. Harold Barger. “Suitable seasoning is very desirable. Of course, if someone told the senior faculty we’d have to teach nothing but Karl Marx, I think there would be a revolt.”<sup>192</sup>

Although the textbook and course content changes bore evidence to the profession’s acceptance of radical subjects, it was not the mark of wholesale or wholehearted approval. As young radicals began to be considered for tenure (usually after 7-8 years of contract) most saw it refused, while others were denied renewal of their contracts. The most publicised of the clashes over hiring between radicals and mainstream economists happened at Harvard, with Samuel Bowles as the protagonist.<sup>193</sup>

<sup>189</sup> Cited in Brazelton (1977), p. 117.

<sup>190</sup> Brazelton (1977), p. 115.

<sup>191</sup> Mermelstein (1970). The book ran a number of editions throughout the decade, with a final third edition in 1976.

<sup>192</sup> Salmans (1970), p. S3-2.

<sup>193</sup> Samuel Bowles born in 1939, earned a BA from Yale University and PhD from Harvard (1965). He was Assistant Professor at Harvard (1965-71) and Associate Professor (1971-74). From 1974 Bowles



Bowles joined the Harvard faculty in 1965. On his arrival, Bowles did not hide his radical convictions; he refused to sign the State teacher's loyalty oath and was threatened with dismissal.<sup>194</sup> Bowles opposed what he saw as "an interference with freedom of speech" and succeeded in defeating the University authorities, remaining in the economics faculty. For this he benefited from "very encouraging support from my own department and elsewhere."<sup>195</sup>

When Bowles was being considered for tenure at Harvard in 1972-73, he was not greeted with the same broad support he enjoyed on occasion of his opposition to the loyalty oath. He received the sponsorship of three of the best known members of the department: Wassily Leontief, Kenneth J. Arrow and Galbraith. "Yet despite such impressive backing, Bowles' promotion was turned down by a vote of the whole senior faculty."<sup>196</sup>

The head of the Harvard department, James Duesenberry, at the time defended the decision as follows: "We haven't made a tenure appointment in four years ... even though we have two positions open. ... When we make a tenure appointment, we are looking for the best appointment we can make from anywhere (not just from within the department)."<sup>197</sup> Duesenberry accepted that the decision was tantamount to an assessment of radical economics: "We have people here who are claiming to have a substantial set of new developments. I simply have to say that, in my opinion, the amount of evidence they have put on the table has just not been that significant."<sup>198</sup>

Duesenberry voiced the standard assessment held by conventional economists. Radical

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has been Professor of Economics at the University of Massachusetts at Amherst. His major works are *Schooling in Capitalist America: educational reform and the contradictions of economic life* (with Herb Gintis, 1976); *Beyond the Waste Land: a democratic alternative to economic decline* (with David Gordon and Thomas Weisskopf, 1983); *Democracy and Capitalism: property, community, and the contradictions of modern social thought* (with Herb Gintis, 1986); and *Recasting Egalitarianism: new rules for communities, states, and markets* (with Herb Gintis, 1998). He is the son of Chester Bowles (1901-1986) former governor of Connecticut, Ambassador to India and Nepal under President Harry S. Truman, undersecretary of state under President Kennedy and then Ambassador to India until 1969.

<sup>194</sup>Loyalty oaths were instituted during the 1950s witch-hunt years, as historian Helen Schrecker has shown "Almost every state, whether or not it investigated its universities or had them investigate themselves, imposed some kind of a loyalty oath on its teachers. ... By the late fifties, thirty-two states required loyalty oaths" (Schrecker (1986), p. 116).

<sup>195</sup>*New York Times* (1966), p. 9.

<sup>196</sup>Lifshultz (1974), p. 28. Bowles received five votes for tenure out of a 24 full professors of the department at the time (*Kennebec Journal* (1973)).

<sup>197</sup>*Kennebec Journal* (1973).

<sup>198</sup>Idem.

economics was seen as “negligible,” not to be rejected, but to be “neglected.”<sup>199</sup>

Bowles was not the only radical at Harvard that failed to be reappointed with tenure. Soon after, the department decided not to reappoint Arthur MacEwan prompting an outcry by graduate students. But contemporaneously, the student protest seems to have succeeded in reversing a similar decision made on Herbert Gintis.<sup>200</sup> As I have shown, Harvard was home to one of the largest groups of radical graduate students and young faculty. Although some students remained in the graduate program, the dismissals effectively dissolved the radical group.

Following the very public case of Bowles, a string of cases of non-renewal of the radicals’ contracts occurred in 1974: four from San Jose State University, one from Lehman College (CUNY) and one from the University of Massachusetts – Boston.<sup>201</sup> The events at San Jose in California were particularly shocking, since it was seen as a centre for radical scholarship in the West Coast. The dismissals there came from an administrative decision, contravening a previous departmental vote.<sup>202</sup> The URPE steering committee interpreted the firings as politically motivated: “The firings are an attempt to get at those teachers who have good and close connections with students – at those who point the finger at capitalism.”<sup>203</sup>

In response to the growing threat placed on the radicals’ employment URPE staged a protest at the AEA meetings in San Francisco, December 1974. “About 60 radical economists, representing the Union of Radical Political Economists [sic], picketed the economic association presidential address late Sunday to protest the firings chanting such slogans as “they say cutback, we say fight back””.<sup>204</sup> To expect some form of response from the AEA was not unreasonable, the previous year John G. Gurley had been elected to the Vice-Presidency of the association and Paul M. Sweezy to its Executive Committee. Furthermore, the Association’s acting president for 1974 (and therefore presiding over the proceedings) was Robert Aaron Gordon,

<sup>199</sup>Solow (1971), p. 63.

<sup>200</sup>Lifshultz (1974), pp. 28-29.

<sup>201</sup>Golden (1975), p. F1.

<sup>202</sup>Rowe (1974), p. D6.

<sup>203</sup>Cited in Golden (1975), p. F1.

<sup>204</sup>Rowe (1974), p. D6.

father of David Gordon, a radical at the New School for Social Research.

With no stated endorsement from URPE, William H. Behn and Henry M. Levin (both colleagues of Gurley at Stanford, and the latter a former collaborator of Bowles on the economics of education) presented to the meeting four proposals on the subject of hiring. The last and most ambitious of the proposals urged “economic departments to take immediate measures to attract and hire permanent (tenure track) economists working in the Marxian economic paradigm”.<sup>205</sup> It was found to be out of order and was never subject to discussion. The first resolution dealt with political discrimination of students. It required that student files (particularly reference letters) be open to faculty members designated by the students and that departments refuse to collaborate on enquiries into the political views of students. It was passed with majority support. The second resolution called for more extensive publicity of job openings (effectively through a more frequent publication of the AEA’s *Job Openings for Economists*), and for all departments to post their job offers; while the former was rejected, the latter part of the resolution was accepted.

For the purpose of the current argument the third resolution was the most significant, as it dealt with political discrimination in hiring practices and made direct reference to the events at San Jose State University and Lehman College as the most recent cases of discrimination. The proposal read: the AEA “shall establish a standing committee, the Committee on Political Discrimination, whose members shall be appointed each year by the President of the Association to collect information on issues of fact in cases involving termination of employment for alleged political reasons.”<sup>206</sup> Not surprisingly, the third proposal proved to be the most controversial.<sup>207</sup> The reference to San Jose and Lehman College was deleted early on in the debate, with the assent of Behn and Levin. Discussion raged on between those that agreed with the creation of the Committee on Political Discrimination and those

<sup>205</sup> *American Economic Review* (1975), p. 442.

<sup>206</sup> *Ibid.*, p. 444.

<sup>207</sup> In the 1972 meetings there had been a motion denouncing political discrimination and asserting the Association’s strong condemnation of political discrimination against radicals in hiring decisions or government grant allocation (*American Economic Review* (1973a)). But it had not been accompanied by direct action by URPE neither did it produce such a clear result as the 1974 creation of the Committee on Political Discrimination.

arguing that the committee would be replacing the functions already held by the American Association of University Professors (AAUP) and thus felt that efforts should be concentrated in allowing the AAUP to work more effectively. But finally, the Behn and Levin proposal prevailed, voted and passed by the majority.

As outlined in the resolution, the President of the AEA would choose the head of the Committee, and Robert A. Gordon appointed Kenneth J. Arrow.<sup>208</sup> The Committee did not report until 1976 and then it did so to the Executive Committee but not to the Business Meeting.<sup>209</sup> In the 1975 meetings David Gordon on behalf of the committee, produced a statement “entitled ‘Taking Political Discrimination Seriously’ and proposing a resolution that would broaden the functions of the Committee,”<sup>210</sup> but a discussion of the proposal was considered out of order for not having arrived in the regulatory thirty days in advance to the meeting. In 1976 and having run for two years, the committee had a total of eight cases under investigation, it reported: “Two investigations have been completed and final reports transmitted to the individual faculty member, his or her academic department, the institution’s president, and the American Association of University Professors (AAUP). The Committee found no definitive evidence of political discrimination in either case.”<sup>211</sup> Arrow explained that there was great difficulty in determining whether discharges resulted from political beliefs, the quality of a professor’s work, or the recession, which had forced some colleges to lay off faculty members.<sup>212</sup>

The proposed extension of the functions of the committee made in 1975 by David Gordon entailed conducting a study on the extent of political discrimination in the hiring practices of departments, modelled on a similar survey study conducted by the Committee on the Status of Women in the Economic Profession (CSWEP). Despite Gordon’s urgings<sup>213</sup> the executive committee only accepted the study in December

<sup>208</sup>Golden (1975), p. F1.

<sup>209</sup>*American Economic Review* (1977).

<sup>210</sup>*American Economic Review* (1976), p. 444.

<sup>211</sup>*American Economic Review* (1977), p. 439.

<sup>212</sup>Epstein (1976), p. A29. The radicals acknowledged the effects of the recession leading to reduction in faculty numbers, but warned that this would serve as a cover for pursuing political firings (*Newsletter* (1974b), p. 5).

<sup>213</sup>*American Economic Review* (1977), p. 441.

1977, with a funding allocation of \$10,000. The study was never conducted with the judgment that the funding was insufficient. By 1977 the Committee on Political Discrimination was no longer investigating cases of dismissal and was forwarding all complaints to the AAUP.<sup>214</sup> Later in the decade according to Jordan Kurland, the acting general secretary of the AAUP, the number of cases of dismissal began to fall,<sup>215</sup> and the Committee on Political Discrimination stopped reporting to the Executive Committee, effectively since 1979.<sup>216</sup>

The aftermath of the dismissals and the slow and ineffectual response by the AEA (elusive in its conclusions and thus unwilling to become involved) only gave credence to the radicals' suspicion towards the profession. To preserve their jobs, the formerly outspoken radicals were attempting to go unnoticed:

With university jobs generally scarce these days, some so-called "closet radicals" have tried to sneak onto campuses by sending in job resumes that hide their radicalism. Others, who already have jobs, try to keep their jobs by having mail from the Union for Radical Political Economics sent to their homes rather than to the office, where anti-radical department chairmen might notice.<sup>217</sup>

The dismissals of radicals occurred contemporaneously to the decline of student power, the radicals' closest allies. Although the relationship between the two is not always explicit and direct, the record of contract renewals and dismissals, a mix of success when accompanied by student unrest or failure when not, sustains this connection.

Only when student unrest was credible would departments and administrators reverse dismissal decisions. In the early 1970s a large number of radicals converged onto Yale's graduate program:

Joined by several younger faculty members, the students began an independent study group to read *Capital* and subsidiary texts. While

<sup>214</sup> *American Economic Review* (1978), p. 450.

<sup>215</sup> Epstein (1976), p. A29.

<sup>216</sup> Lee (2004b) has researched a larger number of cases of dismissal and what can only be termed bullying of radicals by the establishment. I have focused here on the major cases, those that reverberated into the AEA meetings and that led URPE to protest actions.

<sup>217</sup> Golden (1975), p. F1.

pursuing their own self-education, the students asked the Department to offer a number of courses where the problems of imperialism, underdevelopment, Marxian economics, and the history of economic thought could be studied in a formal basis. The students also urged the Department to hire interested and competent faculty to teach these areas not only in the immediate future but also for the long run.<sup>218</sup>

The faculty responded in 1971 by asking third-year graduate student David Levine to teach a course in the History of Economic Thought and an undergraduate level course on Marxian economics.<sup>219</sup> When Levine left for a year in 1972, students circumvented the conventional economics courses by exploiting a loophole in Yale rules. They designated their radical work a tutorial and found a sympathetic faculty member to supervise. That year over 40 tutorials in the same subjects mushroomed at Yale. On Levine's return and given the growing student pressure, the economics department hired him as an assistant professor. It is unlikely that the hiring of Levine would have occurred without the graduate students' actions.

### 3.4.2 Radical departments

Denied tenured posts at the elite institutions where they had obtained their PhDs, many radicals had to find employment in less prestigious Universities or in liberal arts colleges where there was very limited opportunity for research. Dispersed throughout the country, a number of radicals managed to cluster in departments where radical graduate programs could be designed and radical research along academic lines could be pursued.

The legacy of the dismissals and of the falling student radicalism did not prompt the radicals' exclusion from the profession but their migration from the elite campuses to less affluent institutions. The most important achievement of the radicals, and one that I wish to narrate in some detail, was the concentration of

<sup>218</sup>Lifshultz (1974), p. 53.

<sup>219</sup>A major political event at New Haven at this time may have strengthened the radicals' bargaining power. Bobby Searle (co founder with Huey Newton of the Black Panthers) and other Black Panther members were accused of the killing in 1969 of a Panther member under the suspicion that he worked for the FBI. On May Day 1970 at the New Haven Green more than 15,000 protested at the trial coinciding with the national protests over the bombings of Cambodia (Katisificas (1987), p. 118).

radicals at the University of Massachusetts –Amherst (U.Mass.- Amherst). This has remained the most important centre for radical research.

Amherst lies 90 miles west of Boston, the hub of the “five colleges”:

U.Mass-Amherst, Amherst College, Hampshire College, Smith College and Mount Holyoke College. The University of Massachusetts at Amherst was originally and until 1931, the Massachusetts Agricultural College. In 1970 Amherst was still a mainly rural community, “a cow town”,<sup>220</sup> an unlikely destination for urbane radical intellectuals.

The origins of a radical centre for research at Amherst are closely connected with the major affair of dismissal of a radical, that of Samuel Bowles in 1973. Since 1967 the U.Mass.-Amherst department had been in a state of turmoil. It had repeatedly received poor assessments by external committees (in 1967 and again in 1971) despite an investment in hiring “high flyers.” The hiring policy undertaken seems to have focused on “mathematical economists, ... to the avoidance of such traditional fields as macroeconomics, public policy, monetary theory, and economic history”,<sup>221</sup> which angered many in the faculty. In 1971-72 alongside the disputes over hiring that split the faculty, another conflict was added over the firing of a radical teacher, Mike Best. The University administration sidestepped the department and decided not to renew Best’s contract. The head of the department saw the interference as undermining the department’s authority and his own, and resigned.<sup>222</sup>

In a move to solve the divisions in the economics department, the Dean of the faculty, Dean Alfange, was nominated acting head of the Economics Department. Noting a fall in the number of undergraduates in economics, Alfange came to agree with the criticism that hiring had been too narrowly focused.<sup>223</sup> In justifying his own hiring policies, Alfange approvingly referred to Galbraith’s 1972 Presidential Address and his denunciation of a “new despotism,”

which “consists in defining scientific excellence as whatever is closest in

<sup>220</sup> According Herbert Gintis, this is to be taken literally, he recalls that in the 1970s there were cows grazing around the campus (interview with Herbert Gintis, 2003).

<sup>221</sup> Lifshultz (1974), p. 30.

<sup>222</sup> Ibid., p. 30.

<sup>223</sup> Lifshultz (1974), p. 52.

belief and method to the scholarly tendency of the people who are already there. ..." Because of this I had no doubt that the Department needed to be broadened and balanced in order to reflect more widely the professional views that are held in the discipline at large.<sup>224</sup>

After Best's dismissal, Alfange may have also wanted to dispel the image of a department hostile to radicals and engaged in political firing.

Bowles had taken a year off from Harvard in 1972-73 and was at U.Mass.-Amherst's Labor Center. Predicting the problems he would face with the upcoming "tenure or out" decision, Bowles showed interest in joining the University.<sup>225</sup> His bargaining power was patent in that he did not ask for a job but rather for a centre of radical research: "Bowles pointed out that one of the greatest obstacles to the development of radical scholarship in economics was that academics interested in its development were scattered and isolated from fellow colleagues".<sup>226</sup> Bowles thus suggested that alongside his hiring with tenure, the department should also hire Herbert Gintis,<sup>227</sup> Richard Edwards,<sup>228</sup> Stephen Resnick,<sup>229</sup> and Richard Wolff,<sup>230</sup> the first two from Harvard, the other two at the time teaching at City

<sup>224</sup>Idem.

<sup>225</sup>Walsh (1978), p. 34.

<sup>226</sup>Lifshultz (1974), p. 52.

<sup>227</sup>Herbert Gintis born in 1939, earned a BA in Mathematics at the University of Pennsylvania, and an MA and PhD in Economics at Harvard (1969). He taught at Harvard from 1969 to 1974 as Lecturer, becoming Associate Professor Economics in 1973. Joined U.Mass-Amherst in 1974, where he was Associate Professor until 1976 then becoming Professor. Some of his major works are "A Radical Analysis of Welfare Economics and Individual Development" in *Quarterly Journal of Economics* (November 1972); *Schooling in Capitalist America: educational reform and the contradictions of economic life* (with Bowles, 1976); *Democracy and Capitalism: property, community, and the contradictions of modern social thought* (with Bowles, 1986); and *Recasting Egalitarianism: new rules for communities, states, and markets* (with Bowles, 1998).

<sup>228</sup>Richard Edwards born in 1944, earned a BA from Grinnell College (1966), and an MA and PhD at Harvard (1972). He taught at U. Mass-Amherst from 1974, from 1982 as Associate Professor. He moved in 1991 to the University of Kentucky to take the post of Dean to the College of Arts and Sciences, and in 1997 joined the University of Nebraska's College of Business Administration as Professor of Economics. His major works are *Contested Terrain: the transformation of the workplace in the twentieth century* (1979); *Segmented Work, Divided Workers: the historical transformation of labor in the United States* (1982); and *Rights at Work: employment relations in the post-union era* (1993).

<sup>229</sup>Stephen Resnick was born in 1938. He earned a BS at the University of Pennsylvania and a PhD from M.I.T. (1964). Was associate professor at Yale (1963-71) and at City College - CUNY (1971-73), since 1974 he is Professor of Economics at U.Mass-Amherst. In 1989, he was one of the founders of the journal *Rethinking Marxism*. His major works are *Knowledge and class: a Marxian critique of political economy* (with Richard Wolff, 1987); *Bringing it all back home: class, gender, and power in the modern household* (with Richard Wolff, 1994); and *Class theory and history: capitalism and communism in the USSR* (with Richard Wolff, 2002).

<sup>230</sup>Richard Wolff was born in 1942, earned a BA at Harvard, MA at Stanford, and a PhD from



College New York.<sup>231</sup> Like Bowles, Gintis, Resnick and Wolff were hired into tenure track positions (the exception was Edwards who had just finished his PhD).

As Alfange faced criticism for the hiring of radicals, his comments in a memorandum once again resonated Galbraith and the radicals' appraisal of the profession:

It seemed to me impossible ... for the department to continue to remain insensitive to the ferment taking place within the discipline of economics, in which a substantial number of economists – including some of the most prestigious members of the profession – were challenging the dominant neo-classical paradigm and calling into question the ability of the profession utilizing that paradigm adequately to deal with many of the most urgent social problems in the nation and the world.<sup>232</sup>

The new radical presence did not help the Department of Economics to settle into an acceptable *modus vivendi*, one of the reasons why Alfange had taken over as head of department. In 1975 the *New York Times* carried a letter evidencing an environment of suspicion:

The letter charged that two conventional economists were being blocked from receiving tenure by radicals who “occupied a commanding position in the department.” The letter also accused the radicals of seeking to create vacancies in order to fill them with economists of “their own ilk.” Later, the department chairman at the time, Norman D. Aitken, a non-radical, wrote a letter of rebuttal to the *Times* noting that the two conventional economists in question had, in fact, won tenure.<sup>233</sup>

A year later, a formal agreement was reached that would finally bring an end to the disputes. The agreement specified by name the people who were to hold positions of responsibility, crucially on the department's committee responsible for hiring. The

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Yale (1969). He taught at City College - CUNY (1969-73), and since 1974 he is Professor at U.Mass-Amherst. Wolff in his research cooperates with Stephen Resnick, for his major works see the previous footnote. Along with Resnick he was one of the founders of the journal *Rethinking Marxism*.

<sup>231</sup>City College “enrolled more African Americans and Puerto Ricans students than any other “integrated institution of higher education in the United States”. In 1970 the school had adopted a new admissions program which favored the enrollment of pupils from inner city high schools (Roff, Cucchiara, and Dunlop (2000), p. 120). This made it an attractive location for radicals seeking to move closer to the lower classes (interview with Richard Wolff, 2003).

<sup>232</sup>Lifshultz (1974), p. 52.

<sup>233</sup>Walsh (1978), p. 35.

agreement included a hiring plan, identifying the specialities of the new department members to be recruited, and setting a ratio of radicals to nonradicals at roughly three to seven.<sup>234</sup> Helping to settle the fighting was also the naming of a new chairman, Donald Katzner, who had arrived at U.Mass after the radicals and had not taken part in the conflicts.

By 1976 the department of twenty-five members had ten who despite their differences would accept the label of Marxists. They were the largest and most visible group of radicals in academia at the time. Since this group included some of the best-known American radicals, graduate students with strong credentials were attracted to the department, which pleased some conventional economists in the department and the University administration.<sup>235</sup>

Although U.Mass-Amherst hosted the most publicly visible and influential group of radical economists, it was not the only place where radicals were able to maintain radical courses and a radical graduate program. Other departments that merit reference were located at the American University in Washington D.C. and at the New School for Social Research in New York. As evidenced by the geographical distribution of these departments and despite the high mobility of academics, Radical Political Economics remained in the early 1970s primarily Eastern and Northeastern based.<sup>236</sup> On the West Coast there were also groups of radicals at Stanford University<sup>237</sup> and at the University of California at Riverside,<sup>238</sup> but the role played

<sup>234</sup>Ibid., p. 35.

<sup>235</sup>Ibid., pp. 34-35.

<sup>236</sup>It is significant that in 1971 a section entitled "Way Out West" was created in the *URPE Newsletter* with the explicit goal of voicing the problems of Western members and connect them to the strongly-bound radical economics movement in the East and Midwest (Clement (1971)).

<sup>237</sup>At Stanford University in 1975 and "following a four year student struggle, the economics department established a field in "Alternative Approaches to Economic Analysis" (*Newsletter* (1975b)). The department counted with John Gurley, Duncan Foley and Don Harris who had in that year become a tenured faculty member. Although this was a small group in a large department it counted with the support from radicals in other departments, namely Anthropology, Education and the Food Research Institute. See also Christiansen (1974).

<sup>238</sup>At the University of California – Riverside, acceptance of radical economists was won after a two-year battle. The most prominent radicals there were Howard Sherman and E. K. Hunt. The department's chairman Roger Ransom commented on at the presence of 3 radicals in the 11 strong department, "I don't think that makes our department radical ... but it's more than a sidelight.". Ransom's assessment of the radicals was that "I think the radicals are weak on analysis... but they are very strong on laying out the issues and this turns students on.' As a result they swarm to the radicals' classes" (cited in Trombley (1975)).

by radicals in these departments was marginal and precarious.

The creation of the radical departments shows that in the mid-1970s some of the radicals' most effective allies were older and established colleagues. These more conventional economists appreciated the radicals' criticism of the profession and though not necessarily endorsing the radical project for the transformation of the profession, they found it merited consideration and debate. This support was most visible in the hiring of the Amherst radicals. Alfange received

letters and telephone calls from four past presidents of the AEA (one of whom had won the Nobel Prize for economics, another who would receive the same award within the year), a former editor of the *American Economic Review*, and several other well known names in the discipline. They all certified that, although the candidates they proposed were indeed radical, they were highly qualified and competent economists. They all strongly urged Alfange to make the appointments.<sup>239</sup>

At the New School for Social Research the radicals counted upon the support of Robert Heilbroner and at Stanford upon that of former *AER* editor John G. Gurley.<sup>240</sup> Older and established colleagues were unlikely allies if one considers that the radical challenge sought to undermine the establishment's credibility. Yet, the radicals were also in their challenge invested as the spokespersons for a generation that more left-leaning established colleagues admired.

By 1975 radical economics was thus no longer a feature of Harvard, MIT or Yale but a major voice coming out of U.Mass.-Amherst, New School and less prominently of American University, Stanford University and U.C.-Riverside. The departments mentioned were "radical departments" in so far as they supported radical research and despite radicals being always in a minority position. In these new homes a second generation of radical economists would be trained into the 1980s. However, these were not perfect and eternal havens for radicals. Opposition from many of their conventional colleagues would periodically flare, most notably at U.C.-Riverside in

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<sup>239</sup>Lifshultz (1974), p. 52.

<sup>240</sup>Gurley although not involved in URPE and much older than the majority of radicals became a spokesperson for radical political economics in the AEA.

the mid-1980s.<sup>241</sup> Many mainstream colleagues continued to see radicals as an illegitimate competitor for scarce jobs, and tensions would recur when that most valued of prizes in American academia was at stake: tenure.

### 3.5 The academic politics of radical economists

In this chapter I have explored the interplay between student radicalism, the making of radical economics and the mainstream's response to the radicals' emergence. It may seem that I have arrived back at where I started, participant accounts' locating the emergence of radical economics in the New Left. What I want to show in a new light is the claim that radicals were the spokespersons of the sixties' left movement, as these same participant accounts assert. One should be wary not to overplay the radicals' own claims of being the spokespersons of sixties political dissention.

Radical Political Economics began in a modest tone with the call for economics to address what radicals saw as the pressing problems of American society. It was not a new economic theory, it was a divide made to distinguish those that cared for politics and took it seriously, and those that preferred to ignore it. Yet, American society and the campuses were in "civil war" in 1968-70 and the radicals' early political divide was soon developing into something else. Radicals sharpened their differences with the remainder of the profession, further elements were added to strike the distinction, objectivity and neutrality were criticised, subjectivity and partisanship promoted. These features were named as elements of the paradigm of conflict, to replace the traditional, conventional paradigm of the profession. This paradigm carved out a clear identity for the Radical Political Economics community. It effectively prescribed how they were to act: reforming the Universities, displacing the authority of their conventional colleagues, and militanting for social revolution.

The process of increased differentiation was contemporaneous with the rise of student unrest, and it benefited from some student support. Yet, the battle was fought between professional academics, with academic weapons, conferences, papers,

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<sup>241</sup>Interview with Robert Pollin, 2003.

courses; and its objects were paradigms, and epistemologies. It was a faculty war, and the politics which radicals employed were much more academic than they portray them to be. The students (and the “movement” generally) were enlisted for support but were not the authors’ of RPE’s definition, nor is it clear how they may have given even a limited input to the definition. The image I wish to convey is that of academics reaching out to the students, in contrast to the participant account’s view of social movements reaching into economic science, and dictating the transformation.

The 1971 AEA annual meeting was the climactic moment to the radical challenge and the crisis of credibility they championed. The mainstream of the profession, in its response to the radicals, took to defend the standards of science. It downplayed the existence of divisions and of ideology, trying to guide economists on how to insulate economics from politics. The conventional economists hoped to close ranks to preserve the professions’ credibility, and scientific status. The radicals hoped to use the radical students and their protests to wedge a transformation of the university.

Contrary to the radicals’ expectations the student turmoil was short-lived, dying at the close of the Vietnam War and the move of the radical leadership into revolutionary politics. As a result, radical economists were pushed to the margin of the profession. Once facing renewal of contracts, with possibility of tenure, they were dismissed. In some limited contexts, radicals succeeded in their battle for jobs, preserving continuation of radical research at U.Mass – Amherst, New School for Social Research, American University, Stanford, UC-Riverside.

In the following chapter, I discuss another important dimension to mid-1970s events. I will show that radicals were not merely pushed aside. Their political commitments demanded that they abandon their academic critiques and employ their economics for the benefit of the left-wing movements then mushrooming in the U.S.A.. It was these developments at URPE that shaped the radical economics group’s internal structure and forced its intellectual redefinition away from the “paradigm of conflict” they had constructed by 1971.

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## Economics with the people

### The fragmenting of radical identity in the 1970s

#### 4.1 The Union of Radical Political Economics in the 1970s

The historical literature on the U.S.A. in the 1970s is dwarfed by the voluminous debates over the 1960's political and popular culture.<sup>1</sup> Typically the 1970s are portrayed as a decade without a "personality," somewhat of a transition between the liberal and radical 1960s and the conservative 1980s. Because it appears as a watershed period in American history, of major shocks and cultural and political transformation,<sup>2</sup> it merits closer scrutiny than it has been subject of. To look at history by decades is a rather arbitrary device but one that suits well my material, particularly if I am allowed to tweak the start and end of the 1970s. For the purpose of my story, the decade began in 1972 and ended in 1978.<sup>3</sup>

The focus of this chapter is on the Union for Radical Political Economics (URPE) and debates during the 1970s. Although radicals remained active researchers and continued to advance accounts of the workings of the economy, such activity was not as important in redefining what Radical Political Economics was as the Union activities. It was not to the radical departments being formed in the mid-1970s<sup>4</sup> that

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<sup>1</sup>On the sixties there are several useful histories, for instance in this thesis I have drawn extensively from Isserman and Kazin (2004) and Morgan (1991). For the seventies only Schulman (2001) is worthy of note.

<sup>2</sup>Slocum-Schaffer (2003), p. 203.

<sup>3</sup>It has been argued that like 1968, the year of 1973 was a watershed year, when a myriad of events seem to conflate and suggest to the historian a turning of the historical tide (Hollinger (1997), pp. 338-339).

<sup>4</sup>See section 3.4.2 of this thesis.

radicals turned to for a definition of radical economics or of what it was to be a radical economist. URPE had housed the work of defining the radical paradigm, it was again stage to a new transformation of radical identity. In the early 1970s radicals expected that a revolutionary moment was in the making and thus believed that the real political organizing lay alongside the popular movement. To connect with the nascent revolutionary movement, outreach work became for the active membership of URPE the crucial activity that defined the radical economist. URPE was throughout the 1970s more than a professional organization, as it undertook a programme of pamphlet writing and support for social movements. The tasks of this chapter are first to show how and why the outreach work was set up, providing also a description of its content. Secondly, I will chronicle the end of the outreach work and assess its consequences for the radical Union and for the definition of radical economics.

The history of the radicals' activism in the 1970s has not yet been told. One likely explanation is because it is more than a history of academic institutions. Being a history of academics' involvement with the public it fades from the historian's outlook. It does not register in scholarly articles or conference meetings and evidence must be sought elsewhere, in correspondence, interviews, newsletters and pamphlets (some of which are not collected in libraries). The second difficulty is in the analysis of the actors in play. A radical economist may have a clearly defined identity in the professional and academic community but when he travels to the activist arena there is no single translation of his identity, will he be a Democratic Socialist, a Marxist Leninist, a Maoist, a populist? How are we to understand a radical economist's actions, interests and commitments in this public/political context? As I will show throughout this chapter this is not only a problem for the historian, but also the crucial difficulty radical economists had to face in their outreach efforts. Their problem was how to keep the radical economics identity alongside the many identities native to the activist community?

## 4.2 Outreach over academia (1972-75)

Radical student protest was never as widespread as in the aftermath of the 1970 invasion of Cambodia and the subsequent Kent State and Jackson State College killings. And in 1971, radicals achieved professional recognition at the AEA meetings under J. K. Galbraith's Presidency. Yet, in 1972 radicals decided to change URPE's focus away from academia. The new emphasis originated projects directed at the wider popular movements. They intended to inform political practice with radical research and at the same time infuse political relevance into radicals' research. The radicals' Union abandoned earlier engagement with the professions' mainstream and Association. Radicals embraced a new community of reference.

The *Newsletter of the Union for Radical Political Economics* (henceforth *Newsletter*) provided the main outlet for debate and the reporting of activities by the Union. While in 1969-71 the *Newsletter* was populated by reports of new courses and pedagogical debate,<sup>5</sup> from 1972, the pages of the *Newsletter* were increasingly filled with reflections on political strategy and reports of activities with mass movements. In this section I survey the content of these discussions and their interplay with the outreach activities of the Union.

### 4.2.1 Taking over the AEA or Movement Coordinating

In November 1971 the article "The University and Socialism" authored by Frank Ackerman,<sup>6</sup> appeared in the *Newsletter*. Although it was not originally written for

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<sup>5</sup>Some of the pedagogical debates were transferred to the *Review of Radical Political Economics*, worthy of note is the Winter of 1974 issue with papers on "The Content of Introductory Economic Courses" and "The Social Relations of the Classroom". Many radicals had expertise in the economics of education and pursued their research along these lines. The content of the earlier *Newsletter* articles and the *Review* ones were however distinct, the former more akin to announcing or reporting activities and listing course contents (mainly as templates for similar efforts elsewhere), the latter as analysis of the sociology and economics of education.

<sup>6</sup>Frank Ackerman earned his BA. in Mathematics and Economics from Swarthmore College in 1965 and a PhD in economics from Harvard University in 1975. He has taught at University of Massachusetts – Amherst (1982-83); University of Massachusetts – Boston (1984-85); and Tufts University (from 1995). He was original editor of *Dollars and Sense* from 1974. He is a member of the Global Development and Environment Institute at Tufts and has written extensively on environmental issues, notably, *Why do We Recycle? Markets, Values and Public Policy* (1997). He has also written on economic methodology, *The Flawed Foundations of General Equilibrium: Critical Essays in Economic Theory* (with Alejandro Nadal, 2004).



URPE its publication as the main piece in the November *Newsletter* bears proof of an interest in Ackerman's political theorizing.<sup>7</sup>

In the text Ackerman rejected 1960s "new working class" or "youth as class" theories that placed academic radicals in a critical location for sparking social revolution.<sup>8</sup> The author expounded a refusal to identify a single revolutionary group:

Some groups on the left identify some segment of society, such as blue-collar industrial workers, "youth," or black and third-world communities, as the crucial link in a socialist movement. Because the crucial group is most oppressed, most aware of the shortcomings of capitalism, and/or because it is in vital position of power, it is argued that that group must lead the movement, and other groups must be subordinate of, perhaps subordinate to, the leading group.<sup>9</sup>

Ackerman expounded an alternative approach to building a revolutionary movement. Since all sectors of the population experienced the tensions inherent to capitalism, they all had a revolutionary potential. This outlook gave action in the Universities a legitimate revolutionary status, though no longer a central one: "Capitalism has growing need for the products of universities – ideologies and ideologues, researchers, technicians, administrators, and generally well-socialized, skilled, white-collar workers."<sup>10</sup>

Ackerman's strategy towards socialism reflected the views of many URPErs that saw their professional commitments as compatible with the struggle for revolution. The December 1971 meetings of the AEA gave URPErs reasons to be optimistic towards change in the academic establishment. The meeting organised by Galbraith had a significant radical representation in its program, the URPE Women's Caucus had successfully assisted the birth of CSWEP, and an Anti-Samuelson pamphlet distributed in the meetings had been well received.<sup>11</sup> In the aftermath of December 1971, some argued in "An Action Program for URPE" that the Union should engage with the AEA more forcefully. It was felt that "There are no doubt many

<sup>7</sup>It was first published in *Upstart* of the University Radical Union at Harvard.

<sup>8</sup>Ackerman (1971), p. 3.

<sup>9</sup>Ibid, p. 1.

<sup>10</sup>Ibid. p. 2.

<sup>11</sup>Behr et al (1972), p. 16. On the meeting, see page 83 of this thesis.

[economists] who are against racism, imperialism, and the other manifestations of capitalist exploitation. If we can ally with them on this basis then we can more effectively support the struggles of the oppressed peoples and isolate the Samuelsons.”<sup>12</sup> Three areas of activity were recognised: “ongoing fights against bourgeois (pro-capitalist anti-people) ideas in economics, especially in the introductory economics courses”; “ongoing activities around jobs for economists where the principal effort would be in URPE taking leadership in faculty and graduate student union drives”; and “use the annual AEA meetings politically to supplement the above ongoing activities”.<sup>13</sup> According to this view, URPE should provide assistance and advice in: writing pamphlets on theoretical subjects for inclusion in introductory courses (in the absence of a radical economics introductory book); reaching publishers to indicate the existence of a market for radical economics books; and “trying to win control of the *AER* [*American Economic Review*] away from the elite schools and into more progressive hands.”<sup>14</sup> The authors saw events in the History and English academic associations as exemplars: “radical groups in history and English are organized in caucuses and have been successful in fighting for changes in their annual meetings. For example, the Modern Language Association (MLA) has had a radical candidate elected to be president of the association”.<sup>15</sup>

The “An Action Program for URPE” just outlined, was intended for discussion at the 1972 Summer Conference and to serve as a guide for a transformation of URPE’s focus. While in the past URPE had served mainly as a forum for debate and protest against the AEA, the authors of the Action Program in 1972 felt the time had come to “take over the AEA.” However, at that Summer Conference<sup>16</sup> a very different organizational line was adopted, embodied by the creation of the Movement Coordinating Committee.<sup>17</sup> Its manifesto identified five kinds of audiences deserving

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<sup>12</sup>Ibid., p. 16.

<sup>13</sup>Ibid., p. 15.

<sup>14</sup>Ibid., p. 19.

<sup>15</sup>Ibid., p. 20.

<sup>16</sup>URPE holds since its inception a scholarly annual conference at the Allied Social Sciences Associations meetings and a Summer Conference yearly in July/August. The Summer Conference is a place for making decisions and electing officials.

<sup>17</sup>*Newsletter* (1972).

distinct responses by URPE. The first were “libraries and liberals” for which the article dryly announced URPE had nothing to offer. Their advice to the second audience, “teachers that see political activism as talking,” was that they should get their students to see revolt as necessary for changing the outside world. Thirdly came “radicals doing standard research,” for these URPE should show them how to draw radical implications from their research. The fourth audience were “radicals doing radical research” and the Union’s contribution was to make their results more widely known. Finally came those members involved in direct action, to which URPE should provide information and support. In practice the Movement Coordinating Committee called for the organization of workshops and teach-outs, alongside the publishing of pamphlets and literature for wide distribution. Thus in practice the efforts were focused towards the latter three kinds of audiences identified.<sup>18</sup>

That the majority of URPE members endorsed the Movement Coordinating Committee in the Summer of 1972 is evidence that the goal of radicals “taking over the profession” (as proposed earlier that year) held little currency. In the light of Movement Coordinating radicals were not only (or even primarily) professionals, radicals were activists and it was felt that more should be done in support of this role. URPE was asked to extend its activities as a forum for radical scholarship to providing support for mass direct action. The idea was not new, since URPE’s seminal conference in Philadelphia in 1968 there had been support for the radical economist as an advocate.<sup>19</sup> Radical economics informing the social movements and showing the way to revolution was after all central in the radicals’ self-definition. It was radical economics’ usefulness for revolution that justified it over conventional economics.<sup>20</sup> Yet prior to 1972 no initiative had been taken to institutionalise the radicals’ alleged connection with the social movements. URPE had remained a

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<sup>18</sup>The list of desired subjects to be addressed was extensive: state and local loans, US government savings bonds, housing, rent and rent controls; do white collar workers benefit from imperialism?; Nixon strategy for education; radical guide to transportation; worker security; pensions compensation; health and safety; police repression of workers’ organizations; Nixon’s new economic policies; inflation and unemployment; military-industrial complex; pollution – who pays?; welfare- who pays?, who benefits?; and crime – who pays? ( *Newsletter* (1972), p. 4).

<sup>19</sup>Wolff (1969).

<sup>20</sup>See discussion in p. 76 of this thesis.

professional association, organising conferences and publishing a scholarly journal.

The dismissals of radicals could not have been the factor for the transformation of URPE's organising, for these would only be acutely felt later, in 1973-74.<sup>21</sup> The change in URPE's focus came at a high-point of optimism (although it was to prove itself short lived), when a consensus had been achieved on the nature of Radical Political Economics, and in the aftermath of the AEA 1971 meetings. URPE's outreach vocation reflected an additional factor, namely the ideological evolution of the New Left (the SDS graduates) in the early 1970s towards working class politics. What several commentators of 1960s and 70s politics, former-SDSer and Harvard alumnus Todd Gitlin being probably the most vocal amongst them, characterised with dismay as the end of the New Left and the post-1968 conversion of radicals to the Old Left.<sup>22</sup> The conversion was ideological,<sup>23</sup> first from 1968 calling for revolution in America and in the early 1970s, at the high point of social unrest, by adopting Marxist-Leninism as a guide to revolutionary practice.

The ideological reshaping has been portrayed as sweeping through many political denominations and organizations, and not just as the chosen path of former SDS radicals leaving academia. "After battering legal segregation and winning formal voting rights in 1964-65, veterans of the civil rights movement turned their attention to battles for economic equality and political empowerment",<sup>24</sup> such was the new direction taken by Martin Luther King Jr. prior to his assassination in 1968. Deemed by FBI's J. Edgar Hoover as "the greatest [single] threat to the internal security of the country",<sup>25</sup> the Black Panther Party, even though not clearly a Marxist organization, combined strands of black nationalism and Marxism in an eclectic mix:

[The Black Panthers] were the most prominent revolutionary organization in the country during the key transitional years 1968-71, and they proved to be the most important single organization in the transition of thousands

<sup>21</sup> Although the dismissals of radicals and the demise of student power would clearly become factors in URPE's intensification of its activism efforts in 1973-75.

<sup>22</sup> Gitlin (1987).

<sup>23</sup> Despite noted affinities with the Marxist Left, New Lefters in their endorsement of Marxism did not join the traditional organizations of the Old Left, such as the CPUSA or the SWP. Instead, they created their own, of which more later.

<sup>24</sup> Elbaum (2002), p. 46.

<sup>25</sup> Cited in Elbaum (2002), p. 28.

of activists from New Left radicals, Black Power advocates or Third World militants to partisans of Third World Marxism and Leninism.<sup>26</sup>

Following the testimonies of those involved, the adoption of Marxism-Leninism arose from their efforts to understand and oppose the US war in Indochina: "Of all traditions within Marxism, it was Leninism that placed the most emphasis on the imperialist nature of twentieth-century capitalism, on the revolutionary potential of national liberation struggles, on the legitimacy of armed struggle, and on the primacy of building solidarity with oppressed peoples."<sup>27</sup> In a sense these radicals learned their Marxism from the Vietnamese, from the Chinese and from the Cubans whom they admired for their struggle against American imperialism.<sup>28</sup>

The new emphasis on the people and the working class seemed vindicated by events in American society. A new resurgence in union militancy led to unexpected shows of force. In 1969 in Detroit a combative League of Revolutionary Black Workers was created and in New York the concerted actions of 15,000 workers shut down all bridges bringing the city to a grinding halt.<sup>29</sup> Even the patterns of student protest seemed to bear proof of the revolutionary potential of the working class, "while student protest ebbed at the more elite universities after 1970, antiwar and other demonstrations spread further (if less spectacularly) at community colleges and high schools with higher percentages of working class youth."<sup>30</sup>

Radical economists took leadership roles in some of the left-wing formations mushrooming in the early 1970s. Although the radical economist had been defined as a scientist-activist, his actions had been contained to the academic arena; radicals now sought to live out the scientist-activist in a new community. These URPEs urged a redefinition of their Union. The Movement Coordinating Committee was URPE's first response to the new ideological landscape, but after just a year of activity its record was dismal. According to its critics, where local committees had been organised they had failed to function:

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<sup>26</sup>Ibid., p. 65.

<sup>27</sup>Ibid., p. 43.

<sup>28</sup>Interview with Michael Zweig, 2003.

<sup>29</sup>Elbaum (2002), pp. 28, 47.

<sup>30</sup>Ibid., p. 47.

At best, some literature was put out and some speakers directed to needed places, but this failed to develop into a consistent and effective instrument. URPE people, ourselves and the movement co-ordinators included, tended to remain remote from the every day work of building movements for social change. Therefore, our written work and oral presentations tended to be general and abstract, in the sense that the work did not fit well into the particular political-ideological-informational needs of the people requesting help.<sup>31</sup>

In their October 1973 analysis of the Movement Coordinating's failings, Steve Rose and Michael Zweig imputed the failure of the committee on URPE's misguided insistence on acting as a professional organization. They argued that URPE should create a presence independent of and opposed to the AEA and other professional associations:

URPE should not become involved in the internal workings and deliberations of the AEA, seeking to shape that organization. There may be a resolution before the AEA which, taken alone and in the abstract, URPE would support. Still, in the concrete nip and tuck battle with professionalism among radical economists and other socially concerned economists, it would be irresponsible to look at the AEA as the arena for social leverage.<sup>32</sup>

URPE should inform social movements and acting inside the AEA would not develop a "real" movement. The optimism of 1972 and of the "Action Program" had been eclipsed by growing cases of dismissals of radicals and difficulties faced inside academia, the new tone was pessimism. Rose and Zweig warned that:

We have to face the reality that job security cannot be a guaranteed part of the lives of radicals. ... we cannot depend on the AEA to secure our presence ... The only guarantee for our security is connection with strong and growing mass movements on our campuses and in our communities. Again, URPE is led to ties with mass struggle – as the testing ground for our ideas, as the mechanism for social change, and as the source of our security<sup>33</sup>

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<sup>31</sup>Rose and Zweig (1973), p. 10.

<sup>32</sup>Ibid., p. 12.

<sup>33</sup>Idem.

URPE had always opposed professionalism; it had denounced it as a form of elitism, hierarchical and oppressive. Yet, these flaws were not seen as precluding academic work's effectiveness in leading to social change. This had been the content of the radicals' indictment of the profession at the AEA's 1969 meetings protest and implicit in Ackerman's "The University and Socialism".<sup>34</sup> Rose and Zweig's argument was different; they saw professionalism as a misdirection of the radicals' efforts, as a tactical mistake. The only political force was the mass movement of the people and nothing could be achieved in the intellectual confines of academia. The people constituted a new intellectual community providing a check to radical ideas, URPE should be committed to developing the "practice of political economy and testing its ideas in the context of [the mass struggles of the people]." For Rose and Zweig, the Movement Coordinating Committee's support of mass action had not gone far enough because URPEs were still overly committed to academia. Radicals were asked to deepen their involvement with mass movements.

The new organizational line was more than an extension of radical activities. URPE's resources and debates were strikingly redirected, it was not an addition of a new kind of activity, it was a new core to the radicals' work, their most prized commitment. The change was not so extreme as to demand an abandonment of the radicals' academic careers to devote themselves fully to political organizing. Neither was the new line endorsed by the whole of URPE's membership. Some URPEs remained aloof from the activist efforts, they maintained their involvement with the AEA (as dues paying members and participants in its annual meetings) and still saw their activities as academic economists to be contributions in building a socialist movement.<sup>35</sup>

#### 4.2.2 Political Education and Action Coordinator

Only a year after the creation of the Movement Coordinating Committee, at the Summer Conference of 1973, a new outreach project was designed, the Political

<sup>34</sup>See p. 81 of this thesis, Ackerman (1971).

<sup>35</sup>In 1973 at New York there was a strong URPE presence at the AEA meetings to protest against the wave of radical firings (*American Economic Review* (1974)).

Education and Action Coordinator, in shorthand PEAC.<sup>36</sup> It was “established to provide information, analysis, contacts, and valuable links with labor organizations, community projects and movement groups.”<sup>37</sup> The project was run by a collective based in New York. It was entrusted with the task of mobilising URPE members into researching pressing economic problems of the time in radical and action oriented ways. The collective would collate the research contributions of the several members into a “mass pamphlet for political education.”<sup>38</sup>

The first project undertaken by PEAC was on food inflation. Many URPE members attending consumer group meetings and women’s groups found the issue frequently raised. Radicals’ inquiries showed that from January to March 1973 there had been a sharp rise, of 4.6% in the price of food leading to the formation of consumer groups intent on opposing further price increases. A “National Food Research Collective” was set up at the 1973 Summer Conference, and about 50 members throughout the country worked on aspects of the “Food Fact Package.” The pamphlet was concluded in early 1974 and then distributed to local URPE chapters and organizations that requested it.<sup>39</sup>

The *Food Package* included 28 separate fact sheets ranging from one to 14 pages each.<sup>40</sup> The fact sheets fell into three main groups. A first group described the magnitudes and impact of “the food crisis.” A second group provided contextual description and analysis of the structure of the food industry, emphasising the growing concentration of economic power in that sector. The third group criticised the “official explanations” of the food inflation placing them in a political economy context. With each fact sheet bearing on a particular subject (for instance, one was devoted only to trends in the consumer price index of food, another was an analysis of the market structure of the food industry, yet another was a discussion of the impact of unionisation on food prices), they could be used independently or combined with

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<sup>36</sup> *Newsletter* (1973), p. 9.

<sup>37</sup> Keefe (1974), p. 17.

<sup>38</sup> *Newsletter* (1973), p. 9.

<sup>39</sup> *Idem*.

<sup>40</sup> URPE-PEA (1974a). I am indebted to Ruth Inbeck for providing me with copies of the pamphlets and one of the reports written on the pamphlet writing activities. I have had access to what I believe is the final version of the package.



other fact sheets for the writing of popular articles or pamphlets. This design represented the radicals' intention of providing information that could be flexibly used by other groups, to make it serve multiple purposes.

In the varied set of fact sheets of the *Food Package*, radicals estimated the extent of price increases and their negative impact on the real wages of workers, on nutrition and even its effects on starvation and malnutrition of the poor, while arguing that the counterpart was the building up of corporate agro-industry profits. The food sector was identified as one of the fastest growing industries in America, with increasing concentration and mechanisation. As for the causes of food inflation, radicals argued that it resulted from a conjunction of what they labelled "structural dynamics," and "short-term conjunctures". They noted that the concentrated food industry held the consumer to ransom by transferring rises in costs into inflation, and insulating any negative effects on profits. The price increases were compounded by speculation in the futures markets for food and by weather calamities in Asia during that year, raising international demand. The radicals accused the government of not stepping in to protect the poor consumers, and instead keeping a cap on food production to allow prices and profits to rise.

The distribution of the pamphlet package was deemed successful as several thousand copies (over seven hundred in the first few months) were printed and distributed. Interest arose from groups making different uses of the information: "Liberation News Service wrote a series of articles from the food materials which went out to all their subscribers"; "people in community schools, like the Liberation School in Berkeley, have used the food materials in classes"; "the living theatre collective has picked up on the information to make a series of six plays to present in front of supermarkets and at work places when they tour"; and "people organizing in food processing plants came to pick up the material to use for writing a pamphlet."<sup>41</sup> The interest generated by the project prompted further requests for radical economics insights into: "recession, inflation, taxes, Nixon's statements and policies about workers". Many URPE members began to offer articles to the New York collective on

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<sup>41</sup>Weinbaum (1974), pp. 12-13.

such topics as “runaway shops, class war in England, Nixon’s economic policy, housing”.<sup>42</sup>

In 1973-74 a further project was conducted by the PEAC collective entitled *The Energy Crisis: A Matter of Profits*. The “Energy Crisis” was the focus of contemporary debate after October 1973, when Arab members of the Organization of Petroleum Exporting Countries (OPEC) in the midst of the Yom Kippur War, agreed on an oil embargo to all countries supporting Israel in its conflict with Egypt. The OPEC member states further agreed to use their leverage to produce a four-fold rise in international oil prices.

Similarly to the first pamphlet, *Energy Crisis* was “not intended to be read as a book. Rather [radicals noted] that each piece could serve as the basis for a speech, a newspaper article, a study group discussion, a radio broadcast, a videotape program, or a leaflet”.<sup>43</sup> The radicals identified the questions they were trying to answer in the packet, the main one being: how do oil companies reorganise production and sales to maintain high profits and continued expansion? A host of secondary questions addressed the extent of the oil shortage, the origins and use of energy, the structure of the energy industry, and government regulation of the sector. Finally, radicals explored the long-run implications of the crisis for capitalism and imperialism, and for people’s resistance, looking into how people were fighting back. As in the *Food Package*, radicals aimed for a wide ranging discussion of the subject, from its quantitative economic description leading to a political analysis.

The radicals’ concluding analysis of the crisis stressed the international interdependence of the energy business. They began by noting America’s frailty prior to the oil embargo, as a rise in nationalism in the Middle East and in the Third World pointed to a decline in American hegemony, further aggravated by competition from the European and Japanese and a depreciating dollar. In this context the energy crisis had been a godsend, radicals argued: “The energy crisis has pulled the rug out from under Japan and Western European countries while boosting the U.S. back up.

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<sup>42</sup>Ibid, p. 14.

<sup>43</sup>URPE – PEA (1974b).

On the whole, the U.S. is much less dependent on oil imports and is raking in the money selling oil internationally at such astoundingly high prices.”<sup>44</sup> Radicals concluded that prices were being kept high to fuel the profits of the oil business and the expansion of American imperialism.

The content of both the Food and Energy pamphlets reflected PEAC’s stated objectives. Written in non-technical and non-ideological language, they were designed to be used as widely as possible. The 1974 Summer Conference of URPE, a year after PEAC’s creation, re-affirmed “Political Education and Action (PEA) efforts are a correct and essential part of URPE’s overall program of activities.”<sup>45</sup> A slight change of name, with the deletion of the “Coordinator” represented an expansion of activity with the proposal that several coordinator units be established on a regional basis. The functions of the regional PEAs were to receive suggestions on PEA activity for the region and publicise them; encourage participation of movement groups in PEA regional projects; and organize a collective to work on specific projects as they arose.<sup>46</sup> By 1974 there were a number of ongoing national projects, while continuing the distribution of the concluded *Food and Energy* packages, a project on “How to Research a Corporation” was near completion, a “Current Economic Crisis Project” had been initiated and a “Women’s Work Project” was being coordinated from Washington D.C..<sup>47</sup>

The Women’s Work Project was initially run by two cooperating collectives, one based in Washington D.C., the other in New York. It produced in 1974-75 three pamphlets, *Women in Health* (directed at health workers), *Women in Today’s Economic Crisis* and finally *Separated and Unequal: Women in the UAW after WWII*. These pamphlets were shorter than the *Food and Energy* packages, with about 20 pages each, equivalent to the size of some of the larger sections of the PEA

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<sup>44</sup>URPE – PEA (1974b), p. 4.2.

<sup>45</sup>Keefe (1974), p. 17.

<sup>46</sup>Ibid., p. 19.

<sup>47</sup>“How to Research a Corporation” remained “near completion” for the rest of the decade, the “Economic Crisis” project was concluded but in changed circumstances that are discussed later (URPE-PEA (1975)). There was also mention of a “Health Project” conducted from New Haven, Conn. and a “Southern Industrialization & Unionization Project” (Keefe (1974), p. 18) but I found no evidence of their publication.

packages. They were also distinct in style, less was provided in terms of analysis or insights into current political debates, and more focus given to information and data, be it evidence of women's subaltern position at work (with both personal testimonies and statistics), or by giving details of and contacts to unions or action groups that would assist women organizing. The overall focus was on women as workers and developing women's consciousness of their social and economic situation, which gave the pamphlets a grass-roots flavour compared to the *Energy* or *Food* packages which were designed primarily for organizers.

The PEA and the Women's Work Project (conducted under PEA's wing) mobilised many URPE members into building connections with popular movements. The outreach initiatives were meant to educate the movement on the unjust inner workings of the economy but for radical economists they also represented a check on their research. The pamphlet writing was justified on intellectual grounds as it showed radicals the way to politically relevant research and shielded them from irrelevant scholarship. Away from the lecture halls, radicals were seeking a very different intellectual community. The audience was the people, the subjects were current and political and communication was made through pamphlets and teach-outs. There was no hierarchy and no authority. This was the radicals' ideal community.

Was URPE's gamble towards outreach activities a reasonable strategy or was it merely an adventure by a few of its members? The absence of opposition to these activities between 1972 and 1975 and a substantial growth in membership suggests that the new strategy was widely supported. Looking at the membership figures of the organization until 1975 there was a steady increase in its members. In the academic years of 1970-71 and 1971-72 membership grew at about forty percent a year, from 821 members in 1969 to 1643 by 1972. Growth slowed down from 1972 onwards but the number of new members was stable at 700 plus and membership renewal was at around sixty percent. In 1974-75, when the PEAC pamphlet writing activities were at their peak there was even a pronounced increase in both new members and membership renewals.<sup>48</sup> URPE's move away from academia and towards activism

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<sup>48</sup> *Newsletter* (1976).

seemed at the time a secure strategy for extending the organization's influence.

#### 4.2.3 Marxism in the *Review of Radical Political Economics*

The early 1970s ideological reshaping of the American Left towards Marxism and the working class and URPE's outreach efforts also reverberated in changes to the editorial line of the *Review of Radical Political Economics (RRPE)*. The *Review* had been published since 1969, its editorial board voted by the Union's membership at its Summer Conference. It was the obvious platform to debate the intellectual program of radical economics, as it had done in 1971 in the definition of the "paradigm of conflict." The "Revised Statement of Editorial Policy for the RRPE" drafted in the fall 1973 meeting of the editorial board, candidly communicated to "the rest of URPE" a "very interesting development." It announced that a consensus had been reached on the question "What is the RRPE and how should it develop?"<sup>49</sup>

It was felt that *RRPE* should be a "scholarly journal" written for radical intellectuals and that the route toward a mass-media format should be rejected. The negation of this route was justified by the pamphlet writing efforts that were then becoming a core activity of the URPE membership at large. The editorial statement refused to limit the *RRPE*'s interest to "strictly 'economic' subjects" and thus invited contributions that might fall within the confines of other disciplines. The crucial novelty however was that the editorial statement set the *RRPE*'s vocation as developing "a specific radical social science, Marxism."<sup>50</sup>

The editorial board began by denying that earlier debates on a radical paradigm had any content. The new editorial line was a negation of the earlier definition of radical economics. The editorial board argued that earlier efforts by radicals had remained critiques of "bourgeois 'science'" and that a construction of an alternative analysis had not begun in earnest:

Indeed URPE has been defined as a group, both from within and without, primarily in terms of its opposition to capitalist society and bourgeois ideas. Within this perspective it would seem that the real meaning of the

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<sup>49</sup>Editorial Board (1974a), p. ii.

<sup>50</sup>Idem.

desire expressed at the board meeting to work toward the elaboration of Marxist science was the wish to see the URPE membership adopt a common project, that of “building Marxism,” and by doing so to become united in a new and more fruitful way through a common praxis.<sup>51</sup>

The editorial statement reported, “There was virtually no dissension [at the board meeting] to the view that there is no real alternative to Marxism as a science for analysing contemporary human society and that the RRPE should accept that as a given and act accordingly. This fundamental endorsement was very strong and came from many different people”.<sup>52</sup>

The editorial board committed itself to tailoring selection criteria to promote Marxian economics, which it briefly defined in the editorial statement as “an expression of the key role of the working classes in the transformation of modern society” and in more scholarly terms as any literature that references, evaluates or develops, works in the Marxist tradition (the concrete example given being Baran and Sweezy’s *Monopoly Capital* ).<sup>53</sup> Suitable topics of research were also identified: “the historical development, current functioning and emerging contradictions of past and contemporary social systems, the role of methodologies of the social sciences, and the actual and alternative strategies for revolution in America and elsewhere”.<sup>54</sup>

The statement added a measure of caution. It manifested concern that the majority of URPE members might not identify with the common project, and that the true and final assessment of the move could only be performed in the following annual (summer) meeting. The editorial board’s concern proved to be warranted for a number of URPEs made public their opposition to the new editorial line. One prominent critique was raised by Thomas Weisskopf, a member of the editorial board who had been absent at its 1973 fall meeting. He had tried to modify the statement early on, but since this proved without consequence he hoped that the change could be defeated at the Summer Conference.

Weisskopf raised three criticisms to the new editorial line. The first was that the

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<sup>51</sup>Ibid., p. ii-iii.

<sup>52</sup>Ibid., p. ii.

<sup>53</sup>Baran and Sweezy (1966).

<sup>54</sup>Editorial Board (1974a), p. iii.

editorial contained vague generalities and left too many questions unanswered: “the statement is very unclear about just what this new emphasis on Marxism is going to mean in practice.”<sup>55</sup> This criticism was to some extent accepted by the editorial board; in a response to the critics, the board in its spring meeting qualified its move towards Marxism. Firstly the editorial restated its aim, “more emphasis on the Marxian method of analysis,”<sup>56</sup> i.e. neither a wholesale adoption of Marxism as the sole publishable scholarship, nor Marxism’s characterisation as a theory, it was redressed as a method of analysis. Secondly, the Board noted that there was no unanimity between its members on what constituted Marxian analysis:

Some people mentioned an emphasis on historical and dialectical methods of thought, e.g., the idea that social structures change as a result of the conflict among social classes. Others thought the economic theories of value and crisis in Marx’s *Capital* would be important to explore. Still others mentioned the earlier philosophic works of Marx on alienation as an important theoretical starting point for radical political economy.<sup>57</sup>

The editorial board felt that the role of the *RRPE* was to serve as a forum either to explore the usefulness of the Marxian method of analysis, in its many definitions, or to present alternatives that could be shown to be “scientifically superior to that suggested by Marxian methods of research”.<sup>58</sup> The editorial board’s Spring clarification was in all respects a weakening of the earlier statement. The move towards Marxism was restated as not implying that only Marxist theory was acceptable for publication, and multiple definitions of Marxism were shown to be acceptable.

The second criticism raised by Weisskopf was that the editorial board was taking on an unwanted role: “I don’t believe that the board ‘can and should change the Review, beginning now’ when the change is one that is closely linked to questions of ‘organizational redirection for URPE as a whole’”.<sup>59</sup> Weisskopf was therefore denouncing the adoption of Marxism and the new directive stance of the editorial

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<sup>55</sup>Weisskopf (1974), p. 24.

<sup>56</sup>*Newsletter* (1974a), p. 36.

<sup>57</sup>Idem.

<sup>58</sup>Idem.

<sup>59</sup>Weisskopf (1974), p. 24.

board as an imposed redefinition of URPE's polity. His final point argued that the statement was unnecessarily restrictive in its insistence on Marxism as the only form of Radical Political Economics. He noted that: "What unites members of URPE, if anything, is a profound dissatisfaction with American capitalism in general and with the currently orthodox discipline of economics in particular. There is not yet any such unanimity about either the kind of society or the kind of economics that we should be striving for."<sup>60</sup> Weisskopf did not see the way forward as deciding on a shared project. Instead, URPE and the *Review* should remain as forums for diverse approaches, this was for him the organization's strength.

Weisskopf did not demand a full rejection of the new editorial line. In fact, he showed willingness to accept the change if this occurred in what he termed as an "incremental and experimental basis." He proposed that a one-off special issue be prepared following the new editorial line or that a section be created in the *RRPE* with one or two papers appearing per issue and following the new Marxist line. Only with such examples did Weisskopf believe it would be possible for URPEers to make an informed decision on a new editorial policy.

The editorial statement facing widespread criticism was subject to discussion and was finally retracted at the 1974 Summer Conference.<sup>61</sup> A "Note on Editorial Policy" was published in the subsequent *RRPE* issue, in which the board noted "a great deal of criticism, directed both to the process whereby it was issued and the content of the statement".<sup>62</sup> The Board with its withdrawal of the statement issued a *mea culpa*, recognising that it was not in its authority to dictate policy and committing itself to including the full membership of URPE in future editorial decisions. Still, the Board reasserted its belief that the move towards the "*Marrist* approach" was the right direction to be taken.

No further attempts were made to institute Marxism as the theoretical core of the *Review*. Even the idea of creating a special section in the journal for doctrinal debate, to compare Marxism with other approaches, was never pursued. A section

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<sup>60</sup>Idem.

<sup>61</sup>Walsh (1978), p. 37.

<sup>62</sup>Editorial Board (1974b).



entitled “Theoretical Notes and Comments” continued from earlier issues but it gave voice to controversies independent from this episode.<sup>63</sup>

The adoption of Marxism in the “Revised Statement of Editorial Policy” may have been contentious and short lived, but the editorial board’s stated goal of making the *Review* a scholarly journal was never questioned. On this issue there was consensus among the URPE membership. In the early 1970s the status of the *Review* was yet undefined, but the pamphlet writing efforts of PEAC (explicitly) acted as a reason to root the *Review* as an outlet for technical and academic research.

I have wanted to stress the move towards Marxism as the most significant aspect of the *RRPE* debate. Contemporaneous to PEAC efforts in connecting radicals with the social movements the *Review* was aiming for Marxist analysis; both moves reflect the impact on radical economists of the changing political landscape of the American Left. The resistance mounted to changes to the *RRPE* attest that the URPE membership was unwilling to define itself wholly in Marxism-Leninism. Many URPEers remained indifferent to such left-wing party developments, disconnected from the revolutionary political groups. Many URPEers continued to see their professional work as their primary contribution to changing American society. Many URPEers (and often the same) refused to identify Radical Political Economics with Marxist scholarship.

### 4.3 Conflicting interests and identities in URPE (1971-1978)

Despite URPE’s achievements in outreach not all was shared commitment and pleasant cooperation, as the *Review* episode already demonstrated. That URPE was home to a diverse set of personalities and political persuasions was never disputed

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<sup>63</sup>Davis and England (1975) soon after the 1974 debate wrote a polemic for the “Theoretical Notes and Comments” section. This was however wholly unrelated to the Marxism debate, it was an attack on Tom Riddell’s (1973) proposal of having radicals participate in party political work as advisers to Democratic and Republican politicians. Davis and England reaffirmed the PEA line that the only correct strategy was alongside the mass/popular movements. The section was later renamed to “Notes and Comments” in the Winter of 1975, Summer and Fall 1976 issues and from Winter 1977 “Short notes and Comments”.

and throughout the 1970s the Union remained ecumenical in its approach to politics and economic theory. However, this is not to say that diversity was the starting point. In fact, the recognition (if not even celebration) of distinct radical groupings was a later outcome, brought by internecine battles in the mid-1970s which threatened to break URPE apart (as happened to many left groups of the time). Before URPE's move towards outreach, diversity of opinion had not threatened the project of a radical economics identity, forged in reference to the elite of the economics profession. It was in the course of the outreach activities that diversity morphed into consciously organized sub-groups with conflicting and competing social identities, and the single radical economics identity became a chimera. In this section I turn to identifying what the competing identities were, narrating the episodes of conflict and their ultimate resolution.

#### 4.3.1 Women's Liberation in URPE

In the history of women's political movements it is standard wisdom to distinguish between two waves, the first located in the late nineteenth century and beginning of the twentieth century referring to the suffragettes, the second beginning in the 1960s and often labelled the women's liberation movement, the root of current feminist trends. Women's liberation had its genesis in the civil rights movement (in SNCC) and in the student New Left (in SDS). Participation in these movements led women to "an ideology of equality and democracy and a politics characterized by the absence of hierarchy and authoritarian leadership and by community".<sup>64</sup> With a changed sense of self, developed in political activity, they confronted their male comrades when they tried to impose on them traditional gender patterns.

In August 1967 SDS organized a conference in Chicago – the National Conference for New Politics. Its goal was to develop a unified left program and to nominate for the 1968 elections a presidential ticket headed by Reverend Martin Luther King, Jr. and Doctor Benjamin Spock. At the meeting women "devised a resolution requiring that women, who represent 51 percent of the population, receive 51 percent of the

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<sup>64</sup>Breines (1979), p. 498.

convention votes and committee representation”.<sup>65</sup> However, the resolutions committee of SDS refused to introduce the statement. Angered by the men’s response, women decided to meet on their own and concluded that no further efforts should be made at effecting change to SDS, they called for separatism. At their first meeting’s manifesto, they wrote: “We hope our words and actions will help make women more aware and organized in their own movement through which a concept of free womanhood will emerge.”<sup>66</sup>

Following the Autumn of 1967, “women’s groups rapidly emerged in many places, following the social networks and the ‘radical community’ created by New Left activism”.<sup>67</sup> On the basis of the women’s groups created from SDS, women’s liberation organizations began to emerge in 1968:

In January 1968, the New York group Radical Women staged “The Burial of Traditional Womanhood” at an antiwar demonstration in Washington, D.C. In March 1968, a national gathering of radical women occurred in Sandy Springs, Maryland, to discuss goals and priorities. The next month brought a protest of the Miss America contest; massive media coverage created the perception and to some extent the reality of a national movement. In October, the Women’s International Terrorist Conspiracy from Hell (WITCH) hexed the New York Stock Exchange. In November 1968, the first truly national women’s liberation conference was held in Chicago, as women gathered from thirty-seven states to discuss issues in the emerging movement.<sup>68</sup>

Similar to the pattern set at SDS, SNCC and a number of other organizations, political and professional, a Women’s Caucus was created in URPE.<sup>69</sup> It emerged very early on in URPE’s history, in the aftermath of the 1969 AEA Convention: “After the URPE sponsored panel on “The Political Economy of Women’s Liberation” at the New York City convention, a Women’s Caucus was formed which will be open to all women regardless of whether they are URPE members. The purpose of this

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<sup>65</sup> Evans (1980), p. 198.

<sup>66</sup> Ibid., p. 199.

<sup>67</sup> Buechler (1990), p. 33.

<sup>68</sup> Ibid., pp. 70-71.

<sup>69</sup> From interview evidence it seems that the example for the creation of the caucus was taken from similar moves by professional radicals, namely in the Modern Languages Association and not by Women Liberation movements per se. (Interview with Laurie Nisonoff, 2003).

organization is ... to focus on the problems of women.”<sup>70</sup> At its core were a group of radical graduate students from Yale. The Women’s Caucus’ goals were to join women who were isolated from others of their sex and to establish ties with other women’s liberation groups. At the professional level, the caucus sought to target discrimination in employment, by publicising incidents of discrimination and confronting their perpetrators and the AEA. At the academic level it aimed to promote research on the economic and social position of women and to assemble a bibliography on the subject.

Any woman participating in URPE activities could join the caucus: she need not be an economist, activists interested in radical economics or partners of male URPErs were welcomed. A report by Peggy Howard on one of the Caucus’ early meetings in the summer of 1970 testifies to the group’s diverse composition.<sup>71</sup> Such diverse backgrounds meant for Howard that unlike the men, the women’s group did not have a set of common interests and commitments. “Some were members of URPE, and were concerned with discrimination both in URPE and within the economics profession. Others of us resented any obligation to become involved in what was our husband’s or boyfriend’s organization, not ours.”<sup>72</sup>

It was as a weakly bound group that the Women’s Caucus met in the summer of 1971, at Camp Muffy. At the URPE’s Business meeting, a group of women economists decided to demand that the steering committee include two members of the Women’s Caucus, and that more women be added to the editorial board of the *Review of Radical Political Economics* (then there was only one). They also wanted “family” membership to be recognised in URPE (“family” defined broadly as including people living together) which would give voting rights to family members (nearly all female).

The latter proposal was contentious to those that at the time saw URPE as an eminently professional organization - a forum to discuss and develop radical economics.<sup>73</sup> For some men it made no sense to have non-economists as URPE members. The Women’s Caucus saw it differently; they argued that non-economists

<sup>70</sup>Weisskoff (1970). “The Political Economy of Women’s Liberation” was the title of a seminal article written by Margaret Benston for the *Monthly Review* (Benston (1969)).

<sup>71</sup>Howard (1970).

<sup>72</sup>Ibid, p. 4.

<sup>73</sup>Interview with Arthur MacEwan, 2003.

(mostly women) were already an active part of URPE's organizational life (not least as members of the Caucus) and deserved full recognition. Thus the summer 1971 controversy may be seen as the first sign, preceding the outreach efforts, of URPE's move away from a professional based group towards a more open, less academically defined, organization.

The proposed changes to the *RRPE* editorial board faced opposition by the men and a conflict ensued. As retold in the *Newsletter*:

During this debate, a male URPE member made a remark which was interpreted by some women as implying that women were only qualified to speak on women's issues. This incident, which seemed to us symbolic of the treatment of women in URPE, sparked a major encounter. The women decided to caucus, and left the room.<sup>74</sup>

As some of the women economists left the meeting they were (unexpectedly) followed in mass by all the other women present. The leading group of women had intended to discuss a plan of action given the men's response to their proposals, but as all the other women followed them, their exit took also the symbolic meaning of a protest, and a show of solidarity among such a diverse group.<sup>75</sup>

On their return to the meeting, the women announced that the three women nominated for the board would stand as individuals and that they would accept a voting procedure agreed on by the plenary. They further demanded that a committee be set up to determine how voting would take place in the future, taking into consideration the question of regional and sexual representation on committees.<sup>76</sup> Before voting the new editorial board, the women's group proposed that the meeting begin with a session of criticism and self-criticism over the nature of the relationships between men and women in URPE.

Criticism-self-criticism sessions were a staple of radical left movements in the 1960s and 1970s. Based on practices pioneered by the Bolshevik party, mutual criticism sessions were developed by the Chinese Communist Party over the decades

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<sup>74</sup>Horowitz *et al* (1971), p. 3.

<sup>75</sup>Interview with Laurie Nisonoff, 2003.

<sup>76</sup>Horowitz *et al* (1971), p. 3.

of its struggle to power and “represented an attempt either to pre-empt or co-opt the autonomous primary groups which would ordinarily exist in various organizations and throughout society”.<sup>77</sup> The sessions comprised of each person analysing his thoughts and actions comparing them critically with the ideal standard sought for. One was expected not only to criticise ones’ own shortcomings but also to accept criticisms from the group and in turn point to the failings of others.<sup>78</sup>

In the criticism and self-criticism session, over two and a half hours, the women accused URPE men of male chauvinist behaviour. In the *Newsletter’s* report of the events, the authors highlighted “the arguments” raised by men to dismiss the women’s criticisms.<sup>79</sup> According to the report men were impatient with the time spent on a matter they portrayed as relevant only at the level of personal relationships, between them and the women they were involved with.<sup>80</sup> Men argued that they had joined URPE “to discuss radical economics and not to participate in an encounter group.” Women, on the other hand, felt that the male view of politics was restrictive and echoing women liberation’s views argued that personal relationships were political and that radical economics should not allow itself to reproduce oppression within its principal organization.

For women radical change could only occur through collective examination coupled with women’s separatism.<sup>81</sup> According to them, men were paternalistic and domineering, women needed to work together to become independent and self-confident of their abilities. Without husbands and men friends women argued it would be easier to discuss their personal politics.

As a conclusion, the report of the meeting proclaimed that:

URPE is a male-dominated organization and is oppressive to women.

Moreover it has shown very little remorse at being so. We hope that the need for change in the organization is now seen. We will continue to

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<sup>77</sup>Whyte (1974), p. 10.

<sup>78</sup>Ibid., p. 3.

<sup>79</sup>Horowitz *et al* (1971), pp. 4-5.

<sup>80</sup>Another view held by some men was that oppression inside the organization was better characterised as elitism and was forced upon new comers and those from less prestigious backgrounds, which women argued was a wholly different issue (Horowitz *et al.* (1971)).

<sup>81</sup>Ibid., pp. 5-6.

struggle within URPE on the assumption that there are men willing to struggle with us against sexism in URPE. However, unless progress is made within the organization as a whole, some of us may be forced to consider leaving URPE.<sup>82</sup>

The women's reporting of the men's views seems to have been an accurate description of the divisions emerging within URPE. In the same *Newsletter*, alongside the report, was a comment by Howard Wachtel. The author began by noting that URPEers carried a baggage of hostilities, antagonisms, and contradictions thrust upon them by society. While formalism and hierarchy were imposed by the lecture form in Universities and by the AEA, sexism but also racism and elitism were imposed by society generally. He added that these "contradictions," and in particular sexism, manifested themselves in URPE but were not as acute as outside, in larger society.<sup>83</sup> Thus, for Wachtel the issue was not as central and severe as the women had argued.

Despite downplaying the severity of the issue, Wachtel expected future disruptions. He feared that radical organizations were internally weak and could not withstand intense struggle. To prevent the self-destruction of the organisation Wachtel proposed creating "new forms of organizational structure." He suggested that more activities should occur at URPE's regional level, these becoming decisional units with their own general meetings and that the Women's Caucus might constitute itself as a region, if women so wished. He further proposed the creation of a Scheduling Committee, with regional and women's representation, to assist the organization of the meetings at the summer conference. Wachtel's stress in 1971 was on transforming URPE's institutions, arguing that "If the movement does not start dealing with problems of internal structure and only with problems of consciousness and personal liberation, its rate of disintegration will accelerate".<sup>84</sup>

Wachtel's article echoed the portrayal of the men's position provided in the report of the 1971 conference.<sup>85</sup> While women found personal examination (and confrontation) coupled with institutional separatism essential for tackling sexist

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<sup>82</sup>Ibid, p. 6.

<sup>83</sup>Wachtel (1971), p. 7.

<sup>84</sup>Ibid, p. 8.

<sup>85</sup>Horowitz et al (1971).

behavior, men favoured a solely institutional response, ranking the women's caucus as a "region." The silence concerning personal examination in Wachtel's reflections suggests that this proposal in particular was difficult to bear for male URPEs.<sup>86</sup>

The women's criticisms in 1971 was source of discomfort in URPE. Some men responded with hostility, Lawrence Tharp for instance noted his disagreement with the women's solution to sexism, since for him it simply reversed oppression, transferring all the power to women.<sup>87</sup> Tharp accused women of dominating and oppressive behaviour at the conference when they restricted men in their speaking and when the Women's Caucus announced unilaterally changes to the conference program: "Undemocratic actions of this sort are oppressive, whether they come from authoritarian cliques in our workplaces or from women at URPE conferences, and are not consistent with maintaining a non-antagonistic relationship between radical men and women."<sup>88</sup>

The Women's Caucus was broadly successful in obtaining its demands. Alongside the regions, the Women's Caucus elected two representatives to the Steering Committee. Furthermore, several regions began electing one male and one female as their representatives. Four women were elected to the 1971-72, twenty strong, editorial board: Heidi Cochran, Grace Horowitz, Peggy Howard and, Lourdes Surkin.<sup>89</sup>

The Women's Caucus became an active source of activity, organizing several conferences on the political economy of women and in December 1971 targeting the AEA for its gender discrimination, assisting the creation of the Committee on the Status of Women in the Economics Profession (CSWEP)<sup>90</sup>. At the *Review for Radical Political Economics*, one of the initial targets of the caucus' criticism, by 1973-74 there were nine women on the editorial board. In 1974 and until 1978 women took on

<sup>86</sup>I found no evidence of further sessions of criticism/self-criticism over sexism following the 1971 meeting (Private Communication Laurie Nisonoff, 8 June 2005).

<sup>87</sup>Tharp (1971), p. 10.

<sup>88</sup>Ibid. pp. 12-13.

<sup>89</sup>The next academic year, 1972-73, there were seven women at the editorial board: Grace Horowitz, Amy Bridges, Heidi Cochran, Collette Moser, Ann Davis, Laurie Nisonoff and Nancy Hancock.

<sup>90</sup>It was also thanks to the network established by the Women's Caucus that the Women's Work Project was able to function with the success already noted in this chapter, see p. 120



the role of editorial board coordinator, first Lourdes (Surkin) Beneria and from 1976, Constance Blake. Also worthy of note were the special issues put together by women collectives: in July 1972 "The Political Economy of Women" included articles, photos, poems and book reviews; in the Spring of 1976 an issue in two parts, the first on "Women in Changing Societies" and the other of a historical subject, "Women and the Great Depression"; and in the Fall of 1977 an issue on "Women, Class and Family."

My narrative of the outreach activities was circumscribed to the mid-1970s, I have returned to 1971 to discuss the history of the Women's Caucus as the first in a series of similar developments that fissured the radical economics' Union. What I want to highlight is how the radical economics identity was becoming increasingly secondary to divisions emerging among radicals. URPE women were both radical economists and women and they saw themselves separate from the men that were radical economists and men. Even URPE women were not a cohesive unit and their caucus too sectioned over controversies raging within the women's political movement.

The parallel between the women's activities in URPE and the general trends in Women's Liberation should not be surprising. Women URPEers were an integral part of that movement and actively participated in some of its developments. In the women's movement a divide developed between what was labeled socialist feminism and radical feminism:

Socialist feminists sought a synthesis of leftist analyses sensitive to class and race issues, with feminist analyses oriented to gender. Radical feminists asserted the primacy of gender as the root of all hierarchy and domination. Socialist feminists insisted on the need for including working-class women, minority women, Third World women, and an anti-imperialist stance; radical feminists reasserted the primacy of gender and rejected the lingering imprint of Marxism within socialist feminism.<sup>91</sup>

Most URPE women sided with the socialist feminist project and were active members of one of its major organizations, "Marxism-Feminism 1". "M-F 1" was formed in 1973 out of an informal network of women, many of them radical academics but

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<sup>91</sup>Buechel (1990), p. 74.

others involved principally in community organizing, health care, clerical organizing, or other kinds of work.”<sup>92</sup> The purpose of M-F 1 was to promote regular meetings to reflect upon, compare and analyse the possibility of a synthesis between the adherents’ of Marxism and Feminism, “to concretize the ‘connection between the personal and the political’.”<sup>93</sup> Over three years M-F 2, 3, 4 and 5 were created and the network (primarily in New York and Boston) reached 175 members.

In the mid to late 1970s controversy ensued between URPE women over the desirability and content of the fusion of Feminism and Marxism. The public controversy became known after Heidi Hartmann’s “The Unhappy Marriage of Marxism and Feminism” first circulated in 1975 and published in 1979.<sup>94</sup> This debate followed in parallel with the process of disintegration of URPE to which I now turn.

#### 4.3.2 Fighting over the current events newsletter - *Dollars & Sense*

The Women’s separatism pre-dated URPE’s outreach activities. The resolution of the women’s challenge in institutional reform left a lasting mark on the organization but was not source of further severe stress. The same cannot be said of the disputes that followed in the more militant (post-1972) stage of URPE. At the forefront of URPE’s outreach activities and in its new emphasis towards social movements, was the New York PEAC collective. At the conclusion of PEAC’s first (and successful) year of activity a controversy ensued over the publication of a current events newsletter. The controversy was to thrust into sharp relief an aspect of URPE’s new commitment to political involvement; that the organization had become open to the humours of left sectarian infighting.

In June 1974 PEAC members (who were based in New York)<sup>95</sup> wrote to the *Newsletter* claiming priority over the idea of creating a current events bulletin. According to David Barkin and Maarten deKadt, since January 1974, PEAC had initiated contacts with people from around the country cooperating with the

<sup>92</sup>Petchesky (1979), p. 373.

<sup>93</sup>Ibid., p. 374.

<sup>94</sup>Hartmann(1979). See Sargent (1981) for a collection of essays on the controversy.

<sup>95</sup>See section 4.4.2.

pamphlet efforts, to write articles, edit and distribute the bulletin.<sup>96</sup> PEAC also noted their open approach to the project and how they had enlisted the support of a Harvard graduate student, Bill Lazonick. In their narrative of events the authors noted that in April they were surprised by news of a similar project being conducted from Boston. They had been introduced to *Dollars & Sense (D&S)* “a monthly bulletin of economic affairs”, through a letter sent to the New York URPE chapter. The PEAC narrative dramatised the Boston initiative as secretive and of dubious intentions.<sup>97</sup> Barkin and deKadt made much of the inclusion in the *Dollars & Sense* sample issue of reprints from the New American Movement (NAM) publications and that many of the group’s members were active in NAM.<sup>98</sup>

NAM was created in 1970, envisioned “as a broad-based “mass organization” that would “overcome the errors of the New Left”<sup>99</sup> which it identified as anti-leadership, glorification of the Third World and insensitivity to the interests and needs of American workers. NAM’s unofficial manifesto, “Revolution and Democracy,” written by Frank Ackerman and Harry Boyte, distanced the group from the Bolshevik model as inappropriate for the late-twentieth century US, and endorsed a more open and democratic alternative.<sup>100</sup> By 1972, NAM had roughly 1,000 members and about thirty local chapters.<sup>101</sup> In stark contrast with the highly centralised Marxist Leninist parties, NAM’s national office was aimed at serving self-defining local chapters. “NAM saw itself primarily as providing education and political sophistication to . . . other struggles, rather than as aiming to be the leadership organization itself.”<sup>102</sup>

Barkin and deKadt accused the *D&S* group of attempting to drain URPE’s resources, citing from the *D&S* letter which identified the group as:

people who have come together through the Boston chapter of the Union for Radical Political Economics (URPE). However, we intend to rely heavily on URPE chapters around the country to provide us with articles.

<sup>96</sup>Barkin and deKadt (1974a), pp. 16-17.

<sup>97</sup>Though no one disputed the claim that the New York chapter and PEAC had only been informed of *D&S* in April, the portrayal of Lazonick’s role in the affair was disputed (Ackerman (1974), p. 8).

<sup>98</sup>Ibid, p. 17.

<sup>99</sup>Lerner (1998), p. 543.

<sup>100</sup>Elbaum (2002), p. 119.

<sup>101</sup>Ibid., p. 118.

<sup>102</sup>Lerner (1998), p. 544.

Although Dollars and Sense has no organizational connection with URPE, our association with URPE provides us with important sources of support. One URPE chapter has already committed itself to supplying one article a month. We are confident that before we begin publication in the fall we will have other similar commitments.<sup>103</sup>

From this quote, the PEAC members argued that *D&S* was willing to exploit URPE for support yet allowed for no accountability to the organization in return. It was unclear to PEAC and NY-URPE members how the bulletin project would further URPE or radical economics, not least because the close relationship between *D&S* members and NAM threatened the project with sectarian control.

The New York chapter proposed that to solve the impasse both the PEAC and *D&S* bulletin proposals be presented to the URPE Steering Committee for consideration and judgment. However, at the time (April) *D&S* refused to comply, maintaining that the bulletin should remain independent from URPE, and that they did not require its input. The Steering Committee once informed of the problem decided that two members from the Steering Committee, from *D&S* and from PEAC, should meet in May so that a cooperative working relation may be established and maintained.<sup>104</sup>

The May 1974 reconciliation meeting proved inconclusive:

All agreed that the bulletin should be designed to reach movement groups, workers, students, and others who might be interested in a regular analysis of events from a "radical" perspective. Both groups are interested in seeing such a publication emerge relatively soon. The discussions were friendly although no agreement was reached in spite of the clear articulation of the conflicting views.<sup>105</sup>

While PEAC wanted writing and research to be regionally decentralised and URPE's editorial control over the publication, the *D&S* group wanted a centralised, autonomous and independent group to run the bulletin. An additional source of contention was the financial base for the publication, PEAC contrasted their plans for

<sup>103</sup>Cited in Barkin and deKadt (1974a), p. 17.

<sup>104</sup>Barkin and deKadt (1974a), p. 18.

<sup>105</sup>Ibid., p. 19.

broadly based fund-raising to *D&S*'s preference for a few large donations and loans.<sup>106</sup>

PEAC's claim to control over the burgeoning publication was that it had come out (historically) of the Food project's successes, and thus as part of the new challenges PEAC was facing. The NY-based bulletin would be committed to bringing together several groups throughout the country, and thus furthering URPE's growth, while the *D&S* group showed no such concern.<sup>107</sup> Boston's main argument for taking over the responsibility of the current news bulletin was their stress in a quality, timely and readable publication (stripped of left or academic jargon) which could only be achieved under their proposed centralised format.<sup>108</sup> Boston compared to New York was also clearly ahead in the preparation of the publication, with a concluded sample issue.

The record of the discussions suggests that the *D&S* group did not insist that the bulletin should remain independent from URPE, their primary concern was on the structure set up for the publication - centralised in Boston and not overly dependent on outside chapters. After the May meeting the *D&S* group manifested its willingness to make the publication an URPE project reversing their original stance.<sup>109</sup>

Despite the Boston group's change of heart, the run-up to the decisive summer meeting was not peaceful, the New York chapter and PEAC had used the *Newsletter* as a mouthpiece in their campaign against *D&S*. Ackerman responded on Boston's behalf, "I feel hurt, angered, outraged".<sup>110</sup> The author accused the PEAC collective of red-baiting the Boston collective as a front for NAM, when in fact only four out of fifteen people that had worked in *D&S* were members. He further argued that NAM had never behaved in a sectarian fashion in URPE and that it was "unprincipled to assume that all socialist and communist groups are alike, or to attack an individual or group by simply labelling them, without discussing any actual disagreements and

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<sup>106</sup>Ibid, p. 20.

<sup>107</sup>Ibid, p. 19.

<sup>108</sup>Ackerman and Spitz (1974).

<sup>109</sup>Ackerman and Spitz (1974). The Boston group's willingness to work under URPE's fold was communicated to a Steering Committee member in a statement for publication in the June *Newsletter*, however the text was never printed. The explanation for the loss of the manuscript is unclear, it was suggested that it might have been merely an error by the S.C. member, and even the *D&S* group was reluctant to draw from it political implications (Ackerman (1974), p. 7).

<sup>110</sup>Idem.

criticisms".<sup>111</sup> A more severe criticism was targeted at Sandy Orlow, URPE's national secretary, who had written an assessment of the debate in sympathy to the PEAC side. Orlow had depicted the debate as a conflict between distinct views of what URPE was about. He interpreted the Boston group as saying that "URPE started as, has always been, and can be nothing other than a radical professional organization, publishing radical intellectual material, organizing in academia around a radical-bourgeois ideological struggle and other job oriented issues which affect radical professional academics".<sup>112</sup> Orlow had argued that for the Boston group, PEAC were "outsiders" pressing for anti-elitism, anti-sexism, anti-professionalism, and outreach beyond academic circles, which was not truly representative of URPE and would soon die out. Orlow's rhetoric made Boston into an "old URPE," professional and intellectual-oriented, versus the "new URPE" of activism which PEAC epitomised. Ackerman found Orlow's role in the dispute a breach of the ethics demanded from a National representative. He had made no effort to contact the Boston group, to discuss and come to know their views; as a result he had grossly misrepresented the politics of *D&S*. Ackerman reassured that Boston was committed to the struggle against elitism, sexism and professionalism.<sup>113</sup>

The summer 1974 meeting brought a reaffirmation and extension of PEAC's role (becoming PEA) but also the membership's endorsement of the *Dollars & Sense* project. The current events newsletter was to be an URPE project, but along the lines defined in the Boston proposal, centralised and largely autonomous from other URPE structures. Commenting on the 1974 Summer Conference, Nan Wiegiersma, an active member in the Women's Work Project, saw the debates as a battle between "groups of people associated with three political parties (or pre-parties) . . . Progressive Labour Party, Revolutionary Union and New American Movement",<sup>114</sup> while a fourth identifiable political group were the women. In Wiegiersma's assessment a compromise

<sup>111</sup>Ibid., p. 8.

<sup>112</sup>Orlow (1974), p. 21.

<sup>113</sup>Ackerman (1974), p. 9.

<sup>114</sup>Wiegiersma (1974), p. 27. The mid 1970s were a period of a flurry of activity by Marxist Leninist groups to create new communist parties, while building support and discussing political line. In preparation for that goal these groups took on the label of "pre-party formations", see Elbaum (2002).

had been made between the Boston NAM and the New York Revolutionary Union (RU), the latter group being one of the major Maoist organizations of the early 1970s.<sup>115</sup> What distinguished the Maoist movement ideologically was advocacy of violent revolution and the dictatorship of the proletariat; rejection of the Soviet and U.S. Communist parties for alleged “revisionism” (discarding the revolutionary principles of Marxism-Leninism); defence of Stalin as a great revolutionary and belief that after Khrushchev’s criticism of Stalin in 1956 the Soviet Union had moved toward restoration of capitalism and became an antirevolutionary force. Domestically, the Maoist current saw the key to the U.S. revolution as forging an alliance between the “multinational working class” and the “oppressed nationalities.”<sup>116</sup> In the light of Wiegiersma’s comments, the *D&S* dispute was a battle of organized political groups; on the one hand were the Boston-based democratic socialists of NAM, on the other the New York based “new communists” of RU.

In retrospect the current-events newsletter dispute was quickly dealt with, once the Boston collective was willingly brought into the URPE fold. What was significant was a new outlook for URPE as a stage for the maneuvering of political parties. In the new matrix for understanding disputes and for reasoning over subjects, there was no single, united radical group but a tense collection of political interests (originating in outside groups) pursuing their own agendas. URPE members were not solely radical economists set on a theoretical redefinition of the science. They had become in the early 1970s also feminists, Maoists, democratic socialists, and Marxists of various persuasions. For many URPE was not their first home, while it may have defined them as economists it could not define them as political activists. Their commitment to one of the revolutionary movements and parties of the left often demanded a contest with other left groups over strategy, over resources, which resulted in doing battle with radical colleagues.

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<sup>115</sup>Alongside the October League (OL) (as RU, also made up of former SDS members). RU was founded as the Bay Area Revolutionary Union in 1968, grew into a nationwide organization in 1970-71, and in 1975 declared itself the Revolutionary Communist Party (RCP) (Elbaum (1998), p. 473).

<sup>116</sup>Idem.

### 4.3.3 “Whither URPE?” Yellow Springs, August 1975

Despite the successful resolution to the *D&S* controversy at the Summer 1974 conference, tensions in URPE between political groups did not subside. Notably in the New York chapter in the early months of 1975 a confrontation erupted between RU members and other left groupings. The turmoil was all the more serious since the URPE National Office had moved to New York that year and was caught in the crossfire.<sup>117</sup>

URPE’s troubles paralleled growing infighting in the American left. The most detailed historical account of the left-wing groups of this period, *Revolution in the Air*, notes that in contrast to 1973 when new communist groups met under the auspices of the American magazine *Guardian*,<sup>118</sup> sectarian strife became increasingly intense from 1974 onwards, particularly between RU and other groups.<sup>119</sup> From the host of reasons behind the sectarian moment, historian Max Elbaum highlights the belief in Marxist Leninism as a “universal” theory of revolution, as final and unquestionable truth. It was assumed that ideological purity was essential for political efficacy, “Mao declared that ‘the correctness or incorrectness of the ideological and political line decides everything,’ and this dictum was quoted endlessly.”<sup>120</sup> More contextual reasons have also been offered by Elbaum:

“Conditions outside the movement also facilitated this retreat into purist orthodoxies. By 1974-75 the popular movements, which had surged in 1970-73, had ebbed like their late-sixties predecessors, leaving the entire revolutionary left adrift.”<sup>121</sup>

One of the protagonists at the New York troubles commented: “developments in various ‘movement’ groups and coalitions was turning URPE into a sectarian battleground, totally divorced from questions of political economy, and was in fact bringing on ‘narrow sectarian wrecking’ of URPE”.<sup>122</sup> At its April meeting the New

<sup>117</sup> At the 1975 Summer Conference Michael Zweig, Batya Weinbaum and Jeff Keefe (from PEA) were castigated for their behaviour, “disruptive of the SC and irresponsible in his treatment of the National Secretary since the NO move to NYC” (*Newsletter* (1975c), p. 25).

<sup>118</sup> Elbaum (2002), pp. 108-109.

<sup>119</sup> *Ibid.*, pp. 186-187.

<sup>120</sup> *Ibid.*, p. 157.

<sup>121</sup> *Ibid.*, p. 195.

<sup>122</sup> Zweig (1975), p. 22.



York chapter decided to abandon the project of turning URPE into “some kind of general mass ‘movement’ organization” and to re-assert its role as a forum to debate the workings of monopoly capitalism and socialism, by the “broadest number of people actively engaged in studying, writing about and teaching about the economy”.<sup>123</sup> Zweig, formerly an enthusiast of movement organizing, was arguing for a renewed emphasis on intellectual tasks, the mission of URPE being the “development of the most scientific analysis of political economy.”<sup>124</sup>

Zweig maintained his earlier suspicion of professionalism and academia. According to him, URPE’s constituency may have been largely academic but it was not exclusively so, and it was much wider than those they organized, i.e. the parties and movements they were involved with: “URPE members have an ‘audience’ which is broader than those we organize. That is why we correctly publish D&S, arrange speakers and forums on the economy in the community and with unions, and support PEA publications.”<sup>125</sup>

The 1975 April and June *Newsletters* opened a novel front for conflict. A group of URPE members had coalesced around the Chicago URPE chapter with the agenda of furthering activism against academism.<sup>126</sup> Signing their critique as Just Folks,<sup>127</sup> they denounced a “structural antagonism between the natural leadership of radical economics and the duly elected membership”. The authors argued for the existence of two groups within URPE: there were some of the organization’s founders who sought an “alternative AEA”, “they turned to the educated left to find the audience that would validate their academic achievements and would hence provide them with the respect and recognition bourgeois circles were denying them”;<sup>128</sup> and then there were those that tried “to make URPE a place where radical political economics, as a tool

<sup>123</sup>Ibid., p. 23.

<sup>124</sup>Ibid., p. 24.

<sup>125</sup>Ibid., p. 23.

<sup>126</sup>Private Communication Joseph Persky, June 9 2005.

<sup>127</sup>The Just Folks were Jose Alberro (Chicago), Mitchell Berlin (Chicago; Regional PEAC), Herminio Blanco (Chicago), Judy Brody (Women’s Caucus), Amy Bridges (Chicago), Bill Cartmill (Chicago; Regional PEAC), Ed Dessau (Chicago), Jim Hill (Chicago), Lori Helmbold (San Jose; Regional PEAC), Jim Hill (Chicago), Jo Ann Kawell (Chicago), David Landes (San Jose), Joe Persky (Chicago), Jo Anne Preston (Cambridge, Mass.), Gayle Southworth (San Jose; West Coast Organizer) and Al Weinrub (San Jose).

<sup>128</sup>Just Folks (1975), p. 25.

of social change, is learned and developed.”<sup>129</sup>

The shortcomings of URPE were due, according to the Mid-West critics, to its organizational structure which permitted elitism and sexism to permeate the Union, as seen in the emergence of “levels of stardom among our faculty members, and a system in which it is always the same clique that gets exposure in the bourgeois media”.<sup>130</sup> The Mid-West members called for the creation of a “Small Potatoes Caucus” and nomination of spokespersons to fight the influence of the radical superstars. In a later proposal, they called for a regionalised PEA as the core structure of URPE: “It is therefore of the utmost importance that PEA become what it was intended to be originally: an instrument to develop Political Education and Action.”<sup>131</sup> The group wanted more detailed and more widely discussed budgets for PEA and generally more money for the Mid-West, West and South as it was argued that all the funding was being diverted to the East.

A group of URPE members including many of those considered the radical superstars, responded to the Mid-West critics.<sup>132</sup> The group hinted that the accusations over “the ways in which older, more experienced and more established members have been speaking and writing about the economy, fearing that they would be taken as “designated representatives” of the organization”<sup>133</sup> was part of the growing stress inside the Union as political organizations and parties competed to influence it. The group argued that the insinuation that the East was exploiting URPE’s financial resources was absurd, in fact it merely followed from the location of the National Office in New York.<sup>134</sup> For the authors’ the Mid-West challenge was part of a divisive trend that menaced URPE and did not reflect a correct

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<sup>129</sup>Ibid., p. 25.

<sup>130</sup>Ibid. p. 26. The claim that a select number of radicals had been singled out by outsiders as leaders was not contested. Notably, Paul Samuelson’s Op Ed at the *New York Times* in March 1973 referred to Bowles and Sweezy as the heads of radical economics, characteristically conflated with the Marxist movement; and in April 1975, David Gordon was invited by the *New York Times Magazine* to write a large opinion piece on the radical perspective on economic recession.

<sup>131</sup>Alberro *et al.* (1975), p. 16.

<sup>132</sup>The authors of the text were Lourdes Beneria (then coordinator of the *RRPE* ed. board), Sam Bowles, Rick Edwards, David Gordon, Bill Tabb, Mike Reich, John Willoughly, Shaun Hargreaves-Heap, Doug Dowd, and Larry Kahn.

<sup>133</sup>Beneria *et al.* (1975), p. 7.

<sup>134</sup>Ibid, p. 8.

understanding of the organization's problems.

Arguing against the "Small potatoes" proposals, this group felt that investing more resources into PEA would be a mistake, "In our view it would create greater schism to bypass the elected Steering Committee by establishing four paid PEA coordinators", it was all the more so since "an unhealthy division has developed between PEA and URPE".<sup>135</sup> There was a noted threat that PEA may develop into a second URPE. This group, in contrast to the Mid-West critics, did not feel that a reshuffling of organizational priorities was required, or even that some tasks, namely connecting with outside movement groups, should be singled out as more important than others. For "all URPE activities are essential – building the RRPE and PEA, teaching radical political economy within departments of Economics and outside of them, organizing counter-conventions at the AEA meetings and conferences elsewhere, writing and disseminating URPE's work as much as possible and at all levels".<sup>136</sup>

That URPE was in the midst of a crisis was widely recognised. The August 1975 issue of the *Newsletter* was a voluminous collection of articles on the open wounds of the previous months, aptly titled "Whither URPE?" (see Figure 4.1). The Yellow Springs meeting was perceived as the battlefield where the diverse groups with their opposing projects would clash.<sup>137</sup> The result would decide the fate of URPE for years to come.

Three groups were identified in the report of the 1975 Summer Conference, corresponding to the fault lines developing in the previous months.<sup>138</sup> The "small potatoes" group charged the Union of elitism and individualism, and demanded the allocation of more resources to the regions and to activism. The group that had responded to "small potatoes" criticisms in the previous months took on for the

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<sup>135</sup>Ibid., p. 9.

<sup>136</sup>Ibid., p. 7.

<sup>137</sup>The 1975 meeting was the place for all battles, even among the Women Caucus conflict emerged between the Marxists and the Feminists. White (1975), p. 14, claiming that feminist political economy was a negation of URPE:

There is no separate field called 'feminist political economy' because there is no objective basis for it in the political economy of monopoly capitalism. To eliminate the oppression of women requires the elimination of monopoly capitalism - a very long process of class struggle which is not led either by women alone or men alone, but by the working class.

<sup>138</sup>Meyer and Gordon (1975), pp. 5-6.

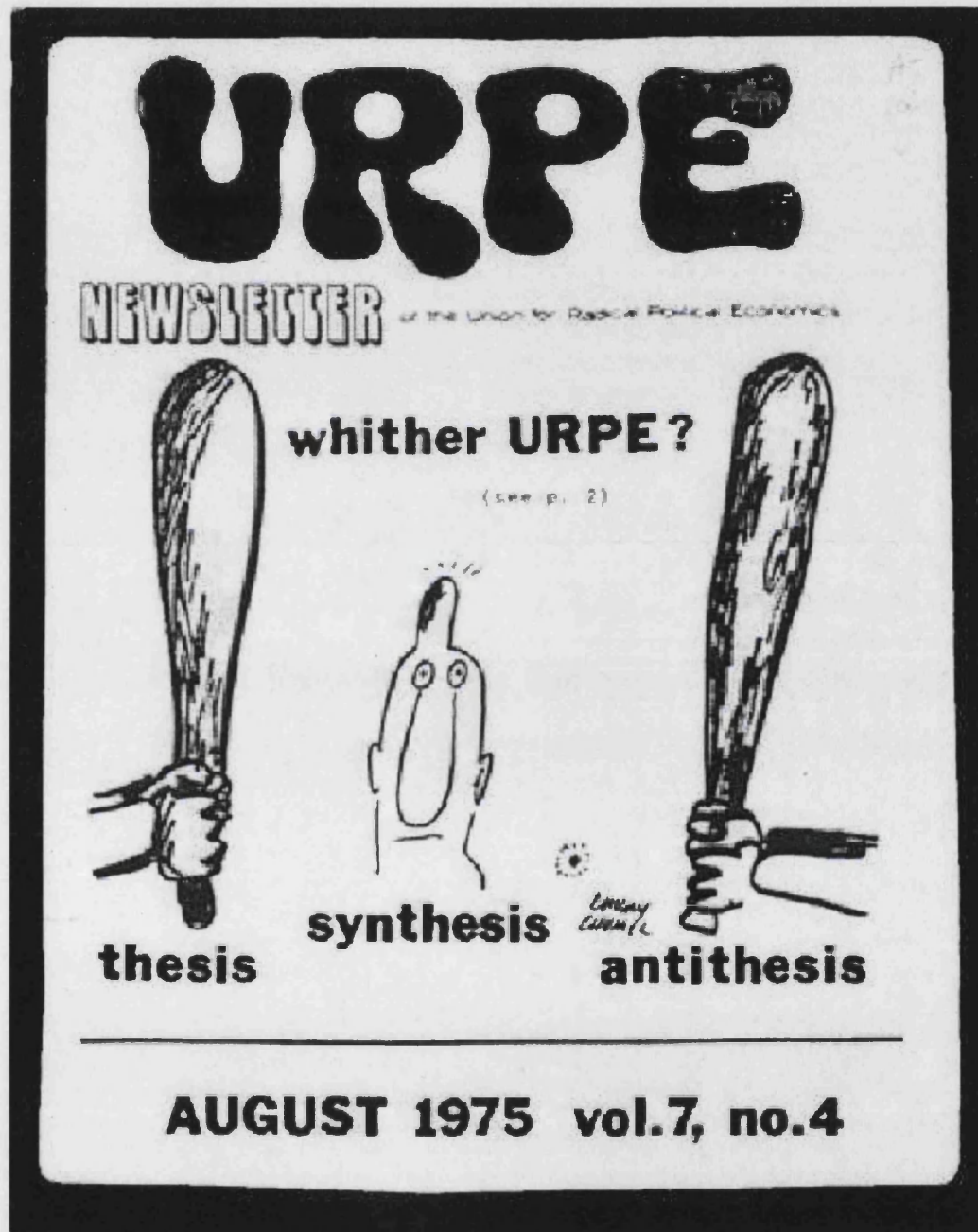


Figure 4.1: Playful dialectic cover of *Newsletter of the Union for Radical Political Economics*, August 1975, containing articles debating the failings of URPE.

meeting the label of “eggplants”<sup>139</sup> and its proclaimed emphasis was to diffuse divisions within the organization. A third group from the New York chapter presented a statement “Struggle for Political Economy” that reaffirmed URPE’s main purpose as that of “doing political economy”, it further argued for abolishing the separate PEA structure and for the creation of a Popular Political Economy Pamphlets series.

The overall outcome of the meeting was a defeat for “small potatoes.” Although they obtained a commitment to prioritise activities by Mid-Atlantic, Mid-West South and Far West chapters, the crucial demand to regionalise and strengthen PEA lost to the New York group’s proposal to abolish the structure. Though deemed a desirable end, agreement on how to push for more “team speaking” and for nomination of “spokespersons” was thin, and nothing in concrete was legislated.

Following the instigation of the New York group, a Popular Political Economy Series was created to replace PEA. Its “Representative Review Board” was not responsible for planning or soliciting members to write. Its role was solely that of judging the materials’ relevance and quality and to decide whether they would carry the URPE seal, and if they were to be funded and/or distributed by URPE. The organizational structure of the new outreach initiative Popular Political Education Editorial Board (PPEEB) was a break from the PEA past. The PPEEB had five members, a steering committee member appointed yearly, and four elected at large at the Summer Conference. Unlike the PEA that was run primarily by a non-elected and independent collective, directed from New York, the PPEEB was fused to the elected steering committee and was made accountable to the Summer Conference. It was thus designed as a check on party political contamination of the pamphlet efforts immunizing URPE against sectarian strife.

The “eggplant” resolutions were adopted (with minor amendments) by the Summer meeting as the standing statement of URPE’s organizational mission, and were used as the basis for a new membership brochure.<sup>140</sup> The adopted resolutions

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<sup>139</sup> “Eggplants” was an allusion to a somewhat popular song by Dr. West’s Medicine Show and Junk Band, written by Norman Greenbaum, “The Eggplant that Ate Chicago” (Personal Communication Joseph Persky, June 9 2005).

<sup>140</sup> Meyer and Gordon (1975), p. 7.

began:

URPE is an association of people devoted to the study, development, and application of radical political economics as a tool for building socialism in the United States. It functions as an umbrella organization, providing a forum and focus for people with many different political inclinations who share that common dedication.<sup>141</sup>

URPE's three major headings of activity were (in the order in which they were named): to encourage the development of Radical Political Economics, namely through the *RRPE*; to "strengthen our primary bases of activity in the universities and other educational institutions" and to "work with many progressive groups in the United States, providing supportive political economic analyses".<sup>142</sup> Popular writing was reaffirmed, "URPE will attempt to develop new activities to make it more responsive to movement, local, and rank and file needs, building upon the lessons",<sup>143</sup> but this was now far removed from being the prime concern.

The solution to the growing disruption inside the organization held by "struggle" and "eggplants" was to contain independent activism and to reaffirm the organization's focus on intellectual and academic tasks; "small potatoes" were left alone in demanding a more directive and muscular organization with a political agenda. Significantly, this was not a mere return to the more intellectual tasks of the past. The 1975 summer conference defined URPE as a grouping of different people, "an umbrella organization" or a "forum." This was to deny that such a thing as a radical political economist existed or that it could come to exist in the future. Under this new outlook URPEers were Marxists, Feminists, Radicals of different kinds united by debate and intellectual partnership.

#### 4.3.4 URPE as an "umbrella organization"

The 1975 meeting represented a major break in the history of URPE. The project of joining radical academics with activism that had replaced the earlier project of a

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<sup>141</sup> *Newsletter* (1975a), p. 10.

<sup>142</sup> *Idem.*

<sup>143</sup> *Idem.*

radical paradigm, was dropped with no substitute in sight. The following years of the organization were a testimony to this loss of direction, inherently also a loss of identity.

As I have noted, PEA was replaced in the URPE Summer Conference of 1975 by the Popular Political Education Editorial Board (PPEEB). Its main function was to publish and distribute the Popular Political Economy Series, and in 1975 it published a reader on economic crisis entitled *Radical Perspectives on the Economic Crisis of Monopoly Capitalism*, initiated and all but concluded under PEA, of which ten thousand copies were distributed in the first year. However, no further projects were initiated or concluded beyond the distribution of the *Crisis* volume. Even the *Newsletter* came into troubles with delays in publishing that year, and only the *Review* continued publication with no noticeable difficulties.

The 1976 Summer Conference was a world apart from the meeting of the previous year. As remarked in the *Newsletter* report: "The mood of the conference can be summed up by saying that it was a very mellow experience." The business meeting "ran in a relaxed, low-key manner".<sup>144</sup> The reasons for the new mood were put down as "people have learned from experience that certain political issues are divisive and for the strength of the organization are best left outside of URPE's politics".<sup>145</sup> In contrast with the 150 attendance of the previous year there were under 90 participants at the 1976 Summer Conference, which sparked a discussion on the "lack of energy" in the organization.<sup>146</sup> It was remarked that older URPE members were no longer taking active roles and the younger members were not getting the "high" that the first generation got from founding URPE.<sup>147</sup>

Facing the failures of the PPEEB, that had not yet properly convened, the Women's Work Project, argued and achieved in the 1976 Summer Conference its independence. In 1977 the Women's Work pamphlets were reprinted and updated, with a new addition in 1978 of *Women Organizing the Office*. The new pamphlet

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<sup>144</sup>Carlson (1976), p. 4.

<sup>145</sup>Ibid., p. 5.

<sup>146</sup>A similar pattern of growing disengagement was identified by the report of the RRPE, ASSA meeting and regional organizing (Carlson (1976)).

<sup>147</sup>Carlson (1976), p. 5.

with over 70 pages was larger than the earlier ones, with three sections: "On the job" collecting testimonies of women's working conditions in clerical jobs; "Our heritage: from Office Clerk to Automated Office" a brief labour history of women,<sup>148</sup> and lastly "Organizing Today", setting out strategies and institutions for women to organize. The Women's Work Project was the only outreach activity that continued into the late 1970s with recognised success, printing 18,500 copies and selling over 13,000.<sup>149</sup> But in 1979, the Women's Work Project decided to discontinue, attributing its demise to URPE's inability to define political direction:

While we think that it is important to take political positions in political education work, URPE does not have a framework for debating the political positions which should be taken. This leads to groups within URPE taking positions which are not shared by or debated with the rest of the organization. I think that URPE politics should be more defined if truly widespread political education work is going to be done in URPE.<sup>150</sup>

The lack of enthusiasm in URPE continued into 1977. Responding to the failure of the PPEEB a new initiative was outlined at the 1977 Summer Conference, the "Economics Education Project."<sup>151</sup> The new activity differed from PPEEB in that members of the Project would actively solicit materials for publication,<sup>152</sup> the previous editorial board had expected to receive manuscripts from interested members, and was intended as a political check on the content of pamphlets. It was becoming clear that the mid-1970s ferment of movement activity was gone. In the absence of spontaneous interest the writing of pamphlets would have to be solicited. In addition, the number of members involved in organising the project was expanded from four to six, elected in the Summer Conference.

The post-1975 outreach activities were no longer part of a grand plan for connecting URPE with mass movements but were poised as "attempts to produce

<sup>148</sup>This section was source of some controversy between feminists and socialists members of the women's group, as they proposed different interpretations of women's history (Wiegiersma (1979), p. 24).

<sup>149</sup>Ibid., p. 23.

<sup>150</sup>Ibid., p. 24.

<sup>151</sup>Hartmann (1977).

<sup>152</sup>Newsletter (1977), p. 5.



politically relevant political economy.”<sup>153</sup> These efforts were also increasingly seen as part of URPE’s vocation to educate. They were another facet of being a radical economist, but never again were they played out as the first priority.

When in January 1978 URPE, through its Economics Education Project, published a new Economic Crisis volume, *U.S. Capitalism in Crisis*, the publication bore little resemblance to the mid-seventies pamphlet work. It was a book collecting independently written scholarly papers over a range of complementary issues, no longer attempting a single and clear message. The preface all but excused itself for the political views represented:

This collection of readings does not present an URPE political position on the current crisis of U.S. capitalism. The selection of articles and the range of views presented is the responsibility of the editorial collective; the authors of the individual articles are responsible for the views presented in their own essays; and the four members of the editorial collective who wrote the Introduction and Conclusion take full responsibility for their views<sup>154</sup>

Despite the disclaimers, the volume was not without political message. But unlike the earlier years’ emphasis on activism, the conclusion was to open a debate on the left, and a long list of unresolved issues was put forward for reflection.<sup>155</sup>

In 1977 facing lack of interest and participation URPE redefined its membership rules, with a new category of membership providing full voting rights and receipt of the *Newsletter* at a much lower cost, an incentive to join the frail Union. And in the following year, 1978, URPE was incorporated.<sup>156</sup> It adopted the status of an educational organization which implied its formal abandonment of any political line, something that was inscribed into its by-laws.

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<sup>153</sup>Ibid., p. 6.

<sup>154</sup>Crisis Reader Editorial Collective (1978), preface.

<sup>155</sup>Ibid., pp. 340-342.

<sup>156</sup>*Newsletter* (1978).

#### 4.4 Divisive activism

Activism has been largely ignored in histories of the radical group. Typically histories are content with asserting activism's presence in the radicals' value system.<sup>157</sup> Yet taking activism as an essentialist feature of radical economics conceals the important history of how it was first interpreted and pursued in URPE c. 1972 and how this understanding collapsed amidst turmoil in 1975. For such a short period, three years, the activist experiment left a lasting mark on URPE and radical economics' identity.

From the outset the move towards activism was divisive. Although the radical identity was solidly cast on the ideal of the scientist-partisan, it was contentious to interpret it as warranting the scientist to move away from academic work and to offer his services to social movements as expert pamphleteer. Such an interpretation came about as a result of a general redirecting of the American Left towards the working class complemented with a disdain for academism and professionalism. While the New York PEAC/PEA increasingly sought to define URPE in a drive towards outreach, the more academic-oriented URPEs coalesced around the *RRPE* or acted independently from the Union. The latter group mounted resistance when, led by the working class emphasis of the time, the editorial board of the *RRPE* decided to re-centre the editorial policy in Marxism.

But the truly disruptive and divisive force was not to be the activists vs academics divide. It came from the outreach enthusiasts. Barely a year into the pamphlet writing efforts, radicals began to look at each other less as fellow radical economists but as members of competing political groups with their distinct ideologies and agendas. The infighting began with the *D&S* controversy and became critical at the New York chapter in the early months of 1975. It was a battle for control and definition of URPE staged by different political persuasions. The radical identity proved powerless to keep the group together in the new non-academic setting.

By 1975 a variety of proposals to resolve the crisis were at hand. The "small potatoes" caucus argued for deepening the activist commitment and create

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<sup>157</sup>See chapter 2 of this thesis, in particular my discussion of Attewell (1984), on p. 46.

institutions that guaranteed that a political line once defined could be enforced. The New York group called for a return to intellectual tasks and to reform outreach into a more modest and contained effort, to be yearly scrutinized at the Summer Conference. Finally there were the “Eggplants” that represented not only a return to intellectual tasks but also to academic ones. The victorious “Eggplant resolutions” restrained activism and communion with popular movements and ultimately reinterpreted it as yet another form of educational work, emptied of ideological content. There was no desire to unite what stood divided by the bitter infighting of the previous year. “Radical economist” had become an “umbrella” identity for the many (and more primary) identities of the party-political left, the Feminists, Socialist Feminists, Democratic Socialists, Populists, Marxists-Leninists of various persuasions, that gained definition in the infighting of the mid-1970s. The years of activism taught radical economists about divisions that they would not easily forget.

# Conclusion to Radical Political Economics

## 5.1 URPE and Radical Political Economics

Radical economics is typically portrayed by radicals as emerging in response to the social movements of the sixties.<sup>1</sup> If the statement is interpreted as noting that radical economists were once student radicals, one of the sixties' movements, then it is merely stating a fact. But if it is meant to signify, as it usually is, that the content of radical economics was somehow directly informed by radicals' involvement with social movements, one risks a misleading generalisation. The political ferment of the 1960s (and 1970s) played a prominent role in the making of radical economics but in often indirect and convoluted ways. I propose that the thread that allows one to string together social context and the specific dynamics of the economics profession for the history of radical economics is the Union for Radical Political Economics (URPE).

The history of Radical Political Economics is, at least until 1978, a history of URPE.

The Union grew out of the organising efforts of University of Michigan economics students protesting against the Vietnam War. Beginning in 1966 in a dispute over the military draft system, economics students had joined together to oppose the University administration and some in the economics faculty. Following this early contact and as calls for student power came to the fore in the late 1960s, these economics students were among those that called for curriculum reform.<sup>2</sup> It has been

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<sup>1</sup>Gintis (1984), Bowles and Edwards (1990) as discussed in chapter 2.

<sup>2</sup>Brazer (1982), pp. 251-252.

suggested that radicals were already by the mid-1960s debating radical economics, at SDS's Radical Education Project, at Free University seminars, and while advocating curriculum reform.<sup>3</sup> But at the end of this thesis, I find it unlikely that the idea of a radical economics, distinct from conventional economic theory, was at the time a subject of debate. URPE was not created in 1968 to give voice to a new economic theory. It followed in its design the program set by the New University Conference of 1967, aiming to support action by radicals as they moved into professional careers.<sup>4</sup> URPE was part of a larger trend of founding radical caucuses in the professions, intended as contact groups for graduating radical students.

The motivation for creating URPE was derived from the unfolding of late 1960s' student activism, in particular the chosen career path of an older cohort of SDSers. The character of the Union was soon reshaped by events specific to the economics profession. The decision taken by AEA officials to hold the 1968 annual meeting in Chicago, angered many in the profession. After the August "police riot" targeting anti-war protesters,<sup>5</sup> many economists demanded a boycott of the city. As an alternative to the AEA Chicago meeting, the Grey Market meeting was organized by Lawrence Klein in Philadelphia.<sup>6</sup> Participation in this meeting changed URPE. In a few days the membership of the Union increased beyond all expectations. Radicals began to believe that more could be done than just act as a contact group.<sup>7</sup>

Brought together in URPE, radicals sought to define a shared intellectual programme. Over conferences, radical courses and articles in the period of 1969 to 1971, URPErs were designing a critique of "conventional economics" and an alternative to it. Radicals' aim was a transformation of the University not merely at the theoretical level, but also in its research and teaching practices. The intellectual programme was invested with the character of a paradigm, a "paradigm of conflict."<sup>8</sup> Radical economics was announced unabashedly as a revolution for economics that

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<sup>3</sup>Lee (2004a), pp. 187-188.

<sup>4</sup>On the New University Conference see p. 65 in this thesis.

<sup>5</sup>Farber (1988).

<sup>6</sup>On the Grey Market see p. 68 in this thesis.

<sup>7</sup>*Newsletter* (1969a); Bluestone (1969).

<sup>8</sup>The most accomplished exposition of this program can be found in the *Review of Radical Political Economics* special issue of July 1971, "On Radical Paradigms in Economics."

would transmit the ferment of the times to what was perceived as an indifferent and complicit profession. Yet, the process of radical economics' definition was strikingly disconnected from the social movements it was said to speak for. It was at URPE's conferences and in the pages of its *Review* that the definition was laboured and ultimately sanctioned. Definition was accomplished in the confines of academia and using well-established scholarly language, as the wielding of the term "paradigm" clearly denotes. The 1960s movement that did play a prominent part in URPE and radical economics' was student power, and the connected protest over the Vietnam War. The conflict in Indochina was a core subject in the radicals' teaching and research.<sup>9</sup> During 1969-70, campus protests escalated in number and in tactics, and administrations resorted to a heavy-handed response, expelling students and calling for police intervention. It is likely that the radicals' support for the student protesters reinforced their disaffection from more conservative faculty colleagues and the profession as a whole, further committing them to challenge the perceived mainstream.

The radical challenge was disruptive of the public standing of the profession. Accounts of the radicals' controversial, at times scandalous, participation at the AEA meetings reached the national media, and their alternative courses were offered at elite universities.<sup>10</sup> Those that responded in defence of the profession and its theoretical products sought to reassert the scientific credentials of economic science. Their strategy was both conservative and accommodating, hoping to contain disruption, keep the dissenters within the professional fold but also marginalise them. Ultimately, the result of the contest was the removal of radicals from centre-stage. The profession denied radicals the route to tenure and reappointment at the institutions that had housed the radicals' emergence and that projected their criticisms to public awareness.<sup>11</sup>

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<sup>9</sup>See Edwards, Reich and Weisskopf (1972) and the August 1970 special issue of the *Review of Radical Political Economics*, "On the War Economy."

<sup>10</sup>On the media portrayal see the articles at the *New York Times* and *Washington Post*, for instance Bender (1970), Salmans (1970) and Harbron (1972). Prominent among the radical courses was Harvard's Social Sciences 125, see Edwards and MacEwan (1970).

<sup>11</sup>Places such as Harvard University, University of Michigan, Yale University, MIT, and Stanford University.

The marginalisation of the radicals, occurring in 1973-76, paralleled the steady decrease in student unrest.<sup>12</sup> The dismissal cases at times took the shape of a contest between the radicals' prime ally, the students, and the established faculty for whom radical economics was of no import and to be neglected. Where student response was strong, as in Yale University, radicals would be hired. But somewhat surprisingly it was with support from established colleagues that radicals secured some success at Stanford and achieved the creation of the U.Mass.- Amherst radical department. Overall, radicals made only modest gains, convinced of the profession's hostility many went "underground" hiding their radical convictions.<sup>13</sup> From the mid-1970s radicals no longer participated in the AEA meetings and abandoned public challenge.<sup>14</sup>

Despite what has been suggested by Frederic S. Lee, the reasons for the radicals' evolving silence should not be put down solely to the dismissals and the mainstream's refusal to sponsor the radical alternative.<sup>15</sup> The marginalisation of radicals in academia co-occurred with a very important transformation in URPE. From 1972 a major section of the Union moved to replace its earlier intellectual focus with activism. Although the *Review of Radical Political Economics* continued to publish scholarly work, the Union was re-directing its resources to publish pamphlets designed for the social movements. The PEA/PEAC pamphlets were unprecedented in URPE and I would suggest also unprecedented in other radical professions, where radicals were in contrast deepening their academic commitments.

The claim that radical economists were responsive to left-wing politics was never more true than in 1972-75. Through URPE's outreach projects radicals interacted with left political groups. In this period radical economists wholeheartedly employed their economics training in service of left political causes. In these efforts, radicals consciously sought outside academia a testing ground for their ideas.<sup>16</sup> The truth

<sup>12</sup>See Lifshultz (1974) on the most media worthy cases of radical firings.

<sup>13</sup>Golden (1975).

<sup>14</sup>URPE has since 1969 held its own convention at annual Allied Social Science Associations meetings, the venue where the AEA holds its annual convention. What I note here is that radicals since the mid-1970s moved to present their work to the URPE sessions only, where before they also participated in AEA sessions.

<sup>15</sup>Lee (2004b), pp. 749, 751-752.

<sup>16</sup>For radicals and the 1970s, and in the logic of their outreach work (Rose and Zweig (1973)), academia and the movement were two very separate entities.

claims of their research were to be examined not by their professional colleagues but by “the people.”

The clearest legacy of URPE’s outreach activities was severe infighting that foiled any hope of returning to the earlier years of communal work. In 1974-75 strenuous schisms developed among the membership and it seemed impossible for URPE to speak again as a single voice.<sup>17</sup> URPE was mirroring the bitter sectarianism of the 1970s American left.<sup>18</sup> The settlement of the internal strife left the Union as an “umbrella organization.” To be able to accommodate the diverse political persuasions, it was defined as being without definition. More importantly, the Union sought to sever its connection with the political left by restraining its outreach work and finally becoming in 1978 an incorporated educational organization.

The aftermath of this period was a dispersed radical community.<sup>19</sup> Different groups now undertook their own activities in isolation from the Union.<sup>20</sup> URPE remained as a professional organization instrumental for maintaining, and reproducing, a network of scholars meeting through publications and conferences. Yet this was very far from the URPE of 1969-71: it abandoned the project of designing and promoting a radical paradigm; and it was unwilling to battle the AEA.

The focus of this thesis is on the emergence and definition of dissent. Self-definition continues as long as the group exists and is subject to change, any cut-off point to the narrative will be arbitrary. Still, one can take heed in choosing watershed moments where a stage in the history of the radical community runs its full course. The late 1970s signify such a moment. Two characteristics may be seen as distinguishing pre and post-1980 radical economics. URPE was no longer at the core of radical activity, except for the *Review of Radical Political Economics* which

<sup>17</sup>One need only compare URPE’s 1971 statement on Nixon’s economic policy published by the *Review of Radical Political Economics* in August 1972, with the *Crisis Reader* of 1978.

<sup>18</sup>Elbaum (2002).

<sup>19</sup>One may question that given such lack of common purpose, if the term “community” is still appropriate.

<sup>20</sup>For instance the Centre for Popular Economics was created at U.Mass – Amherst, staffed with URPErs and with goals very similar to URPE’s outreach activities, but wholly independent from the Union (Magnuson (1979)). And soon *Dollars Sense* gained full independence from URPE (interview with Arthur MacEwan, 2003).



continued as a prominent outlet for radical scholarship.<sup>21</sup> The radical economics departments, notably U.Mass.-Amherst, became the stage for radical debate. New adherents to radical economics were not recruited through URPE, they were typically the graduate students of the older radical generation based at U.Mass.-Amherst, New School, U.C.-Riverside and American University.<sup>22</sup> The second striking change was that controversy between radicals was no longer explicitly political or directed at reshaping the radicals' institutions (i.e. URPE). The focus became primarily intellectual, addressing subjects that would interest few outside the radical group, and even within the group only the more theoretically inclined. For instance, in the early 1980s there was a flurry of research on the falling rate of profit, discussing its theoretical and empirical validity.<sup>23</sup> In another example, the department at U.Mass.-Amherst became divided in two factions, one led by Stephen Resnick and Richard Wolff, the other by Samuel Bowles and Herbert Gintis, over the role of postmodern Marxism in radical theory.<sup>24</sup> Radicals engaged in controversies that were played out within the confines of their community. These efforts were of a character very different from the 1970s radical challenge to mainstream economics.

## 5.2 Boundary work and radical identity

To conclude my discussion of the history of Radical Political Economics I wish to review the changes operating in the radicals' identity, in what bound radicals as a community. In 1968, what University of Michigan radicals shared with those that joined them in URPE was the experience of student activism. They were bound by

<sup>21</sup>Although alternative outlets also began to appear: *Capital and Class* (in 1977); *Research in Political Economy* (1977); the *Cambridge Journal of Economics* (in 1978), and somewhat later *Rethinking Marxism* (in 1989). The *Review of Radical Political Economics* was not hegemonic as the journal for publishing radical work.

<sup>22</sup>That was the experience of Robert Pollin who joined URPE at the behest of his teacher David Gordon (interview with Robert Pollin, 2003).

<sup>23</sup>The debate was in part a response to the Okishio Theorem of 1961, that disproved Marx's law of the falling rate of profit. However, debate over the theorem only began in earnest in the late 1970s, early 1980s (Cullenberg (1994), pp. 59-67).

<sup>24</sup>Impressed by the work of Barry Hindress and Paul Q. Hirst's *Pre-capitalist modes of production* (1975), Resnick and Wolff began to research Louis Althusser's Marxism and the postmodern strains it motivated (interviews with Wolff, Resnick, and Gintis, 2003). The debate that split the U.Mass.-Amherst department began in the pages of the *Review of Radical Political Economics* in 1979 (Resnick and Wolff, 1979; Gintis, 1979).

the culture of the sixties, which they celebrated at URPE's yearly Summer Conference in parties, music and debating. They identified with a suspicion of authority and of the professional commitments of their elders.

Events in 1968 and 1969 moved to the top of radicals' agenda the need to challenge and change their profession. The AEA failed to respond to the August 1968 events in Chicago by not boycotting the city, and it remained silent over the War in Vietnam and the civil rights struggles. Adding to this official negligence, administrations and faculty responded to the student activism of the late 1960s with repression. Estranged from the profession, seeing it increasingly as an adversary, radicals were seeking to undermine its authority. Their challenge was not a rejection of science or of the value of knowledge, instead their aim was a refashioning of scientific practice. In their criticisms radicals portrayed themselves as the best future for economics. I want to draw attention not only to the content of the challenge but on how it reshaped the radicals' identity, what they saw as their shared interests, and defining attributes.

To understand radicals' design of their challenge and how it shaped radical's identity I use the concept of boundary work. I follow Thomas Gieryn in his metaphor that likens boundary work to the design of a cultural map:

Boundary-work stands in the same relationship to what goes on in laboratories and professional journals as a topographic map to the landscape it depicts; both *select* for inclusion on a cultural or geographic map those features of reality most useful for achieving pragmatic ends (legitimizing authority to knowledge claims or hiking through wilderness).<sup>25</sup>

In the terms proposed by Gieryn, radicals' challenge amounted to drawing a new map of knowledge. Following this topographic metaphor I wish to describe what referents were part of the radicals' and mainstream's maps, and how each map prescribed movement within science.<sup>26</sup>

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<sup>25</sup>Gieryn (1995), p. 406.

<sup>26</sup>Despite evoking a topographical metaphor Gieryn never attempts to draw a visual map of the controversies he studies (Gieryn (1999)), neither have later authors working with this methodology (see for instance, Kinchy and Kleinman (2003)). Their analysis is literary and no visual aids are

The radicals' earliest statement of their challenge, appearing in 1968 and enshrined in URPE's original prospectus, was a call to lead economics into politically relevant research. It was not a critique of economic theory and its heuristics, it sought to rewrite the questions posed by economists.<sup>27</sup> What radicals depicted was a cultural landscape where the economics profession was distant from the social problems of the day. As John Weeks stated, the source of economics' state lay in the "politics of economists."<sup>28</sup> In the radicals map, the distance between science and society existed because the *mainstream economists* had moved away from awareness of society and its ills into the land of "irrelevancy." Economists were free to travel in this cultural landscape. While *radical economists* moved towards society and its problems and thus became *political*; the conventional economists were those that distanced themselves from society, and thus became *irrelevant*.<sup>29</sup> Therefore, radicals were proposing a map of the politics of economists. Science was not demarcated, because "science" was what scientists did "science" was mobile. Economists could apply their scientific efforts across this landscape without changing the scientific status of their work.

The episodes of radicals conflict with faculty and administrators over the campus unrest intensified in 1969-70, and so the debates between radicals and mainstream at the AEA conventions became ever more bitter. Radicals began to argue that theirs was an identity empty of intellectual content, and that they should define their own economics. This became their goal in the 1969-71 period as they designed courses and debated within URPE. Radicals raised the stakes from a critique of the subjects studied by economists, to a critique of the profession, its institutions and ideas as a whole.<sup>30</sup> As the debate between Michael Zweig and John Weeks testifies, some radicals resisted such an ambitious challenge. However, by 1971-72 even the critics

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provided. Following this work I have also shied from using illustrations. To depict these maps visually requires deciding on shapes, textures and many more relationships between the maps' contents than are available in the record of the controversies. I found that to supply an illustration I would have to accompany it with a cumbersome justification of the many decisions needed for its construction, and I judged that this would add little insight into the subject of study.

<sup>27</sup>See URPE's Prospectus, in URPE Secretariat (1969).

<sup>28</sup>Weeks (1971).

<sup>29</sup>This was the dominant message of the papers of the Conference in December 1968 with the exception of Zweig's "New Left Critique of Economics."

<sup>30</sup>Evidenced in the 1969 storming of the AEA Business meeting (*American Economic Review* (1970)).

had been converted to a wholesale rejection of prevailing economic theory.<sup>31</sup>

In their new challenge radicals mapped a very different landscape of science and its relationship with society. As before science and politics overlapped, they were not demarcated. What stood separate were *scientific paradigms* located in the cultural map, thus, theirs was a map of the politics of paradigms (see table 5.1.).<sup>32</sup> As an outcome of philosophy of science, the existence of two competing paradigms (the radicals' and the mainstream's) implied a battle for hegemony, where only one paradigm could prevail.<sup>33</sup> Yet, radicals did not draw much attention to this aspect, their major stress was to show how paradigms were interlocked with the struggle for social change in American society.

The radicals located their paradigm linked to the *revolutionary movement* in America, as a science for the oppressed and their emancipation. The paradigm was part of the "movement."<sup>34</sup> In contrast, they placed the professional mainstream as implicated with the *conservative forces* in society, as part of the ideology of the *status quo*. Both the paradigms and the social forces were bound and were irrevocably in conflict and distant in the cultural map. The radical map had no place for *neutrality*, the radicals thus denied the mainstream refuge. Such referent did not exist in the radical map, instead, the mainstream paradigm was said to be in *partisan* support of the *status quo*. While the mainstream's was a concealed partisanship, the radical paradigm on the other hand was overtly partisan.

Besides the paradigms overlap with the progressive or conservative forces of American society, what separated them was their adherence to *subjectivity* or *objectivity*. Radicals located traditional economics overlapping with objectivity. For them, objectivity was also objectification or alienation. The alleged mainstream's denial of the subjective was with counter-cultural overtones made into a denial of

<sup>31</sup>Zweig (1971b[1969]), Weeks (1971[1970]), Zweig (1971c), Weeks (1972).

<sup>32</sup>The design of the radical paradigm and its contrast to the mainstream one were the subjects of the July 1971 Special Issue of the *RRPE*, see p. 76 of this thesis.

<sup>33</sup>To evoke Kuhnian paradigms was a very contemporary weapon to deploy, one that economists shared with radical historians and other radical professionals. For more on the dissenters use of paradigms, see section 10.1. of this thesis.

<sup>34</sup>Fusfeld (1973), p. 145.

“life” itself.<sup>35</sup> The traditional economics paradigm was said to depoliticise and de-socialise the scientist while training students to become “machine-like,” - objectivity was used interchangeably with the notion of an ideology of the *status quo*. In contrast, radicals located themselves in subjectivity.<sup>36</sup> Their scientific approach would allow the scientist to be close to the ills of society and hence revolt against injustice. Its goal was to teach people how to live without alienation.

The mainstream’s response to the radical challenge focused explicitly on the alleged overlap between politics and science (or scientists). Mainstream economists denied that politics and economics were one. For them, they stood separate. But the mainstream also denied that there was a clearly defined boundary between the two. One could be near to science and far from politics, but there was no means of being wholly immune to the influence of society. By conventional economists’ own admission there was a difficult balance to strike, one should be close enough to identify the social ills in need of response and yet, also ensure that economists work in near isolation from partisanship and societal commitments.<sup>37</sup> Politics was not a social space where two separate classes (the oppressed and the oppressor) were clearly demarcated. Instead, for the mainstream, politics was part of economists’ “personal tastes,” alongside for instance with personality, it was the starting point of any scientific voyage.

To move away from the influence of personal tastes and to become closer towards the ideal of objective, neutral scientific knowledge, the scientist had to pose sensible, quantifiable questions and then open his work to professional criticism as an antidote to personal idiosyncrasies.<sup>38</sup> The mainstream mapped a path of “knowledge of technique and acquaintance with data”<sup>39</sup> that economists should travel towards science. This route to science was part of a single paradigm of economics, the mainstream denying radicals’ claim that they offered a new paradigm. The radicals were seen as abandoning science to do politics, what Solow called “cant and

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<sup>35</sup> Behr et al (1971), p. 25

<sup>36</sup> Ibid., p. 39.

<sup>37</sup> Solow (1971), p. 64.

<sup>38</sup> This was the core message of Solow (1970).

<sup>39</sup> Solow (1971), p. 65.

role-playing". In the mainstream's map, the radicals, with their claims of joining politics and economics, were located outside science. They had abandoned the path towards knowledge and were engaged in exercise of their "personal tastes."

Table 5.1: Boundary maps of radicals and mainstream.

Maps	Location of radicals	Location of mainstream
Radicals' map of the politics of <i>economists</i>	In society, addressing its problems.	Away from society, in irrelevancy.
Radicals' map of the politics of <i>paradigms</i>	In partisanship and subjectivity. With the revolutionary movement and the oppressed.	In false neutrality and objectivity. With the status quo and their ideology.
Mainstream's map of the practice of <i>economists</i>	In personal and political commitments. Close to ideology.	In quantification and knowledge of technique. Close to objective knowledge.

The translation of each contender's argument onto a cultural map highlights their enlisting of allies. It shows how radicals were exhibiting their distance from the remainder of the profession. While the economics profession was made the enemy, deeply committed to the status quo; radicals boasted their partisanship to social revolution. According to the radical map, the greater one's distance from the profession the greater one's closeness to the "movement." In radicals' definition of the "movement" their students held a paramount role: "college and university teachers work in one of the centers of radical social activity in the United States. Radical teachers should, therefore, view their work as part of a wider radical movement. They should design their courses to be relevant to the concerns and needs of that movement."<sup>40</sup> The University was a place of social experimentation, where liberation and the battle against alienation could begin. Teachers and students, together, would liberate themselves of prejudices and of oppressive social relations through curriculum

<sup>40</sup>Edwards and MacEwan (1970), p. 361.

and grading reforms. The radicals sought to enlist the support of their radical students.

The historical record demonstrates that students in revolt were in fact close to radical economists. Students were not only an ally in radical rhetoric, they did act as an ally. Throughout the apex years of student unrest, radical economists were outspoken advocates for the students, and many students joined URPE and became involved in radical economics. Students through protest or argument assisted the creation of radical courses and the hiring of radical economists. As the post-1971 years of decay in student activism demonstrated, the fate of radical economists was bound to the fate of student unrest. Having not secured other viable sources of support, radicals were left open to dismissals and marginalisation which followed in the mid-1970s. They did benefit from support from older established colleagues, an unlikely and unsolicited ally. But despite the fine sounding names, such as J. K. Galbraith, J. G. Gurley, K. Arrow, W. Leontieff, this did not amount to a sufficient base of support in the profession to ensure radicals' success. In fact the majority in the profession were convinced by the opposing cultural map, drawn as a defence against the radical challenge.

Those that spoke in "defence of economics," or as they often stated it in "defence of science," were primarily aiming at accommodation and reassurance of economics' scientific credentials. Their argument highlighted how radicals had diverged from the correct path. They did not seek so much to make radicals into an enemy. The aim was to deny the radicals' dangerous boundary work and to impose the conventional mode of debate in the profession, well-contained from political digressions and questioning of economics' credibility. The strategy, I argue, appealed to professional economists that benefited from the profession's status as neutral and objective. If the mainstream's goal was to convince the radicals to rejoin the professional discourse of objectivity and neutrality then it failed, but it is doubtful this was what was intended. What they achieved was the containment and marginalisation of the radicals, under Solow's dictum: "we neglect radical economics because it is

negligible.”<sup>41</sup> It is noteworthy that economists outside the debate behaved according to what was outlined by the mainstream spokespersons. When radicals were denied tenure at Harvard, the faculty chairman noted that the radicals’ achievements were “not significant”.<sup>42</sup> It appears that the bulk of the profession joined in the defence of economics, unconvinced by the promised benefits of the radicals’ alliance with the “movement” and revolution.

The process of radical economics’ self definition was underscored by the controversy between radical economists and their conventional colleagues. Radicals came to see themselves as challengers and their identity was built to articulate the challenge. Radical Political Economics was in 1971 no longer an empty label, it had been filled with meaning. The radicals’ shared past as student activists - an initial identity - was not as important as the development of a (radical) economics for revolution – their new identity.

There is continuity between the radicals’ paradigm building period, up to 1971, and their post-1972 involvement in outreach work. The radical (URPE) efforts of connecting with the popular movements were a living out of the radical identity, of the scientist-revolutionary. The radical paradigm passionately called for a University beyond the campus, with the scientist mixing with the people to test his ideas, to allow society to inform his research.<sup>43</sup> What was new was to interpret such connection without the mediation of the University or its restless students. The radical paradigm was a programme for the University, but the radical activists of PEAC/PEA were acting away from the campuses through URPE, with no connection to their professional roles.

I have argued in the previous chapter that radicals and URPE in the mid-1970s were moving away from the academic community into the political activist community. As the record shows the radical identity proved unsuited to the new environment. It failed both to provide guidance for action and it failed to maintain a sense of shared interest and purpose. Political line was strenuously debated and

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<sup>41</sup>Solow (1971), p. 63.

<sup>42</sup>James Duesenberry in *Kennebec Journal* (1973), p. 32.

<sup>43</sup>This theme runs from Wolff (1969) to Behr et al (1971).



revised, never consensually settled, and divisive solidarities began to populate URPE, fragmenting the radical economics community.

The 1970s left-wing political arena was ostensibly ideological. URPEers looked not at each other's economics views but at political affiliation and ideological belief.<sup>44</sup> It was not in their economics that radicals differed, though there were differences between those zealously Marxist and those that were suspicious of Marx,<sup>45</sup> where radicals differed was in their politics. They clashed not on debating the economy, but on the political directing of URPE. It was in politics that divisions emerged. In under two years, sections of the URPE membership were in New York battling over control of the organization's resources. Commitment and solidarity was due primarily to the ideological group and not to the radical economics identity. URPE began to resemble yet another battleground for feminists, socialist feminists, democratic socialists, and Maoists to face each other.

It is significant that radicals did not battle it out to the end, and knowing all too well what was in sight if the conflict was allowed to continue, i.e. the wrecking of URPE, they negotiated a settlement.<sup>46</sup> The 1975 Summer Conference, issuing a new Prospectus for the Union, emptied URPE of its political focus, towards what were clearly academic and educational goals.<sup>47</sup> URPE was an "umbrella" organization. The goal of designing a single radical economics, of identifying a radical economist, was abandoned and no further discussions followed along those lines. There were no further conferences on the subject, or special issues in the *Review of Radical Political Economics*. The organization that had originated the radical economics identity and paradigm to fight the profession, was working without a map defining the landscape of economics and prescribing radicals movement within it.

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<sup>44</sup>At the *Dollars & Sense* controversy the most damaging argument made by New York about Boston was their commitment to the political group New American Movement, see p. 136 of this thesis.

<sup>45</sup>As in the *RRPE* 1973 editorial board debate. see p. 122 of this thesis.

<sup>46</sup>Zweig (1975).

<sup>47</sup>The careful tone of the Crisis Reader of 1978 epitomises the new stage (URPE-EEP (1978)).

## Part III

# POST KEYNESIAN ECONOMICS

# 6

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## Introduction to Post Keynesian Economics

### 6.1 Participant histories of Post Keynesian Economics

The second community of dissenters which I have chosen to study is Post Keynesian Economics. Post Keynesian economists' relationship to their history is nothing less than the converse of that of radical economists. Post Keynesians passionately debate their intellectual origins. Their research, in contrast with the technical style dominant in the economics profession, is paved with incursions into the history of economic doctrine. Some authors have favourably argued for "the organic way which history of thought is embedded in Post Keynesian economics."<sup>1</sup> According to this view, history of thought is pursued to inform present day economics, building on a "historian's understanding of older texts", "studying different historical contexts and the theories developed to address them helps economists to build up the judgment necessary for developing theories appropriate to new contexts."<sup>2</sup> How and why this Post Keynesian heuristic works is seldom fully articulated. What stands out clearly is the noble standing that history of thought holds in Post Keynesian Economics.

Post Keynesians have produced a far-reaching and detailed body of work on their own intellectual origins, but often this research has been subordinated to the demands of contemporary controversies in economics. Some of this historical work is then underpinned by generously interpretative and impressionistic use of evidence,

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<sup>1</sup>Dow (2002), p. 319.

<sup>2</sup>Ibid., pp. 321, 330.

where justificationist myth tries to pose as history. As an illustration to this style one need only recall Paul Davidson's often repeated account of Sir John R. Hicks's conversion to Post Keynesian Economics. Davidson proposed the following tale:

With his IS-LM equational system, the young Hicks of the 1930s became a forefather of what Joan Robinson would call "Bastard Keynesianism" . . . In the mid-1970s, however, his Hicksian equational system failed to provide any workable solution to the inflation problem plaguing the free world. Hicks – now Sir John Hicks, a Nobel Prize winner – apparently recognized the errant ways of his neoclassical youth . . . By the winter of 1980-81, Hick's conversion away from neoclassical economics towards a Post Keynesian analytical approach was complete.<sup>3</sup>

It is almost with teleological fatality that Davidson reveals the young neoclassical Hicks ultimately accepting the truth of Post Keynesian Economics, in his wiser later years as a Nobel Prize laureate. The evidence however is slim. For instance, concerning the last and crucial twist to the tale, Hicks in 1980-81 did write a piece to the *Journal of Post Keynesian Economics* critical of his IS-LM work but it was far from an endorsement of Post Keynesian Economics.<sup>4</sup>

Beyond the noted cases of justificationist historicizing, a more difficult hurdle to overcome in reading this secondary literature arises from the fact that these are participant accounts - Post Keynesians writing their own history. From the vantage point of the group and its culture many questions about the past are never posed and researched. There are many silences. One very important question for my current concerns is: how did the group emerge? Intellectual participant accounts such as the ones we are offered by the members of the group, have not much to say on the subject.

This chapter does not attempt to survey three decades of Post Keynesian controversies on the history of economics. I take a much more focused inspection of the literature. What I am urged to do is to sketch some of the claims made on the emergence of Post Keynesian Economics. These will be valuable clues for motivating and framing my own approach to the historical problem of emergence.

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<sup>3</sup>Davidson (1991), p. 27-28.

<sup>4</sup>The comment that Davidson extracted from Hicks' 1980-81 article was rather subdued in its rejection of past commitments, "I have, however, not concealed that, as time has gone on, I have myself become dissatisfied with it [IS-LM]" cited in Davidson (1991), p. 28.

## 6.2 The Keynesian generations

Post Keynesian intellectual histories are populated by the giants of economic thought. Although some authors reach back to David Ricardo and Karl Marx to begin their narratives, the majority position tends to start with John Maynard Keynes, “Post Keynesian economics starts with the work of Keynes, specially his *General Theory of Employment, Interest and Money*. This work threw the gauntlet and challenged established economic thinking in numerous ways.”<sup>5</sup> Keynes’ work has been characterised as a critique of neoclassical economics. Post Keynesians have portrayed the *General Theory* (and to some also his *Treatise on Money*) as a major intellectual break with the mainstream of far reaching importance for economics. Keynes’s revolution<sup>6</sup> is the first and central theme of Post Keynesian accounts of their emergence.

Post Keynesians contest what they see as the establishment’s reading of Keynes embodied in the Hicksian IS/LM analysis.<sup>7</sup> This interpretation of Keynes is known among Post Keynesians as the “Neoclassical synthesis” or dismissively as “Bastard Keynesianism”:

the central message of the synthesis is the view that long-term equilibrium is determined by the supply side and may be understood through the analysis provided by classical economics, whereas short-term departures from equilibrium are the result of demand fluctuations and market imperfections, in particular the sluggish movement of wages and prices in response to changes in demand.<sup>8</sup>

It was Joan V. Robinson that coined the term “Bastard Keynesianism” in a 1964 review of Harry Johnson’s *Money, Trade and Economic Growth*, “arguing that what Johnson and other ‘bastards’ of his generation saw as weaknesses were in fact strengths – to wit, a sense of time, of the structure of society and of economic life as a

<sup>5</sup>Holt and Pressman (2001), p. 2.

<sup>6</sup>Steindl (1997).

<sup>7</sup>Debates over Keynes’ theory: its content, how it should be developed, and how the economics profession responded to his work are raging to this day and are not exclusive to Post Keynesian economists. For a recent debate on the subject of the IS/LM see the 2004 supplement to the *History of Political Economy*, volume 36.

<sup>8</sup>McKenna and Zannoni (1997), p. 463.

process.”<sup>9</sup> The economists traditionally associated with the mainstream version of Keynesianism are Paul A. Samuelson, John R. Hicks, Franco Modigliani, Don Patinkin, Robert M. Solow and Milton Friedman.<sup>10</sup>

Post Keynesians characterise the Neoclassical synthesis as a (mostly American) movement that prevented the economics profession from discarding those traditional ideas, that Keynes had shown to be flawed, notably traditional notions of equilibrium “in which *all* markets (including the labour market) clear, [as] an accurate description of the outcome of tendencies in the economic system”.<sup>11</sup> For Post Keynesians, neoclassicals forced Keynes’s ideas into their mathematical apparatus where equilibrium and certainty reigned and this did irreparable damage to his intended message.<sup>12</sup> It is a hallmark of this secondary literature “to speak for” Keynes, the weight of the argument placed first in showing Keynes’ revolutionary intentions and then interpreting how Keynes would have sought to fulfil them in today’s debates.

In Post Keynesian historical reflections, following the work of Keynes came the contributions of the first Post Keynesians, described as his close associates at Cambridge, Joan Robinson, Richard Kahn, Nicholas Kaldor and Piero Sraffa but also the Polish economist, Michal Kalecki.<sup>13</sup> This first generation “inspired a new generation of economists in Cambridge during the 1950s and 1960s,” namely Tom Asimakopulos, Pierangelo Garegnani, Luigi Pasinetti and Geoff Harcourt.<sup>14</sup> In America two economists, J. K. Galbraith and Alfred S. Eichner, are “the bridge between the European and American Post Keynesians” by developing “insights of Robinson on imperfect competition.”<sup>15</sup> The first generation of American Post Keynesians is said to have been followed by another led by Sidney Weintraub and Paul Davidson and with Hyman Minsky and Basil Moore.<sup>16</sup> The third generation of Post Keynesians is currently active and has founded its work on the insights of

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<sup>9</sup>Harcourt (1987), p. 204.

<sup>10</sup>Holt (1997), pp. 508-9.

<sup>11</sup>Harcourt (1987), p. 204.

<sup>12</sup>Davidson (1991), p. 22.

<sup>13</sup>Holt (1997), p. 509.

<sup>14</sup>Holt and Pressman (2001), p. 4.

<sup>15</sup>Idem.

<sup>16</sup>Ibid., p. 5.

previous authors.

In Post Keynesian histories each generation of authors is said to build on the work of the previous one. What is proposed by these accounts is a genealogy to describe Post Keynesian theory development. Continuity is assured through the professor-student relationship, an institutional link that is redressed intellectually into an originator-developer connection.

The added dimension of space to the genealogical sequence, i.e. the existence of European and American branches, has been used to represent the existence in Post Keynesian Economics of distinct approaches. The Europeans, often referred to as Neo-Ricardians or Sraffians, are described as concerned with the study of income distribution and models of the economy labelled as real economy (no-money) models. They are said to have argued “that income allotments are not determined by the neoclassical theory of marginal productivity and of factor inputs, but by macroeconomic aggregates and social and political forces.”<sup>17</sup> The Americans on the other hand are said to place their prime stress in the role of money and finance for a modern and complex economy riddled with uncertainty: “In an economy where the future is unknown, contractual agreements through institutional arrangements are needed to incur the factor inputs necessary for efficient production through historical time. Such contractual agreements require money and liquidity, so that entrepreneurs are able to meet their contractual liabilities before production takes place.”<sup>18</sup>

The genealogical net (mixing generations and geography) is said to account for all theoretical positions in Post Keynesian Economics. The seminal systematization of this net was proposed by Omar Hamouda and Geoffrey C. Harcourt in the mid-1980s.<sup>19</sup> This survey proposed three branches, adding to the above mentioned a Kaleckian-Robinsonian branch. I have chosen not to discuss this work here since their reading of the group’s history, although still cited with admiration, has been replaced by the two-branch description.<sup>20</sup> Each branch characterised as a succession of

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<sup>17</sup>Holt (1997), p. 509.

<sup>18</sup>Ibid., p. 511.

<sup>19</sup>Hamouda and Harcourt (1988).

<sup>20</sup>Hamouda and Harcourt’s proposal is discussed in detail in section 8.5.2.

authors, has its own methodology, its own set of core ideas and reading of Keynes' original critique. One could say there is an intellectual DNA based on which new contributions are founded. Critically all branches converge retrospectively to Keynes, this lineage establishes the Post Keynesian group as the "true" follower of Keynes's revolutionary ideas.

The major work on the history of Post Keynesian Economics was authored by John Edward King, *A History of Post Keynesian Economics since 1936*.<sup>21</sup> Unlike earlier accounts, King was primarily concerned with a study of history and he seeks to push aside economists' presentist concerns. Still, King's book was nonetheless primarily an intellectual history in line with the major themes noted above – Keynes was maintained as the main figure for the Post Keynesian group/theory, and a genealogical structure accounted for further theory developments. These two themes were quite explicitly enunciated from the outset. As suggested by the title, the book began with a discussion of Keynes and his 1936 *General Theory*. As a methodological choice, King adhered to a view from the history of sociology, that schools of thought were structured in terms of: a "founder-leader," a set of "converts" who build on the master's innovative message and finally the "foot-soldiers" who are in charge of the organizational work, for instance the editing of journals.<sup>22</sup> This can be easily likened to the three generations scheme presented above, where the labours of later authors seem to be downplayed in homage to the innovative leaps of earlier generations.

King placed Keynes' *General Theory* at the beginning of his account, however he did not argue that Keynes' message was unambiguous: "the final version of the *General Theory* that went to the publishers in January 1936 ... differed substantially from earlier drafts and was itself less than entirely coherent."<sup>23</sup> According to King, Keynes' work was not coherent for contemporary Post Keynesian standards and proved fatally ambivalent on crucial theoretical issues, despite this it became the

<sup>21</sup>King (2002) The book was the outcome of an extensive research involving interviews conducted with Post Keynesian authors (King (1995a)), a annotated bibliography of Post Keynesian works (King (1995b)) and editing a collection of Post Keynesian most significant texts (King (2003)).

<sup>22</sup>King cites Tiryakian (1977) as his entry point into the history of sociology. King acknowledges that this structure is evocative of a religious sect which he accepts as innate to any school of thought (King (2002), p. 3).

<sup>23</sup>Ibid., p. 12.



starting point for a generation of Cambridge economists to explore and develop. Before surveying the work of these authors, King introduced into his narrative another founding father, Michal Kalecki, a Polish economist who had arrived at Keynes' most important insights prior to the publication of the *General Theory*, and had in some respects already gone beyond him. King tells the reader that Kalecki's work was well received in Cambridge by Maurice Dobb and Joan V. Robinson and had a lasting influence in the latter's work.<sup>24</sup>

In King's history, the first generation of Cambridge economists was said to be striving for "a generalization of the *General Theory*."<sup>25</sup> Keynes by his own admission had presented only a short period theory, and Roy Harrod's 1939 "An essay in dynamic theory" published in the *Economic Journal*, was the first among a series of attempts to formalize a Keynesian long-period theory of growth. According to King, it was as a critique of Harrod's growth model and with strong Kaleckian influences, that Robinson in 1956 wrote the *Accumulation of Capital*.<sup>26</sup> And it was also by following Harrod's model that Kaldor and Pasinetti developed a Post Keynesian theory of income distribution.<sup>27</sup>

King argued that in the 1960s Post Keynesians' work moved from the heading of growth theory to that of capital theory. The first departure from (neoclassical) marginal productivity theory was located in Robinson's 1953-4 *Review of Economic Studies* article, but King then adds that it was only with Sraffa's 1960 book *The production of commodities by means of commodities* that the critique fully matured.<sup>28</sup> The 1960s were marked by what became known as the Capital Controversies, where Post Keynesian Economists attacked the orthodox theory proposed by Franco Modigliani, Paul Samuelson and Robert Solow of MIT.

King argued that the first Post Keynesian work done outside Cambridge occurred in the late 1950s and early 1960s. First in Sidney Weintraub's, 1958, *Approach to the Theory of Income Distribution*, followed by Hyman Minsky's work on financial

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<sup>24</sup>Ibid., pp. 49-50.

<sup>25</sup>Ibid., p. 56.

<sup>26</sup>Ibid., p. 62.

<sup>27</sup>Ibid., p. 70.

<sup>28</sup>Ibid., p. 92.

instability as the cause of economic crisis and in Paul Davidson's theory of money and finance in his 1972 book, *Money and the Real World*.

In King's narrative the 1970s were years of battle between Post Keynesian Economics and the neoclassicals, a struggle that King saw as being lost by Post Keynesians, "It had indeed been a comprehensive defeat, most apparent in the utter failure of Cambridge political economy to reproduce itself."<sup>29</sup> Somewhat contradictorily, King noted Post Keynesian Economics' continued expansion throughout the globe. With already a strong group in America in the late 1970s, economists from other nations began to contribute to the approach, King lists Australia, Austria, Canada, France and Italy.

King's history was written as a meticulous knitting of works, by describing models and theoretical positions manifest in the literature and arguing for their intellectual connections. The study was ambitious in its attempt to cover nearly sixty years of work and in alluding to the main Post Keynesian texts. The ordering of the materials was made to suggest a tentative development of ideas, progressing in clarity, elegance and scope. In this walk to intellectual maturation there were steps backwards. It was a slow process where authors sometimes took the wrong route, as was the case of Joan Robinson with her 1937 *Introduction to the Theory of Employment*, which King identified as a largely "Bastard Keynesian" text.<sup>30</sup> In time authors corrected their path and found their way back to the advancement of Post Keynesian Economics. But despite the order he imposed on the historical record, King avoided an ostensibly "whig" account by concluding that Post Keynesian development has fallen short of complete success. Post Keynesian Economics for King is not a coherent field; as evidence to this claim he surveyed debates reflecting long unresolved tensions between branches of the Post Keynesian family tree.

King's history effectively voiced claims that Post Keynesians have made concerning their intellectual ancestry, it tracked and registered these tentative intellectual connections. That King was keeping with the established narratives of

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<sup>29</sup>Eminent Post Keynesians at Cambridge when retiring were not replaced by like-minded dissenters but by the "neoclassical enemy" (ibid., p. 133).

<sup>30</sup>Ibid., p. 25.

Post Keynesians has been noted in several reviews of the book, for instance Eric Tymoigne and Frederic S. Lee commented: "King's story of Post Keynesian economics from Keynes to the capital controversy, although well told, is all quite familiar."<sup>31</sup> In the same review it was remarked how much of King's insights were drawn from the oral tradition of the Post Keynesian group.<sup>32</sup> What this tells us is that King did not examine the accuracy of his collected oral history against the historical record. His account was indistinguishable from the oral history and as such from participant accounts. King wrote a history of Post Keynesian Economics as Post Keynesians know it. It was a history about the Post Keynesian family, how authors should be ordered and how one should rank their contributions. King's history was also restrictively intellectual: any sense of a Post Keynesian community living in society is lost, replaced by Post Keynesians ideas disembodied of social history.

While conservative by keeping with the consensus established in participant accounts, King did break some new ground by exploring and clarifying some intellectual claims in great detail. Most notably, King courageously diagnosed ambiguities in Keynes's message which most Post Keynesians would be reluctant to admit, and as a result King suffered the attack of one of the most prominent members of the Post Keynesian community, Paul Davidson.<sup>33</sup>

Undoubtedly because of Post Keynesians' passionate relationship with their own history, King's book has been widely discussed and became a focus of controversy. It is telling that most of the disagreements arose on questions of emphasis. Members of the Neo-Ricardian or European branch felt that their position had not been adequately represented,<sup>34</sup> those reading from an American perspective wanted less detail on Cambridge economists and more attention given to the work of Davidson and Weintraub;<sup>35</sup> yet others would have wanted more detail on the second generation

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<sup>31</sup>Tymoigne and Lee (2004), p. 276.

<sup>32</sup>Ibid., p. 275.

<sup>33</sup>Davidson (2004) was a lengthy review of King's history. King (2005) is a point by point response to Davidson, who as editor of the journal reserved for himself the last word on the discussion (Davidson (2005)).

<sup>34</sup>Trezzini (2003), pp. 121-122.

<sup>35</sup>Bateman (2004), p. 582.

of Post Keynesians.<sup>36</sup> These responses reflect long-standing feuds within the Post Keynesian group concerning its history.

Of particular interest, in the debate over King's history, was the contention by Tymoigne and Lee that King had not provided an adequate moral to his narrative. King concluded his study with the diagnosis that Post Keynesian Economics was a failed paradigm given the unsettled Post Keynesian internal controversies. Tymoigne and Lee argued against King's assessment, proposing that the true measure of Post Keynesian success or failure ought not to be theoretical coherence but whether the group survived in the hostile environment of economics, "the formation of such community is in fact an outstanding accomplishment."<sup>37</sup> This critique echoed Frederic S. Lee's own work on the history of Post Keynesian Economics.

### 6.3 Punished by orthodoxy

Frederic S. Lee, an established Post Keynesian price theorist,<sup>38</sup> has undertaken a study of the history of Post Keynesian Economics that deserves to be singled out from other accounts, as Lee broke with the traditional intellectual character of earlier histories. Paramount in Lee's research was the notion of network, which, compared to contemporary uses of concept in the sociology of science was rather narrowly defined.<sup>39</sup> It denoted participation in meetings, membership of associations, mailing lists, publishing in journals or teaching in the same graduate programs: "a Post Keynesian economist is defined solely in terms of participating in identifiable Post Keynesian events."<sup>40</sup> Interestingly, Lee posed the question of how Post Keynesian emerged, within his frame how the Post Keynesian network emerged.

Lee's history of Post Keynesian Economics (PKE) was part of a broader project, a historical study of contemporary heterodox economics.<sup>41</sup> This research comprised

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<sup>36</sup>Fontana (2005), p. 410.

<sup>37</sup>Tymoigne and Lee (2004), p. 279.

<sup>38</sup>Lee (1998).

<sup>39</sup>Such as for instance, in the "sociology of translation" (Callon (1999)).

<sup>40</sup>Lee (2000b), p. 141. What makes an event Post Keynesian is not addressed.

<sup>41</sup>In addition to his work on radical economics, reviewed earlier in this thesis, he has also focused on the *Conference of Socialist Economics* (Lee (2001)), and *Association of Heterodox Economics* (Lee (2002a)). As I have noted in the introduction to this thesis, the terms "heterodox" and "dissent" are

both analysis and critique of the economics profession for its alleged exclusion of heterodox economists. Polemically, Lee contended that twentieth century economics was characterised by the unending effort of neoclassical economics to purge the profession of its competitors. As a result of their de facto exclusion from the AEA annual conference and the *American Economic Review* (*AER*) and other mainstream journals, heterodox economists gathered in three much smaller professional associations, the Union for Radical Political Economics, the Association for Evolutionary Economics and the Association for Social Economics.<sup>42</sup> The heterodox networks were therefore a reaction to the mainstream's repression.

Lee's account of Post Keynesian Economics history drew extensively from the correspondence between Alfred S. Eichner and Joan V. Robinson, which he collected and published.<sup>43</sup> In light of this archive, Lee argued that Eichner was the main driver in the creation of the Post Keynesian network.<sup>44</sup> The formative period of the Post Keynesian network was located to the period from 1971 to 1980.<sup>45</sup> Lee traced the beginning of the Post Keynesian network to a meeting organised by Eichner and Luigi Pasinetti under Robinson's sponsorship at the 1971 AEA meetings in New Orleans. The meeting attended by seventeen economists was held with the aim of discussing ways by which heterodox economists may begin to open-up economics to non-neoclassical approaches.<sup>46</sup> It was, according to Lee, with the desire to re-open economics to heterodox discourse that the PKE network grew. He further argued that it was only in 1976-77 that a social network of Post Keynesians became fully formed. The core of the Post Keynesian group consisted of ten economists – Paul

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used interchangeably in economics. Lee's "heterodox groups" is more inclusive than the sixties and seventies dissenters, it encompasses for instance Grants economics and institutionalists which I do not consider.

<sup>42</sup>Lee (2000a), p. 22.

<sup>43</sup>"Correspondence: Working out the Megacorp and Oligopoly, 1969-1971"; "Correspondence: Organizing the U.S. Post Keynesians and Macrodynamics, 1971-1972"; "Correspondence: Neoclassical Economics and the Post Keynesian Paradigm, 1973-1976" in Warren J. Samuels (2000), *Research in the History of Economic Thought and Methodology*, volume 18c, pp. 79-204.

<sup>44</sup>Other important promoters of Post Keynesian Economics were not given sufficient attention, such as Geoffrey Harcourt, Jan Kregel, and Paul Davidson.

<sup>45</sup>Following the period of creation of a social network, Lee notes the building of institutional structures from 1977 to 1982; and from 1978 to 1995 the reproduction and development of the Post Keynesian social structures (Lee (2000b), p. 142).

<sup>46</sup>Lee (2000a), p. 22.

Davidson, Alfred Eichner, Jan Kregel, Hyman Minsky, Basil Moore, Ed Nell, Roy Rotheim, Steve Rouseas, Howard Sherman, and Sidney Weintraub – who were involved in or attended at least two Post Keynesian “events” between 1971 and 1977 and also subscribed to and/or were on the editorial board of the *Journal of Post Keynesian Economics (JPKE)* when it was founded in 1978.<sup>47</sup>

Lee’s crucial insight was that the origins of Post Keynesian group lay not in the 1930s as suggested by intellectual accounts, but in the early 1970s. Lee did not set out to displace earlier (intellectual) accounts. In his history, the heterodox networks were said to emerge in response to the repression of heterodox ideas by the mainstream. Hence, for Lee heterodox ideas must have pre-dated the heterodox networks. Still, I would argue that in light of Lee’s findings Post Keynesian participants’ intellectual histories become increasingly untenable. If Post Keynesian ideas had existed since Keynes’ 1936 *General Theory*, as an independent and alternative body of theory, why would one have to wait for three decades for them to be enunciated? Why would one have to wait for the repression of the mainstream to create a Post Keynesian group that would advance this alternative? Lee’s work unwittingly casts doubt over previous research.

But Lee’s work is not without its shortcomings. It offered a meta-narrative that binds the history of heterodox groups together. Its characteristic claims were the repression imposed by the economics’ establishment, and its counterpart: the heterodox community tradition of self-support that successfully resisted the attack.<sup>48</sup> Lee has argued that his work must be read as a recovery of heterodox memory, on which a heterodox identity could be based.<sup>49</sup> The author conceded that such a heterodox identity is a destination and it is far from reflecting the present.<sup>50</sup>

<sup>47</sup>Lee (2000b), p. 150. In this analysis one can see the hallmark of Lee’s approach to networks, the register of attendance to meetings is equated to commitment to the group.

<sup>48</sup>According to Lee, the counterpart to the establishment’s attack is the self-support from fellow heterodox economists, mutual aid was crucial to survive the mainstream’s repression (Lee (2002b), p. 53).

<sup>49</sup>Lee (2000b), p. 142.

<sup>50</sup>For Lee, the historical tradition of self-support is maturing into a convergence of theoretical positions and institutions (Lee (2002b), p. 55). Seen by the author as a major development in this direction, the *Association for Heterodox Economics* hopes to transform what was once a fragmented heterodox “community in which the component parts seemed at times to be at war with each other or simply did not acknowledge that others existed.” into a “community where pluralism, not division

Regrettably, to strike the unifying narrative of heterodoxy Lee remained silent over the controversies and conflicts between and within dissenting groups. His history of PKE made no provisions to how internal disputes may have shaped the community/network, despite the historical record being saturated with such debates. Lee has made little of this record, in his discussion of Post Keynesian meetings he bypassed the details of their contents, the discussions undertaken, and the roles played by its participants.

## 6.4 Externalising the history of Post Keynesian Economics

In my review of the histories of Post Keynesian Economics I have kept intellectual and institutional histories separate. They correspond to largely independent narratives. Intellectual histories locate the origins of Post Keynesian Economics to the “revolutionary message” of Keynes and to the lineage of scholars that have followed in his stead. Institutional histories, namely that proposed by Lee, target the repression by the mainstream, in denying access to journals and academic appointments, as the motive force in creating the Post Keynesian network. Comparing the two accounts I have argued that the origins of Post Keynesian Economics have not yet been adequately scrutinized.

I do not contest the claim that the PKE network was created in the 1970s, Lee’s work is invaluable in making this point. His publication of the Eichner-Robinson correspondence<sup>51</sup> revealed a rich chronology of meetings, of institutions gained and lost, that any future history must address in detail. Although King’s intellectual history was overwhelmingly restricted to the reading of texts and their connections, paying little attention to meetings, personalities, or institution building, he too singled out the 1970s as the crucial decade of PKE and mainstream confrontation.<sup>52</sup>

However, the conditions surrounding the formation of the Post Keynesian

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*exists.*” (Lee (2002a), p. 39).

<sup>51</sup>Lee (2000a).

<sup>52</sup>King (2002), chapter 6.

“network” are not so well established. Not least because so much of this body of literature is written by the group’s participants and with presentist concerns. Lee downplayed conflict within and between dissenting groups. He dismissed the need to treat these divisions by providing evidence of cooperation between dissenters and arguing for a current trend of convergence between the groups. Recently, such a tendency has been exemplified by Davidson’s review of King’s history, where he appeared to want to purge Post Keynesian history from anything that did not relate to his particular and current definition of the field.<sup>53</sup> As I noted in the beginning of this chapter, Post Keynesians see history as a vehicle for internal controversy. Such approach to history only conflates the difficulty to understand the past. To stay clear of this instrumentalist baggage, the historian needs to be careful when handling these participant histories as secondary material. In fact, they may prove much more insightful as primary material.

Following Lee, this thesis begins the account of Post Keynesian emergence in the late 1960s but I am aware that the conditions of this emergence have yet been adequately addressed. I also take Post Keynesians’ writing of history as an important element in the making of the group, in the making of their identity.

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<sup>53</sup>Lavoie ((2005), p. 372) replies to Davidson’s critique and rightly points out that King’s definition of Post Keynesian Economics was akin to Davidson’s 1970s and early 1980s definitions of the field. In a later chapter I will discuss in detail Davidson’s boundary work.



## Atlantic alliance

### The emergence of Post Keynesian Economics

#### 7.1 Capital theory, Cambridge economics and survey-history

The historical scholarship on the origins of Post Keynesian Economics offers us two disconnected bodies of work. On the one hand, a detailed genealogy of Post Keynesian ideas ignoring even the existence of a Post Keynesian community with a genesis and a history. On the other, a history of the Post Keynesian network, meticulous in its chronology of events but silent concerning the intellectual content of the meetings, and the agenda of the actors that inhabited the network.<sup>1</sup> It was left unexamined how Post Keynesians developed a sense of shared interests and purpose in the constitution of a Post Keynesian community.

What I aim to do in this chapter is to describe the intellectual element in the constitution of the Post Keynesian community: the construction of a Post Keynesian identity. This identity gave dissenters a narrative perception of who they were, what they stood for and the nature of the profession they inhabited. I aim to show that it is within this identity that dispersed critiques and authors were assembled into a new sociological entity inside the profession, Post Keynesian Economics.

I trace the origins of the Post Keynesian identity to the “Cambridge Capital Controversies” of the 1960s. I argue that a distinction ought to be made between the capital debates of the 1960s, briefly discussed in section two of this chapter, and the

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<sup>1</sup>See previous chapter for a rendition of this claim.

later readings given to the debates in 1968-1969, as it is the latter that is of most interest to the present discussion. The main claim of this chapter is that the emergence of a Post Keynesian identity was not so much a product of the Capital debates but of the historical accounts that strove to make sense of them as they waned into a stalemate. It was in these narratives that Cambridge England redrew the intellectual space of economics, contesting the canon of the profession and presenting an alternative to it. It was in the reinterpretation of the canon and its narratives that the group was to find an identity.

I argue, in section four of this chapter, that the organizational efforts of the 1970s that created the Post Keynesian community were grounded on the identity provided by the “Cambridge Capital Controversies”. The identity was critical for the formation of the group by mediating the coalition between the distinct dissenting approaches that became known as Post Keynesian Economics. I conclude by arguing that to this day the controversies have kept a prominent place in Post Keynesian Economics’ identity.

## 7.2 The capital debates of the 1960s

Over four decades, the capital controversies have continued to motivate an outpouring of reflection and analysis. Hindsight affords many benefits in assessing the terms and consequences of the debate but it may also cloud our understanding of how the debate was perceived by its contemporaries. For the purpose of my argument it is crucial to recover the “uncertainty” contemporaries felt over what was being debated and what was to be concluded. This section places the debate in its context.

### 7.2.1 The 1961 MIT seminar

Joan V. Robinson<sup>2</sup> circa 1960 was known in US academia for her 1933 *Economics of Imperfect Competition*, the publication of which alongside Edward H. Chamberlin’s

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<sup>2</sup>Joan Violet Robinson (1903-1983) born in Surrey, England, studied at Cambridge (MA in 1927) where she taught from 1932 to 1971, becoming Professor of Economics in 1965. Main works include *Economics of Imperfect Competition* (1933); *The Accumulation of Capital* (1956) and *Aspects of Development and Underdevelopment* (1979).

*The Theory of Monopolistic Competition*, transformed equilibrium value analysis (price formation).<sup>3</sup> Soon after publication of this work and following J.M. Keynes' *General Theory* in 1936, Robinson moved away from price theory to devote herself to work on growth theory.<sup>4</sup> Such later work was received with less enthusiasm by American readers. In a review of Robinson's 1956 book *Accumulation of Capital*, Abba P. Lerner commented: "that this line of thought does not seem to the present reviewer to be too helpful", recommending the book as a good exercise for graduate students given the many and demanding models expounded in its pages.<sup>5</sup>

In 1961 Robinson visited the US: she was at MIT and Harvard, she also lectured at Northwestern, Chicago and Washington State Universities, University of Colorado and addressed the Midwestern Economic Association.<sup>6</sup> It was her visit to MIT that is worth noting for the focus of this thesis, as she engaged the Faculty with the criticisms arising from Piero Sraffa's<sup>7</sup> work and the ongoing discussions at Cambridge UK. That same year Pierangelo Garegnani<sup>8</sup> was at MIT and he is credited for having approached Paul Samuelson<sup>9</sup> and Robert Solow with the same critical challenge.<sup>10</sup>

The challenge began with the claim that any quantity measure of capital required the assumption that capital was, or could be approximated as a single homogeneous good. The alternative conceptualization of capital, as money, required a set of relative prices to account for the heterogeneity of capital goods, and in a capitalist economy

<sup>3</sup>It also initiated a famous debate over which of the two approaches was the most adequate. See White (1936) for a contemporary review and discussion of the two approaches.

<sup>4</sup>Robinson (1937a, 1937b, 1952, 1956).

<sup>5</sup>Lerner (1957), p. 693. Inspection of reading lists published by the American Economic Association (Stigler and Boulding (1953); Fellner (1946)) show Robinson referenced but not for her later work on capital theory, references are to Robinson (1937-1938; 1941).

<sup>6</sup>Turner (1989), p. 171.

<sup>7</sup>Piero Sraffa (1898-1983) born in Turin, Italy, graduated from the University of Turin, taught at Perugia (1924-26), Cagliari (1926-27) and Cambridge (1927), from 1927 to 1983 was Fellow of Trinity College, Cambridge. He was editor of the *Works and Correspondence of David Ricardo* (with M. H. Dobb) (1951-1973) and his major work was the *Production of Commodities by Means of Commodities: Prelude to a Critique of Economic Theory* (1960).

<sup>8</sup>Pierangelo Garegnani born in 1930 in Milan, Italy. Has a BA from the University of Pavia (1953) and a PhD from Cambridge (1959) under the supervision of M. H. Dobb. Garegnani taught at Sardinia (1962-66), Pavia (1966-70), Florence (1970-74), Rome (1974-92) and Cambridge (1973-4), he is since 1992 Professor of Economics in Rome.

<sup>9</sup>Paul Anthony Samuelson born in 1915 in Indiana, USA. Has a BA from the University of Chicago (1935) and an MA and PhD from Harvard University (respectively 1936 and 1941). He made his entire career at the Massachusetts Institute of Technology (MIT) becoming professor in 1947 and professor emeritus in 1986. In 1970 he received the Nobel Memorial Prize in Economics.

<sup>10</sup>Hodgson (1997), p. 97.

such prices depended on the rate of profit.<sup>11</sup> Thus in the standard aggregate production function, which is a function of capital *a priori* measured in price terms, the determination of profits that followed led to circular reasoning. Sraffa's work was the source of this insight having claimed to show a host of disturbing properties of such aggregate production functions, namely reswitching or double switching and capital reversing.<sup>12</sup> Reswitching corresponded to the possibility that the same method of production may be the most profitable at more than one rate of profit, even though other methods were more profitable at values in between. Capital reversing corresponded to the value of capital moving in the same direction as the rate of profit.<sup>13</sup>

### 7.2.2 From the 1962 to the 1966 symposia

It was Robinson's seminar that prompted the controversy.<sup>14</sup> In 1962 the British *Review of Economic Studies* (*RES*) held a "Symposium on production functions and economic growth," which included papers by Kenneth J. Arrow, Nicholas Kaldor, James A. Mirrless, Paul A. Samuelson, Robert M. Solow, Joan Robinson, J. E. Meade, D.G. Champernowne, J. Black, Richard Stone and J.A.C. Brown. The major piece of the volume was a paper by Samuelson entitled "Parable and Realism in Capital Theory: The Surrogate Production Function" which was dedicated to "Joan Robinson on the occasion of her memorable 1961 visit to MIT."<sup>15</sup> In the paper the author formalized a "surrogate production function" that was safe from the Sraffian results,

<sup>11</sup>Throughout the chapter I shall refer to the term "rate of profit" following the Cambridge England use, since this is the side of the debate that I am following more closely, the neoclassical side preferred "rate of interest" to label the return to capital, see Cohen and Harcourt (2003), fn.7, for the distinction.

<sup>12</sup>Sraffa (1960).

<sup>13</sup>Birner ((1996), pp. 227-229) has argued that Sraffa's book was not the igniter of the controversies. This is consistent with evidence of a cold reception which greeted Sraffa's book in the US. A reviewer commented: "This is a terse book." (Reder (1961), p. 688) and assessed that had it been published in the late 1920s or 1930s it would have constituted a major theoretical innovation but work in Von Neumann models had now anticipated much of what Sraffa was saying (ibid., p. 695). It was Robinson and Garegnani's challenges made to Samuelson at MIT that led the latter to take notice of Sraffa's reswitching. The controversy begins when Samuelson engages with the criticism.

<sup>14</sup>I interviewed a former MIT graduate student that was present at the 1961 seminar. He recalled the polemical tone Robinson gave to the lecture, claiming that neoclassical economics was defunct. He also added that only Samuelson and a few others seemed to follow her argument, he had not at the time (interview with Stephen Resnick, 2003).

<sup>15</sup>Samuelson (1962), p. 193.

thus preserving what the author called neoclassical parables, namely: an association between lower rates of profit and higher values of capital per man employed; an association between lower rates of profit and higher capital-output ratios; an association between lower rates of profit and higher sustainable consumption per head; and in competitive markets, a distribution of income between profit-earners and wage-earners explained by knowledge of marginal products and factor supplies.

As Robinson was later to confess: "For several years, everyone (except Piero Garegnani) was somewhat baffled by the surrogate production function. Then, in 1965, a fortunate accident occurred. A disciple of Professor Samuelson claimed to have proved that reswitching can never occur".<sup>16</sup> David Levhari's<sup>17</sup> 1965 piece in the *Quarterly Journal of Economics (QJE)* was an attempt to sweep the Sraffian criticism away. In Robinson's words Levhari's contribution was a fortunate accident because it was soon shown to contain a formal error, prompting a new series of exchanges. The Harvard based *QJE* in 1966 gave a follow up to the Levhari article under a symposium on "Paradoxes in Capital Theory."<sup>18</sup> The symposium included a counter example to Levhari's theorem by Pasinetti,<sup>19</sup> two articles discussing the significance of reswitching,<sup>20</sup> a refutation of the non-switching theorem by Morishima<sup>21</sup> and Samuelson and Levhari's admission that the non-switching theorem was false.<sup>22</sup>

The concluding piece in the *QJE* symposium was Samuelson's "Summing up," where the author both recognized the possibility of reswitching, and provided an intuition for it based on an "Austrian circulating capital model."<sup>23</sup> In Samuelson's concluding remarks he praised the Sraffian critics for demonstrating reswitching as a logical possibility of which economists needed to be aware, he ended: "If all this causes headaches for those nostalgic for the old time parables of neoclassical writing,

<sup>16</sup>Robinson (1975), p. 37.

<sup>17</sup>David Levhari was a graduate student of Samuelson at MIT. Levhari concluded his PhD thesis entitled "Essays on optimal economic growth" in 1964.

<sup>18</sup>"Paradoxes on Capital Theory: A Symposium" in the November 1966 issue of the *Quarterly Journal of Economics*.

<sup>19</sup>Pasinetti (1966).

<sup>20</sup>Bruno *et al* (1966); Garegnani (1966).

<sup>21</sup>Morishima (1966).

<sup>22</sup>Samuelson and Levhari (1966).

<sup>23</sup>Samuelson (1966), p. 573.

we must remind ourselves that scholars are not born to live an easy existence. We must respect and appraise, the facts of life.”<sup>24</sup>

For a reader more familiar with the controversies, my description may seem rather narrow. I made note of only two sets of exchanges (the *RES* and *QJE* symposia), in what was later portrayed as a debate “raging” over three decades. The reason for this narrowing of focus is that I wish to present the debate in its immediate temporal context, that is, as it was perceived in the mid-1960s, before the reconstructions of the debate produced by the late 1960s and early 1970s surveys.

The reader that I am interested in is American. As we shall see particularly in the next chapter but also later in this one, Post Keynesian Economics despite its intellectual connections with Cambridge England was of American design, a socio-historical fact not often noted in Post Keynesian histories due to their intellectual focus. For the American audience, the American *QJE* exchange was better known than the British *RES* one. Hence, without the surveys and the histories as guides to travel through the literature, the reader of the mid-1960s may have recognized a debate but would not have easily considered it part of a larger controversy. Furthermore, it would not have been easy for such a reader to appreciate the significance of what was being debated. It is against this uncertainty concerning the content and significance of the debate that the surveys emerged.

### 7.2.3 The 1969 survey

The symposia mentioned above had a significant impact in MIT and Cambridge, and in fact all the main participants in the symposia were connected to these two institutions.<sup>25</sup> These participants had a reading from their own experience of what was under discussion and what could be concluded, but the same is not necessarily true for the remainder of the profession. As E. Roy Weintraub notes: “from the present we look back and see order, but that order was not always evident to those who were in the field at the time.”<sup>26</sup> The survey article provides order as it

<sup>24</sup>Ibid., p. 583.

<sup>25</sup>See Cohen and Harcourt (2003), p. 200, for a list of the main participants.

<sup>26</sup>Weintraub (1991), p. 129.

reconstructs past work so that it may be seen as leading somewhere, to knowledge, or in this case to the debate's resolution.

The first survey appearing in the *Journal of Economic Literature* (*JEL*) in 1969 was apparently not commissioned as an account of the debate. There was an earlier request made by Mark Perlman, editor of the *JEL*, to Jack Hirshleifer for a survey on capital theory: "stressing the developments in capital theory in the last five to ten years.". Hirshleifer accepted the request and added: "I follow the Fisher tradition which considers most of the traditional debates about 'capital' irrelevant. What all this leads to is that I would be more comfortable if the assigned topic were, say, 'inter-temporal decision and equilibrium' rather than 'capital theory'." Compromise was met with the title proposed by Perlman: "Some Modern Aspects of Capital Theory: Inter-Temporal Decision and Equilibrium."<sup>27</sup> However, by July 17 Hirshleifer wrote with the news that he would not be able to write the piece.

Perlman was in Australia for the Summer of 1968. In G.C. Harcourt's<sup>28</sup> words: "So there Perlman was, with the second issue of this new journal and no one to do the major survey article. Wilfred [Prest] said, 'Don't worry, go to Adelaide and ask Geoff Harcourt. He's good on capital theory, he'll do it for you.' So Perlman came over and spent a day talking me into it."<sup>29</sup> In a letter formally commissioning the survey, Perlman asked Harcourt to write a survey "reviewing recent developments in capital theory" covering the preceding decade and then added: "I am particularly interested that American readers should have the opportunity to see where Mrs. Robinson's views come from and how they may be contrasted with the views of other writers."<sup>30</sup>

The evidence suggests that Perlman was not looking for a survey of the capital debates. The *Journal of Economic Literature* had just been created by the American

<sup>27</sup>April 24<sup>th</sup>, 1968; May 14<sup>th</sup>, 1968; May 20<sup>th</sup>, 1968. Letters in Mark Perlman Papers, The Economists' Papers Project, Rare Book, Manuscript, and Special Collections Library, Duke University, Durham, North Carolina, Box 24.

<sup>28</sup>Geoffrey Colin Harcourt was born in Melborne, Australia in 1931. Harcourt has a BCom, MCom from Melbourne (1954, 1956) and a PhD (1960 - initially with Nicholas Kaldor but in the end under the supervision of Ronald Henderson) and a LittD from Cambridge (1988). He taught at Adelaide (1958-85) where he became Professor in 1967, from 1982 to 1999 he held a Readership at the University of Cambridge.

<sup>29</sup>King (1995a), p. 176.

<sup>30</sup>28<sup>th</sup> August, 1968. Letters in Mark Perlman Papers, The Economists' Papers Project, Box 24.

Academic Association as an outlet for book reviews and the occasional survey of the economics literature. Foremost, Perlman was looking for a survey on the broad subject of capital theory and was willing to negotiate the survey's focus with the assigned writer.<sup>31</sup>

### 7.3 The Cambridge Capital Controversies

It was Harcourt's survey to write. It is beyond question that the text bore his personal mark, both his experience in capital theory and his humorous and polemic style of writing. But the text was also a collective undertaking. Harcourt had to draw on a store of meaning with which to make sense of the debates. Thus in this section I examine which resources Harcourt drew to support his survey writing. I also examine the content of the survey against other readings offered in the early 1970s on the debates.

#### 7.3.1 Writing the survey

To write the survey Harcourt divided it into five self-contained working papers and sent them to some of the participants of the debate to gauge early reactions to his interpretations.<sup>32</sup> That is, in order to write about the controversies Harcourt engaged in a dialogue with participants from both sides of the debate. By looking at these exchanges<sup>33</sup> we are able to better appreciate what views were represented in the survey.

In August 1968, Harcourt wrote to Robinson with the news of the commissioned project. In Robinson's reply she outlined what she thought ought to be the content of the survey: "Various attempts have been made to move from the one-commodity world to a multi-commodity world, but these have been finally discredited in the double-switching controversy."<sup>34</sup> This excerpt (underlined by Harcourt in the letter)

<sup>31</sup>I should note however that I have asked Perlman whether this was the case and his reply was inconclusive (Private communication Mark Perlman, 5<sup>th</sup> July 2003).

<sup>32</sup>Harcourt (2000), p. 278.

<sup>33</sup>Harcourt (1969) acknowledges the people that had access and input to the working papers, however the original list as found in Harcourt's personal correspondence was more extensive including 28 names.

<sup>34</sup>Letter from Harcourt to Perlman, 13<sup>th</sup> August 1968. Geoff C. Harcourt Personal Papers. I am



outlined a historical position that as we shall see, was reflected in the final text of the survey. The double-switching controversy was the subject of Harcourt's first working paper and that the other sections/working papers were written around it.<sup>35</sup> Harcourt later re-organized them in chronological order, since in writing them he moved from the present to the past.

In September 1968, with the plan of the survey in view, Harcourt wrote to Perlman:

The selection of issues that I have made is influenced by my own association with the puzzles as they have arisen and there is in my bibliography a distinct Cambridge England, Cambridge America preponderance. This may seem to some readers a distortion of what in the long run will turn out to be the more important problems but I think it fits in reasonably well with your view that American readers should have an opportunity to see where Mrs. Robinson's views come from, etc.<sup>36</sup>

The focus of the survey was therefore moving away from treating "recent developments in capital theory" in the preceding decade to treat the views of the two Cambridges, England and America.<sup>37</sup>

The imprint of a Cambridge England perspective was strong both in style and content. It was derided by some of the readers of the working papers, from an Oxford economist: "[I] would go through the paper eliminating all the light-hearted remarks that go very well in a Cambridge seminar but do not read so well in print",<sup>38</sup> while praised by others, from a Cambridge England economist: "I found your exposition clear and interspersed with carefully chosen puns that enliven it nicely. Actually I found in it many of the characteristics which I normally associate with Joan Robinson."<sup>39</sup> From the Cambridge England readers of the working papers came

indebted to G. C. Harcourt for having allowed me to study his private records and correspondence on the subject of the 1969 survey.

<sup>35</sup>Working paper 2 was: "The rate of profit"; Working paper 3: "Solow on the rate of return"; Working paper 4: "Wicksell effects" and Working paper 5: "Malleability, Fossils and Technical Progress" (Private communication G. C. Harcourt, 6<sup>th</sup> April 2004).

<sup>36</sup>Letter from Harcourt to Perlman, 6<sup>th</sup> September 1968. G.C. Harcourt Personal Papers.

<sup>37</sup>Hahn and Matthews's (1964) survey of growth theory was then seen as the standard to match.

<sup>38</sup>Letter by W.M. Corden to Harcourt, 16<sup>th</sup> January 1969, G.C. Harcourt Personal Papers.

<sup>39</sup>Letter by L. Pasinetti to Harcourt, 14<sup>th</sup> June 1969, G.C. Harcourt Personal Papers.

encouragement and corrections, such as points of priority and clarification,<sup>40</sup> from Cambridge America readers came mostly indifference. Solow saw Harcourt's working papers as grounded on old misunderstandings between him and Robinson, and one-sided in its indictments of ideological bias and error.<sup>41</sup> Solow concluded:

To tell you the truth, I've retired from this debate. It just makes time away from useful pursuits. It infuriates Joan that I won't dance any more, but you get enough of the stately sarabande after a while. Nothing ever changes and I keep thinking that I could be estimating the demand curve of peanut butter or doing something else that might actually help the world and advance economics.<sup>42</sup>

A third view expressed in this correspondence was that of estrangement from what was being discussed:

I think you take the disputants (on both sides) a little too much at face value, and you take Paul Samuelson (and somewhat less so, Bob Solow) as the canonization of neoclassical economics. That is perhaps how both Cambridges view it, but does not do justice to the less polemic (but no less neoclassical) economists, (e.g. Meade).<sup>43</sup>

Taken together the comments by readers of the working papers show a survey taking shape as a distinctly Cambridge England assessment of the debate.

### 7.3.2 The survey and its narrative

Harcourt's final survey paper entitled "Some Cambridge Controversies in the Theory of Capital" was divided into four sections: "Malleability, Fossils and Technical

<sup>40</sup>For instance Dobb in a letter of 6<sup>th</sup> January 1969 to Harcourt, established Sraffa's priority over reswitching, according to Dobb even though in print Sraffa only discussed it in 1960, it was a result informally discussed in Cambridge much earlier. In Harcourt's private records some of the working papers' early drafts show extensive comments by Robinson, Pasinetti, and others.

<sup>41</sup>Perlman also requested Samuelson's comments on the survey, "Although there are a number of interpretations that I might not deem optimal, I think it worthy of publication. (...) Survey items like this should represent the opinions of the author and not be subject to overmuch auditing and refereeing." (letter from Samuelson to Perlman, 22<sup>nd</sup> January, 1969). A harsher assessment came from another reviewer: "It is a rather one-sided (Robinsonian) survey of the literature written primarily for Cambridge, England. I don't think it really exposit well the Robinsonian position to those that don't know it or presents the M.I.T. side of it fairly." (Zvi Griliches to Perlman, January 22, 1969). The two letters in Mark Perlman Papers, The Economists' Papers Project, Box 24.

<sup>42</sup>Letter from Solow to Harcourt, 8<sup>th</sup> January 1969, G.C. Harcourt Personal Papers.

<sup>43</sup>Letter from J. Stiglitz to Harcourt, 11<sup>th</sup> December 1968, G.C. Harcourt Personal Papers.

Progress”; “Solow on the Rate of Return: Tease and Counter Tease”; “A Child’s Guide to the Double-Switching Debate” and “The Rate of Profit in Capitalist Society”, with a technical appendix on Wicksellian effects, it followed closely the five working papers.<sup>44</sup>

The first section discussed a series of exchanges prompted by Robinson’s 1953-4 question: How to measure capital?<sup>45</sup> Robinson is placed at the start of the series of contributions, but the main protagonists were neoclassical economists: some responding directly to Robinson’s question,<sup>46</sup> others surveyed to show different responses to the same question even though Robinson’s challenge was not their motivation. Harcourt reconstructed work on growth models as a series of attempts to answer Robinson’s question. Despite all these efforts, Harcourt suggested that the Robinson problem remained unanswered; no satisfactory measurement of capital was ever presented by the neoclassicals.

At the core of the second section of his paper lay a debate between Solow and Robinson. According to Harcourt, Solow had in his De Vries lectures of May 1963<sup>47</sup> “[prefaced] his analysis with a discussion of why there are recurring controversies in capital theory. He gives two reasons, one of which is ideological - the social function of providing an ideological justification for profit which in the 19th century was the non-Marxist backlash; the other reason is analytical - it’s difficult.”<sup>48</sup> To escape the ideological weight of the debates Solow had drawn a “conceptual distinction between the imputed return to capital and the income of capitalists”.<sup>49</sup> Robinson attacked Solow, interpreting his work as “an attempt to justify the marginal productivity theory of distribution and, in particular, the microeconomic proposition that in a competitive capitalist economy labor is paid its *full employment* marginal product.”<sup>50</sup> Thus, the purpose of this section was not to shed light on another theoretical debate

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<sup>44</sup>Harcourt (1969).

<sup>45</sup>Robinson (1953-54).

<sup>46</sup>Champernowne (1953-54); Solow (1955-56); Swan (1956).

<sup>47</sup>Solow (1963).

<sup>48</sup>Harcourt (1969), p. 380.

<sup>49</sup>Idem.

<sup>50</sup>Ibid., p. 382.

but to "highlight some of the causes of the controversy,"<sup>51</sup> namely ideological ones.

The third section of Harcourt's survey paper presented the reswitching controversy as the high point of the debate and intimately linked these ideas to Sraffa's *Production of Commodities by Means of Commodities*.<sup>52</sup> Harcourt placed Sraffa not as a third independent entity holding a distinct theoretical construct but as a contributor on the Cambridge, England side of the debate. Sraffa's work was shown as concerned "with a view of economic life and especially of the production process, price formation and income distribution which was more akin to that taken by the classical economists – Smith, Ricardo, Marx – than by the neo-classicals – Jevons, Wicksteed, Marshall".<sup>53</sup> In doing so, Harcourt suggested a link between Sraffa and Robinson's own interest in (re-)introducing Marx to economics. Harcourt thus added the Sraffian reswitching and capital reversing ideas to the arsenal of the Cambridge England theorists. The early 1960s debates in the *RES* and *QJE*, and Samuelson's "Summing up"<sup>54</sup> were presented by Harcourt as the point at which neoclassicals were forced to abandon their neoclassical parables and therefore aggregate production functions and the marginal productivity theory of distribution.

Finally we are lead to a section on the rate of profit. The statement that connected the sections was: "that, in order to determine the rate of profit, further factors have to be introduced from outside the production system," such factors were the "saving propensities" in the work of Nicholas Kaldor, Luigi L. Pasinetti and Robinson. Harcourt after having presented a Cambridge England critique to neoclassical economics gives the reader an alternative economics - a Cambridge England alternative.<sup>55</sup>

Harcourt molded together several distinct instances of debate into his survey. All were broadly connected to the concept of capital in growth and distribution, thus

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<sup>51</sup>Ibid., p. 380.

<sup>52</sup>Sraffa (1960).

<sup>53</sup>Harcourt (1969), p. 378.

<sup>54</sup>Samuelson (1966).

<sup>55</sup>It is arguable if what Harcourt presented was an alternative. Such a claim is certainly rejected by neoclassicals but it is also subject to debate by those who came to stand side by side with the Cambridge England critics. Harcourt himself wrote on the possibility and existence of such a complete alternative, see King ((2002), chapter 10) for a review of this literature.

justifying the choice of title for his survey, multiple capital controversies and not a single controversy.<sup>56</sup> While Perlman had commissioned a survey on developments in capital theory over the preceding decade, Harcourt went as far back as 1953 to argue that the controversies of the 1960s should be seen as part of a broader critique set out by the work of Robinson, Kaldor, Pasinetti and Sraffa, or following the author's label: Cambridge, England. Harcourt's survey was not an inquest into a sub-field of theory rather, his presentation was a narrative of emergence. In the narrative Robinson had challenged the mainstream that promptly reacted, but to no avail. Harcourt told us the mainstream's efforts were unable to meet her challenge (section 1 of the survey). The debate with Solow was made to show the ideological motivation of the controversy, and its political and social content, Harcourt described Solow's attempt at escaping the ideologically content of the debate as a failure (section 2 of the survey). He then brought in Sraffa to administer the final and definitive blow, one that was written as a logical refutation of neoclassical belief, and grounded his claim in the fact that Samuelson had conceded defeat (section 3 of the survey). The stage was set for something to happen, and Harcourt presented an alternative that he claimed was immune to the problems raised by the critics (section 4 of the survey).

In contrast to the strong claims made in the body of his article, Harcourt's conclusion may at first sight seem conservative: "We break off in midstream and few issues are settled. A key one relates to marginal productivity and its role in distribution, about which as we have seen there is a complete cleavage of opinion on the significance of double-switching results for this issue."<sup>57</sup> This open-ended conclusion was an invitation for the reader: the critique/alternative that Harcourt had narrated as emerging was an incomplete edifice to which the reader could contribute. And Harcourt predicted that the debate would surely move into more fundamental areas "for it is the *general methodology* of neoclassical analysis, rather than any particular result, which basically is under attack".<sup>58</sup> With the writing and

<sup>56</sup>The title of the survey also resonated Harry Johnson's 1950s survey "Some Cambridge Controversies in Monetary Theory" (Johnson (1951-52)).

<sup>57</sup>Harcourt (1969), p. 398.

<sup>58</sup>Idem.

rewriting of the history of the controversies the theoretical importance of reswitching and its place at the center of the controversies changed. By 1971 Robinson was downplaying the importance of reswitching and in 1975 her plea was to move the debate into methodology and abandon its technical emphasis (namely issues concerning reswitching).<sup>59</sup>

My account here is designed to highlight, as the most important feature of Harcourt's survey, the narrative that lay in the background. The author constructed a parting of ways and the formation of two distinct camps. In the language adopted throughout this thesis it was an instance of boundary work. In this narrative we see how two sides take shape, a neoclassical one where aggregate production function theorists struggle to solve the puzzles of dealing/measuring capital, and a Cambridge England side where there is an abandonment of such attempts and the search for new solutions, initiated through Robinson's *Accumulation of Capital* and the later work of Kaldor and Pasinetti.

### 7.3.3 Harcourt's critics

A book followed the 1969 survey, the plan of which closely resembling that of the article, it contained five chapters with the same titles as the article's sections.<sup>60</sup> The book was a more elaborate rendition of the survey's argument, the author aiming to "extend the assertions into what I hope are persuasive or, at least, respectable

<sup>59</sup>Robinson (1971; 1975). No methodological reenactment of the debate followed, neoclassicals did not engage with Robinson's redesigned critique (Samuelson and Solow commented on Robinson's "Unimportance of Reswitching" but merely to discuss the significance and status of reswitching for neoclassical theory (Robinson (1975); Samuelson (1975); Solow (1975)). Her call was met only in later reconstructions of the debate that began to identify two parallel critiques set out by Cambridge, England: one was the reswitching result which was the core issue debated in the *RSE* and *QJE* symposia; the other was Robinson's equilibrium versus history critique, presented as a deeper methodological challenge to the mainstream (see Harcourt (1975; 1976)). And Harcourt by 1994 commented on the controversies: "I think the methodological nature of Joan Robinson's critique is the dominant one." (King (1995a), p. 178) As the controversies were being reconstructed in later histories and surveys, new readings were being attached to them.

It is not my task in this chapter to assess these later reconstructions. What matters for the present argument is to distinguish these later readings from the Harcourt (1969; 1972) ones where the equilibrium vs history critique was not dominant.

<sup>60</sup>Harcourt (1972). However, the contents of the article's section I: "Malleability, Fossils and Technical Progress" were broken into two book chapters: "Search for a will-o'-the-wisp: capital as a unit independent of distribution and prices" and "Treacle, fossils and technical knowledge".

arguments.”<sup>61</sup> What was added to the book was an introduction that summarized the argument and highlighted as its main lesson the “fundamental cleavage” between the two Cambridges. The clash of views was portrayed as rooted in ideological and political differences and in distinct intellectual lineages. It was said that while the neo-classicals followed the tradition of the marginalists, Cambridge England followed that of the classicals (David Ricardo and Karl Marx).

To understand what is entailed by Harcourt’s position and to assess the significance of the survey’s narrative for the economic literature of the time one needs to first consider the major resource he used for his boundary work, the canon. Harcourt’s boundary work was predicated on the selection, ordering and interpretation of texts and was meant to identify the crucial materials for the discipline. In so doing he was redefining the canon. It is significant that Harcourt made allusions to “founding fathers” of economic theory such as Ricardo and Marx and offered new interpretations of their work, namely by placing them as alternative and opposite to Alfred Marshall and Leon Walras (the marginalists). Alongside the classicals the Cambridge England texts were read as a revolutionary approach overcoming the problems endemic to all previous literature. It followed that Cambridge England should replace Cambridge Massachussets. The construction of the present required a new interpretation, a reconstruction, of the past, a new history for economics. The survey should be read as an attempt at the reconstruction of the discipline’s canon, and in the process mapping in economics those approaches that should be adopted and that should replace those that existed.

But Harcourt’s attempt at reconstructing the canon failed to gather the acceptance required to produce a sweeping reshaping of the discipline. Soon after its publication the survey was challenged. In 1974 three surveys were published that portrayed the controversies in ways that disagreed with Harcourt’s 1969 survey and his 1972 book. By presenting alternative ways to read the controversies, these three surveys underline the particularity of Harcourt canonical map and assert it as but one

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<sup>61</sup>Harcourt (1972), p. vii.

of many competing interpretations available in the discipline.<sup>62</sup>

John Hicks writing in 1974 in the *American Economic Review* also recognized two competing visions in economics, but the author had his eyes on a longer history. Hicks made a distinction between a Fundist position in capital theory with its roots in Adam Smith and British classical authors (capital is a fund and capital's value is derived from the value of future net products) and a Materialist position going back to Cannan, Marshall, Pigou and J.B. Clark (where capital consists of physical goods whose value is the market value of the capital goods).<sup>63</sup> Hicks however saw no destructive tension between the two since they were both worth pursuing. Both definitions had their place and importance, one was forward looking, the other backward looking. In Hicks' understanding, debates about capital were therefore often a product of misunderstanding, but one with deep roots in the history of economics, namely that one had to choose between the two views. For Hicks, the 1960s Capital controversies were just another instance of this misunderstanding. In his version of the never-ending capital debates, the English side was seen as taking a Fundist position, while the American one a Materialist stance. So in Hicks' account there was none of the novelty in approach that Harcourt associated with the Cambridge England side. Rather, what motivated the analytical standoff was not discovery and theory development but two groups working under distinct and well-established conceptualizations of capital.

A more direct challenge to Harcourt came from Joseph Stiglitz and his 1974 review of Harcourt's book. Stiglitz engaged with the Cambridge England theories and gave voice to the neoclassical criticisms of this approach. In this account, critique did not run solely from England to the USA. Stiglitz identified a number of weaknesses in the Cambridge, England approach: for instance, their models were not amenable to be extended to include more than two factors (beyond labor and capital).<sup>64</sup> The

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<sup>62</sup>As Samuels notes, in economics it may be that plurality of interpretations is more frequent than a single view: "Throughout its history, at every point in time, economics, has been comprised of multiple schools of thought. Moreover, each school of thought (and its theories and doctrines) has been given multiple interpretations." (Samuels (2001), p. 485).

<sup>63</sup>Hicks (1974), p. 309.

<sup>64</sup>Stiglitz (1974), p. 895.



author also charged the Cambridge England theorists with rejecting any attempt at empirical verification.<sup>65</sup> Stiglitz did recognize an emerging theory in the Cambridge England group but made it the object of fierce criticism. This stands in contrast with Harcourt's survey where neoclassicals were reacting to a challenge but never responded with criticism towards the alternative approach. Stiglitz presented the new theory as empty of promise, negating Harcourt's identification of an alternative.

Of all the pieces here discussed Mark Blaug's discussion of the *Cambridge Revolution* was the most akin to Harcourt's in its structure. It too identified the existence of competing theories, the neoclassical and the Cambridge England one, the latter defined as composed by the contributions of Kaldor, Pasinetti and Robinson.<sup>66</sup> It too assumed the need for comparison and judgment, and saw the controversies in this light.

The Cambridge UK theories are certainly logically consistent, even if they do not always hang together in a logically consistent, total framework of theories. They are possibly more realistic in some of their basic assumptions, although the statement is itself highly ambiguous. But they are not simpler, they are not more elegant, they are totally incapable of producing testable predictions. Whatever is wrong with neo-classical economics (and who doubts there is much to complain of?), it wins hands down on all possible criteria.<sup>67</sup>

What is noteworthy in Blaug's discussion is that his theory appraisal did not attach laurels of victory to the logically consistent one over the alternative. Unlike in Harcourt's survey, Samuelson's recantation was not sufficient to sustain the claim that the Cambridge England theory had "won" and therefore should be adopted.

The comparisons between Harcourt's 1969 article and these three 1974 surveys reveal the controversial nature of the claims embedded in Harcourt's construction of a canonical narrative. The comparison with Hicks' paper shows Harcourt's stress on novelty and the importance of the debate in sustaining his claim that a theoretical alternative is emerging. The comparison with Stiglitz's article brings out a hidden

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<sup>65</sup>Ibid., p. 901.

<sup>66</sup>Blaug (1974), p. 61.

<sup>67</sup>Ibid., p. 85.

dimension of the controversies obscured in Harcourt's survey, namely that of criticisms directed from the US/neoclassical side to the UK side. Harcourt's article had portrayed only the attack of a challenger on the entrenched *status quo*. Finally, the comparison with Blaug leads one to question the resolution of the controversies as Harcourt presented it. While Harcourt saw an admission of defeat by the neoclassicals, Blaug saw an alternative that had not proven itself and a critique that had little impact when considering the neoclassical edifice in all its breadth and strength of analysis.

These comparisons underlie how Harcourt's 1969 survey constructed the controversies to identify the formation of a new space for theory development, a space filled with promise. Harcourt set the beginning of the controversies at the point when the first line is drawn between the two groups, though according to Hicks, that debate was a long running one. Harcourt portrayed the demarcation line splitting the field of economics as coming from the Cambridge England side, while in Stiglitz the demarcation was being drawn by critiques originating from both sides of the debate. As the culmination of this process of definition Harcourt identified a victory over the old theory, whereas Blaug argued that there was no victory and so no new viable space.

#### 7.4 The Post Keynesian Economics alliance

Harcourt through his survey of the capital controversies aimed to identify a new space in economics. Carving out the literature he invested this space with an ancestry – the classical economists, Keynes and the Cambridge economists; proven critical powers – victory in debate over MIT; and promise of the future economics – a field yet unexplored of great potential. This space was to become Post Keynesian economics, Harcourt called it “neo-Keynesian.”<sup>68</sup> To be effective it would have to attract a sizable following and secure an institutional presence. My task in this section is to show how Harcourt's “Cambridge capital controversies” were used to create the Post

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<sup>68</sup>More about labels to follow in the next chapter.

Keynesian community and have continued to play a role in its social reproduction.

#### 7.4.1 Contacts between American dissenters and Cambridge

Despite its critics Harcourt's survey and book<sup>69</sup> also enjoyed a wide and favorable response. From the editor of the *JEL*, Perlman wrote to Harcourt: "Many students have written to me asking for reprints and I imagine that it has become standard stock in virtually all graduate courses in macroeconomic analysis (it's been stolen from our library – a sign of success the librarian tells me)."<sup>70</sup>

One of the most significant effects of Harcourt's survey was in prompting the contact between groups of dissenting economists in the USA and Cambridge, England economists. One case that I wish to discuss in greater detail is that of Paul Davidson,<sup>71</sup> partly because of the role this author later played in the history of the group<sup>72</sup> but also because there is archival evidence that allows us to follow his changing perception of the discipline during the period.

In 1967 in the aftermath of the *QJE* symposium, Davidson corresponded with Samuelson: "I enjoyed your seminar yesterday but I am sorry I didn't get into a further discussion of the income distribution aspects of the controversy between the two Cambridges. I find something to agree with and some areas of disagreement with in both schools of thought.", adding that he had written two articles on the subject one for the *Economic Journal* where he attempted "to put neo-classicism in a Keynesian prospectus" and another in *Econometrica* which "attempts to blend Keynes' views of the *Treatise* and *The General Theory* into a modern theory of capital accumulation".<sup>73</sup> Both articles were however agnostic towards the debate and

<sup>69</sup>Harcourt (1969; 1972).

<sup>70</sup>Letter from Perlman to Harcourt, 26<sup>th</sup> November, 1969. In G.C. Harcourt's Personal Papers.

<sup>71</sup>Paul Davidson born in 1930 in New York, USA, obtained a BS in Chemistry from Brooklin College in 1950, an MBA from City University, NY, in 1955 and a PhD in Economics in 1959 from the University of Pennsylvania. Davidson taught at Pennsylvania (1955-1958), at Rutgers University (1958- 1986) and from 1986 has held the Chair of Political Economy at the University of Tennessee. He is founding and current editor of the *Journal of Post Keynesian Economics* (since 1978). His major works include *Aggregate Supply and Demand Analysis* (with E Smolensky, 1964); *Money and the Real World* (1972) and *International Money and the Real World* (1982).

<sup>72</sup>Of which more later, chapter 8 is devoted to debates within Post Keynesian Economics.

<sup>73</sup>Letter from Davidson to Samuelson, May 2<sup>nd</sup>, 1967 in Paul Davidson Papers, at Rare Book and Manuscript Library, Duke University.

were instead devoted to a study of the labour market following an aggregate demand-aggregate supply analysis<sup>74</sup> and the role of money and Keynesian uncertainty to the investment/capital accumulation decision.<sup>75</sup> Davidson in 1967-68 should be properly characterised as writing largely outside the debate.

Also in 1967 Davidson began to correspond with Joan Robinson presenting her with his work on the demand for finance. A long theoretical exchange was to follow. At Robinson's instigation the discussion ran into themes of the capital controversies. In this Davidson favourably compared the "Cambridge school" to "Samuelson's neo-classical muddle"<sup>76</sup> but hesitated to embrace it. While Robinson emphasized the importance of introducing the rate of profit into a theory of investment and capital, Davidson argued that to be fully convinced some work had still to be done in linking profit to the theory of effective demand.<sup>77</sup> Davidson resisted the approach, reporting the results of the correspondence to his economics mentor Sidney Weintraub,<sup>78</sup> who replied:

I suggest that you write her [Robinson] a kind note indicating your acceptance of her view on profit, and rather complete agreement, while proposing to work out some of the details and qualifications at a later date. There is every reason for you to make a friend of her, though many find her equally hostile to friend and foe.<sup>79</sup>

As Davidson moved closer to the Cambridge England perspective in his correspondence with Robinson, in 1969 he received a draft of Harcourt's *JEL* survey which prompted the comment: "Received your latest manuscript and a fast reading leaves me very impressed. I would very much encourage you to develop this into a

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<sup>74</sup>Davidson (1967).

<sup>75</sup>Davidson (1968).

<sup>76</sup>Letter from Paul Davidson to Joan Robinson, August 30<sup>th</sup>, 1967 in Turner (1989), p. 186.

<sup>77</sup>Letter from Paul Davidson to Joan Robinson, King's College Library, Cambridge, Joan Robinson Papers, JVR/vii/14/29.

<sup>78</sup>Sidney Weintraub (1914-1983) was born in Brooklyn. He studied at LSE and New York University where he received his PhD in 1941. Weintraub taught at St. Johns University (then in Brooklyn) and was occasional visiting teacher at the New School for Social Research prior to his move in 1950 to the University of Pennsylvania. He was a founding member of the *Journal of Post Keynesian Economics*. His main works include *Price Theory* (1949); *Income and Employment Analysis* (1951); *An Approach to the Theory of Income Distribution* (1958); *Keynes, Keynesians and the Monetarists* (1973).

<sup>79</sup>Letter from Weintraub to Davidson, August 29<sup>th</sup>, 1967 in Paul Davidson Papers, The Economists' Papers Project, at Rare Book and Manuscript Library, Duke University, Box 6.

book. Because of the comments you have made some of the points you have made on the controversy make some sense to me.”<sup>80</sup>

The Harcourt survey article and book,<sup>81</sup> which were contemporaneous to this trans-Atlantic correspondence, added status to the Cambridge England positions, reinforcing the contact already made between dissenters in Britain and the USA. As E. Roy Weintraub has argued, surveys amongst other things, validate the importance of the subject for the wider academic audience and may set directions for theory development.<sup>82</sup> Harcourt’s survey told, as its main moral, that Cambridge England was a powerful critique of the mainstream in need of further work,<sup>83</sup> an invitation American dissenters’ took seriously:

now that a seemingly mortal blow to the neoclassicals has been delivered, creating a tremendous redundancy in the human capital stock in this side of the ocean, the American school will try to restore its capital values by drawing the Anglo-Italian school into an endless controversy as to which of the two simplified unrealistic models – the Cambridge, Massachusetts or the Cambridge, England one – is the least unrealistic.<sup>84</sup>

Paul Davidson was in 1969-70 calling for the construction of a new and “realistic” economics, portraying a mainstream that had lost credibility from the Cambridge England attack. The new theoretical development however had to respect the boundaries drawn by the controversies; it had to fit the theoretical space defined by Harcourt’s survey. A sizable portion of the Davidson-Robinson correspondence during the early 1970s was focused on the introduction of profit into Davidson’s analysis. In 1970-71 he visited Cambridge, where he wrote his book *Money and the Real World*: “In this book I try to integrate this monetary view with the growth theories of Harrod and the neo-keynesians”<sup>85</sup> and his chapters XI-XII in *Money and the Real*

<sup>80</sup>Letter from Davidson to Harcourt, 9<sup>th</sup> January 1969 in Paul Davidson Papers, The Economists’ Papers Project, Box 3.

<sup>81</sup>Harcourt (1969; 1972).

<sup>82</sup>Weintraub (1991), p. 132.

<sup>83</sup>As noted by King ((2002), p. 121): “It seemed for a while to many critics of neoclassical economics as though the enemy had been taken on and beaten, on its own ground (...). There was the real prospect (...) “Post Keynesian” economics might constitute a potential lethal threat to orthodox thinking.”

<sup>84</sup>Letter from Paul Davidson to Joan Robinson, 15<sup>th</sup> of December 1969, Joan Robinson Papers, King’s College Library, JVR/vii/114/88.

<sup>85</sup>Davidson (1972). Letter from Paul Davidson to Luigi Pasinetti, 15<sup>th</sup> November, 1971 in Paul

*World* were his self-recognized attempt to integrate his work with the Cambridge England theory.<sup>86</sup>

Though the case has been made for Davidson, other authors also came to reference the Controversies and to side with the Cambridge England position. In 1969 Alfred S. Eichner,<sup>87</sup> then an Assistant Professor at Columbia University, wrote to Robinson.<sup>88</sup> The content of their ensuing correspondence was centered on Eichner's theory of oligopoly. His approach focused on the microeconomics of the firm, while Robinson by then worked under a macro one, for Eichner these were compatible. The discussion that followed focused on laying down a synthesis of the two approaches<sup>89</sup> and resulted in Eichner's adherence to theoretical positions of Cambridge England.<sup>90</sup>

The early 1970s were a time to downplay differences and highlight what was shared: "As Hy Minsky told me recently: "There are so few of us, that we must be careful not to make issues of the little differences in our approaches in the literature. We must continually attack the orthodox and not be trapped into quarrelling amongst ourselves."<sup>91</sup> In the late 1960s, early 70s, the work of American Post Keynesians was framed to a large extent by the controversies. It was not just the work of Davidson but also the work of Edward Nell, Tom Asimakopulos, Donald Harris, and David Laibman.<sup>92</sup> This literature explored the significance of reswitching and attempted to develop models under Cambridge England assumptions. The themes of the capital controversies were a means to mediate a joining of forces

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Davidson Papers, The Economists' Papers Project, at Rare Book and Manuscript Library, Duke University, Box 5.

<sup>86</sup>Letter from Paul Davidson to Joan Robinson, 25<sup>th</sup> September, 1970. Joan Robinson Papers, King's College Library, JVR/vii/114/125.

<sup>87</sup>Alfred S. Eichner (1937-1988) was born in Washington D.C. He obtained his PhD in Economics from Columbia University in 1966. He taught at Columbia (1961-71) at the State University of New York at Purchase (1971-1980) and at Rutgers University (from 1986). His major works include *The Emergence of Oligopoly: Sugar Refining as a Case Study* (1969); *The Megacorp and Oligopoly. Micro Foundations of Macro Dynamics* (1976) and *The Macrodynamics of Advanced Market Economies* (1987).

<sup>88</sup>Lee (2000a), p. 20.

<sup>89</sup>This desire is expressed by Eichner, "It seems clear, at least to me, that I don't fully know what I am talking about when I discuss the post-Keynesians and what their analysis encompasses. I shall certainly try to correct for this deficiency over the next years as I do the final revising of the manuscript [his work on oligopoly]." Letter from Eichner to Robinson, April 1969, Joan Robinson Papers, King's College Library, JVR Misc 79/4.

<sup>90</sup>Lee (2000a), p. 21.

<sup>91</sup>Letter from Davidson to Paul Wells, 28<sup>th</sup> December, 1972 in Paul Davidson Papers, The Economists' Papers Project, Box 6.

<sup>92</sup>Nell (1967a; 1967b), Asimakopulos (1969), Harris (1973; 1978), Laibman and Nell (1977).

between critics from both sides of the Atlantic ocean.

The controversies were also a valuable teaching tool, as Donald Harris commented: “the JEL article has had a powerful effect in exposing the neo-neoclassicals and the meaning of the Cambridge contribution – in particular, among graduate students, which is where it matters.”<sup>93</sup> Jan Kregel<sup>94</sup> recalls that for his formation as a Post Keynesian the “basic influence was that when I was a student at Rutgers, Paul Davidson gave an advanced seminar on the Cambridge Capital Theory Controversies, so that I was introduced relatively early to those controversies. And at the same time, these were linked to the *Tract on Monetary Reform*, the *Treatise on Money* and the *General Theory*.”<sup>95</sup> Through his contact with Robinson, Davidson sent Kregel to Cambridge for the academic year of 1968/9. In Davidson’s words; “Kregel was a PhD student of mine that I sent to Cambridge to be tutored by Joan Robinson on the reswitching controversy. He successfully defended his PhD dissertation on the Cambridge Capital Controversy at Rutgers on April 14, 1970.”<sup>96</sup> Kregel’s dissertation work was followed by two books on the work of Joan Robinson and the Cambridge England approach to growth theory.<sup>97</sup> On capital theory Kregel adapted a series of lectures he gave to undergraduates in Holland into a book.<sup>98</sup>

The positions drawn in Harcourt’s interpretation of the capital controversies played directly into the turmoil developing in American campuses.<sup>99</sup> Harcourt was a vocal opponent to the Vietnam War in Australia<sup>100</sup> and he saw a link between intellectual controversy and political belief. His stress on the ideological basis of the neoclassical argument<sup>101</sup> led him in his 1972 book to claim:

<sup>93</sup>Letter from Don Harris to Harcourt in May 29<sup>th</sup> 1970, at G.C. Harcourt’s Private Correspondence.

<sup>94</sup>Jan Allen Kregel was born in 1944 in Dallas, Texas. He received his PhD in Economics from Rutgers University in 1970. He taught at the University of Bristol (1969-72), University of Southampton (1973-9), at Rutgers University (1977-81) at Rijkuniversiteit Groningen (1980-5) and at the John Hopkins University School of Advanced International Studies at Bologna (since 1985). His major works include *Rate of Profit, Distribution and Growth: Two views* (1971), *The Reconstruction of Political Economy: An Introduction to Post-Keynesian Economics* (1973); *Theory of Capital* (1976).

<sup>95</sup>Kregel (2000).

<sup>96</sup>Davidson (2004), p. 256.

<sup>97</sup>Kregel (1971; 1973; 1972).

<sup>98</sup>Kregel (1976). Interview with Jan Kregel, 2003.

<sup>99</sup>On student unrest, see chapter 3 of this thesis, particularly pp. 55-59.

<sup>100</sup>Harcourt was a founding member of the Australian Executive Committee of the Campaign for Peace in Vietnam in South Australia and for two spells, its Chairman (Harcourt (1999), p. 45).

<sup>101</sup>Harcourt (1969), pp. 380-386.

It is my strong impression that if one were to be told whether an economist was fundamentally sympathetic or hostile to basic capitalist institutions, especially private property and the related rights to income streams, or whether he were a hawk or a dove in his views on the Vietnam War, one could predict with a considerable degree of accuracy both his general approach in economic theory and which side he would be on the present controversies.<sup>102</sup>

Robinson was recognised as a radical<sup>103</sup> and received invitations to lecture at US Universities. These invitations that came both from students and colleagues, were met by lectures from Robinson on the capital controversies and economic methodology. Appreciation for Robinson's work was more felt in the student population than by the academic staff.<sup>104</sup> One such invitation came from Anwar Shaikh who had begun graduate work in economics at Columbia in late 1967. With the contact with Robinson (in 1970) and after reading Harcourt (1969), Shaikh moved from studying trade and development to write a thesis on "theories of value and theories of distribution".<sup>105</sup> It was in line with the controversies that Shaikh wrote his "Laws of Algebra and Laws of Production: the HUMBUG production function."<sup>106</sup>

The capital controversies as they unfolded had touched a latent discontent in American economics and this became manifest in the rising correspondence between Robinson and dissenting American economists. For these American dissenters the controversies, as interpreted by Harcourt, defined both a place of departure; and a direction to follow, namely the construction of an alternative to replace a "defeated" mainstream. What these contacts show is a theoretical alliance taking shape, distinct theoretical approaches uniting under the banner of an emerging school of thought.

<sup>102</sup>Harcourt (1972), p. 13.

<sup>103</sup>Between 1950 and 1983 Robinson wrote fourteen pieces for the socialist *Monthly Review* (Turner (1989), p. 85). The texts voiced her views on the Soviet Union, China, Korea and Cuba, motivated by her visits to these countries all through the 50s and 60s. She also participated in debates sponsored by the *Monthly Review*: in 1961, "Has capitalism changed?", in 1967, on the changes in economic organization taking place in Russia.

<sup>104</sup>Although coming from a sympathetic colleague the following quote from a letter by Sidney Weintraub echoes this growing interest: "You will be interested in the fact that I am being "petitioned" by our second year graduates to offer a course built around your "type" ideas: they are in near rebellion at the econometrics and math-econ to which they have been subjected by our substantially MIT crowd." Letter from Sidney Weintraub to Joan Robinson, November 29<sup>th</sup>, 1973 in Turner (1989), p. 182.

<sup>105</sup>Interview with Anwar Shaikh, 2003.

<sup>106</sup>Shaikh (1974).



The controversies proved to be a much-warranted instrument to create “the necessary critical mass of academically situated economists to bring the post-keynesian revolution to this country.”<sup>107</sup>

#### 7.4.2 Beginnings of Post Keynesian institutions

The emergence of a Post Keynesian institutional network has been researched by Frederic S. Lee. What I now show is that this network building was founded on the contacts stimulated by the Capital Controversies and parallels the theoretical alliance described in the previous section. I argue that it was the desire by these dissenters to further a theoretical perspective/alliance that they envisaged would change the profession and not, as Lee argues, a desire to defend themselves from the mainstream’s attacks.<sup>108</sup>

In 1971 J.K. Galbraith, as president-elect of the American Economic Association (AEA), invited Joan Robinson to deliver the Richard T. Ely lecture at the annual meeting of the Association. Eichner saw this invitation as a positive sign towards a change in American economics: “Perhaps, however the time is ripe for a post-Keynesian Revolution in this country. Your invitation to visit this country is certainly a hopeful sign.”<sup>109</sup> Robinson in this visit was the catalyst for the first Post Keynesian meeting, more precisely, the first of a series of meetings that lead to the development of Post Keynesian institutions.

In November 1971, Robinson was at Columbia “at the invitation of Anwar Shaikh and other graduate students following the upheavals the previous spring over the Cambodian invasion.”<sup>110</sup> During her visit she met with Eichner and Pasinetti, the three discussed the present state of economics in America and decided that a letter by Robinson should be sent to sympathetic economists thus setting up a meeting at the coming ASSA (Allied Social Sciences Association) meetings in New Orleans.<sup>111</sup>

<sup>107</sup>Letter from Alfred S. Eichner to Joan Robinson, July 3<sup>rd</sup>, 1974. Joan Robinson Papers, King’s College Library, JVR Misc 79/4.

<sup>108</sup>As discussed in the previous chapter, Lee (2000a, 2000b).

<sup>109</sup>Letter from Alfred Eichner to Joan Robinson, April 1971, in Lee (2000a), p. 106.

<sup>110</sup>Eichner note in Lee (2000a), p. 113. In an interview Shaikh denied any connection between Robinson’s visit and the Cambodia protests (interview with Anwar Shaikh, 2003).

<sup>111</sup>A letter from Alfred Eichner to J.K. Galbraith, February 17<sup>th</sup>, 1972, in Lee (2000a), p. 125,

The meeting on the 28<sup>th</sup> of December 1971 was attended by seventeen economists out of the original twenty to thirty that received Robinson's letter. There was little agreement on what could be done to change the landscape of American economics. Still, "there was agreement on one point: everybody at the meeting felt that *The American Economic Review* was closed to heterodox economists and that something should be done about it."<sup>112</sup> Eichner was designated to contact Galbraith, President of the AEA, in the name of the group, and present him with the conclusions of the meeting, namely their shared concern over the biased assessment of non-neoclassical articles sent to the *AER*.

To keep the group informed about the developments of the protest a mailing list was created.<sup>113</sup> By January of the following year (1972) Eichner had begun editing a newsletter, which he sent to the participants of the New Orleans meeting. In one of early newsletters a Post-Keynesian Theory bibliography was sent, under the heading of "classics" it included the works of Keynes, Robinson, Kaldor, Harrod, Kalecki, Sraffa and Dobb, under the heading of "others" the list of references was headed by Kregel's two books on capital controversy themes; Harcourt's capital controversies book and Davidson's book written in Cambridge and "integrating" his work with the Cambridge England theory.<sup>114</sup> The bibliography is in line with the canonical view portrayed in Harcourt's survey argument, labelled "post-Keynesian" and inclusive of dissenters from both Cambridge England (the "classics") and the USA (the "others").

The existence of a mailing list also encouraged the organization of sessions at the ASSA annual meetings, "we feel that there is too little awareness, even among those presents a transcript of Robinson's letter:

As a result of my visits to a number of North American Universities over the last few years, I am much concerned over the way economics is being taught. A vulgar version of neoclassical theory continues to be offered in the graduate schools – although I find that many of the professors themselves privately admit that it is not intellectually defensible – while students are forced to pretend to accept whatever they are taught in class.

I thought that perhaps the convention of the American Economic Association this month might provide an opportunity to discuss with economists from the United States what can be done to remedy this situation.

<sup>112</sup>Lee (2000b), p. 143.

<sup>113</sup>For more on the AER protests see Lee (2000a).

<sup>114</sup>"Post-Keynesian Theory" in Paul Davidson Papers, The Economists' Papers Project, Box 2.

disenchanted with neo-classical economics, as to the possibilities which some of the recent work growing out of Cambridge post-Keynesian growth theory offer as an alternative to the conventional analysis.”<sup>115</sup> The meetings, the first Post Keynesian sessions, were initially held under the auspices of the Union of Radical Political Economics (URPE) in 1972, 1973 and 1974,<sup>116</sup> but Eichner also made efforts to have these included in the AEA label in the program, in 1974 he proposed a session to the President of the AEA:

Post-Keynesian theory, based on the pioneering work of Roy Harrod, Joan Robinson, Nicholas Kaldor, Piero Sraffa, Michel Kalecki and Luigi Pasinetti, represents a coherent, fully developed alternative to neo-classical theory which predominates in this country. Unfortunately, it has received little exposure in this country and, as a result, American economists who might be receptive to it have had little chance to judge on its merits. The re-switching controversy, which has had some airing in this country, presents a much distorted and inadequate view of the theory, in much the same way that the 19<sup>th</sup> century debate over the wages fund doctrine did in the case of marginal productivity theory.<sup>117</sup>

Departing from the Controversies as the most visible instance of their (adopted) critique, Post Keynesians were organizing means to voice their alternative economics, Eichner signalled that there was (in 1974) more to Post Keynesian theory than reswitching. The letter to the AEA President further listed speakers for the session Edward Nell, Tom Asimakopoulos, Donald Harris and Paul Davidson.<sup>118</sup>

One of the less recognized results of the 1971 meeting between Robinson and American dissenters was the idea of an “anti-neoclassical” book. The project was entrusted to Ed Nell and the envisaged title was: *Growth, Profits and Property – Essays in the Revival of Political Economy*. Although by 1976 Nell had collected all

<sup>115</sup>Letter from Eichner to Davidson, June 27<sup>th</sup> 1973 in Paul Davidson Papers, The Economists’ Papers Project, Box 2.

<sup>116</sup>Lee (2000a, 2002b).

<sup>117</sup>Letter from Eichner to Gordon, March 6<sup>th</sup>, 1974 in Paul Davidson Papers, The Economists’ Papers Project, Box 2.

<sup>118</sup>Letter from Eichner to Gordon, March 6<sup>th</sup>, 1974 in Paul Davidson Papers, The Economists’ Papers Project, Box 2. I have no record of the Post Keynesian session taking place.

the essays<sup>119</sup> publication only came in 1980.<sup>120</sup> The book is introduced with the paragraph:

This book of essays was conceived at the 1971 convention of American Economics Association, at a meeting called by Joan Robinson for those dissatisfied with orthodox economics. Hence in a very real sense, this book is a tribute to her, and to the influence she has exercised over the profession. Her influence in one form or another has been decisive in shaping a new and critical approach to economics. All of us would like to take this opportunity to honor her contributions to the field.<sup>121</sup>

In reference to the book: "The reason [for the book's importance] is that it completes and clarifies a critique that has been going on for some time at the heart of economics. But in addition to critique, it states for the first time most of the aspects of a positive, new paradigm."<sup>122</sup>

The burst of organizing initiated by the 1971 meeting and followed by sessions at the ASSA, by the edition of a newsletter, and edition of an anti-neoclassical book, constituted the genesis of the Post Keynesian group. The organizing was made possible by the contacts established from 1967 to early 1970s between American dissenters and their shared desire to further Cambridge England theory. As an

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<sup>119</sup>There were a number of differences between the 1976 version first submitted and the published one. In Part IV on Macroeconomics papers by John Eatwell and Alfredo Medio were added. Part VI "Welfare, Capitalism and Socialism" was renamed to "Property and Value" and Victor Lippitt and Howard Sherman's "Comparative Economic Systems: A Radical Approach" was replaced by a paper by David P. Ellerman. The published version also included a new section (VII: Marxism and modern economics) with papers by Kregel "Marx, Keynes, and social change: is post-Keynesian theory neo-Marxist?" and Frank Roosevelt's "Cambridge economics as commodity fetishism".

<sup>120</sup>The book was nearly cancelled in 1979 since it was seen by Cambridge University Press as being too long and therefore too expensive (leading them to demand reduction of the number of essays, shortening some and asking for authors to give up their royalties), problems with the payment of the art work of the book proved to be a further complication.

<sup>121</sup>Nell (1980).

<sup>122</sup>From Davidson's letter of support for the book, sent to Cambridge University Press. (undated), in Paul Davidson Papers, The Economists' Papers Project, Box 6.

institutional alliance it reinforced the theoretical alliance operated in the early 1970s.<sup>123</sup>

## 7.5 Revisiting the Controversies and the Post Keynesian identity

The great transformation of the profession implied in the Post Keynesian reading of the controversies didn't occur. In time, the capital controversies slipped away from the memories of the majority of the profession. As Geoff Hodgson documents with the assistance of citation indexes, the profession as a whole was never much caught by the capital critique. The controversies and the Sraffian assault "[were] not only bypassed, but became irrelevant for the new core theory of neoclassicism. Sraffian results have no apparent critical effect on game theory. Losing their status even as internal criticism of the mainstream core, they became less and less relevant."<sup>124</sup> And, by early 1980s, the focus of Post Keynesian work had also moved away from the battles of capital theory. Debates inside the Post Keynesian field were now on the subjects of methodology and the role of uncertainty in economic processes (prompted, according to King, by a rediscovery of J. M. Keynes through the publication of his *Collected Works*).<sup>125</sup> And their debates with the mainstream were most notably on monetary theory, where they offered a critique of monetarist policies.<sup>126</sup>

However, and unlike the rest of the profession, the capital controversies were not forgotten by Post Keynesians. Time and time again this episode was revisited and reassessed and therefore also relived. What I wish to discuss in this section are the uses given to the capital controversies by a new generation of economists coming into Post Keynesian economics. I show the role the controversies played in the

<sup>123</sup>The "theoretical alliance" is therefore, in this context, shorthand for changing positions in theory. "Institutional alliance" referred to the meetings, newsletter, joint sessions and book edition. I see this distinction as merely pragmatic, a way to organize my evidence, since I find no methodological justification to keep them separate, they occur during the same period, they follow and reinforce each other, they are one and the same, network building.

<sup>124</sup>Hodgson (1997), p. 104.

<sup>125</sup>King (2002), pp. 181-182. An account of this reshifting of emphasis reflects also power struggles inside Post Keynesian economics, which will be discussed in the next chapter.

<sup>126</sup>As King documents, in King (2002), chapter 8.

reproduction of the group. The controversies continued to provide direction for new research and, as a narrative, they identified the origins and the role that the group should strive to play in the profession, namely that of a critic and alternative to the mainstream.

The ways through which the new generation came to learn of the controversies are varied. Older Post Keynesians taught the controversies to their students in courses on macro theory and on the history of economics.<sup>127</sup> The promotion of summer schools, starting in 1981 in Trieste, also allowed a younger generation to come into contact with the history of the capital debates. The summer schools were an opportunity for Post Keynesians from Europe and America to join in discussion. Post Keynesians brought their graduate students to the summer schools for an intensive course on what was Post Keynesian economics and what were its most recent theoretical developments. Not only was Harcourt present at these meetings but his 1972 book on the capital controversies was included in the reading lists of the school.<sup>128</sup>

The new generation of Post Keynesians used the Capital Controversies as a motivation for their work. They brought new denunciations of the limitations of neoclassical models,<sup>129</sup> extended the models of the Cambridge England side<sup>130</sup> and responded to critiques raised by the neoclassicals.<sup>131</sup> Further contributions re-explored the significance of the reswitching<sup>132</sup> and discussed new ways of presenting the Cambridge argument more in tune with the 1980s increasing focus on Keynes' work.<sup>133</sup> The critique was even reproduced as an attack on other fields of neoclassical theory, namely neoclassical demand theory: "Reswitching in capital

<sup>127</sup> As comes out in reading King's interviews of Post Keynesians (King (1995a)), Rutgers University's graduate program trained some of the most influential Post Keynesians of the 1980s generation. The controversies were taught at Rutgers in the early 1980s in the history of economics course taught by Nina Shapiro (interview with Radhika Balakrishnan 2003). I should note that some students already knew of the controversies from earlier reading of dissenting literature and were convinced "the capital controversy effectively destroyed neoclassical theory and [were] willing to use the controversy in a number of ways to show it." (Private communication Fred Lee, October 11, 2003).

<sup>128</sup> Harcourt (1972). Sidney Weintraub Papers, at The Economists' Papers Project, Rare Book, Manuscript, and Special Collections Library, Duke University, Durham, North Carolina. For more on the Trieste Summer School see the next chapter.

<sup>129</sup> Baldone (1984).

<sup>130</sup> Fazi and Salvadori (1985).

<sup>131</sup> Ahmad (1986).

<sup>132</sup> Halevi (1985).

<sup>133</sup> Fleck and Domenghino (1986).

theory led to the destruction of the neoclassical parables regarding the relationship between prices, production and distribution. Likewise, reversal (and reswitching) of commodity bundles (...) undermine the orthodox consumer theory.”<sup>134</sup>

A further use given to the capital controversies was to revisit the debate for methodological insights. As younger Post Keynesians revisited the moment where the demarcation between post Keynesian economics and neoclassical economics was drawn, they saw themselves as drawing lessons over the main differences between Post Keynesian economics and the mainstream. I provide two examples of this use.

The first example is a debate in the pages of the *Journal of Post Keynesian Economics* from 1980 to 1985 and its main question was: Why did the capital controversies fail to produce a “Kuhnian scientific revolution”? The question was raised by Sheila Dow.<sup>135</sup> She depicted the controversies as showing the weaknesses of the neoclassical paradigm and prompting a crisis, yet they were not followed by a paradigm shift. In her paper Dow stressed the incommensurability of two paradigms, the neoclassical and the Cambridge England one, to explain neoclassical economics’ survival of the crisis. Two years later, Andrea Salanti took issue with Dow. The author argued that the Cambridge School “has not yet been able to provide a sufficiently general and/or “value-appealing” theory to dislodge the neoclassical standard.”<sup>136</sup> Salanti saw the controversies in a different light: since the Cambridge School provided an incomplete alternative, the effects of its critique were only partial. Dow’s reply claimed Salanti held a positivist view, naively expecting theories to be testable and refutable. Dow reasserted her stress on the value-ladenness of paradigm assessments.<sup>137</sup>

This debate continued with Avi Cohen interpreting the value-ladenness of the two paradigms as competing methodological approaches and therefore distinct evaluative outlooks on theory.<sup>138</sup> According to Cohen, the critique of the Cambridge UK stressed the requirement to explain historical processes and underplayed the need for

<sup>134</sup> Watts and Gaston (1982-3), p. 287.

<sup>135</sup> Dow (1980).

<sup>136</sup> Salanti (1982), p. 129.

<sup>137</sup> Dow (1982).

<sup>138</sup> Cohen (1984).

prediction; this was in stark contrast with the methodological and evaluative outlook of neoclassicism. He concluded that Post Keynesians should “be explicit about their methodology, so as to avoid the kind of misunderstandings surrounding the Cambridge controversies. (...) [and] use that methodology to provide good historical and theoretical explanations of economic phenomena.”<sup>139</sup> Cohen’s contribution was challenged by Michael Bernstein. Like Salanti, Bernstein saw the Post Keynesian alternative as incomplete, “attention should be focused on the development of a coherent and systematic theory.”<sup>140</sup> Bernstein concluded that the methodological reengagement that Cohen had proposed would not produce the necessary transformation of the profession. Cohen’s response<sup>141</sup> was to reject Bernstein’s reading of his article. He (Cohen) argued that his methodological re-engagement was not a strategy to battle neoclassical opponents, but a means to bring out the underlying issues dividing the profession.

A second example of exploration into the history of the controversies in search for renewed methodological insight was undertaken by John B. Davis. Davis derived from the capital controversies one of the three key methodological principles of Post Keynesian economics, which was portrayed as “a response to the failures of the neoclassical theory and its inadequate characterisation of economic activity.”<sup>142</sup> The principle he drew from the Cambridge controversies was that:

Contrary to the neoclassical conception of economic science as the steady accretion of analytical technique that renders past theory obsolete, post Keynesian theory allows for the re-examination of a variety of past theories as the needs of contemporary investigation bring forward new problems of characterization.<sup>143</sup>

The above examples show the Capital debates discussed as a starting point for the Post Keynesian (Cambridge) paradigm and the critique it raised as a shattering of the neoclassical edifice that should have brought its demise. In these examples,

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<sup>139</sup> Cohen (1984), p. 627.

<sup>140</sup> Bernstein (1985), p. 610.

<sup>141</sup> Cohen (1985).

<sup>142</sup> Davis (1987), p. 552.

<sup>143</sup> *Ibid.*, p. 563.



young Post Keynesians discuss the history of Post Keynesian economics through the history of the controversies. Significantly, the capital controversies are said to provide methodological lessons for the future of Post Keynesian Economics, as an alternative to mainstream economic theory.

The latest reviewing of the controversies in 2003 maintains this outlook over the Cambridge Capital debates:

While many of the key Cambridge, England, combatants stopped asking questions because they died, the questions have not been resolved, only buried. When economists decide to delve again, we predict controversies over these questions will be revisited, just as they were time and again in the 80 years prior to the Cambridge controversies<sup>144</sup>

Cohen and Harcourt invite the reader to use the controversies to understand the nature of the profession and the nature of orthodox economic theory, and suppose that the controversies may assist economics in overcoming the mainstream theory's shortcomings. The main thrust of the Cohen and Harcourt review article is that differing outlooks in both "ideology and methodology, two subjects most economists would rather avoid"<sup>145</sup> fuelled the controversies and prevented their resolution. This explanation brings together old and new readings of the debate: the older emphasis on ideology present in Harcourt's 1969 review article and Cohen's stress of differing methodological perspectives inside the discipline.<sup>146</sup>

Cohen and Harcourt's 2003 survey testifies to my claim that the capital controversies were instrumental to bring together older and younger generations of Post Keynesians. Addressing the canon established by Harcourt's reading of the controversies, and providing it with new readings meant that, a younger generation appropriated the history of the controversies as their own. Revisiting the controversies allowed them to position themselves side by side with older Post Keynesians and share the identity that the controversies' history provided, namely Post Keynesian economics as inheritor to a devastating critique of the mainstream

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<sup>144</sup>Cohen and Harcourt (2003), pp. 211-212.

<sup>145</sup>Ibid., p. 210.

<sup>146</sup>Cohen (1984).

and as the key to an alternative economics. Born out of a reinterpretation of the canonical history of economics Post Keynesian identity is grounded on this canonical alternative which is hence continuously revisited.

## 7.6 The emergence of Post Keynesian Economics

Post Keynesian economics was the product of an alliance. It was primarily an alliance between American economists and the designed Cambridge England school of Harcourt's survey (which Robinson also promoted). Post Keynesian economics was the combination of a group of economists with a shared theoretical agenda, which in the early 1970s began to: reference each other, to attend each other's meetings (for instance, seminars in Columbia in 1976), to protest against the AEA together, and later (1978) to set up and publish in a *Journal of Post Keynesian Economics*.

Examination of these efforts should also make clear that the bulk of this Atlantic alliance resided in America. Robinson did not remain an enthusiast of these efforts for long, and other Cambridge Economists were never militant supporters.<sup>147</sup> The organising was American-led, the alliance was made between Americans.<sup>148</sup>

I have located the source of the alliance activity to 1969 and Harcourt's survey-article and in 1972 book on the capital controversies. Out of the unsettled exchanges Harcourt redrew the economics canon, he repositioned authors and texts to reveal what others had failed to recognise, a new theory in economics about to supplant the old. This was indeed a powerful message particularly for the "revolutionary craving" times of the late sixties, early seventies. Post Keynesians were not social revolutionaries but they wished for a revolution in economics nonetheless. Cambridge England positions were following the controversies, being adopted and developed by American dissenters. Adopting a canon, a new map of economics, is a

<sup>147</sup>It is interesting to note that Robinson when she wrote a textbook in 1973 (Robinson and Eatwell (1973)), did not make much of her American allies in its content, although she requested their help to promote the textbook in America, see King and Millmow (2003) for a history of the textbook's ill fate. Robinson did comment on American economics, notably in her "What are the questions?" (Robinson (1977)) but she remained primarily attuned to debates in Cambridge (Robinson (1980), pp. 123-130).

<sup>148</sup>The Americanization of Post Keynesian Economics is partly discussed in the next chapter.

political act.<sup>149</sup> By siding with Cambridge England, American dissenters were joining with the Cambridge England canon and the authority that it then commanded, voiced in the *JEL*, included in reading lists, applauded by students. By siding with the Cambridge England canon they could then join a growing alliance of dissenters. A theoretical harmonization between approaches was being sought, manifested in the pronouncements in the authors' correspondence, and later reflected in their writings.

The Cambridge Capital Controversies hold a prominent place in Post Keynesians' identity. The controversies are periodically remembered and retold. They are passed on to later generations in Post Keynesian courses, oral or written histories. This episode in the history of economics is to this day, the foundation for Post Keynesians. Similarly to the 1970s where it was reason to join American and British dissenters, it now justifies young and older generations of Post Keynesians is furthering the Post Keynesian approach.

Interestingly the intellectual innovation that bred Post Keynesian Economics was not Keynes' *General Theory*, neither was it Sraffa's *Production of Commodities by Means of Commodities*. What first bound Post Keynesians was a reading of the economics literature. It was a vision of the past, a history that explained the future. It was frequently in battling over that past that Post Keynesian economics developed, the subject of the next chapter.

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<sup>149</sup>Samuels (2001), p. 487; Brown (1993), p. 65, which I will discuss in detail in section 9.2.

## Many labels

### The battle for self-definition

#### 8.1 Conflict in Post Keynesian Economics and the history of a label

In 1972, Paul Davidson approvingly paraphrased Hyman Minsky<sup>1</sup> as saying, “There are so few of us, that we must be careful not to make issues of the little differences in our approaches in the literature. We must continually attack the orthodox and not be trapped into quarrelling amongst ourselves.”<sup>2</sup> However, tensions between dissenters were present in the early history of the Post Keynesian community. In the following year Minsky reviewed Davidson’s *Money and the Real World*, prompting the following remarks by the latter:

With friends like these who needs enemies! (...) I certainly would agree with your statement that MRW [*Money and the Real World*] is “a large step forward but it does not signal our arrival at the destination;” but I do not think you are correct in indicating that it is not a paradigm shift. (...) I would have thought we could keep our squabble out of the printed literature or if they are to appear, put them in the context of how this work suggests the need for further research in these areas.<sup>3</sup>

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<sup>1</sup>Hyman P. Minsky (1919-1996) born in Chicago, USA. He earned a BA in Mathematics from the University of Chicago and PhD in Economics from Harvard University. Taught at Carnegie Mellon University (1947), Brown University (1949-1957), University of California Berkeley (1957-65), and at Washington University in St. Louis, Missouri (1965-90). After his retirement he was involved with the organization of the Jerome Levy Economics Institute (Bard College). His major works include *John Maynard Keynes* (1975), *Can ‘It’ Happen Again?* (1982) and *Stabilizing an Unstable Economy* (1986).

<sup>2</sup>Letter from Davidson to Paul Wells, December 28<sup>th</sup>, 1972. Paul Davidson Papers, The Economists’ Papers Project, at Rare Book and Manuscript Library, Duke University, North Carolina, U.S.A., Box 7.

<sup>3</sup>Letter from Davidson to Minsky October 16<sup>th</sup>, 1974. Paul Davidson Papers, The Economists’ Papers Project, Box 4.

Whereas, the subject of the previous chapter was to show how the Capital Controversies mediated a coalition of dissenters that defined the outline of a Post Keynesian community, the current chapter investigates how internal conflicts shaped its intellectual and institutional definition.

This chapter is about conflicting definitions of Post Keynesian Economics, the context of these disputes and its actors. My main entry-point into internal controversy is a history of the label of “Post Keynesian Economics”. For this purpose it is useful to discuss the label as a “classification,” an “ordering or arrangement of objects into groups or sets on the basis of their relationships,”<sup>4</sup> the objects of interest in the present case being theories and authors. For the label of Post Keynesian Economics, we have a case of self-definition. It was the group that defined its own label and the features that distinguished the objects of its class from objects of other classes in the field of economics. Keeping with the recurring metaphor of this thesis, reclassifying or relabelling *is* redrawing the map of economic theory, and in the process drawing in allies and drawing out enemies. Post Keynesians are thus seen as constantly seeking to redefine their own intellectual and social boundaries in debates where the label was one of the core subjects of contention.

This chapter distinguishes evolving definitions of the label Post Keynesian but also notes the existence of different labels and their spellings such as “post-Keynesian,” “Anglo-Italian theory,” “Cambridge School,” “Neo-Keynesian” and “Post Keynesian”. Each section devotes attention to the debates that marked the specific period for the group (first subsections) followed by an analysis of the texts that came to define the label for that period (second subsections). The first uses of the label appeared at the pre-history of the Post Keynesian group and are discussed in section 8.2. The following section deals with the formation of the group in the first half of the 1970s and how it came to appropriate the label and redefine it. Section 8.4 deals with the second half of the 1970s, the culmination of which was the creation of the *Journal of Post Keynesian Economics*. This period also corresponded to changes in the balance of power inside the group and with it came a new definition for the

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<sup>4</sup>Sokal (1974), p. 1116.

label. In the early 1980s, and prompted by efforts to extend the Post Keynesian network internationally, a debate ensued that motivated a further redefinition of the label; this is the subject of the final section.

## 8.2 Before the Capital Controversies

The term “post-Keynesian” first appeared in a book published in 1955 and edited by Kenneth K. Kurihara, a macroeconomist from Rutgers University.<sup>5</sup> The collection of essays printed in the volume was meant as an “extension of Keynes’ *General Theory* by a group of well-known economists.”<sup>6</sup> J. M. Keynes’s work was portrayed as a frame of reference upon which to search for “criticisms, further explorations, imaginative extrapolations and novel insights, yet all trying to add something”.<sup>7</sup>

For authors in the volume, the sympathy that Keynes’s work merited had its limits. Keynes had provided indispensable “tools of analysis” and had raised important questions but he had not answered them all. Furthermore, new questions had arisen for which Keynes could provide little help, Kurihara noted in his introduction: “many would now consider [Keynes’s] analysis less than adequate for meeting such special problems as cyclical forecasts and controls, persistent inflation, the maintenance of full-employment booms, secular growth, nonlinear structural relations, and macrofunctional distribution.” These problems were said to lie outside the scheme of the *General Theory* and called for fresh approaches, as those proposed in the book.<sup>8</sup> More important was the statement that this extension of the *General Theory* was “only now possible.” The passing of time had given perspective as to what were Keynes’s contributions to economics and what were the problems of Keynesian economics. Since Keynes’s contributions had then been fully assimilated one could engage in answering some of the questions he had left unanswered and draw

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<sup>5</sup>Kurihara (1955).

<sup>6</sup>Kurihara (1955), p. vii. The contributors were: Howard R. Bowen; Martin Bronfrenbrenner; Richard Brumberg; Dudley Dillard; Shinichi Ichimura; Lawrence R. Klein; Kenneth K. Kurihara; R. C. O. Matthews; Franco Modigliani; Gerald M. Meier; Anatol Murad; Don Patinkin; Paul P. Streeten; Lorie Tarshis; Mabel F. Timlin; Shigeto Tsuru and William S. Vickrey.

<sup>7</sup>Idem.

<sup>8</sup>Ibid., p. vii.

support from some of his “tools of analysis” for answering new questions.<sup>9</sup>

Paul Samuelson in the concluding paper of a book entitled *Keynes' General Theory – Reports from Three Decades*, also made use of the term for “A Brief Survey of Post-Keynesian Developments.”<sup>10</sup> One can read from the preface the book's stated motivation:

The immediate stimulus to the compilation of this volume was chronological. What better way to celebrate the *General Theory's* first quarter century than to ask some of the distinguished economists who responded to Keynesian economics at the doctrine's inception to place in print their present evaluations of the Keynesian revolution or “revolution”?<sup>11</sup>

In his “brief survey” Samuelson offered “a personal interpretation of some major trends in macroeconomic analysis that have grown out of the *General Theory*”.<sup>12</sup> Samuelson played with the idea of how a second edition of the *General Theory* might look and he listed contributions that had picked up from Keynes and developed his insights, of which the crucial one was that “economic equilibrium need not produce full employment.”<sup>13</sup> The author also wanted to make note of contributions that could be seen as valuable additions, for instance a theory of the wage and price level that was lacking in Keynes's *General Theory*. The paper was a reading of the breadth of contemporary macroeconomic theory (it included references to Simon Kuznets, Robert Solow, Nicholas Kaldor, Milton Friedman and others) and how it might be related to Keynes' work.

John E. King in his history of Post Keynesian Economics characterizes these uses of the term as carrying “chronological rather than a doctrinal meaning.”<sup>14</sup> King is correct in that the term was not a reference to a section of the economics discipline as it was later to denote, instead it was a description of economics as a whole. But it

<sup>9</sup>The articles covered such diverse topics as econometric research, modeling and a comparison between Marx and Keynes.

<sup>10</sup>Leckachman (1964); Samuelson (1964).

<sup>11</sup>Leckachman (1964), p. v. The book included papers by D. G. Champernowne, Gottfried Haberler, Roy Harrod, Abba Lerner, Austin Robinson, W. B. Reddaway, Paul A. Samuelson, Paul M. Sweezy, Jacob Viner.

<sup>12</sup>Samuelson (1964), p. 332.

<sup>13</sup>Ibid, p. 332.

<sup>14</sup>King (2002), p. 9.

was also more than just chronological in meaning. As Samuelson and Kurihara's uses exemplify, when Keynes' legacy was under scrutiny the label "Post-Keynesian" was made to take the place of "Keynesian." When comparing Keynes' economics with their own research, authors underlined how the profession had transcended Keynes, fully accessing his contributions, adopting some, rejecting others and going so much further than the *General Theory*.

Joan V. Robinson first recorded use of the term was in the preface to her second volume of *Collected Economic Papers* of 1960: "The bulk of the present volume was written within the last five years [i.e. 1954-59], and all but one piece in the last eight. It belongs to the field of what is sometimes called post-Keynesian economics."<sup>15</sup> She was adhering to the standard use of the time. However in an article in 1964 "Pre-Keynesian Theory After Keynes",<sup>16</sup> Robinson redefined her list of labels. According to Robinson, Keynes had been responsible for a fundamental criticism of neoclassical economics; he had not so much presented a complete new system, but a powerful yet partial critique of the orthodoxy. Therefore, the work of Keynes sectioned the history of economics, those that had not understood his critical insights lived in a "pre-Keynesian" world of ideas, those that had understood it, moved onto a "post-Keynesian" one. The article indicted neoclassical economics as being built upon "pre-Keynesian" ideas.

Thus for Kurihara and Samuelson the profession was one, united in having advanced beyond Keynes. For Robinson in 1964, some economists ("neo-neoclassical" ones as she also called them) had not understood Keynes' message, they were lost in a pre-Keynesian past. Her message was that to move forward one needed to return to Keynes. To Robinson, Keynes was the beginning of a critique that she was aiming to conclude and that grounded new enquiries better suited to understand the world. In all these uses of the term, Keynes was a landmark in the history of economics that set about a change in the profession, redesigned the issues, questions and tools of

<sup>15</sup>Robinson (1960), p. vi. In the second edition (1974) Robinson compares "Post-Keynesian Analysis" and "Neo-Classical Capital". The use of the term is different here, as we shall see it was in line with the definition attached to the term in 1974, bearing the mark of the Capital Controversies episode.

<sup>16</sup>Robinson (1964).



economics.

### 8.3 Post Keynesian meetings (1971-1978)

The previous chapter discussed the early contacts between American dissenters and Joan Robinson in connection with the Capital Controversies. It was shown that a theoretical alliance was formed mediated by the reading of the debates provided in Harcourt's surveys.<sup>17</sup> The theoretical alliance creating Post Keynesian economics was effected by the adoption of Cambridge England ideas in America and by efforts at developing and extending these theoretical perspectives. As the alliance was taking shape there wasn't yet a label to designate the approach.<sup>18</sup> But as the relationship between British and American dissenters developed, the need for a label to denote the emerging approach began to be felt. It was negotiated between American dissenters (who disliked "Anglo-Italian") and Cambridge England (disliking "Neo-Keynesian"), the compromise was finally met with "post-Keynesian."

Evidence of this negotiation can be found in Paul Davidson's correspondence with Robinson. While debating methodology and the Capital Controversies, Davidson referred always to the "Cambridge school"<sup>19</sup> while Robinson talked of an "Anglo-Italian theory"<sup>20</sup> to denote the Cambridge England side of the controversies. In later letters Davidson came to adopt Robinson's label but there is no evidence of a discussion over the issue in the correspondence. Alfred S. Eichner on the other hand, in his correspondence with Robinson manifested displeasure with Robinson's preferred term "Anglo-Italian." He wrote: "Perhaps you might be able to clarify for me how the group should most properly be delineated. Should they be known as the Cambridge Neo-Keynesians, as I have been wont to term them, or as the Anglo-Italian school, as you refer to them in your last letter?"<sup>21</sup> Eichner argued that

<sup>17</sup>Harcourt (1969; 1972).

<sup>18</sup>For convenience in the previous chapter I adopted the term "Cambridge England" as in Harcourt's surveys (1969; 1972).

<sup>19</sup>Letter from Paul Davidson to Joan Robinson, August 30<sup>th</sup>, 1967 in Turner (1989), p. 186.

<sup>20</sup>Letter from Joan Robinson to Paul Davidson, September 3<sup>rd</sup>, 1969, *ibid.*, p. 189.

<sup>21</sup>Letter from Joan Robinson to Alfred S. Eichner. This letter, undated, was a reply to a 9<sup>th</sup> of June letter. Joan Robinson Papers, Kings College's Library, Cambridge, U.K., JVR Misc 79/4. "Neo-Keynesian" was the label used in Harcourt (1969).

the term “Anglo-Italian” would never “catch on” in the United States. By April 6, 1971, he was using “post-Keynesian” while Robinson continued to use “Anglo-Italian.”<sup>22</sup> Eichner further promised Robinson that he “would not refer to it again as the neo-Keynesian theory.”<sup>23</sup>

Some of the exchanges that led to the changes in labels have been lost.<sup>24</sup> However, Eichner’s correspondence with Robinson is prefaced by a note, that might elucidate what motivated the change: “My distinct recollection, though I do not see it in the correspondence that survives, is that she objected to neo-Keynesian because it sounded too much like ‘neoclassical’.”<sup>25</sup>

By this time, as I have shown, Robinson had already used the term “post-Keynesian,”<sup>26</sup> but in the context of her correspondence with Eichner it was employed with new meaning. In 1971 it was no longer a reference to the profession and its reception of Keynesian ideas but an allusion to a set of theoretical contributions, those of the Cambridge England side of the Capital Controversies.<sup>27</sup>

### 8.3.1 Alfred S. Eichner and Jan Kregel as spokespersons

It is significant that Eichner was the protagonist for the definition of the label as “post-Keynesian” since he was soon to take on the role of main publicist of the approach. One of the points that will be recurrent in this chapter is that the need for labeling was more acutely felt by the institutional promoters of the group, by those aiming to stand as its spokespersons. Eichner and Luigi Pasinetti were the instigators of the first meeting between American dissenters receptive to Cambridge England views. The meeting took place on 28<sup>th</sup> of December 1971 in New Orleans at the American Economic Association (AEA) convention.<sup>28</sup> The meeting was not what

<sup>22</sup>Letter from Alfred S. Eichner to Joan Robinson, on 16<sup>th</sup> April, 1971. Joan Robinson Papers, JVR Misc 79/4.

<sup>23</sup>Turner (1989), p. 191.

<sup>24</sup>In the late 1970s Robinson destroyed virtually all her correspondence (interview with Jan Kregel, 2003).

<sup>25</sup>Lee (2000a), p. 113.

<sup>26</sup>Robinson (1964).

<sup>27</sup>Referring to the work of Robinson on profit and accumulation (Robinson (1956)) and the work of Kaldor (1960a; 1960b; 1966) and Pasinetti (1962) on distribution.

<sup>28</sup>A very important AEA convention in this history of dissent, referred to in chapter 3 and 7, pp. 83, 206 of this thesis.

Eichner expected from it, the consensus achieved by the dissenters present was limited to a protest over the *American Economic Review's* (*AER*) editorial policy, which was perceived as closed to non-neoclassical economists. Eichner suggested in the meeting that “post-Keynesian” be adopted as the label for the group, but the suggestion was met with hostility by many participants.<sup>29</sup>

Eichner and Edward Nell, a year later, organized a session at the Union for Radical Political Economics (URPE) meetings held in Toronto, entitled “The possibility of an alternative to the neo-classical paradigm: a dialogue between Marxists, post-Keynesians and Institutionalists.”<sup>30</sup> The session reviewed Nell’s anti-neoclassical textbook, had a paper by Minsky, “An Alternative to the Neo-Classical Paradigm: One View”, and one by Eichner with the title: “Outline for a new paradigm in economics.” Eichner’s goal was to tackle the disagreements made evident in the 1971 meeting and look for grounds for consensus. He argued that the widespread dissatisfaction with the neoclassical paradigm sprang from two beliefs shared by all dissenters: “that the conventional analysis provides a faulty and incomplete explanation of how the economic system itself functions and, two, that it ignores the critical interaction between economics and other aspects of society.”<sup>31</sup> The critiques by Marxists, neo-Marxists, Keynesians, post-Keynesians and institutionalists were deemed compatible, though they presented different emphasis depending on author and tradition. For Eichner, what prevented this critique from being more effective was the absence of a paradigm.<sup>32</sup> It would enable each school to pursue distinct lines of enquiry that were seen most promising, but also yield a *shared* basic model of the economy and society.

Eichner discussed change in economics resorting to Thomas Kuhn’s philosophy of science.<sup>33</sup> This was contemporaneous with similar debates happening within URPE.<sup>34</sup>

<sup>29</sup>Lee (2000b), p. 146.

<sup>30</sup>Letter Alfred S. Eichner to Paul Davidson, 12<sup>th</sup> December, 1972. Paul Davidson Papers. The Economists’ Papers Project, at Rare Book and Manuscript Library, Duke University, Box 2.

<sup>31</sup>Eichner (2000 [1972]), p. 147.

<sup>32</sup>“What has so far been missing, and thus what has prevented these critiques from sweeping the textbooks clean of error, has been the lack of an alternative paradigm” Eichner (2000 [1972]), p. 147.

<sup>33</sup>Kuhn (1970).

<sup>34</sup>Recall the 1971 special issue of the Review of Radical Political Economics “On Radical Paradigms in Economics” discussed in chapter 3 of this thesis, p. 76.

In Kuhn's account a whole discipline could be transformed: a discipline in crisis would generate a new approach (paradigm) that would replace the prevailing *status quo*. It was the dissenters' perception that neoclassical economics was in crisis, as stated in Robinson's Richard T. Ely lecture of 1971 to the AEA.<sup>35</sup> They reasoned that if no scientific revolution was underway it was because the alternative paradigm was not yet in place, not yet complete. Kuhn's account of discontinuity in science was once again turned from a descriptive statement to a programmatic one; to overcome the mainstream the dissenters had to construct a consistent alternative in the form of a paradigm.

Eichner's conference paper was a sketch of how an anti-neoclassical paradigm could be designed.<sup>36</sup> The first task was to model the larger society's functions so that the role of the economic system might be better understood. Secondly, some variant of a Ricardo-Sraffa general equilibrium growth model would be used to analyse market economy. Marx's economics would be seen as a particular version of this general class of models, while the work of Keynes stood as a refinement of the model. The paper was as an appeal for a rallying of approaches against the mainstream, in a designed paradigm that made the different perspectives compatible. Eichner reported on the meeting: "I would like to add that the session reinforced my belief that it is possible to develop an alternative to the neo-classical paradigm, that the differences between Marxists, neo-Marxists, Keynesians, post-Keynesians, institutionalists and grants economists are not necessarily irreconcilable." However it would not be easy given "This tendency to talk past one another was certainly evident in Toronto."<sup>37</sup> It was decided that the dialogue in search of a new paradigm inclusive of Keynesian, Marxist and Institutional approaches was to be pursued in local discussion groups established for that purpose, designated Political Economy Clubs.

Eichner's appeals at URPE came in a period where the Union and radicals were

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<sup>35</sup>Robinson (1972).

<sup>36</sup>Eichner (2000 [1972]).

<sup>37</sup>Letter from Eichner to Davidson, 7<sup>th</sup> February 1973. In Paul Davidson Papers, The Economists' Papers Project, at Rare Book and Manuscript Library, Duke University, North Carolina, U.S.A., Box 4.

redirecting their efforts away from strictly intellectual tasks.<sup>38</sup> Even more troubling for Eichner's efforts was the growing sectarianism within URPE, and the exemplar of this new stage of intolerance was Anwar Shaikh's expulsion from the URPE-New York chapter for his sympathies with Sraffa and other Cambridge authors.<sup>39</sup> Eichner was therefore even unable to organise a Political Economy Club in New York City.

Frederic S. Lee comments that Eichner became disillusioned with the possibility of a single paradigm uniting the different heterodox perspectives, and it was only "with Robinson's encouragement, Eichner regained his determination to promote Post Keynesian economics now tempered with a more realistic perception of how difficult it would be."<sup>40</sup> In 1973 at the New York URPE annual meetings Eichner organized a workshop: "Post-Keynesian theory as a teachable alternative macroeconomics."<sup>41</sup> Eichner, again with Nell's support, compiled a post-Keynesian bibliography that was distributed in the meeting and through the newsletter that had been first issued after the New Orleans meeting with Robinson. The 1973 meeting signalled a change in Eichner's efforts. His aim was no longer that of creating a paradigm uniting radical and Keynesian approaches but that of developing post-Keynesian theory as the alternative paradigm.

Eichner's next step in developing and promoting post-Keynesian economics in the U.S.A. was to announce its existence to the profession. For this he requested the assistance of Jan Kregel, who had been at Cambridge as a graduate student of Robinson (and had concluded a doctorate on the Capital Controversies).<sup>42</sup> Eichner wanted them both to write a survey article that presented the post-Keynesian paradigm and publish it in a major journal.<sup>43</sup>

Contemporaneously to Eichner's efforts in the U.S.A., Kregel was promoting the

<sup>38</sup>On radicals' outreach work see chapter 4.

<sup>39</sup>Interview with Anwar Shaikh, 2003.

<sup>40</sup>Lee (2000a), p. 28.

<sup>41</sup>The workshop was chaired by Ed. Nell, with talks by Ed Nell on "The implications of the Sraffa model", a paper by Al Eichner on "Development and Pricing in the Megacorp" and a further paper on Cambridge theory.

<sup>42</sup>His thesis was published under the title: *Rate of Profit, distribution and growth: two views* (1971). The text referred to "neoclassicals" vs the "Keynesians" in what were Harcourt's (1969, 1972) respectively "Cambridge Mass" and "Cambridge England".

<sup>43</sup>Interview with Jan Kregel, 2003.

new approach in Britain. In the early 1970s, Jan Kregel published two articles in the *Bulletin for the Conference of Socialist Economics*, the journal of a group of radical economists that were organizing in Britain.<sup>44</sup> Like Eichner, Kregel was looking for an audience in the radical community. What is striking in these articles, is the use of terms “post” and “pre” Keynesian in accordance with Robinson’s (1964) understanding of them. Pre-Keynesian were those ideas that had survived in neoclassical economics, ignoring the critical insights of Keynes: “In the less ambitious sense of short-term cycles or crises there is a large pre-Keynesian literature which was made largely nugatory by the analysis of the *General Theory*.”<sup>45</sup> Post-Keynesian contributions were those that understood the content of Keynes’ work: “A radical Keynesian must then mean someone who believes that no advance has been made in monetary theory since around 1938. In commonly accepted terminology such a position would be that of ‘post-Keynes’.”<sup>46</sup>

In 1973 Kregel published the *Reconstruction of Political Economy: An Introduction to Post-Keynesian Economics*.<sup>47</sup> In this text the meaning of post-Keynesian was no longer a chronological one as given in his previous articles but a brand for a new approach, its main goal was “to link Keynes’ own theory with the long-period theory of the classical political economists. The possibility of such a relation has become obvious with the post-Keynesian construction of a long-period theory based on Keynes’ short-period theory which closely resembles, in both content and concern, the classical theory of Ricardo and Marx.”<sup>48</sup> The book dealt in detail with the work of Robinson on growth theory. In two short sections headed: “Alternative Post-Keynesian Analyses,” Kregel discussed Nicholas Kaldor’s “stylised facts” method and distribution models, followed by Luigi Pasinetti’s work on distribution. The alternative analyses made up a single approach, an alternative to neoclassical economics, Kregel concluded:

<sup>44</sup>Kregel (1972; 1974). For a history of the Conference of Socialist Economics see Radice (1981) and Lee (2001).

<sup>45</sup>Kregel (1972), p. 59.

<sup>46</sup>Kregel (1974), p. Kregell.

<sup>47</sup>Kregel (1973).

<sup>48</sup>Kregel (1973), p. xv.

the post-Keynesian approach is not identical in its approach or application to specific areas or problems. We have tried to show, however, that the general base of the theory is unified and coherent in that it stems from the work of Keynes and is potentially able to analyse all the problems that classical political economy found of interest. In this sense it qualifies as a true alternative to the existing neoclassical orthodoxy. It remains that it will not answer all the questions posed by neoclassical theory. If it did it would not be an alternative. At the same time it provides a method of approach and a point of view widely different from the neoclassical, offering questions that the neoclassical approach cannot treat. It is in this sense that it becomes a true alternative.<sup>49</sup>

Eichner's desire to work with Kregel for promoting the post-Keynesian approach was justified given Kregel's earlier efforts at presenting post-Keynesian economics as an alternative to the neoclassical establishment.<sup>50</sup> They shared the vision of an emerging approach in economics. Furthermore, Kregel's 1973 book provided a detailed exposition of Robinson's work and so he was well qualified to bring into Eichner's survey-article Robinson's views on theory.

### 8.3.2 Post-Keynesian as an American extension of Cambridge England

In July 1974, Eichner approached Mark Perlman, editor of the *Journal of Economic Literature (JEL)*, with the survey article authored jointly with Kregel: "Our purpose in writing this article was to provide a guide to the post-Keynesian literature on matters other than the capital controversy, a literature which seems largely unknown to American economists."<sup>51</sup> Eichner in this statement placed the capital controversies, a publicly recognised event to readers of the *JEL*, within the class of post-Keynesian theory. He argued that that there was more to post-Keynesian theory than what had been presented in the Capital debates. Perlman had sponsored Harcourt's capital controversies survey of 1969, and appeal to the subject was likely

<sup>49</sup>Kregel (1973), p. 204.

<sup>50</sup>Kregel (1972; 1973; 1974).

<sup>51</sup>Letter from Alfred S. Eichner to Mark Perlman, July 31<sup>st</sup>, 1974 in Mark Perlman Papers, The Economists' Papers Project, at Rare Book and Manuscript Library, Duke University, North Carolina, USA, Box 24.

to have favourable resonance.

The Eichner and Kregel survey was subjected to two referees, one supportive and one very negative, however, Perlman showed resolve in publishing it: "As some of our mutual friends may have told you, I am quite interested in seeing that members of the American Economic Association get regular and useful exposure to the economics work done elsewhere, particularly in Cambridge on the Cam."<sup>52</sup> In face of mixed reviews, it was ultimately the intellectual authority commanded by Cambridge that secured the article's publication.

Eichner and Kregel's survey was entitled: "An Essay on Post-Keynesian Theory: A New Paradigm in Economics." The starting point of the essay was to assert the significance of Keynes' "critical insights into the workings of a modern, technologically advanced economy".<sup>53</sup> In a footnote the authors clarified the choice of label:

The term neo-Keynesian has also been used. It should be added that Keynes's is only one of the contributions upon which the new approach is based. Certainly the work of Kalecki has been no less important than that of Keynes; and to the extent that the new approach rests on the theory of value which grows out of the work of John von Neuman and Piero Sraffa, those names need to be mentioned as well. Indeed, to Marxists the work of Sraffa is seen as being the most fundamental of all, and they are likely, to refer to the new approach as neo-Ricardian.<sup>54</sup>

The paper promised a review of post-Keynesian literature, highlighting the salient features of the new approach. The authors grouped these under four headings: growth dynamics, distributional effects, the Keynesian constraints and the microeconomic base. The means by which each of these was addressed differed. While Keynesian constraints were presented as an exemplar, a way of approaching a problem, the other three were presented by reference to models and theories.

<sup>52</sup>Letter from Mark Perlman to Alfred S. Eichner, December 16<sup>th</sup>, 1974 in Mark Perlman Papers, The Economists' Papers Project, Box 24.

<sup>53</sup>Eichner and Kregel (1975), p. 1293.

<sup>54</sup>Ibid, p. 1293. The reference to the term "neo-keynesian" appears because that had been used in Harcourt's 1969 *JEL* survey. The inclusion of Kalecki and Von Neumann seems to have been at Robinson's suggestion: "I think a paper on these lines would be very useful. The general introduction is excellent but I think both Kalecki and von Neumann should be mentioned along with Keynes. It would be truly remarkable to see this published in the U.S.A." Letter from Robinson to Eichner 11<sup>th</sup> September 1974, Joan Robinson Papers, JVR Misc 79/4.



Post-Keynesian growth dynamics was said to have originated in Roy Harrod's work in the 1930s which had been revised in light of Joan Robinson's *Accumulation of Capital* and the contributions of Kaldor and Pasinetti, including class and distributional considerations for the determination of the rate of savings.<sup>55</sup> Eichner and Kregel stressed what they saw as "analysis done in historical time" and "recognizing the dynamic nature of the economic system." Distributional effects were closely connected with studies of growth and were again referenced to the work of Pasinetti, Robinson and Kaldor. Distribution was seen as an integral part of all economic explanation and analysis, highlighting the distinct economic behaviour of capitalists and workers with regard to saving and consumption. These first two sections corresponded to materials connected to the Capital Controversies.<sup>56</sup> The discussion of the "Keynesian constraints," the third section of the paper, asserted the centrality of money for modern economies. The section discussed how to analyse the macroeconomic aggregates, it placed investment as the primary determinant of economic activity and savings as adjusting to the former. The main contributions discussed in this section were those of Kregel, Davidson, Minsky and Tom Asimakopoulous. Finally, the fourth section addressed the microeconomic base of post-Keynesian economics by presenting a theory of non-competitive pricing grounded on the Michal Kalecki's mark-up and addressing the nominal wage as exogenously determined. The main contributions referred to in this last section were those of Donald Harris and Eichner.<sup>57</sup>

The survey presented "post-Keynesian theory" as an approach originated in the 1950s and 1960s at Cambridge (sections 1 and 2 of the survey) and joined by American economists who developed and added to it (sections 3 and 4). The survey can therefore be read as a progress report of the alliance between British and American dissenters. The authors' desire to showcase "post-Keynesian theory" beyond the Capital Controversies was a desire to show their own work as a valuable

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<sup>55</sup>Harrod (1939); Robinson (1956); Kaldor (1960a) and Pasinetti (1974).

<sup>56</sup>It would be a perfect match with Harcourt's canonical interpretation of growth and distribution theory (Harcourt (1972)) if not for Eichner and Kregel's inclusion of references to their own work for post-Keynesian distribution (Eichner and Kregel (1975), p. 1299).

<sup>57</sup>Harris (1974) and Eichner (1973; 1976).

addition to an emerging theory.

Eichner and Kregel noted that neoclassical economists did recognise this literature by commenting and exploring its insights, but they remarked that the neoclassical reading was at odds with the post-Keynesian perspective. Post-Keynesian theory should be treated as a “new paradigm” surfacing in the midst of an economics profession in crisis. The emerging paradigm as an alternative to neoclassical economics was the focus of the survey’s final section. The inclusion of a table comparing post-Keynesian theory and neoclassical theory (see Figure 8.1) asserted that economists were faced with a choice. The authors argued that while the purpose of post-Keynesian theory was to explain the real world, the purpose of neoclassical theory was to serve as basis to optimal decision-making.<sup>58</sup> The survey article concluded that the post-Keynesian paradigm was still developing but from the onset it was clear it was better suited to answer the concerns of the day.

TABLE I

<i>Aspect</i>	<i>Post-Keynesian Theory</i>	<i>Neoclassical Theory</i>
Dynamic properties	Assumes pronounced cyclical pattern on top of a clearly discernible secular growth rate	Either no growth, or steady-state expansion with market mechanisms assumed to preclude any but a temporary deviation from that growth path
Explanation of how income is distributed	Institutional factors determine a historical division of income between residual and non-residual shareholders, with changes in that distribution depending on changes in the growth rate	The distribution of income explained solely by variable factor inputs and the marginal productivity of those variable factor inputs
Amount of information assumed to be available	Only the past is known, the future is uncertain	Complete foresight exists as to all possible events
Conditions that must be met before the analysis is considered complete	Discretionary income must be equal to discretionary expenditures	All markets cleared with supply equal to demand in each of those markets
Microeconomic base	Imperfect markets with significant monopolistic elements	Perfect markets with all micro units operating as price takers
Purpose of the theory	To explain the real world as observed empirically	To demonstrate the social optimality if the real world were to resemble the model

Figure 8.1: Comparing post-Keynesian and neoclassical theory, in Eichner and Kregel (1975), p. 1309.

<sup>58</sup>Ibid, p. 1310.

This survey came at the end of a series of debates and institutional efforts led by Eichner during the early 1970s. It mirrored the successful alliances forged in this period. The project for an anti-mainstream paradigm enlisting Marxists, Institutionalists and Keynesians had failed, however an alternative paradigm was possible when constructed from the Capital Controversies with additions by American dissenters (see Table 8.1.).

Table 8.1: Label's definitions and spokespersons until 1975.

Period	Spelling	Spokesperson	Refers to
1950s-60s	Post-Keynesian	...	Economics
Late 1960s	Anglo-Italian	Robinson	Cambridge, England side of Capital Controversies
Early 1970s	Post-Keynesian	Eichner	Paradigm of Cambridge, England and their American following

#### 8.4 *The Journal of Post Keynesian Economics* (1977-82)

Eichner took on himself to be the promoter of the Cambridge approach. This mandate allowed him to redraw the boundaries inherited from Harcourt's survey of the Capital Controversies. He was drawing attention to the efforts of American economists in joining Cambridge England, he was connecting the American group to the renowned Cambridge authors. It is not surprising then that Eichner and Kregel's survey for the *JEL* stressed American developments over Cambridge England subjects.

Eichner's standing as spokesperson for Post-Keynesian economics was as good as the means he used to make himself heard. His mouthpieces were the annual URPE meetings, the post-Keynesian newsletter he edited and his own writings, prominently his survey at the *JEL* in 1975. As the institutions supporting the Post Keynesian

community changed in the late 1970s so did Eichner's standing as spokesperson. The most significant institutional change of this period was the creation of a Post Keynesian journal. It is at this point that the label changes ("post" is capitalized and the hyphen deleted) though the explanation for the change only came later.

#### 8.4.1 Sidney Weintraub and Paul Davidson as spokespersons

The idea of a dissenting journal appears to have originated early in the 1970s, out of a failure by dissenters in gaining access to the mainstream outlets. The 1972-73 protest towards the *AER* led by Eichner, agreed to in the 1971 meeting, proved to be without major consequences. As President of the AEA, Galbraith after receiving Eichner's protest letter asked the editor of the *AER*, George Borts, to respond to the dissenters' claim. His reply was that there was no discrimination and that the work dissenters had submitted simply did not live up to the quality standards of the *AER*. He further suggested that "a separate journal to identify the heterodox and divergent points of view" be formed and "a group to sponsor sessions at the annual meetings of the AEA" be set up.<sup>59</sup> Borts challenged Eichner to present evidence that papers were being discriminated against on the basis of ideological bias.

Davidson had also engaged in efforts of his own, following a debate between John G. Gurley and Robert Solow and published in the 1971 May issue of the *AER*,<sup>60</sup> he proposed to Galbraith to set a ten percent quota for "radical" economic papers. The response from Borts was: "There is no doubt that setting a quota of 10 percent as Davidson suggests would attract more articles in this area, but I don't think they would be the good ones. Moreover, every branch of economics can lay claim to a share of space in the Review."<sup>61</sup>

In September 1972 the *AER* editor responded to the criticisms by printing an editorial statement where he reaffirmed the editorial policy of encouraging "perfect

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<sup>59</sup>Letter from George Borts to Alfred S. Eichner, August 8<sup>th</sup> 1972 in Paul Davidson Papers, The Economists' Papers Project.

<sup>60</sup>This exchange figured prominently in chapter 3 of this thesis, p. 85.

<sup>61</sup>Letter from Davidson to Galbraith, 21<sup>st</sup> September 1971, and letter from George Borts to J.K. Galbraith, 4<sup>th</sup> October, 1971. Letters in Paul Davidson Papers, The Economists' Papers Project.

freedom in all economic discussion.”<sup>62</sup> At the Toronto AEA business meetings it was decided the *AER* should institute anonymous refereeing. Borts somewhat resisted the decision by implementing the new rule only to half of the refereed manuscripts and arguing that it had produced no effects. However, by January 1974 all manuscripts were anonymously refereed.<sup>63</sup> The changes were not enough to convince the protesting group, which continued to feel unfairly excluded from the *AER* and consequently without a scholarly outlet to present their research to the profession.

After the publication of the Eichner and Kregel survey article, meetings between American post-Keynesians intensified. It then seemed possible to organize locally in New York in addition to the yearly sessions at the ASSA meetings. So, in 1976 Eichner organised in Columbia a set of seminars on post-Keynesian topics.<sup>64</sup> The purpose was to bring together the then few post-Keynesian economists in working out what post-Keynesian economics was. The seminars were short-lived, six in total, with divisions emerging between the participants and the final seminar was held in December.<sup>65</sup>

Eichner and Davidson given the problems faced by the seminars sought to organise a conference. It was held at Rutgers University in April 16<sup>th</sup> 1977 on “Post-Keynesian theory and inflation policy,” with the purpose of serving “as a forum for the exchange of ideas and scholarship by those economists whose work falls within the tradition established by Keynes, Kalecki, Robinson, Kaldor and Sraffa.”<sup>66</sup> The conference lasted two and a half days and was attended by 65 economists from the USA and Canada.<sup>67</sup>

<sup>62</sup> *American Economic Review* (1973b), p. 503.

<sup>63</sup> Somewhat independently, in March 1974 and since John G. Gurley and Paul M. Sweezy were in the AEA's Executive Committee, Borts was ordered to take in a radical economist into his Board of Editors, he chose U.Mass.-Amherst's Stephen Resnick.

<sup>64</sup> The seminars included papers by John Eatwell, Paul Davidson, Sidney Weintraub, Harvey Gram and Basil Moore, see Lee (2000a), p. 30.

<sup>65</sup> In a letter to Robinson in Nov 9<sup>th</sup>, 1976, Eichner reports on the first seminar. It was attended by about 25 people and had John Eatwell presenting a paper where he combined the Kaleckian analysis of aggregate demand and distribution with the Sraffian value equations in order to analyze fiscal policy. Davidson in the meeting contested the Kaleckian interpretation of aggregate demand and the overall validity of Eatwell's efforts. Joan Robinson Papers, JVR Misc 79/4.

<sup>66</sup> Lee (2000a), p. 31.

<sup>67</sup> There were four sessions at the conference, the first entitled “The monetary aspect of inflation” having as chairman Davidson and as panellists H. Minsky and B. J. Moore; “The international dimension” with L. Tarshis as chairman and J. Burbridge and A. S. Eichner as panellists; “Incomes Policy

Sidney Weintraub's participation in the conference convinced him that the project of a journal for Keynesian economics was possible.<sup>68</sup> Creating a journal focusing on dissenting Keynesianism was a viable option since the doors of the *AER* seemed forever closed and given the group's surprising expansion witnessed in the conference. The *Journal of Post Keynesian Economics* saw its first issue in the Fall of 1978. The journal was Sidney Weintraub's project and he invited his former student Paul Davidson to share with him the editorial responsibilities. In the editors' statement of purposes we can read its goals, to: "encourage evolving analysis and empirical study to contest the conformist orthodoxy that now suffuses economic journals in the United States", "committed to the principle that the cumulative development of economic theory is possible only when the theory is continuously subject to challenge."<sup>69</sup> The definition of the camp of acceptable papers was made in the negative, with the refusal of all those neoclassical models that: "(1) assume automatic full employment; (2) ignore the vital presence of a public sector; (3) postulate perfect certainty; and (4) provide only "long run" steady state solutions."<sup>70</sup>

As a definition of Post Keynesian economics the statement was sufficiently vague to imply a broadening of the Post Keynesian camp. But what sort of broadening was it? Who was to be included? These questions are important because the journal and its editorial policies held the power to reshape the field of Post Keynesian Economics by giving voice to some authors and perspectives over others.

#### 8.4.2 A broader Post Keynesianism?

To better understand the content of this change one needs to look into the work of Paul Davidson and to his changing views on the landscape of economics. Unlike Eichner, Davidson was no enthusiast of Kuhn's philosophy of science, he preferred the more traditional reference to "schools of thought." In 1972 Davidson had identified

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as a Stabilization Device" chaired by S. Weintraub and with R. Marris and L. Seidman and finally "Beyond Post-Keynesian Theory: the Unexplored Issues" with E. Nell as chair and J. Kregel and S. Rousseas in the panel and with G. C. Harcourt as commentator.

<sup>68</sup>Turner (1989), p. 196.

<sup>69</sup>Weintraub and Davidson (1978), p. 3.

<sup>70</sup>Idem.

five schools of thought (see figure 8.2.) characterised along such dimensions as “Politics,” “Money,” “Wage rate and income distribution,” “Capital Theory,” “Employment Policy,” “Inflation” and “Government Role”. Davidson further characterised his work and that of the “Keynes’s school” as developed along three basic assumptions:

that in the real world (1) the future is uncertain (in the sense that Knight and Keynes used the term), (2) production takes time and therefore, if production is to occur in a specialization economy, someone must make a contractual commitment in the present involving performance and payment in the uncertain future, and (3) economic decisions are made in the light of an unalterable past, while moving towards a perfidious future<sup>71</sup>

In 1972, what was to become under Eichner’s labelling the “post-Keynesian paradigm” was identified by Davidson as the “neo-Keynesian school.”<sup>72</sup> Despite Davidson’s stated efforts to integrate Cambridge England views into his work,<sup>73</sup> he nonetheless preferred to locate himself in the “Keynes’ school” at the centre of politics and economics.

Ten years later, in 1982, following the creation of the *JPKE*, Davidson produced a similar table of schools of thought (see figure 8.3).<sup>74</sup> The significant change is the inclusion of a brace under some of the schools to identify the formation of the Post Keynesians:

Post Keynesian economists are primarily an amalgam of those from the Keynes and Neo-Keynesian schools, but are also joined by some right-leaning members of the socialist-radical group, such as Galbraith. Moreover, certain left-leaning members of the neoclassical Keynesian school have exhibited some sympathy for Post Keynesian analysis in recent years (e.g. John Hick’s writings of the late 1960’s and 1970’s).<sup>75</sup>

<sup>71</sup>Davidson (1972), p. 7.

<sup>72</sup>Recall that this was Harcourt’s (1969) use of the term.

<sup>73</sup>See p. 202 of this thesis.

<sup>74</sup>Davidson (1980) lists in socialist-radical the authors “Galbraith, Bowles, D. Gordon, all Marxists,” in Neo-Keynesians “Robinson, Kaldor, Sraffa, Pasinetti, Eichner, Kregel, Harcourt,” in “Keynes” “Harrod, Shackle, Weintraub, Davidson, Minsky, Wells, Vickers,” in “Neoclassical synthesis Keynesian” “Solow, Samuelson, Tobin, Clower, Leijonhufvud, J.R. Hicks” and finally in “Monetarist-Neoclassical” “Friedman, Brunner, Meltzer, Parkin, Laidler.”

<sup>75</sup>Davidson (1980), p. 152.

Table 1.1  
A TABLE OF POLITICAL ECONOMY SCHOOLS OF THOUGHT

	<i>Socialist-Radical</i>	<i>Neo-keynesian</i>	<i>Keynes</i>	<i>Neoclassical-Bastard Keynesian</i>	<i>Monetarist-Neoclassical</i>
Politics	Extreme left	Left of centre	Centre	Right of centre	Extreme right
Money	Real forces emphasised – money merely a tool for existing power structure	Real forces emphasised, money assumed to accommodate	Money and real forces intimately related	Money matters along with everything else	Only money matters
Wage rate and income distribution	Wage rate basis of value. Income distribution the most important economic question	Money wage is the linchpin of the price level. Income distribution very important	Money wage rate fundamental; income distribution question of less importance	Wage rate one of many prices. Income distribution is the resultant of all the demand and supply equations in a general equilibrium system. Income distribution a matter of equity, not of 'scientific' inquiry.	
Capital Theory	Surplus generated by reserve army	Surplus needed over wages	Scarcity theory (quasi-rents)	Marginal productivity theory and well-behaved production functions	
Employment Theory	Any level of employment possible. Assumes growth in employment overtime. Full employment creates crisis for capitalism	Growth with any level of employment possible, although growth at full employment emphasised	Any level of employment possible; full employment desirable	Full employment assumed; unemployment is a disequilibrium situation	Full employment assumed in long run; no explicit short-run theory of employment
Inflation	Primarily due to money wage changes, but can also be due to profit margin changes	Due to money wage or profit margin changes	Due to changes in money wages, productivity and/or profit margins	In long-run primarily a monetary phenomenon being related to money supply via portfolio decisions. In short run may be related to Phillips curve	Primarily a monetary phenomenon in the sense of being related to the supply of money via portfolio decisions
Government role	Socialise the capitalist sector	Laissez-faire except for macroeconomic controls over incomes	Laissez-faire except for macroeconomic controls over money, investment decisions, and the earnings system	Laissez-faire except for externalities and some ad hoc macro-controls	Laissez-faire

Figure 8.2: Paul Davidson's "Table of Political Economy" in his 1972 *Money and the Real World*, p. 4.

But there were other (minor) changes to Davidson's "Table of Political Economy." "Neo-keynesian" was renamed to "Neo-Keynesian," i.e. the "K" capitalized, and "Neoclassical-Bastard Keynesian" was replaced by "Neoclassical Synthesis-Keynesian." Now (partially in the case of neoclassical Keynesian school) grouped under Post Keynesian economics these schools of thought deserved greater respect, thus the small "k" to capital "K" change and the substitution of "bastard" for "synthesis."

Davidson's clearest statement (in the late 1970s and early 1980s) on the definition of Post Keynesian Economics appeared in 1980. It was part of a symposium entitled "The Crisis in Economic Theory" marking the fifteenth anniversary of the journal *Public Interest*. The debate was prefaced by Peter F. Drucker, according to whom economics had undergone four major "paradigms" shifts<sup>76</sup> and it was on the throes of a fifth "scientific revolution," with the demise of

<sup>76</sup>The first revolution from the Cameralists and Mercantilists to the Physiocrats, the second with the Physiocrats replaced by the Classicals, a third with the advent of the neoclassicals, from the



TABLE 1.1 A Table of Political Economy

	<i>Socialist-Radical</i>	<i>Neo-Keynesian</i>	<i>Keynes</i>	<i>Neoclassical Synthesis-Keynesian</i>	<i>Monetarist-Neoclassical</i>
Politics	Extreme left	Left of centre	Centre	Right of centre	Extreme right
Money	Real forces emphasized - money merely a tool for existing power structure	Real forces emphasized, money assumed to accommodate	Money and real forces intimately related	Money matters along with everything else	Only money matters
Wage Rate and Income Distribution	Wage rate basis of value. Income distribution the most important economic question	Money wage is the linchpin of the price level. Income distribution very important	Money wage rate fundamental. Income distribution question of less importance	Wage rate one of many prices. Income distribution is the result of all the demand and supply equations in a general equilibrium system. Income distribution a matter of equity, not of 'scientific' enquiry	
Capital Theory	Surplus generated by reserve army	Surplus needed over wages	Scarcity theory (quasi-rents)	Marginal productivity theory and well-behaved production functions	
Employment Theory	Any level of employment possible. Assumes growth in employment overtime. Full employment creates crisis for capitalism	Growth with any level of employment possible, although growth at full employment emphasized	Any level of employment possible. Full employment desirable	Full employment assumed. Unemployment is a disequilibrium situation	Full employment assumed in long run. No explicit short-run theory of employment
Inflation	Primarily due to money wage changes, but can also be due to profit margin changes	Due to money wage or profit margin changes	Due to changes in money wages, productivity and/or profit margins	In long run primarily a monetary phenomenon being related to money supply via portfolio decisions. In short run may be related to Phillips curve	Primarily a monetary phenomenon in the sense of being related to the supply of money via portfolio decisions

} Post Keynesian

Figure 8.3: Paul Davidson's "Table of Political Economy" in his 1982 *International Money and the Real World*, p. 2.

Keynesian theory and policy of the preceding 30 to 50 years. In the *Public Interest* symposium, under a section "Radical Critique," Paul Davidson authored a paper: "Post Keynesian Economics: solving the crisis in economic theory." The paper is of significance since it gave a *JPKE*'s editor's definition of Post Keynesian Economics.

Davidson began by arguing that Keynes' revolution had been aborted by the failure of neoclassical economists to comprehend Keynes' "logic". Post Keynesians were those that understood Keynes, "Post Keynesians (...) share the view that Keynes provided a revolutionary new *logical* way of analysing a *real world* economy."<sup>77</sup>

Davidson added in a footnote:

There is no hyphen between the words "Post" and "Keynesian." In the past there has been a lack of uniformity in labelling schools of thought; hence perceptive readers will note that others have used the term Post-Keynesian (with the hyphen) to designate the school that I have labelled Neo-Keynesian. In the order to provide a nonambiguous

neoclassicals to the Keynesians was the fourth (Drucker (1980), pp. 7-9).

<sup>77</sup>Davidson (1980), p. 152, emphasis in original.

terminology I have eliminated the hyphenated term from this text and request that others do the same.<sup>78</sup>

Davidson explained his change in label differently to different audiences. If in the *Public Interest* volume he portrayed it as a clarification, for Post Keynesians he described it as a broadening of the approach. Robinson, when she received the early issues of the *JPKE*, praised Davidson's work and commented: "I am rather bothered by the way Al Eichner set up as the spokesman for what all Post Keynesians think and he then slips in fancy ideas of his own. I hope you are keeping an eye on him."<sup>79</sup> Davidson replied:

I know what you mean regarding Al Eichner's attempt to straight jacket what Post-Keynesian school means. If you will note that Al always uses a hyphen between the Post and the Keynesian and we at the Journal have specifically removed the hyphen. It is a subtle way to suggest that Post Keynesian in the view of the *JPKE* is much broader than Post Keynesian in the view of Al Eichner.<sup>80</sup>

The nature of this "subtle" change was acknowledged by Eichner who, in a footnote to his first *JPKE* article "A post-Keynesian short-period model", wrote:

The appearance of this journal [*JPKE*], as well as the series of articles in *Challenge* magazine beginning with the May of 1978 issue, are further evidence that a distinct body of post-Keynesian theory is coming to be recognized. It should be noted, however, that the present journal, by omitting the hyphen, hopes to encompass a much broader approach than the one identified here as "post-Keynesian."<sup>81</sup>

Despite what may be taken from statements by the protagonists, I suggest that there was more to Davidson's redefinition than a mere broadening of the scope of Post Keynesian Economics. Davidson's *Public Interest* article characterized Post

<sup>78</sup>Idem.

<sup>79</sup>Letter from Robinson to Davidson, 2<sup>nd</sup> April 1979. Joan Robinson Papers, JVR/vii/144/8.

<sup>80</sup>Letter from Paul Davidson to Joan Robinson, 3<sup>rd</sup> May 1979. Joan Robinson Papers, JVR/vii/114/173. It is also likely that Weintraub and Davidson's choice of label reflected a desire to differentiate their dissenting approach from Kurihara's conventional Keynesian 1955 book, mentioned earlier in this chapter. The need for this differentiation was all the more poignant given that Kurihara and Davidson were colleagues at Rutgers University.

<sup>81</sup>Eichner (1979b), p. 38.

Keynesian models along axiomatic lines and a reading of these does not suggest that a broadening of the group was implied. Davidson noted three propositions shared by all Post Keynesian models: the economy is a historical process; in a world where uncertainty is unavoidable, expectations have an unavoidable and significant effect on economic outcomes, and political and economic institutions play a significant role in shaping economic events.<sup>82</sup> This was Davidson's 1972 definition of the "Keynes's school." Following Davidson, Keynes and the Keynes's school should become the centre of economics and the defining core of Post Keynesian Economics.<sup>83</sup>

Weintraub, co-editor of the *JPKE*, was not as vocal in discussing the definition of Post Keynesian Economics. Yet, he seemed to be in agreement with Davidson's definitional work. He joined Davidson in castigating Samuelson for describing himself a "Post-Keynesian" in the 1980 edition of *Economics*. The criteria for disallowing Samuelson's use of the label were those outlined in Davidson's writings.<sup>84</sup>

The aim of Davidson's definitional work was unlike Eichner's continued efforts. Through books, seminars and the graduate work of his students Eichner kept his long time pursuit of paradigm construction.<sup>85</sup> He was not only trying to extend post-Keynesian economics to new fields of research but also working out a synthesis that might unite the different perspectives and preferences of the diverse group of economists. In the words of William Millberg, one of Eichner graduate students in the late 1970s: "He really had 'this grand design', . . . , which was an attempt both to give an overview of Post Keynesian economics and to synthesise the conflicting strands."<sup>86</sup>

Davidson did not seek a unifying paradigm. He did not call for the *construction* of a new approach that united the varied contributions, fusing them into one. Instead, he aimed to *stabilize* the alliance activity of the previous decade by noting a shared communality in Keynes and the axioms of the "Keynes's school", departing

<sup>82</sup>By institutions Davidson meant money and contracts, in particular spot and forward price contracts. (Davidson (1980), pp. 158-164). This definition is very close to Davidson's (1972) definition of the "Keynes" school.

<sup>83</sup>Notably Davidson (1980; 1982).

<sup>84</sup>Davidson and Weintraub (1981). Similarly Weintraub (1978-79; 1979) pressed the same buttons than Davidson in defining Post Keynesian Economics, notably in the stress given to uncertainty and Keynesian "animal spirits."

<sup>85</sup>Eichner (1979a).

<sup>86</sup>King (1995a), p. 53.

from these diversity was apparently allowed. Davidson's definition provided stability because unlike Eichner's anxious calls for new formulations of an (elusive) paradigm, it demanded nothing of Post Keynesians. Their communality in Keynes was said to be a *fait accompli*. Keynes was raised to a new prominence as the foundation, and as what bound Post Keynesian economists together. That Davidson became the dominant spokesperson of the Post Keynesian community can be seen from how discussions were re-centred on how to interpret Keynes and his legacy and no longer, along Eichner's vision, on designing a single paradigm that encompassed all authors (see Table 8.2.).

Table 8.2: Label's definitions, spokespersons until 1982.

Period	Spelling	Spokesperson	Refers to
1950s-60s	Post-Keynesian	...	Economics
Late 1960s	Anglo-Italian	Robinson	Cambridge, England side of Capital Controversies
Early 1970s	Post-Keynesian	Eichner	Paradigm of Cambridge, England and their American following
Late 1970s	Post Keynesian	Davidson	Communality between Cambridge England, American following and other dissenting Keynesians

### 8.5 The Trieste summer school (1981-85)

Recently, Davidson has claimed that his late 1970s, early 1980s broad definition of Post Keynesian Economics was merely a strategy to battle the mainstream.<sup>87</sup>

Davidson's statements in 2005 are a defence of his current definition of Post Keynesian Economics over his own earlier pronouncements. Portraying a continuity in his "true beliefs" (inevitably his current ones), for Davidson the past was no more

<sup>87</sup>Davidson (2005), p. 395.

than an accidental deviation. To give credit to Davidson's present reflections obscures the conditions which lead him from a federation of adjacent schools of thought in 1980-82, to his current definition of Post Keynesian Economics as a narrowly inscribed and distinct school of thought. To attribute such changes in definition merely to Davidson's change of mind is evading the issue of why he changed his mind. Once again, change in the definition of Post Keynesian Economics followed from changes in its institutional make-up. With institutional change came a new set of spokespersons who used their power to redraw the boundaries of Post Keynesian Economics. What marked the early 1980s in the history of the Post Keynesian community was the organization of a Summer School in Trieste, Italy.

### 8.5.1 Many spokespersons

Kregel was in Rome, Italy, in the late 1970s working as adviser to the federation of employers. Kregel with Pierangelo Garegnani devised the project of an informal school, which was facilitated under Italian law, for Cambridge-related dissenting ideas. Sergio Parinello, a colleague of Garegnani in Rome, had contacts with the education authorities in the autonomous region of Trieste and secured the organization of a Summer School.<sup>88</sup>

A Roundtable in Udine-Trieste, Italy, held from September 22<sup>nd</sup> to 28<sup>th</sup> in 1980<sup>89</sup> was organized to discuss the creation of an international summer school, "The general format of the School would be such as to bring together students and teachers interested in pursuing specific aspects of economic theory and policy critically and constructively in an informal setting."<sup>90</sup> For the purpose of organizing the Summer School, a Centre of Advanced Economic Studies was created.<sup>91</sup> It was projected that

<sup>88</sup>Interview with Jan Kregel, 2003.

<sup>89</sup>The sessions at the Roundtable were: "An Evaluation of the Theory of Value and Distribution from the Classics to the Modern Neo-Ricardians"; "The Principle of Effective Demand in the Short and Long Periods"; "Monetary Theory and Modern Economics"; "Modern Economic Theory as a Basis for Economic Policy"; Economic Growth and Development: A Developed or Developing Country Problem?"; Discussion of International Summer School"; "The Theory of International Exchange and the Problem of Natural Resources"; "Organizational Session for the School". Letter from Angelo Mazollo to Paul Davidson, March 11, 1980, Paul Davidson Papers, The Economists' Papers Project, Box 4.

<sup>90</sup>Idem.

<sup>91</sup>It included Giampaolo de Ferra (Rector of University of Trieste), Pierangelo Garegnani (University

25 to 30 students<sup>92</sup> and 20 to 25 teachers would attend. The organizing committee was composed by Pierangelo Garegnani, Jan Kregel and Sergio Parrinello. There were four broad areas of study defined for the school: “a) value and distribution; b) money and effective demand; c) theory and policy of development and international economics, d) fact and experience in economic policy.”<sup>93</sup> The format of the school would comprise a lecture series, seminars (with teachers and students in discussion) and a conference as its conclusion.

The first International Summer School in Trieste was held from the 23<sup>rd</sup> of August to the 3<sup>rd</sup> of September of 1981 and was concluded with a “Conference on Distribution, Effective Demand and International Economic Development” on the 4<sup>th</sup> and 5<sup>th</sup> of September. The papers from the conference with comments by discussants were published by Kregel.<sup>94</sup> It is the examination of these comments that is most telling for the present history since these evidence a debate within the school.

The first paper of the conference volume was Krishna Bharadwaj’s “On effective demand: certain recent critiques.”<sup>95</sup> In this text the author aimed to bring the critical insights from classical economists to bear on Keynes’ theory of effective demand. Bharadwaj argued:

While there is no denying the need to incorporate real world considerations and the fact that expectations and uncertainty are endemic in such situations, there is also the need to provide theoretical structures that lead to firm and meaningful results with real-world applications. It has been argued here that a firmer theoretical scaffolding may be available in the surplus theory which offers greater promise in this direction.<sup>96</sup>

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of Rome), Jan Kregel (University of Groningen), Angelo Marzollo (CISM and University of Udine), Sergio Parinello (University of Rome –Chairman of the Centre). Letter from Sergio Parinello to Sidney Weintraub, 6<sup>th</sup> December, 1980 at Sidney Weintraub Papers, The Economists’ Papers Project, at Rare Book and Manuscript Library, Duke University.

<sup>92</sup>Of the students it was thought that six would come from the USA, one from Australasia, one from Canada, four from Latin America, 4 from India and other Afro-Asian countries, five from Italy, seven from other Western European countries and two from socialist countries. Letter from Sergio Parrinello to Sidney Weintraub, 6<sup>th</sup> December, 1980, Sidney Weintraub Papers, The Economists’ Papers Project.

<sup>93</sup>Letter from Sergio Parinello to Sidney Weintraub, 6<sup>th</sup> December, 1980 at Sidney Weintraub Papers, The Economists’ Papers Project.

<sup>94</sup>Kregel (1983a).

<sup>95</sup>Bharadwaj (1983).

<sup>96</sup>Bharadwaj (1983), p. 24.

Bharadwaj was advancing a critique originated by Garegnani.<sup>97</sup> He had engaged Robinson in a controversy over Keynes' concept of effective demand within a Sraffian system of production.<sup>98</sup> Garegnani denied that Keynes had anything to contribute to a long-period Sraffian theory of interest/profit. This critique was enunciated as an alternative approach – the surplus approach.

The claims made by the surplus approach were contested by Asimakopulos and Minsky. Asimakopulos argued that Bharadwaj had not understood Keynes' account with regards to uncertainty and distribution.<sup>99</sup> Furthermore, he denied Bharadwaj's crucial distinction between a short and long-period. Minsky was more clearly critical of Bharadwaj,<sup>100</sup> he saw the author operating a flawed separation between the financial and the real world: "One great advance that I have always associated with Keynes is that he held that we cannot dichotomise the financial and the "real" when it comes to understanding capitalism."<sup>101</sup> Evidence of a debate in the first conference of the Summer School was also contained in the second paper of the volume "Effective Demand: Origins and Development of the Notion" by Kregel. The paper was introduced with the comment: "During the last few days [of the summer school] much has been said and written about "long-period" and "short-period" theories of "effective demand" ."<sup>102</sup>

The controversy expanded in the years that followed. In the Trieste Summer School there were increasingly bitter disputes. A few years later, Kregel spoke vividly of the mood:

The faceoff that was to take place between you [Davidson] and Eatwell was replaced by the two of us (...) this time I think I clearly got the best of him, including some fancy footwork which got me the right to the last reply of the session. Musu supported me very strongly, while Nell was dispersive. Harcourt also was very strongly on our side in first session and Roncaglia supported Minsky and attacked Vianello and the "Surplus people" as they are now called in the policy session. Piero [Garegnani] left

<sup>97</sup>Garegnani (1979a).

<sup>98</sup>Robinson (1979); Garegnani (1979b).

<sup>99</sup>Asimakopulos (1983), p. 31.

<sup>100</sup>Minsky (1983).

<sup>101</sup>Minsky (1983), p. 49.

<sup>102</sup>Kregel (1983b), p. 50.

a day early.<sup>103</sup>

But debate also continued outside the school's walls. The Summer School ignited a flurry of exchanges between members the group, which were printed in the pages of the *Cambridge Journal of Economics*. It developed into a battle over the intellectual legacy of Cambridge. On the one hand were those that identified with Keynes and critiqued Sraffa. On the other those that found Keynes to be a neoclassical and held Sraffa as the crucial reference point.

One exchange that I wish to highlight appeared in the *Cambridge Journal of Economics* volume dedicated to Joan Robinson in 1983. John Eatwell wrote on "The long theory of employment" arguing that Keynes lacked an adequate critique of output theory, but added that such critique was possible following the Capital Controversies. He wrote: "it must be admitted that the analytical imperfections in Keynes's theory have contributed to the decline [of Keynesian theory]. The structure needed to be reworked; the new, innovative elements (the principle of effective demand) retained, and the insidious remnants of orthodox theory discarded."<sup>104</sup> Further blame was assigned by Eatwell to some of Keynes' followers for taking a short-run interpretation of his work, "The 'short-run' interpretation of *The General Theory* provided a convenient means of absorbing 'Keynesian economics' into the mainstream of orthodox theory."<sup>105</sup> An alternative long period theory of employment needed to be designed, a "theory of output that may be considered the concomitant of the long-period or 'normal' theory of value and distribution."<sup>106</sup>

Kregel was the one to respond against such views. His article was aimed at answering an emerging critique that rejected

certain aspects of Keynes' work which give a "central role to uncertainty and expectations" for, it is suggested, such factors involve theoretical "weaknesses" which facilitated "the subsequent rehabilitaton of the orthodox long-run relation between savings and investment" in the

<sup>103</sup>Letter from Jan Kregel to Paul Davidson, 19<sup>th</sup> September 1984, Paul Davidson Papers, The Economists' Papers Project, Box 3.

<sup>104</sup>Eatwell (1983), p. 270.

<sup>105</sup>Ibid, p. 272.

<sup>106</sup>Ibid, p. 271.



“neoclassical synthesis” and the “neo-neoclassical” theory of long-run growth.<sup>107</sup>

Kregel saw no flaws in Keynes’s theory, instead, Keynes was the sound foundation for all future work. The paper, written for a volume dedicated to Joan Robinson, made of Robinson a guide to travel the literature. Robinson’s early work was characterised as a search for a long-period theory to supplement Keynes’s short-period analysis. According to Kregel, her efforts only led her to see such a project as undesirable. With Robinson Kregel criticised the Eatwell project: “Joan Robinson’s position [abandoning the search for long-period analysis] would seem to reflect a realistic scepticism concerning application of stationary neoclassical theory or the natural positions of the surplus approach to the analysis of the problems of accumulation and employment.”<sup>108</sup>

Richard Arena, a participant of the Summer School, later characterized the positions that developed in the Trieste Summer School. According to Arena, there were two main positions, the “post-Keynesian theory” and the “surplus approach theory.” The former included authors such as Davidson, Weintraub, Minsky, Eichner, Kregel, Robinson, Kaldor and Pasinetti’s 1960s work. The latter included the work of Garegnani, Milgate, Vianello and others.<sup>109</sup>

Post-Keynesian theory was described by Arena as an analysis of monetary production economies in both the short and the long-period. The existence of uncertainty over the future of the economy is the reason for the institution of money, which was seen as a refuge against the insecurity of investment by permitting arbitrage between productive and speculative activities. The short period analysis revolved around the principle of effective demand and allowed the determination of production and employment levels while the long-period was centred on a theory of the level of investment and liquidity preference.<sup>110</sup>

According to Arena, the surplus approach saw itself as an inheritor of classical

<sup>107</sup>Kregel (1983c), p. 343.

<sup>108</sup>Ibid, p. 360.

<sup>109</sup>I note that the account given here is focused on Arena’s (1987) two main groups, he further lists dissenting authors and positions inside each group but it is not my aim to discuss this.

<sup>110</sup>Arena (1987), pp. 209-212.

theory, with Marx as the most developed contribution. For these authors the task at hand was to construct a classical theory of the long-period taking some elements from Keynes, notably the role played by variations of income for the process of equalization of investment and savings, but an abandonment of the notion of the marginal efficiency of capital as determining investment. Their long-period theory would provide a framework to treat problems of distribution, production, accumulation and money. Critically it had no place for uncertainty and expectations.

The Summer School debate became the all consuming focus of Post Keynesians' work in this period. The solidarities constituted in the debates developed into institutional form. In 1983, a group of "surplus people" from Cambridge created the journal *Contributions in Political Economy*, a yearly journal that exists to this day.<sup>111</sup> In 1985 another journal titled *Political Economy: studies in the surplus approach* was created. The organization of the summer school was redesigned to reflect the various constituencies. Following the summer school's second year<sup>112</sup> "the Scientific Committee of the Center for Advanced Economic Studies, considering the opportunity of a more agile organization of the School, approved a Working Regulation which provides for a division within the competence of the Committee into three sections."<sup>113</sup> Three areas of competence were created, section A, devoted to "the surplus approach; criticism of marginalism; the theory of effective demand and the long period" was coordinated by Pierangelo Garegnani; section B, focusing on: "the Post Keynesian approach; criticism of marginalism; money and finance" was coordinated by Jan Kregel;<sup>114</sup> finally section C, "analytical economics," devoted to the themes of "development and technical progress; economics of international trade and natural resources; mathematical economics with a critical perspective"

<sup>111</sup>Its three-person editorial board from its inception is composed by John Eatwell, Murray Milgate and Giancarlo de Vivo.

<sup>112</sup>In 1982 the school was held from the 18<sup>th</sup> to the 29<sup>th</sup> of August with a conference on "Theories of Accumulation and the control of the economy" in 30 and 31 August. The conference had two papers by Garegnani, commented by Kregel, Nell, Parrinello, and Vianello; and another by S. Weintraub, commented by Bharadwaj, Davidson, Harris and Pasinetti.

<sup>113</sup>Letter from Parrinello to Weintraub, 13<sup>th</sup> January 1983. Sidney Weintraub Papers, The Economists' Papers Project.

<sup>114</sup>Its Advisory Board included: Davidson, Harris, Minsky, Nell, Pasinetti, Steiger, Vicarelli, Weintraub and Sylos Labini.

coordinated by Sergio Parrinello.<sup>115</sup>

With such strenuous cohabitation the Summer School was held only until 1985.<sup>116</sup> However, it seems the assessment by some was not wholly negative:

We [Paul and Lousie Davidson] really had become addicted to it [Summer School]. If it disappears an important unifying force in Post Keynesian economics will be gone. I really believe that it did a tremendous good in developing young people from all over the world. Of course, not the entire class, but even if only 10 to 25 % of the class each summer was inspired by the Summer School, that did a world of good for Post Keynesian Economics.<sup>117</sup>

The summer school had ran its course in the expansion of the group, it had allowed a number of graduate students to come into contact with the work and discussions that characterised Post Keynesian economics but it had also significantly transformed it. A surplus' approach had consciously taken shape and led a critical charge on other theoretical perspectives. Soon, two positions had formed increasingly independent and embittered. Although, it has been alleged that most of the ill temper resulted from Garegnani's zealous pursuit of the surplus approach, it was a transformation more permanent than the vagaries of personality.

### 8.5.2 Post Keynesian Economics or "horses for courses"?

The impact of the Trieste Summer School and the discussions it ensued can be best assessed in later pronouncements over the definition of Post Keynesian Economics. In

<sup>115</sup>Letter by Sergio Parinello to Weintraub, 13<sup>th</sup> January 1983, in Sidney Weintraub Papers, The Economists' Papers Project. The third Summer School was held in August 1983 and concluded with a conference entitled: "The economic dynamics of resources, technology and employment – theories and policies for open economies", from the 1<sup>st</sup> to the 3<sup>rd</sup> of September. The 1984 Summer School ran from the 27<sup>th</sup> to the 31<sup>st</sup> of August. It concluded with the Conference on "Streams of Economic Thought: A critical appraisal of the last four summers' work in Trieste-Udine" between the 1<sup>st</sup> and the 3<sup>rd</sup> of September. The 1985 Summer School was held between the 29<sup>th</sup> of August and the 3<sup>rd</sup> of September. That year's conference was on "Alternative Policy Analyses for Third World Development" on the 4<sup>th</sup> and 5<sup>th</sup> of September.

<sup>116</sup>"The Summer School for next year is off. PG [Pierangelo Garegnani] has refused to accept any of Sergio's proposed changes and without them neither he nor I will organise the School for next year. I have now proposed that the Centre be dissolved, but we couldn't do it in the last meeting (...). We then need to decide if we want to form a new one, or if the thing has run its useful course." Letter from Kregel to Davidson, 23<sup>rd</sup> November 1985, Paul Davidson Papers, The Economists' Papers Project.

<sup>117</sup>Letter from Davidson to Kregel, 5<sup>th</sup> December 1985. Paul Davidson Papers, The Economists' Papers Project.

August 1987, at a conference entitled “New Directions in Post-Keynesian Economics” held at Great Malvern, one such attempt was put forward by Omar Hamouda and Geoff Harcourt. The conference was asked to “consider the past achievements and future prospects of post-Keynesianism.”<sup>118</sup> Hamouda and Harcourt’s text was a overview of the field, “It considers the different strands of post-Keynesianism and questions whether an integrated and coherent research tradition can emerge. After clarifying what post-Keynesianism means to different groups, the authors conclude that attempts to integrate the three identifiable strands are doomed to failure.”<sup>119</sup> Hamouda and Harcourt’s proposal<sup>120</sup> is still seen as an influential statement on the nature of Post Keynesian Economics, it has been reprinted numerous times and has been extensively cited.

What Hamouda and Harcourt put forward was a new definition for Post Keynesian Economics:

Post Keynesian economics is thus a portmanteau term which contains the work of a heterogeneous group of economists who nevertheless are united not only by their dislike of mainstream neoclassical theory and the IS/LM general equilibrium version of the “Keynesian” theory but also by attempts to provide coherent alternative approaches to economic analysis. (...) We say “approaches” because we may identify several strands which differ from each other both with regard to method and with regard to the characteristics of the economy which are included in their models.<sup>121</sup>

Hamouda and Harcourt identified three strands inhabiting Post Keynesian Economics. The first was the “American Post Keynesians” who included Sidney Weintraub, Davidson, Kregel and Minsky:

The implications of the *Treatise [on Money]* and the *General Theory* were the base on which the American post Keynesians built. They stressed uncertainty, the necessary integration of money from the start of analysis of the workings of the economy, the central position of the money-wage as

<sup>118</sup>Pheby (1989), p. ix.

<sup>119</sup>Idem.

<sup>120</sup>I shall use the 1988 *Bulletin of Economic Research* version of the 1987 conference paper, the paper was also published in the 1989 book collecting contributions to the Malvern conference (Pheby (1989)).

<sup>121</sup>Hamouda and Harcourt (1988), p. 2. Although Pheby hyphenised the label, it is interesting to note that Hamouda and Harcourt used the *JPKE* version.

both the major determinant of the price level and of the stability (or instability) of the economy, and the stock-flow interrelationship of the process of capital accumulation<sup>122</sup>

This definition is evocative of Davidson's pronouncements on Post Keynesian Economics and the Kregel side at the Trieste Summer School battles.<sup>123</sup> The second strand referred to the work of Garegnani, Bharadwaj, Eatwell, Milgate and Pasinetti, know as the neo-Ricardians. They were said to argue for "a theory of a *long-period* level of income and employment (in the sense of the ultimate outcome of persistent forces) that should be placed alongside the classical theories of value and distribution".<sup>124</sup> Finally the third strand was said to correspond the work of Robinson and her followers,<sup>125</sup> and was characterised as holding that

the real wage is historically determined by the state of the class war (amongst other factors), and it determines in turn the *maximum* rate of profits available. Whether what is *potentially* there is realized *in fact* as a rate of profits and a rate of accumulation depends upon the forces of effective demand.<sup>126</sup>

Post Keynesian Economics was defined by Hamouda and Harcourt as a reunion of approaches which shared two general characteristics. Firstly, the approaches were represented as strands coming out of classical political economy; by Keynes' critique of Marshall, by Sraffa's extension of Ricardo's model economy, and by Kalecki's development of Marx. A set of ancestors, of canonical figures were thus identified: Ricardo, Marx, Keynes, Kalecki, Sraffa and Robinson. The references to these founding parentage were aimed at showing a common tradition shared by all Post Keynesians. A second characteristic was that all the strands advanced critiques of neoclassical economics.

<sup>122</sup>Ibid., p. 6.

<sup>123</sup>Davidson's (1980; 1982); Arena (1987).

<sup>124</sup>Hamouda and Harcourt (1988), p. 9.

<sup>125</sup>The three strands did not cover the whole spectrum of contributions, certain authors overlap the barriers, bridging strands: Kaldor and Robinson are seen as important for all strands; Pasinetti and Goodwin for the Neo-Ricardians and Kaleckians; and Shackle for the American Post Keynesians and Kaleckians.

<sup>126</sup>Hamouda and Harcourt (1988), pp. 12-13.

The authors were justifying a reunion of the different approaches under the banner of Post Keynesian Economics, they suggested that approaches “differ from one another, not least because they are concerned with different issues and often different levels of abstraction of analysis.”<sup>127</sup> Though recognising that at times disagreement emerged in the Post Keynesian camp, implying that strands overlapped in their interests and research, the authors stressed that their emphases are essentially different. They warned that paradigmatic unity should not be sought in synthesizing the different strands in order to see if a coherent whole emerged, this would be looking for “another box of tricks.” The dreaded weakness, diversity, was redressed as a strength. They noted that “the policies that may be rationalised by post Keynesian analysis are very much geared to concrete situations, the historical experiences and the sociological characteristics of the economies concerned.”<sup>128</sup> Diversity which was an obstacle to the creation of a single approach, was turned into a necessary feature of the pluralist theory. This prospect was dubbed a “horses for courses” approach.

The redefinition of Post Keynesian Economics as a “horses for courses” approach was a response to the schisms brought about by the Trieste Summer School. It identified a shared ancestry that would accommodate approaches that refused unity in terms of any theoretical precepts. The connection between this proposal and the Trieste events has been explicitly noted by Davidson.<sup>129</sup> But more significantly, the argument of Hamouda and Harcourt’s survey had been anticipated in 1982 by Harcourt.<sup>130</sup> His article titled “Post-Keynesianism: Quite Wrong and/or Nothing New” was originally a seminar given on the 16<sup>th</sup> of October 1981 at the Reserve Bank of Australia, as Harcourt returned from the first Summer School. Already in this 1981-2 article, Harcourt identified three strands in Post Keynesian Economics: the two battling factions of Trieste and a third where he located himself. He also anticipated the “horses for courses” argument, but this earlier article compared to the 1988 one was distinctively more optimistic, since Harcourt claimed that shared

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<sup>127</sup>Ibid., p. 25.

<sup>128</sup>Idem.

<sup>129</sup>Davidson (2004), p. 263, footnote 21.

<sup>130</sup>Harcourt (1985). I cite from the 1985 reprint of the *Thames Papers in Political Economy* 1982 article.

intellectual commitments existed between the strands. He noted a “preoccupation with time” arising from Robinson’s critique of equilibrium and advocacy of models in historical time; secondly the “importance of social relationships and institutions in post-Keynesian theory.”<sup>131</sup>

In the early to middle 1980s it became increasingly difficult to hear Post Keynesians speak at a single voice. In America the dominant definition of Post Keynesian Economics was still that coming from the *JPKE*. Both Davidson and Kregel argued for a definition of Post Keynesian Economics centred on Keynes’s legacy. Kregel in 1983 defined Post Keynesian Economics as the “generalization of the *General Theory*”, and he discussed in detail the Post Keynesian interpretation of Keynes.<sup>132</sup> The following year, at a session in the AEA annual meetings, Kregel was arguing that Cambridge economists had missed Keynes’s major message, “Keynes’ *General Theory* was exclusively concerned with a monetary economy in which changing beliefs about the future influence the quantity of employment. Yet money plays no more than a perfunctory role in the Cambridge theories of growth, capital and distribution developed after Keynes.” According to Kregel it was only with the work of the American Post Keynesians that Keynes’s original message had been recovered.<sup>133</sup> In 1984 in “Reviving Keynes’s revolution,” Davidson interpreted Keynes’ revolutionary message in terms his concept of “non-ergodicity” and concluded: “Post Keynesians are trying to build on the logical foundations of Keynes’s real world analysis to resolve modern day economic problems. They invite all who possess open minds to undertake the further evolution of Keynes’s logical heresy”.<sup>134</sup>

To the above authors Hamouda and Harcourt’s definition was implausible. It sought to unite groups that did not wished to be joined. Still, the “umbrella” definition<sup>135</sup> of Post Keynesian Economics has proven to be a resilient one. It still reigns over many discussions on the history and definition of Post Keynesian

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<sup>131</sup>Harcourt (1985), pp. 130-139.

<sup>132</sup>Kregel (1983d), pp. 34-37.

<sup>133</sup>Kregel (1985), pp. 133, 138.

<sup>134</sup>Davidson (1984), p. 574.

<sup>135</sup>Davidson has called it “large tent”, Davidson (2005).

Economics.<sup>136</sup> It holds an appeal to those that saw an identity with Cambridge and the whole of its legacy, not just Keynes, but also Sraffa, Robinson, and Kaldor. It appealed to those that sought to federate the dissonant voices. This was of course Eichner's emphasis in his efforts to create a "post-Keynesian paradigm." Eichner in 1983 was arguing against those that sought to distinguish Sraffian from Keynesian economics:

While some post-Keynesians may disagree, I would argue that both types of analysis, short-period as well as long-period, are essential and indeed are complementary to one another. This is the basic methodological position underlying the econometric model of the American economy which my colleague, Leonard Forman, and I are constructing in an attempt to demonstrate the empirical validity of post-Keynesian theory.<sup>137</sup>

And Eichner was not alone. Under his sponsorships Alessandro Roncaglia wrote on Sraffa's work as "playing a decisive role in the development of post-Keynesian theory."<sup>138</sup> Peter M. Lichtenstein researched Sraffa's work as the "Post-Keynesian theory of value and price."<sup>139</sup> Eichner died in 1988, the year of publication of the Hamouda and Harcourt article. With Eichner's death the paradigm building project was abandoned but Hamouda and Harcourt's "horses for courses" took its place as the means to unite the feuding groups.

## 8.6 Meaning, institutions and spokespersons

The history of the self-definition of Post Keynesian Economics offers us no simple narrative. Unlike the standard set in the historical scholarship on Post Keynesian Economics I attempt no narrative of progress or of decadence.<sup>140</sup> Instead, I underline discontinuities in the history of the Post Keynesian group and its boundaries. Post Keynesians debating the definition of their approach redrew their social and

<sup>136</sup>See the discussion on the secondary literature in chapter 6.

<sup>137</sup>Eichner (1983), pp. 2-3.

<sup>138</sup>Roncaglia (1979), p. 87.

<sup>139</sup>Lichtenstein (1983). This book was published by M.E. Sharpe owing to Eichner's negotiation of a series of post-Keynesian works.

<sup>140</sup>See Tymoigne and Lee (2004) review of King (2002), discussed in p. 177 of this thesis.



intellectual boundaries with new intellectual and institutional programs. To the end of the period I have examined, the subject of Post Keynesian Economics' definition remained unsettled.

Until 1972 three labels were being used interchangeably with the same content, "neo-Keynesian," "Anglo-Italian theory," and "post-Keynesian." Curiously the label that was soon adopted to denote the Cambridge England side of the Capital Controversies and its American supporters was the one that was already in use by the mainstream of the profession under another meaning, "post-Keynesian." From 1978, while "post-Keynesian" still remained in use, the new label "Post Keynesian" came to dominate. By 1985-88, although no further changes to the label occurred, no meaning for the label had secured hegemonic assent: on the one hand there was the *JPKE* understanding, on the other the "horses for courses" of Hamouda and Harcourt.

The observed discontinuity in the meaning of Post Keynesian Economics arises because each proposed definition was a response by different actors to different contexts. Eichner's "post-Keynesian paradigm" was part of his early 1970s solution to expand the alliance between Cambridge economists and American dissenters. He sought the support of other discontents within the profession, and announced the emerging approach as an alternative to neoclassical economics. In contrast, Weintraub and Davidson's "Post Keynesian Economics" was designed to stabilize the alliance activity of the previous years, suggesting that further attempts to homogenise approaches were unwarranted. In addition it favoured the more conventional audience that Davidson and Weintraub sought to enlist as contributors to the *JPKE*. Finally, the "Post Keynesian" of the early to middle 1980s was an outcome of the Trieste Summer School disputes. It was a reaction to the emergence of the "surplus people" seeking to redefine the content of Post Keynesian Economics.<sup>141</sup> The definitions of Post Keynesian Economics were pragmatic responses to changes in the group's institutional environment. I suggest that these were the expansion and contraction of Post Keynesian boundaries in response to the changed context.

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<sup>141</sup>For the purposes of this thesis, American-centred, the emergence of the "surplus people" is an exogenous event. I have not studied what led to formation of the "surplus approach." There is regrettably no material on this subject though it is one that deserves to be researched.

Each definition of Post Keynesian Economics discussed in this chapter had its distinct origins but one should not fail to see that they were often perceived as alternatives. In fact, changes to the label were markers of difference between competing proposals. Ostensively, the “Post Keynesian” of the *JPKE* had lost the hyphen to indicate distance from Eichner’s earlier “post-keynesian” efforts. It was a battle of spokespersons, of those that took on the role of interpreting the content of the approach, its reasons to be and its future. That some spokespersons were more successful than others can only be attributed to the institutional power they commanded. Eichner spoke for the approach, with the occasional support of Nell and Kregel, for most of the 1970s with no competitors. When Davidson came into the scene and took on role of spokesperson, Eichner was virtually displaced. Davidson had the platform of the *JPKE*, he was a tenured professor at Rutgers University and later even employed Eichner at his department. Although Eichner had organized seminars at the ASSA meetings, and had M.E. Sharpe’s commitment to publish “post-Keynesian” books and articles in the magazine *Challenge*, he held a precarious academic post and could never command the same resources as Davidson.<sup>142</sup> In the battle of spokespersons at Trieste, the result was not so clear. Davidson and Kregel did not have similar success in displacing the challenge of the “surplus people.” The spokespersons at Trieste were equally matched. They argued with similar numbers of graduate students, publishing records and academic credentials.

The history of the Post Keynesian community is one of competing spokespersons, of their personalities and the institutional platforms from which they voiced their vision for Post Keynesian Economics. The conflict in the history of Post Keynesian Economics’ self-definition reflected competing strategies for the boundaries of the group - who were to be its allies, who were to be its enemies. In the concluding chapter of my study of Post Keynesian Economics, I wish to discuss in greater detail how this boundary work, with its making and breaking of alliances, shaped Post Keynesian identity.

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<sup>142</sup>On Eichner’s professional career, see Lee (2000a), p. 32-35.

## Conclusion to Post Keynesian Economics

### 9.1 Cambridge, Keynes, and Post Keynesian Economics

The subject of my research is the emergence of dissent in post World War II economics. I began by arguing that the intellectual participant accounts that dominate the historical scholarship can mislead on the origins of Post Keynesian Economics.<sup>1</sup> I do not dispute that if Post Keynesians reference and seek to develop the work of the Cambridge left Keynesians then some connection exists between the seminal ideas and the later developments. However, this intellectual link is often taken to mean that the historian must search for the source of the initial revolutionary ideas to the detriment of the study of more recent record.

Consequently, the origins of Post Keynesian Economics have typically been located to a distant past, often 1936, the date of J. M. Keynes' *General Theory*. I have shown that the question of the emergence of Post Keynesian Economics is not that of the tentative development of Cambridge economists' revolutionary vision, or that of locating its first and true source. Instead my analysis concerns how the Cambridge authors' work came to be seen as revolutionary.

In the early 1960s, the work of Cambridge economists was not debated as a distinct body of theory. Cambridge economics (then defined as the work of Joan V. Robinson, Piero Sraffa, Nicholas Kaldor, and Luigi Pasinetti on the subjects of growth and distribution) came to be seen as an alternative to neoclassical economics only after the debates on capital theory between Cambridge economists and members of the MIT economics faculty. Crucially, it was Geoffrey C. Harcourt, in an influential

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<sup>1</sup>See chapter 6 of this thesis.

survey article for the *Journal of Economic Literature*, that identified the work of Cambridge economists as an alternative to neoclassical economics. Harcourt's piece tapped into growing discontent in American economics. In the years that followed, American dissenters sought to join their research with that of the Cambridge authors; they corresponded, met in seminars, and visited the University of Cambridge.<sup>2</sup> This was an American passion; Americans were the primary promoters of this challenge to neoclassical theory and to this day dominate the Post Keynesian group in numbers. The community of Post Keynesians was thus formed in the early 1970s on the belief that Cambridge held a foundation for an alternative economic theory. The group originated with the goal of challenging the mainstream.

Intellectual histories of Post Keynesian Economics today crown Keynes as the originator of the Post Keynesian revolutionary message. Interestingly, this was not the early to mid-1970s perception. For Alfred S. Eichner, Cambridge was important because it was residence to Robinson, Sraffa, Kaldor and Pasinetti and not because of Keynes.<sup>3</sup> Keynes takes a prominent role in Post Keynesian Economics only with the creation of the *Journal of Post Keynesian Economics (JPKE)* in 1978. Thus Post Keynesians' appreciation of Keynes as originator of the approach and their devoted discussion of his work is more recent than suggested in the intellectual histories.<sup>4</sup> These participant histories have projected onto the past the cultural norms of the present, notably Keynes as the undisputed founder of the Post Keynesian approach.

It was Paul Davidson's early 1980s redefinition of Post Keynesian Economics that placed the "Keynes's school" as the intellectual core of the group.<sup>5</sup> Prior to this, Keynes had been paired with Michal Kalecki, on the understanding that the former had provided a critique but had fallen short of creating a viable alternative, which

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<sup>2</sup>In chapter 7 I devoted particular attention to the correspondence of Eichner and Davidson with Robinson, and noted Kregel and Davidson's visits to Cambridge.

<sup>3</sup>This was noted in my discussion of Eichner and Kregel (1975), but the observation can be made in all of Eichner's later work in the "post-Keynesian paradigm".

<sup>4</sup>J. E. King does note that Post Keynesians' work on Keynes was concentrated in the 1980s but seems unaware that this represents a discontinuity in Keynes's standing in Post Keynesian identity (King (2002)).

<sup>5</sup>See his table of schools of thought reproduced in p. 236 of this thesis, and my discussion later in this chapter.

Kalecki had more fully developed.<sup>6</sup> Davidson, and possibly Weintraub, disagreed.<sup>7</sup> For them the alternative was to be found in Keynes. But until the Trieste controversies Davidson's statements implied only a change of emphasis. Despite the "Keynes' school" providing its essential character, Post Keynesian Economics was still said to be a collection of theories and authors, encompassing the contributions of many Cambridge economists.

At Trieste, Pierangelo Garegnani critiqued the features that Davidson had inscribed as Keynes's revolutionary insights and sought to appropriate for himself the legacy of Sraffa and of Cambridge.<sup>8</sup> To this, Davidson and Kregel responded by making Keynes the beginning and end of all things Post Keynesian, and in their battle with the "surplus people" other references were dropped as tainting Keynes's true message. Garegnani succeeded in framing the debate as a choice between Keynes's short period approach and Sraffa's long period one. Since Robinson and Pasinetti had argued in favour of a Sraffian conceptualization of the economy,<sup>9</sup> for Davidson and Kregel to include them in their Post Keynesian Economics would appear as an acceptance of Sraffa. That Keynes was being made distinct from other Cambridge authors, became explicit when in 1984 Kregel argued that Cambridge economists had missed Keynes's crucial message.<sup>10</sup>

I have extended the scope of this research well into the 1980s to provide closure to the very significant change that began in 1978-79 with the creation of the *Journal of Post Keynesian Economics* and with Garegnani's contemporaneous attack on Keynes, while laying a claim to Sraffa's legacy. To have finished my narrative in 1980 would have captured this process in its infancy. As I have shown, it developed in 1981-85 at

<sup>6</sup>At Robinson's suggestion in Eichner and Kregel (1975), see p. 229 of this thesis.

<sup>7</sup>As I have noted although Weintraub never openly debated the definition of Post Keynesian Economics, his pronouncements as editor evidence agreement with Davidson's views. In Davidson and Weintraub (1981) he endorsed the distinction between Post-Keynesian Economics and Post Keynesian Economics, taking the latter to mean "real world" economics.

<sup>8</sup>His critique appeared famously in the recently created *Cambridge Journal of Economics*, Garegnani (1979a).

<sup>9</sup>Pasinetti (1981), Robinson's recollections on her 1970s work are illustrative of her desire to further Sraffa's insights Robinson (1980).

<sup>10</sup>Kregel (1985), p. 133. Today, Davidson argues that him and Weintraub are the first Post Keynesians, whereas Robinson and Sraffa are more distant family (fore-fathers) (Davidson (2004), pp. 247, 251).

the Trieste Summer School and when I looked even further ahead to 1987-88 it was to provide an assessment of these developments.<sup>11</sup> Still, as I have noted in my discussion of radical economics, there is no definitive closure to the task of defining and redrawing the boundaries of a dissenting group. In late 1980s and 1990s the Post Keynesian group continued to undergo change. In Britain with Economic and Social Research Council funding, the Post Keynesian Study group was founded by Victoria Chick and Philip Arestis. In the USA, facing faculty opposition at Rutgers University, Davidson moved to the University of Tennessee where he began to organize his own Summer School and Conference.<sup>12</sup> All these developments were source to further changes to the anatomy of power and to the boundaries of Post Keynesian Economics, inevitably, also to the role that the Cambridge legacy was seen to play.

My remarks in this section distinguish my account of Post Keynesian emergence from that offered in participant histories. I make clear that Post Keynesian Economics' relationship with Cambridge was far more contingent than current historical scholarship indicates; in the period I have researched opinion shifted as to the content and role played by the Cambridge legacy. What I have not yet done is to discuss explicitly how these changes that have held such a prominent place in my narrative are related to the concepts of boundary work or to the identity of Post Keynesian Economics.

## 9.2 Boundary work and Post Keynesian identity

This thesis has examined Post Keynesians' pronouncements on the label and the intellectual definition of their community. These were authoritative statements on the identity of the group. Not all Post Keynesians were involved in the process of self-definition. It is significant that those that actively debated the meaning of Post Keynesian Economics were those that set themselves up as promoters of the group, heading its institutional campaign, whom I have called spokespersons. The discursive

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<sup>11</sup>The major assessments were Arena (1987) and Hamouda and Harcourt (1988).

<sup>12</sup>The contents of the conference were zealously published by Edward Elgar (Davidson and Kregel (1989; 1991; 1994; 1999)).

power of defining Post Keynesian Economics was always in line with institutional power. The majority of Post Keynesians seemed to accept the spokespersons mandate.<sup>13</sup>

Recalling my earlier discussion of boundary work, I want to suggest that the debates on the definition of Post Keynesian Economics comprised of cases of boundary work. The aim in defining Post Keynesian Economics was to denounce the mainstream as flawed science or non-science and ultimately take its place.<sup>14</sup> Post Keynesians were mapping the field of theory, stressing as crucial for the future of economic science some elements at the expense of others, and re-fashioning relationships between theories and authors. The label's changing meanings represent a process of inclusion and exclusion, to create allies and enemies.

The Post Keynesian boundary work was of a very different kind from that performed by radicals. Firstly, the mainstream did not react strongly to the Post Keynesian challenge. There were numerous exchanges between the notables of the profession and the Cambridge economists but little engagement between the former and the American dissenters, who constituted the Post Keynesian group proper.<sup>15</sup> Consequently, I will not contrast the Post Keynesian map with the mainstream response (there was none) but contrast instead the different maps proposed by Post Keynesians. Secondly, the referents in Post Keynesian Economics map-making were characterised by an interlocking of economic content and intellectual ancestry. The focus of the radicals had been on the politics of science and science's relationship to society. Post Keynesians' focus was on an assessment of past authors and their contributions, notably the canon of economic literature.

In the late 1960s American dissenters became convinced, notably by Geoffrey C.

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<sup>13</sup>In my interviews with Roy Rotheim and Victoria Chick, they seemed aware of the "Davidson view" and the "Harcourt view" and expressed sympathy for one or the other (interview with Roy Rotheim, 2004; interview with Victoria Chick, 2005).

<sup>14</sup>The more frequent argument was to say neoclassical economics was outdated, a pre-keynesian paradigm. Only Eichner made the claim that neoclassical economics was not scientific. Eichner (1983) argued that "Economics was not a science" because neoclassical economics failed empirical testing, he hoped to show that the "post-keynesian paradigm" would fare much better in that crucial criterion. The design of a "post-Keynesian" macro-econometric model was Eichner's primary focus in the mid-1980s.

<sup>15</sup>For instance see the symposium "Appraising Post Keynesian Economics", at the 1979 Annual Meetings of the AEA, *American Economic Review*, 1980, 70(2), pp. 10-28, organized by the then recently created *Journal of Post Keynesian Economics*.

Harcourt and Joan Robinson's reading of the capital controversies, that from Cambridge, England a new economics could be built. In the early years of the 1970s they linked their research to the work of Robinson, Sraffa, Kaldor and Pasinetti.<sup>16</sup> Through their interest in Cambridge these dissenters joined in meetings to discuss their challenge to the mainstream and the contours of a new economics. Among this early group, Eichner most militantly took on the task of announcing and enunciating the emerging approach. His 1975 article for the *Journal of Economic Literature*, authored with Jan Kregel, followed from Harcourt's earlier discussion of the capital controversies and updated it with additions by American dissenters.<sup>17</sup> More importantly, Eichner presented "post-Keynesian theory" as a paradigm and he listed several criteria along which to distinguish the new approach from that of the mainstream.

From 1972, he became convinced that "post-Keynesian theory" was a new paradigm for economics. In the URPE 1972 meetings at Toronto and through his efforts to create Political Economy Clubs across the USA, he argued for a paradigm inclusive of many brands of dissent (Marxist, Institutional, Keynesian). But a year later, in the aftermath of the failed Political Economy Clubs, the paradigm was reconsidered to become solely Keynesian, combining the insights of Cambridge economists and American Keynesians.

Eichner mapped the general field of economic theory as comprised of *two competing paradigms*.<sup>18</sup> The space of each paradigm was filled by groups of models of similar characteristics, which were described under the headings of: "dynamic properties," explanation of income distribution, information available in the economy, "conditions that must be met before the analysis is considered complete," and microeconomic base. The above characteristics struck the claim that the neoclassical paradigm was not designed to explain the real world. The neoclassical paradigm was said to expound no growth or steady-state growth models of the economy, where

<sup>16</sup>This was the subject of chapter 7.

<sup>17</sup>Eichner and Kregel (1975)

<sup>18</sup>Although Eichner recognised the existence of radical and institutional economics, he never mentioned these as paradigms. It is likely he believed these did not stand as viable alternatives to neoclassical economics to the same extent as "post-Keynesian theory."



there was perfect foresight, perfect markets with no monopolization, and where income distribution was explained restrictively with reference only to the availability and productivity of factor inputs.<sup>19</sup> Neoclassical economics was thus more a theory “to define an optimal decision rule than to explain the world as it is.”<sup>20</sup> The distinction between the *real world* and the idealized *world of the social optimum*, was a crucial one to which all other later instances of boundary work would return.<sup>21</sup> The paradigms occupied these very different (very separate) cultural worlds. The location of the “post-Keynesian paradigm” in the real world made it the strongest candidate to solve the ills of the American economy.

Eichner did not revise his definition of “post-Keynesian theory.” After 1975 he held on to his vision of a new paradigm developing from the insights brought out in the work of Cambridge economists and extended by American authors. He sustained a dichotomy in his map of economics, it was split into a neoclassical orthodoxy and a “post-Keynesian” alternative. In the second instance of Post Keynesian boundary work, undertaken following the creation of the *Journal of Post Keynesian Economics* in 1978, this duality gave way to a plurality of entities occupying economics.

The main spokesperson for the new definition of Post Keynesian Economics was Paul Davidson and he argued, in coherence to what he had expressed in 1972, that the landscape of economics should be mapped by *distinct schools of thought*. Davidson in 1982 listed five schools of thought: Socialist-Radical, Neo-Keynesian, Keynes, Neo-Classical synthesis Keynesian, and Monetarist-Neoclassical. He described them in terms of their views on money, wages and income distribution, capital theory, employment theory and inflation. Davidson listed the schools in order of their political beliefs, from extreme-left to extreme-right. The effect of this ordering was to place at *the centre of economics*, the “Keynes school,” within which

<sup>19</sup>Eichner and Kregel (1975), p. 1309.

<sup>20</sup>Ibid., p. 1310.

<sup>21</sup>This distinction also echoes radicals’ claims concerning the economics orthodoxy’s irrelevance towards the urgent social ills of America. However, radicals interpreted it in relation to the politics of science, for radicals it was a matter of how economics was related to social struggles. For Eichner it was a result of the realism of the models economists chose to employ, the Cambridge models were the most realistic.

Davidson counted himself.<sup>22</sup>

There was no mention of a paradigm, instead Post Keynesian Economics was a reunion of authors and approaches. As I have argued, underlying this definition was the creation of the *JPKE*. To ensure its future the publication needed to appeal to a large audience, which imposing paradigm coherence would have severely restricted. Post Keynesian Economics was presented as eminently non-intrusive. It encompassed the whole of the Neo-Keynesian school (the Cambridge economists, Eichner and other Americans) and the Keynes School (where Davidson placed himself and other American dissenters) and part of the Socialist-Radical and Neo-Classical synthesis schools. This created the impression of Post Keynesian Economics as a broadly based movement, only one out of five schools of thought was completely excluded from it, the Monetarist – Neoclassical school. The politics of Post Keynesian Economics was also broadly defined, from left-wing to right of centre economists, but its core was placed at centre/left of centre politics. Davidson's definition did not seem to strike much of an attack on the mainstream. The neoclassical mainstream was portrayed as a fringe, a minute part of the profession at the "extreme right." In Davidson's map the mainstream was marginal. However, as the Trieste Summer School opened up a debate over the meaning of Keynes' work and the role he might play in an alternative to neoclassical economics, Davidson changed his views on how the boundaries were to be drawn.

Garegnani and the "surplus people" initiated the debate and imposed the duality between *short period* and *long period analysis*. They saw their position in terms of a dichotomy that was said to fully characterise the field of economic theory. Upon this primary distinction others were woven. Davidson and Kregel, spokespersons for Post Keynesian Economics, accepted this boundary. For them on the side of short period analysis was Keynes and the Post Keynesians with their stress on the uncertainty of future economic outcomes and their stress on considering the economy as a

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<sup>22</sup>Davidson (1982), p. 2. Whereas in the radicals' mapping the political element was the primary distinction and from it followed all other distinctions, for Davidson, politics was not primary. In the contrary, economists' politics followed from their views of the economy, from their theory. This did not make him into a "conventional economist" which would argue economic views and politics were orthogonal, Davidson proposed a "third way."

*money-economy*. On the side of the long period analysis were the “surplus people” but also the neoclassical mainstream, all adopting a variation of a general equilibrium model, with no consideration for money, and taking the future to be certain or liable to statistical modelling.<sup>23</sup>

Recalling Davidson’s earlier mapping of economics, as a set of five schools of economic thought, the new understanding of Post Keynesian Economics narrowed it to the “Keynes’s school.” Davidson’s Neo-Keynesian school, so close to the core of Post Keynesianism, was expelled. This was not just a hardening of the boundaries between the schools of thought, it was a transformation of the space. There were again only two entities occupying the economics profession, the Post Keynesian Economics of the short period initiated in Keynes’ revolutionary analysis, and the long period Walrasian analysis of the “surplus people” and the neoclassicals unsuitable to study the real world. Remarkably, this was a return to the distinction rehearsed by Eichner of two worlds: the *real world* and the idealized *world of the social optimum*.

Interpreting these controversies as instances of boundary work highlights Post Keynesians’s enlisting of allies. Paramount among these allies were dead economists – Ricardo, Marx, Marshall, Keynes. To be able to speak for these past giants of the profession was a powerful resource to endow the group with credibility and hence attract a larger following. To understand how this was the case one must consider the role of the canon in economics and how it underscores the identity of the economics profession and the identity of Post Keynesian economics.

In the history and methodology of economics there has recently been a growing appreciation for the existence and role played by the canon in economics.<sup>24</sup> The concept of the canon is borrowed from literary theory where it refers to “the texts that are valued as works of literary merit and in so doing [the canon] helps define the

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<sup>23</sup>For the “surplus people,” the short period analysis was a misreading of Keynes, undertaken by the mainstream and some American dissenters (although Keynes himself had erred by using a Marshallian framework). The only correct reading of Keynes was through Sraffa’s work and that lead to the surplus or long period approach. See my discussion on chapter 8 of the Trieste debate, p. 244.

<sup>24</sup>Interest for the subject has been stimulated by the work of Vivienne Brown on Adam Smith (Brown (1994)) which had as notable follow up a conference in Athens, in 1997, entitled “The canon in the history of Economics” (Psalidopoulos (2000)).

nature and scope of literary practice and literary theory".<sup>25</sup> It is through chronologies of texts and authors past, that some disciplines record their own achievements and advances. "The canon presents a view of the discipline that epitomizes and underscores the discipline's own sense of identity and intellectual tradition."<sup>26</sup>

The concept of canon need not be restricted to literary theory. In sociology for instance, a "classical theory" defined with reference to a set of "founding fathers" namely Karl Marx, Emile Durkheim and Max Weber,<sup>27</sup> embodies that discipline's canon. These authors and their collaborators' works are identified as the most valuable contributions of the past that modern work must reference and develop. The interpretation and reinterpretation of these founding fathers' texts "influence what kind of discussion counts as a sociological theory, what theoretical language sociologists are to speak in, and what problems are most worth speaking about."<sup>28</sup> Although today it is arguable whether the main part of the economics profession recognises an economics canon,<sup>29</sup> it clearly did in the 1960s and 1970s. Some of the major protagonists of the discipline of this period related their work to readings of older greats.<sup>30</sup>

Post Keynesians' boundary work claimed as their ancestry Ricardo, Marx, Keynes, Kalecki, Sraffa, Robinson and Kaldor; in an attempt to constitute themselves as part of a long tradition in economics. This strategy was not new to the history of economics, fifty years earlier this had also been the strategy taken by the institutionalists. They wished to undertake a "reclamation" of the American Economic Association. For this "they were placing themselves in this longer American tradition that stretched back to the 1880s."<sup>31</sup> To effect their 1920s

<sup>25</sup>Brown (1993), p. 65.

<sup>26</sup>Idem.

<sup>27</sup>Connell (1997), pp. 1511-2.

<sup>28</sup>Ibid., p. 1512.

<sup>29</sup>Samuels (2001).

<sup>30</sup>Paul A. Samuelson's *Collected Scientific Papers* have large sections devoted to his history of economics writings in this period (Samuelson (1972), pp. 640-711; Samuelson (1977), pp. 841-946). Another towering member of the profession, Milton Friedman, became involved in the late 1960s in a controversy with Don Patinkin concerning a Chicago economics tradition (see Tavlas (1998)). Mark Blaug's famous textbook on the history of economics focused on the economics canon was also from this period, first edition in 1964, second in 1968 (Blaug (1964)).

<sup>31</sup>Rutherford (2000), p. 303.

challenge to the economics profession, the early institutionalists posed themselves as a long and accredited tradition in economic thought. This legitimizing strategy is also present in Post Keynesian Economics. But unlike the early institutionalists, Post Keynesians further denied the soundness of the mainstream's reading of the canon. They attempted to deligitimize their adversaries. When they argued that the mainstream had misread Keynes and scorned them as "bastards," they were denying the mainstream's claim to Keynes' intellectual lineage. Consequently, for the profession to recover its sense of identity and tradition, to once again be truly Keynesian, it would have to become Post Keynesian.

The profession was not converted to the Post Keynesian reading of the canon. In spite of its ineffectiveness as a weapon to transform economics, the Post Keynesian reading of the canon and the links of ancestry it established played a continuous role in the history of the group. It was with reading and re-reading of these canonical authors and texts that Post Keynesians engaged in conversation, it was through these readings that alliances within the community were made and broken. In this thesis I have shown how the Capital Controversies have been revived by generations of Post Keynesians pouring through its canonical texts and authors.<sup>32</sup> I have made the case that the debates on the reading of such canonical figures as Keynes and Sraffa reshaped Post Keynesian Economics in the 1980s. It is by sharing the parentage of these canonical figures that Post Keynesians come to see themselves as part of a single group.<sup>33</sup> Post Keynesians are not alone in having their adopted intellectual ancestry underscore their sense of identity, for example Austrian economists find their identity in discussions over the work of Menger, Wieser, Mises, and Hayek.<sup>34</sup> And in institutional economics "Veblen's influence, whether direct or indirect, was one of the major elements providing a commonality and a bond between the members of the institutionalist movement."<sup>35</sup>

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<sup>32</sup>See section 7.5 of this thesis.

<sup>33</sup>Hamouda and Harcourt (1988) claim of a shared ancestry is an accurate description of Post Keynesians relationship to their past. It is more doubtful whether this ancestry need imply a "horses for courses" methodology or any direct intellectual content. For a discussion of the Hamouda and Harcourt methodological claims, see Backhouse (1988).

<sup>34</sup>Vaughn (1994), pp. xi-xii.

<sup>35</sup>Rutherford (2000), p. 284.

Post Keynesians defined themselves in opposition to the mainstream, as challengers. They did not redraw the outer boundaries of the profession that define its relationship to the world outside the campuses, as radicals had done. Instead they redrew the inside of the profession into a Post Keynesian camp and a defunct neoclassical one. Post Keynesians sought as their ally the canon of economics. While their boundary work was met by the mainstream's silence, Post Keynesians never abandoned their stance as challengers. The Post Keynesian identity had become fixed by the episodes of boundary work as that of a contender to the mainstream. Furthermore, the canonical figures, once weapons to battle the mainstream, retained their role as mediators of the relationships between the group's members. Being a Post Keynesian was to discuss Cambridge economics.

## Part IV

# CONCLUSION

## Crisis, dissent and settlement

Comparing two cases of dissent in economics

### 10.1 Crisis and revolution in economics

In 1962 Thomas S. Kuhn introduced the notion of scientific revolution to the discourse on science. Kuhn favoured a metaphor between the patterns of political and scientific change. He described scientific change as occurring in a sequence of malfunction – crisis – scientific revolution, “In both political and scientific development the sense of malfunction that can lead to crisis is pre-requisite for revolution.”<sup>1</sup> Kuhn described the period when the role of the paradigm within a discipline is attenuated: “by proliferating versions of the paradigm, crisis loosens the rules of normal puzzle-solving.”<sup>2</sup> As crisis deepens scientists commit themselves to one of the alternative paradigms, and the scientific community becomes divided between competing camps or parties.

To dissenting economists, reading Kuhn’s philosophy of science in the 1970s, it may have seemed prophetic. They had witnessed economics, in the early 1960s self confident and self-congratulating, fall into a state of factious controversy and retreat from public life. If Kuhn was right, following this crisis an epoch making change to economics was in the coming.

Dissenters (particularly the young radicals) had been educated in a period of proud confidence in the profession and its achievements. The main proponents of the new science of growth pronounced “the promise of modern economic policy, managed

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<sup>1</sup>Kuhn (1970[1962]), p. 92.

<sup>2</sup>Ibid., p. 80.



with an eye at maintaining prosperity, subduing inflation, and raising the quality of life".<sup>3</sup> Economists heralded the "obsolescence of the business cycle": "As of this writing [November 1969], the nation is in its one-hundred-and-fifth month of unparalleled, unprecedented, and uninterrupted economic expansion."<sup>4</sup> The "age of the economist" was proclaimed:

Economists have become the high priests of a world of money, wealth, and aspirations for material goods. . . . It would be hard to name another discipline that has exerted as much influence on the modern world. . . . economics is the only social science with a generally accepted body of theory whose validity almost every practitioner would accept.<sup>5</sup>

To anyone who had witnessed the "golden age" of economics, the late 1960s and early 1970s must have seemed a world apart.<sup>6</sup> The American Economic Association (AEA) meetings of this period were assaulted by many brands of dissent. In 1967 the Association was first petitioned on the war in Vietnam, an effort that was deemed in violation of the AEA charter. In 1968, the August "police riot" at the Chicago Democratic Party Convention drove a wedge in the professional ranks. That year, two meetings of economists took place at the same time: the AEA official meeting in Chicago, and a Grey Market Meeting in Philadelphia in boycott of the official meeting. In 1969, as I have reported, scandal broke with the radicals taking over the floor of the business meeting to indict the profession of complicity with the evils of American society.<sup>7</sup> In a more cordial tone, a group of black economists presented a "Statement of Concern of the Caucus of Black Economists to the American Economic Association," demanding greater representation to fight social, racial and professional bias.<sup>8</sup> The radicals were not the only critics attacking the mainstream during this early period. In 1971, Mancur Olson and Christopher Clague paired the radicals with the emerging Chicago-Virginia school (with James Buchanan and Milton Friedman as

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<sup>3</sup>Heller (1966), p. 116.

<sup>4</sup>Okun (1970), p. 31.

<sup>5</sup>Fusfeld (1966).

<sup>6</sup>Coats (1992b) suggests plausibly that the economics profession's over exposure may have created false expectations over its possible achievements and made the public too educated in its limitations. For him it was this public trial that brought about the crisis.

<sup>7</sup>These events have been discussed in chapter 3 of this thesis.

<sup>8</sup>Spratlen (1970).

figure-heads), politically at the extreme right of the profession.<sup>9</sup> The profession was braced in fencing off attacks from both the left and right of the political spectrum, voiced publicly in the December 1970 AEA meetings with disputes between monetarists, Keynesians and radicals.<sup>10</sup> In 1971, the meetings presided by J. K. Galbraith had a substantial radical presence and were host to the first Post Keynesian meeting. At Galbraith's invitation Joan V. Robinson famously attacked the profession. A year later Galbraith's Presidential Address produced a similar critical charge.<sup>11</sup> In the Galbraith meetings, women economists accused the profession of gender bias and founded the Committee on the Status of Women in the Economics Profession.<sup>12</sup> This is a list of deepening crisis.

Unable to quiet the dissenting voices, the economics profession was in a spiral of decreasing credibility. The state of disarray was recognised by "the veritable orgy of public self-flagellation provided by some of the economics profession's leading spokesmen",<sup>13</sup> the Richard T. Ely lectures and the presidential addresses at the AEA meetings became pulpits to self-doubt about the state of economic knowledge.<sup>14</sup> It was not just Galbraith and Robinson that expressed concern over the soundness of economics practice. In 1970 Harry G. Johnson, following statements that year about a monetarist revolution in the making, spoke of the benefits of monetarism to curb the "nonsense" that had accumulated in the Keynesian orthodoxy.<sup>15</sup> Wassily Leontief, President to the AEA in 1970, spoke of how "an uneasy feeling about the present state of our discipline has been growing in some of us who have watched its unprecedented development over the past three decades."<sup>16</sup> Kenneth Arrow's Presidential Address in 1973 was an "expression of discontents and expectations", and he sought an answer to the uncertainties about economics in the economics of

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<sup>9</sup>Olson and Clague (1971).

<sup>10</sup>Bender (1970).

<sup>11</sup>Robinson (1972), Galbraith (1973).

<sup>12</sup>More on the 1971 AEA meetings see pp. 83, 206.

<sup>13</sup>Coats (1992b), p. 462.

<sup>14</sup>A.W. Coats lists another set of complaints pointing to less doctrinal and more technical deficiencies in economics, Coats (1992b), p. 464.

<sup>15</sup>Johnson (1971), p. 13.

<sup>16</sup>Leontief (1971), p. 1.

uncertainty.<sup>17</sup> But probably the most explicit acknowledgement of economics' crisis of credibility came in Walter Heller's 1974 Presidential Address, where he proposed that "going against our current fashion of telling the world what's wrong with economics, I offer a modest contribution to the immodest subject of what's right with economics."<sup>18</sup>

It is difficult to identify in late 1960s economics an "anomaly" as Kuhn defined it: a violation of the "paradigm-induced expectations that govern normal science,"<sup>19</sup> a puzzle of difficult resolution that is only solved by modifications to the paradigm. There was no single unresolved puzzle underlying the crisis in economics, consistent throughout the period. From early on the monetarist critics focused on the orthodoxy's failings in tackling inflation, but the radicals stressed the irrelevance of economics in addressing the problems of poverty, war and imperialism. There was no agreement among dissenters on the nature of the problem with economics. Only later in the crisis of economics did some convergence in concerns begin to emerge. In 1971, contemporary economic policy prescriptions came under wide scrutiny.<sup>20</sup> And even later, circa 1973-4, the attention of the mainstream and of the dissenters turned to the problem of stagflation (inflation accompanied by slow economic growth).<sup>21</sup>

Economics was not alone in its crisis; with it were also the other social sciences. As I have noted in my discussion of the emergence of Radical Political Economics, the troubles in economics were part of the sixties American "civil war."<sup>22</sup> It was social crisis that fed intellectual dissent. More importantly, for the radicals it was the prospect (however unreasonable it may seem to us now) of social revolution that wrecked the consensus in economics. Radicals designed their science for social revolution.<sup>23</sup> The same pattern was manifest in radical history. In a study of the historical profession, Peter Novick has noted that Staughton Lynd the spokesperson

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<sup>17</sup>Arrow (1974), p. 1.

<sup>18</sup>Heller (1975), p. 1.

<sup>19</sup>Kuhn (1970 [1962]), pp. 52-53.

<sup>20</sup>See the AEA symposium "Have Fiscal and/or Monetary Policies Failed?" in *American Economic Review*, 1972, 62 (1/2), pp. 11-30.

<sup>21</sup>Coats (1992b), p. 462.

<sup>22</sup>Isserman and Kazin (2004).

<sup>23</sup>Particularly in Zweig (1971a) and in URPE's pronouncements in the early 1970s, before and during its outreach efforts.

for the radical historians, abandoned concerns for scholarship to adopt what he termed “guerrilla history.” Many radicals believed they were on the eve of an “American October” revolution.<sup>24</sup>

The expectation of social revolution has long been noted to favour the emergence of dissent. This point has been made in the literature on religious radical ideas.<sup>25</sup> In seventeenth century England the widespread belief in the coming of a millennium, i.e. in a future thousand-year golden age of peace, and prosperity, typically initiated by the Second Coming of Christ, promoted radical action and ideas among the people.<sup>26</sup> The expectation of the millennium, with its utopian image of social transformation, further weakened the bounds of authority to benefit the challengers of the social and religious order. In 1960s and 1970s America, the enthusiasm for a profound transformation of society was mirrored by equally profound intellectual critique.<sup>27</sup>

As a description of the crisis of economics, Kuhn’s structure of scientific revolutions is not very helpful. It was not a Kuhnian-type anomaly that led to the feeling of malfunction and fed the crisis. Instead, what triggered the spiral of deepening crisis seems to have been the “millenarianism” or “revolutionarism” of 1960s America. Kuhn is relevant to this history for other reasons. He clearly informed many of the dissenters’ views about scientific change, as members of the radical, Post Keynesian, and Austrian economics communities engaged in the design of new paradigms for economics.<sup>28</sup> Where Kuhn wrote: “when paradigms change, the world itself changes with them”, radicals read that they could assist the world to change by inventing a new paradigm.<sup>29</sup> But more importantly, as both radicals and Post Keynesians witnessed the crisis in which the profession was immersed, Kuhn’s philosophy seemed to assure the imminence of change to the dissenters’ advantage, and further pressed them to raise the temper of their critique. An intellectual and

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<sup>24</sup>Novick (1988), p. 432.

<sup>25</sup>A literature I discussed in the introduction to this thesis, see section 1.3.

<sup>26</sup>Hill (1991), p. 33-34.

<sup>27</sup>This argument is clearly applicable to the radicals which were among the first in the wave of late sixties, seventies dissenters. It is not reasonable when considering the Post Keynesians, although some among its ranks shared in the revolutionary enthusiasm of the radicals.

<sup>28</sup>As I have discussed in chapters 5 and 9, on the Austrians see Vaughn (1994), p. 112.

<sup>29</sup>Kuhn (1970[1962]), p. 111. See Peabody (1971).

institutional remaking of economics was on the horizon.

## 10.2 Settling dissent

I have noted how at the end of my period of study, dissenters had failed to effect a major change to the profession. In the 1980s both dissenting communities were increasingly isolated from the rest of the profession and fragmented by internal controversy. Radicals, following URPE's involvement with outreach work, abandoned efforts of defining the "radical economist" and splintered into independent groups. Radicals were unable to manage their divisive political commitments into a single project for economic science. They were also unwilling to undertake another challenge to the profession as they had done in 1969-71. Through the outreach activities many had found ways of connecting their involvement in politics with their research in economics without the mediation of URPE.<sup>30</sup> Others found a community of radicals in other social sciences with whom to research and debate.<sup>31</sup> Post Keynesians after the Trieste Summer School also became increasingly differentiated and groups of authors developed their research in relative independence. Post Keynesians, unlike the radicals, never abandoned their critique of the mainstream of economics. But they too were becoming in the mid-1980s increasingly centred on their community, partly due to an affluence in conferences, summer schools and graduate research on Post Keynesian Economics. In the 1980s a string of new journals appeared. Adding to the *Journal of Post Keynesian Economics* and the *Cambridge Journal of Economics* both created in 1978, were the annual *Contributions to Political Economy* beginning in 1983; the bi-annual *Political Economy – Studies in the Surplus Approach* began in 1985, the quarterly *Review of Political Economy* was created in 1989, and the following year the quarterly *Journal of Evolutionary Economics*.<sup>32</sup> The new

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<sup>30</sup>Some turned to party political activity, others to movement groups such as feminist collectives, other still continued their involvement in *Dollars & Sense* or in U.Mass – Amherst Centre for Popular Economics (interview with Arthur MacEwan, 2003).

<sup>31</sup>For instance, the case of Marxism Feminism groups, see p. 134 of this thesis.

<sup>32</sup>Also early in this period several journals became amenable to the Sraffian and Post Keynesian ideas *Australian Economic Papers*, *Cahiers de Economie Politique*, *Economia Politica*, *Economie Politique*, *Kyklos*, *Metroeconomica*, and *Manchester School*.

outlets provided greater visibility to the proposals of the dissenters and created a self-sufficient environment of intellectual exchange.<sup>33</sup>

One can therefore find reasons internal to the dissenting groups for their increasing isolation from the main body of the profession. However, to explain the isolation of the dissenters one also needs to consider the role played by the economics profession. The mainstream's response to dissent was not a major element to my narrative. I have looked primarily to how dissenters constituted themselves into communities and how they designed their challenge to the profession. But I have noted in passing some of the immediate responses of the mainstream to the dissenters. Examining this record further highlights how this is a political history.

Of the two groups that I have studied, the radicals were the best placed to make themselves heard. Some amongst their ranks had family connections to the political and professional establishment and they were educated and hired to some of the elite Universities of the American East, namely MIT, Harvard and Yale Universities. Radicals also benefited from powerful allies among senior members of the profession, of which Leontief, Galbraith and John G. Gurley stand out.<sup>34</sup>

Radicals had in the early 1970s, a disruptive participation in the AEA annual meetings in full gaze of the national media. In 1969 they stormed the business meeting to read a statement of critique of the profession.<sup>35</sup> In 1972 they passed a (diluted) resolution condemning political discrimination in hiring practices. In 1973 they nearly succeeded in obtaining the AEA's opposition to the recent military coup in Chile.<sup>36</sup> The radical proposals were either ruled out of order, with resort to the Charter of the Association, "The Association as such will take no partisan attitude, nor will it commit its members to any position on practical economic questions", or were discussed and then defeated by the business meeting. But in 1974 with Gurley as Vice-President and Paul M. Sweezy as a member of the executive committee,

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<sup>33</sup> At times it also promoted differentiation between groups. Some journals were often narrow in their orientation, for instance *Political Economy- studies in the surplus approach* published only 6 volumes and ended in 1990.

<sup>34</sup> These are subjects I have discussed in chapter 3.

<sup>35</sup> See p. 81 of this thesis.

<sup>36</sup> *American Economic Review* (1973a; 1974).

radicals obtained a commitment by the *American Economic Review* to take into its editorial board one radical, Stephen Resnick. In the business meetings that year, radicals assisted in the creation of the Committee on Political Discrimination. After these apparent victories by the radicals, a change to the bylaws of the Association was introduced:

A resolution adopted at the annual meeting in which less than five percent of the membership of the Association has voted thereon shall be submitted within ninety days to a vote by mail ballot if a majority of the Executive Committee determines that, because of the nature or consequences of the resolution, all members should have the opportunity to participate in the final decision.<sup>37</sup>

Effectively this move transferred the power from the business meeting to the executive committee, from an organ which was liable to be taken over by dissenters to one of more conventional preferences.

The strategy of the mainstream was one of containment. Many prominent members of the AEA sought to diffuse attempts by the radicals to politicize the Association, but were reluctant to take any further step that might jeopardise the profession's stake to political neutrality. The radicals' political passions were not a legitimate reason to expel them from the AEA, that could be construed as a violation of the Association Charter's dogma of non-partisanship. As I have argued, the mainstream (notably Robert Solow) did not seek to brand radical economics as non-science.<sup>38</sup> Their aim was to protect the credentials of the economics profession as objective knowledge. Those that spoke for the mainstream repeated to the radicals that the standards for debating and resolving intellectual disputes were carefully drawn research questions and technical rigor, and not a language that mixed analysis with advocacy. These were rules of conduct radicals explicitly rejected.

What was most dangerous and in need of curbing was the spread of the idea that economists could not be trusted to provide politically neutral insight. In this respect the Post Keynesians were not as dangerous an adversary as the radicals. The former

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<sup>37</sup> *American Economic Review* (1976), p. 466.

<sup>38</sup> See section 5.2 of this thesis for a detailed rendition of this argument.

accepted the rules of scholarly exchange, the limits of how and of what the scholar could speak.<sup>39</sup> It was only in protesting the alleged bias against unorthodox views by the *American Economic Review* (*AER*) that Post Keynesians prompted a response by the profession's Association. In their protest they were placing the profession's standards of objectivity in check, they argued that editors and referees were acting as advocates in defence of a doctrine. From January 1974 the *AER* began to submit articles anonymously for refereeing, which was enough to diffuse charges of bias. The new policy however was not enough to induce an opening up of the *AER* to dissenting perspectives as Post Keynesians had envisaged. The editorial line was not changed to give greater representation to non-neoclassical views, and the editor George Borts remained in his post despite criticisms levied against him.

The investigations of academic freedom cases by the Committee on Political Discrimination invite a comparison with Mary O. Furner's study of academic freedom cases at the time of the professionalisation of American social science, in the late nineteenth century. This comparison reveals that the mainstream's behaviour in the 1970s had a very well established precedent. Furner shows that the professionalization of social science, and notably of economics, brought the containment of advocacy to the confines of professional expertise. The first concern of the professional economists was to secure their claim to objective knowledge, "The value of objectivity was emphasized constantly in both training and professional practice, until it occupied a very special place in the professional ethos."<sup>40</sup> But professionals did not abdicate participation in social reform, in fact they "wrapped their reform intentions in a mantle of professional prerogative that shielded them from consequences of advocacy which would otherwise have been too severe to risk."<sup>41</sup> In addition, professional economists were able to secure a substantial degree of social autonomy under the claim "that specialists in a discipline were the only ones qualified to judge each

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<sup>39</sup>In the early days of the Post Keynesian community they followed the radicals in accusing the profession of ideological bias; this was a crucial element of Harcourt's narrative of emergence (1969; 1972). The American Post Keynesians were however more reluctant to take this left-wing route and in their work such claims were abandoned.

<sup>40</sup>Furner (1975), p. 323.

<sup>41</sup>Ibid., p. 322.



others' work and ability."<sup>42</sup> The emerging ethics of professional conduct explain how some nineteenth century violations of academic freedom succeeded in invoking the outrage of the AEA and fellow economists (such as in the case of Edward Ross at Stanford University) while others failed (such as in the case of Edward Bemis at the University of Chicago).<sup>43</sup> What distinguished these cases was that the former respected the limits of professional expertise, the latter did not.<sup>44</sup>

The profession in the nineteenth century would not rally in support of a radical being fired for his political views when those fell outside the limited range of his expertise. The profession in the 1970s behaved in the same manner. The Committee on Political Discrimination when faced with the University administrations' charge of incompetence levied against the radicals, could have challenged such claims and accused the administrations of violating academic freedom, this was a possibility both in the 1890s as in the 1970s.<sup>45</sup> But such course of action would be seen as legitimizing the radicals' approach to advocacy, which did not respect the limits of professional expertise. To support the radicals would undermine the professions' claim to be an "objective advocate." The 1970s Committee's work was in the end inconclusive, both because it was difficult to judge on the conflicting claims of administration and radicals, but also because it worked to avoid further scandal. The committee did not anger the administrations by judging in favour of the radicals' claims of political discrimination, but neither did it anger the radicals already too disillusioned by the profession to expect much from it.<sup>46</sup>

The 1970s dissenters remained in the economics profession not because their work was object of widespread interest, but because the cost of rejecting them was to cast further doubt in the public over the political passions of the experts. Instead dissenting work was merely branded a worse sort of economic science, in Solow's words "we neglect radical economics because it is negligible."<sup>47</sup> Ultimately, the

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<sup>42</sup>Ibid., p. 244.

<sup>43</sup>Ibid., chapters 8 and 9.

<sup>44</sup>This is a simplification of Furner's narrative. She also considers the reputation of the victim, the political context of the case and even geographical differences in the standing of universities.

<sup>45</sup>Ibid., p. 182.

<sup>46</sup>On the issue of the Committee on Political Discrimination, see p. 96 of this thesis.

<sup>47</sup>Solow (1971), p. 63.

mainstream facilitated the isolation of the dissenting communities because it treated their work with indifference.

The mainstream of the profession as can be gauged by the behaviour of the AEA, was carefully defensive and never overtly repressive towards dissent. It limited its response to attacks on the credentials of objectivity and neutrality so essential to the profession's standing in American society. As the comparison with the nineteenth century illustrates, this was an old game being played under old rules. The ultimate prize that the mainstream was seeking throughout this crisis was to restore the credibility of academic economics.

### 10.3 Comparing identities

This thesis is about the emergence of dissent in American economics in the 1960s and 1970s. Part of my focus has been on the creation of dissenting identities. When I discussed the "identity of radical economics" and the "identity of Post Keynesian Economics," the concept denoted the communities' sense of belonging to a distinct entity within economics. The dissenting identities provided members of the group, the communities, with a sense of shared interest and purpose, which distinguished them from those external to it.

Frederic S. Lee has argued that the "heterodox economist" is without identity, because he is missing a sense of his past, denied by the hegemony of mainstream over the history of the profession.<sup>48</sup> For Lee, the role of the historian is to offer the missing identity. For instance, his history of radical economics

takes the reader on a transformational journey, via a narrative, where, in spite of repression, Marxian and radical economics survive and grow. This brighter, more hopeful journey culminates in the formation of URPE. By the end of the article, the reader will have experienced what it concretely meant to be a radical economist, and this is in part what constitutes the historical identity of a radical economist.<sup>49</sup>

Lee argues that all dissenting groups share a single past of repression, and a single

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<sup>48</sup>Lee (2004a), p. 178.

<sup>49</sup>Ibid., p. 179.

history justifies a reunion of the independent groups under a single identity. For the author, in place of “radicals,” “institutionalists,” or “Post Keynesians” one ought to see only “heterodox economists.”<sup>50</sup> This view seems to imply that existing identities are false ones, predicated in a flawed knowledge of the past. Lee with the history of heterodox economics wishes to participate in reshaping the communities which he studies. My approach is very different, I researched identity as it was perceived by the members of the dissenting groups and whilst Lee clearly takes the side of the dissenting groups, I withhold allegiance.

I have used the notion of identity to register the emergence of dissenting communities. There was no radical economics until the creation of the Union for Radical Political Economics, and the subsidiary belief of a political divide distinguishing the young radicals from their established elders.<sup>51</sup> Similarly, there was no Post Keynesian Economics prior to a belief in Cambridge’s revolutionary new theory, which brought dissenters into cooperation to change American economics.<sup>52</sup> We can only speak of dissent once a dissenting identity emerges.

In spite of being both dissent, the identities of the two communities were clearly distinct. The focus of radicals’ identity rested in politics and the social turmoil occurring outside the walls of academia. Their purpose was to bring the profession into the ferment of American society, to make knowledge effective for social change. Radicals saw themselves as the voice of future social revolution. For Post Keynesians the focus of their identity was the Cambridge past. They saw themselves as responding to a profession that had dismissed the revolutionary message of Cambridge, Keynes’s groundbreaking critique and the alternative economics developed by his former students. Post Keynesians saw themselves as the voice of the Cambridge tradition.

Given the distinct identities it is not surprising to find these were fairly independent communities. That does not mean that no attempt was made to join the two. As I have shown Eichner early on attempted a dialogue with the radicals,

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<sup>50</sup>Lee (2002a; 2002b).

<sup>51</sup>The subject of chapter 3.

<sup>52</sup>The subject of chapter 7.

suggesting that radical analysis become a sub-class of the “post-Keynesian paradigm.” However, he very soon abandoned these efforts when radicals’ responded with hostility to the proposal.<sup>53</sup> On the side of the radicals, Howard Sherman and his students at the University of California – Riverside have argued that Post Keynesian Economics is a variety of Radical Political Economics.<sup>54</sup> But the most stable bridge between the two groups has arisen following radical interest in the subjects of money and finance, where they have turned to the work of Hyman Minsky on the financial instability of capitalism. Today some radical authors see the two communities and their approaches as compatible,<sup>55</sup> but this was not the case in the 1970s and 1980s. The radical James R. Crotty’s appraisal of Post Keynesian Economics in 1979 concluded with the telling criticism of Post Keynesians’ unrealistic political vision: “Like Keynes’ own theory, it fails to appreciate the immense power that the corporations derive of their exclusive control of capital investment and thus of jobs and income.”<sup>56</sup> A further critique of Post Keynesian Economics that deserves mention was put forward by Frank Roosevelt, in his doctoral research at the New School for Social Research, titled: “Towards a Marxist Critique of the Cambridge School.” Roosevelt was responding to Harcourt’s claim that Cambridge economics was a development in the tradition of Marx. Focusing on the work of Robinson and Sraffa, he pointed out “how the Cantabrigians diverge from Marx in their method of differentiating societies” and argued “that they mystify the defining characteristics of capitalism and fail to grasp what the struggle for socialism is all about.”<sup>57</sup> As Kregel wrote during this period, any similarities between Post Keynesian Economics and radical economics were “only superficial.”<sup>58</sup>

Using the concept of boundary work I have drawn attention to the dissenters’

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<sup>53</sup>See p. 225 of this thesis.

<sup>54</sup>Sherman (1987), p. 4.

<sup>55</sup>Interviews with Jim Crotty and Robert Pollin, 2003. I should note that Hyman Minsky’s good standing in Post Keynesian Economics during the 1980s was unchallenged. But in the 1990s this may have changed. Davidson has recently expressed misgivings on taking Minsky as a Post Keynesian (Davidson (2004), p. 252).

<sup>56</sup>Crotty (1980), p. 25.

<sup>57</sup>Roosevelt (1980), p. 277. The article was originally published in 1975 in the *Review of Radical Political Economics*.

<sup>58</sup>Kregel (1980), p. 267.

design of a challenge to the mainstream, with its choice of friends and foes. I have spoken of maps of culture.<sup>59</sup> They allowed me to interpret the dissenters' intellectual challenge in relation to their institutional efforts and to the context of 1960s and 1970s American society. For instance, radicals' map of the "politics of paradigms" allowed me to clarify how radicals' challenge was fused to the growing revolutionary temper of American society.<sup>60</sup> And Paul Davidson's map of the "many schools of economics" was evidence of how the editors of the *Journal of Post Keynesian Economics* hoped that this institutional platform might serve to expand the Post Keynesian community.<sup>61</sup> I have followed Thomas Gieryn's work on the topographic metaphor in studying scientific controversies. This thesis does not seek to add to Gieryn's methodology. Instead, my contribution is to show that this metaphor is effective in following a history of controversies, of recurring clashes between dissenters and mainstream, translated into a succession of maps. My study of the history of dissenting communities saw many maps in the making, asking: how and why this or that ally was included?; Why this enemy was inscribed in this particular way? The contrast between maps is telling to understand the history of dissent and its relationship with the *status quo*. For instance, radicals' abandonment of a map of the "politics of economists" for one of the "politics of paradigms" strikingly reveals their new ambition of undermining the economics profession as source of neutral and objective expertise.

In line with current science studies literature, I take an outsider's account of the history of dissent, one that stresses how contingent the meaning of "radical" or "Post Keynesian" economics was. As they were made, identities and communities were also remade. In particular, I have noted how their design of a challenge to the profession reshaped the dissenting communities – the interplay between identity and boundary work. Radicals' early identity was the source of resources for their boundary work, namely by supplying the emphasis on the politics of science. But the episode of boundary work ultimately forced a restructuring of the radical community, under a

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<sup>59</sup>In sections 5.2 and 9.2 of the thesis

<sup>60</sup>See p. 161 of this thesis.

<sup>61</sup>See p. 263 of this thesis.

new understanding of what were radicals' shared interests and programme for action. In 1971 the identity became grounded on the promise of a science for social revolution, their "paradigm of conflict." The new radical identity was the ideal of the "scientist-revolutionary." It was this new identity, coupled with the belief on imminent social revolution that motivated in 1972-73 the URPE emphasis on outreach work.<sup>62</sup>

Post Keynesian identity was also subject to change. Although Cambridge as ancestry was never displaced as the core of the Post Keynesian identity there was discontinuity over which of the Cambridge economists to include as a parent. In 1975 Alfred S. Eichner's "post-Keynesian paradigm," the first Post Keynesian challenge to the profession, established a Cambridge ancestry composed of the protagonists of the Capital Controversies – Joan Robinson, Piero Sraffa, Nicholas Kaldor and Luigi Pasinetti. In the late 1970s Paul Davidson initiated another instance of boundary work and the Post Keynesian identity was further redefined. Although the other Cambridge authors were kept as ancestors, Keynes became the principal figure of the Post Keynesian tradition. This was seen as facilitating a broadening of the group's membership. But when in 1981-85 at the Trieste Summer School, Davidson's interpretation of Keynes came under dispute; a new episode of boundary work sought to define a more restricted ancestry. In the late 1970s Keynes had been used to broaden the group, in the mid-1980s Keynes was used to evict the "surplus people." Davidson and Jan Kregel's new reading of Keynes justified a rejection of all other Cambridge economists as Post Keynesian ancestors. These episodes of boundary work, either primarily driven as challenges to the profession or as disputes internal to the community, resulted in changes to the identity of Post Keynesian Economics.<sup>63</sup>

Dissenting economics offers the historian a wealth of controversy. These were communities in a permanent flow of change, that seemed almost to refuse definition. They teach us a great deal about how controversies develop in modern social science, and show the multiple resources that can be marshalled to do so – from the canon to

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<sup>62</sup>These were the subjects of my discussion in chapter 5.2.

<sup>63</sup>These were the subjects of my discussion in chapter 9.2.

the student body. Dissenters' distinct identities and interests teach us that there were many avenues leading to dissent in 1960s and 1970s American Economics. But foremost, this history is evidence of how in the "harder of the soft sciences," economists have fought over the boundaries of their discipline.

# A

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## Interviews and oral history

I began this research with the benefit of prior knowledge in Post Keynesian Economics. I had studied Post Keynesian Economics during my undergraduate training in economics. While undertaking postgraduate work at the University of Cambridge, I had heard Geoffrey Harcourt lecturing on the history of Post Keynesianism and attended Tony Lawson's critical realist workshop. These gave me a measure of insight into how to approach my reading of the literature and into the oral history of the group. For Radical Political Economics I had no similar benefit. I had to rely solely on the secondary literature, which is remarkably thin, at least when compared to the Post Keynesian one. My research strategy had to address the insufficiencies of my prior knowledge of the groups. Since for Post Keynesian Economics I had a good grasp of current controversies and the tributary secondary/historical literature, I set my focus on a study of archival materials. I was increasingly convinced that this had been an adequate choice as the archives revealed a different past from that offered in participant accounts or in the oral history. For Radical Political Economics, given the absence of archival sources and the fact that many of the protagonists are still active academics, I interviewed those that I identified from the secondary literature as the main protagonists in the group's history. The interviews would serve as a guide for my reading of the radical literature and would allow for invaluable access to personal files.

There is a vast literature on interviewing and its uses in sociology,<sup>1</sup> ethnography,<sup>2</sup> and history.<sup>3</sup> The approaches are distinguishable in their goals and anxieties. While

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<sup>1</sup>Bauer and Gaskell (2000).

<sup>2</sup>Hammersley and Atkinson (1996).

<sup>3</sup>Thompson (2000).



ethnographers' interests lie in recovering meaning and in reconstituting culture, they are concerned with the obstacles to access and understanding of distant languages and practices.<sup>4</sup> Sociologists on the other hand, typically aim at data collection and are led to discuss the design of the interview/survey in yielding reliable factual content.<sup>5</sup> Oral historians zoom in on the reliability of memory, such as in the much noted "telescoping" of events, and on the making of narratives.<sup>6</sup> Oral historians overwhelmingly adopt the method of life history interviewing. The advantages lie in eliciting history in the informant's own terms, not attempting to divert the subject's experience of the past. My interest in interviews as road maps to the history of each group (notably of the radical one), set the life history approach as the most adequate.<sup>7</sup>

In preparing for the interviews, I examined similar uses of interviews in the history of economics. There are several collections of "conversations" worth exploring as a sampler of what can be done with interview materials. The standard of this work I found to be rather varied. In David Colander and Harry Landreth's *The Coming of Keynesianism to America* for instance, the interviewer prompted the subjects to be discussed, effectively listing events and themes to the interviewee for commentary.<sup>8</sup> This interviewing strategy was successful at keeping a clear narrative for the reader, but only because of the interviewer's dominance and at the cost of trampling the voice of the informant. John E. King's *Conversations with Post Keynesians* is yet another example of an approach I found unsuited for my goals, for in these conversations controversy overwhelms history. The material pertains to running polemics between Post Keynesians and it primarily assisted in clarifying current debates (Kaleckian vs Neo Ricardians vs American Post Keynesians), but undermining any attempt at glimpsing their past.<sup>9</sup> Arjo Klamer's *Conversations with Economists* was to my reading the most significant achievement and the closest in design to what I intended to undertake. Klamer's questions were open ended and

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<sup>4</sup>Fontana and Fey (2000), pp. 654-655.

<sup>5</sup>Gaskell (2000).

<sup>6</sup>Portelli (1991).

<sup>7</sup>Morrissey (1998), Anderson and Jack (1998).

<sup>8</sup>Colander and Landreth (1996).

<sup>9</sup>King (1995a).

emerged from the interviewee's own narrative;<sup>10</sup> this brought out the informant's sense of history as a crucial element for the historian to consider.

Because of limited financial resources I had to couple my interview work with archive research at the office files of the Union for Radical Political Economics and at the 'The Economists' Papers Project at the Rare Book, Manuscript, and Special Collections Library of Duke University.<sup>11</sup> After two years of secondary and primary research I had produced a list of radical economists (and of a few prominent Post Keynesians) with information on their current occupation, email addresses, and brief notes on their role in the history of Radical Political Economics. The majority I found were located on the U.S.A.'s East Coast, in New England and New York. I therefore planned a set of interviews constrained by location and cross-sectioning of my listed pool of authors. I tried to balance the number of radicals from what I perceived to be the major groups, those educated at Harvard (Herbert Gintis, Arthur MacEwan, Stephen Marglin, Patricia Quick), Michigan (Barry Bluestone, Michael Zweig) and Yale (Stephen Resnick and Richard Wolff). While these represented the "founding" generation of radicals, I also chose to interview a later generation with more diverse, less well documented backgrounds (Jim Crotty, Robert Pollin, Anwar Shaikh).

I contacted my potential interviewees by email two months ahead of my departure to the US. In the email I briefly outlined the topic of my doctoral research, as an exploration in the history of dissent in post-World War II economics, with my request to conduct an interview on the themes of the respondent's life work and history. I also provided in the email means for the respondent to verify my credentials, namely by contacting my Ph.D. supervisor. No one refused to be interviewed. However, there were several non-respondents that despite an additional email still did not reply (Samuel Bowles, Edward Nell, Dimitri B. Papadimitrou). There were also instances when following an early positive reply, respondents later expressed unavailability for a meeting at the dates suggested (Phillip Arestis and

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<sup>10</sup>Klamer (1984).

<sup>11</sup>I was awarded by the Central London Research Fund a two-month travel and subsistence grant to finance the interviews. I also received a Economists' Papers Project grant from Duke University to financially support my research at their archives.

William Millberg). In the course of my research-trip a few more names were added to my list of interviewees, although I was able to contact and interview only a few of them (Radhika Balakrishnan, Laurie Nisonoff).

At the start of each interview I set out my commitment to notify the informant prior to any use of quotations and to only do so upon their expressed consent. I also requested to record the conversation, in five cases the interview was not taped (Balakrishnan, Crotty, Quick, Resnick, Wolff) either at the interviewee's request or because the meeting was conducted in a public place unsuited for the recording. All the recording was done in mini-disc digital format and the recordings were not deposited. The majority of interviews were conducted either at the informant's house or at their office, four of the non-taped interviews were conducted at public places. Interviews lasted from forty minutes to a maximum of two hours, although in two cases the conversation continued outside the interview setting for more than three hours (Nisonoff and Wolff).

Ideally, life history interviewing requires several sessions, of approximately two hours each, but from the outset it was clear that it would be harder to negotiate access on that basis, as well as creating further difficulties for the scheduling of the research trip. Instead, I aimed for single two-hour interviews designed as life history but with a few semi-structured questions on topics of paramount interest.<sup>12</sup> For each interview I outlined a schedule of topics and questions, partly general, dealing with life history, partly specific, departing from my prior knowledge on the informant's historical role as evidenced in the written record. All questions were open-ended and I tried to approach my specific topic in connection with the unfolding of the life history, addressing subjects when possible in their chronological order. Inevitably, I revised my question and topic schedule informed by the archive work that I was performing alongside the interviews and by the information and experience I was gaining from the interviews.

Given the difficulty in undertaking an additional interview collecting trip to the United States, I later conducted a few interviews by telephone. As it has been noted

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<sup>12</sup>Gaskell (2000).

unlike the face-to-face interview, it is harder in this context to make more probing questions and impossible to take on evidence from the body language of the interviewee.<sup>13</sup> I consequently adjusted my question and topic schedule. I made fewer and more direct questions, usually in order to address a limited subject (for instance with Roy Rotheim, I wanted to discuss his time as a student at Rutgers University).

I did a summary transcript of the interviews, avoiding a cumbersome verbatim transcription, I summarized every thirty minutes into about 500 words. I transcribed very few quotes from each interview. I was concerned primarily in accessing the meanings given to events and documents, and extracting the respondent's narratives of the group's history, elements that were revealed only by repeated listening to the recordings. Because of my "narrative use" of the interviews, their imprint is not always apparent in the thesis text.<sup>14</sup> When I use the interviews as evidence I duly reference the source in footnotes. But my debt to the interview evidence lies also beyond these references, as it underpins my understanding of texts and events, and my choices in exploring some events over others.

Finally, below I provide a list of the people I interviewed and location and date of interview.

Radhika Balakrishnan, in New York City, USA, on the 16<sup>th</sup> of June 2003.

Barry Bluestone, in Boston, USA, on the 11<sup>th</sup> of June 2003.

Victoria Chick, in London, UK, on the 16<sup>th</sup> of June 2005.

Jim Crotty, in Amherst, USA, on the 9<sup>th</sup> June 2003.

Herbert Gintis, in Northampton, USA, on the 6<sup>th</sup> of June 2003.

Jan Kregel, in Cambridge, UK, on the 18<sup>th</sup> of September of 2003.

Arthur MacEwan, in Cambridge, USA, on the 10<sup>th</sup> of June 2003.

Stephen Marglin, in Cambridge, USA, on the 28<sup>th</sup> of May 2003.

Laurie Nisonoff, in Amherst, USA, on the 6<sup>th</sup> of June 2003.

Robert Pollin, in Amherst, USA, on the 9<sup>th</sup> June 2003.

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<sup>13</sup>Seldon and Pappworth (1983), p. 14.

<sup>14</sup>Jovchelovitch and Bauer (2000).

Patricia Quick, in New York City, USA, on the 16<sup>th</sup> of June 2003.

Stephen Resnick, Boston, USA, on the 11<sup>th</sup> of June 2003.

Roy Rotheim, telephone interview, on the 14<sup>th</sup> of September 2004.

Anwar Shaikh, in New York City, USA, on the 17<sup>th</sup> of June 2003

Richard Wolff, New Haven, USA, on the 2<sup>nd</sup> of June 2003.

Michael Zweig, in New York City, USA, on the 18<sup>th</sup> of June 2003.

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