



TRADITION & DISCOVERY

The Polanyi Society Journal

Volume XLVII

Number 3

October 2021

Tradition & Discovery

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Submission Guidelines

Submissions: All manuscripts should be submitted as a Microsoft Word file attached to an email message. Articles should be no more than 6000 words in length (inclusive of keywords, abstract, notes, and references) and sent to Paul Lewis at lewis_pa@mercer.edu. All submissions will be sent out for blind peer review. Book reviews should be no more than 1000 words in length and sent to Jean Bocharova at jbocharova@msjc.edu.

Spelling: We recognize that the journal serves English-speaking writers around the world and so do not require anyone's "standard" English spelling. We do, however, require all writers to be consistent in whatever convention they follow.

Citations:

- Our preference is for Chicago's parenthetical/reference style in which citations are given in the text as (last name of author year, page number), combined with full bibliographical information at the end of the article. One exception is that Polanyi's major works may be cited parenthetically using the following abbreviations (with abbreviations italicized):

CF *Contempt of Freedom*

KB *Knowing and Being*

LL *Logic of Liberty*

M *Meaning*

PK *Personal Knowledge*

SEP *Society, Economics, and Philosophy*

SFS *Science, Faith, and Society*

SM *Study of Man*

STSR *Scientific Thought and Social Reality*

TD *Tacit Dimension*

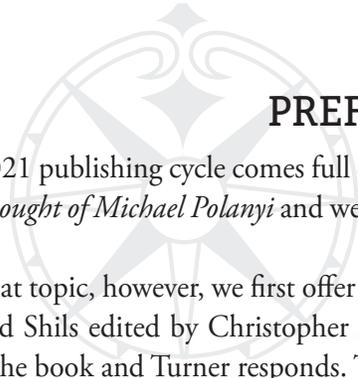
For example: Polanyi argues that (*TD*, 56). Full bibliographical information should still be supplied in the references section since many of us may work with different editions of his works.

- Endnotes should be used sparingly and be placed before the reference section.
- We do recognize that Polanyi's work connects with scholars who work in diverse disciplines that use different style guides. To the extent that our software allows, we will accept other styles (e.g., APA or MLA) so long as the author is consistent and careful in following it. The main point, of course, is to give the reader enough information to locate and engage your sources. Manuscripts that are not careful and consistent in style will be returned so that the author can make corrections, which may delay publication.

For more information see <http://polanysociety.org/Aims-and-Scope-9-12-18.htm> and <http://polanysociety.org/TAD-Submissions&Review-9-12-18.htm>

Tradition & Discovery is prepared by Faithlab in Macon, GA.

Tradition & Discovery is indexed selectively in *The Philosopher's Index* and *Religious and Theological Abstracts* and is included in both the EBSCO online database of academic and research journals and the Directory of Open Access Journals. *Tradition & Discovery* is also available online at www.polanysociety.org.



PREFACE

This last issue of the 2021 publishing cycle comes full circle. We opened 2021 with a forum on Gábor Bíró's book, *The Economic Thought of Michael Polanyi* and we close with further comments from Eduardo Biro and a response from Bíró.

Before we get to that topic, however, we first offer commentary on *The Calling of Social Thought*, a book on the work of Edward Shils edited by Christopher Adair-Totef and Stephen Turner. Struan Jacobs and Peter C. Blum review the book and Turner responds. To find out what Shils has to do with Polanyi, be sure to read these essays.

Finally, Alessio Tartaro continues his analysis of how Polanyi's thought develops over time, this time examining Polanyi's early criticism of positivism.

As always, be sure to check out www.polanyisociety.org for the latest news about meetings, scholarship of interest, and other happenings. Remember, too, that the deadline for submitting dues for 2022 is December 31, 2021.

Paul Lewis

NOTES ON CONTRIBUTORS

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Struan Jacobs (struanjacobs@gmail.com) taught social theory over many years at Deakin University, Australia. He researches Polanyi's social thought in relation to the ideas of significant others, including Karl Popper, Friedrich Hayek, Edward Shils, and Thomas Kuhn.

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RECOVERING THE THOUGHT OF EDWARD SHILS

Struan Jacobs

Keywords: Edward Shils, social theory, civility, liberalism, tradition, collective self-consciousness

ABSTRACT

*This article provides an extended review of *The Calling of Social Thought*, a collection of essays about the thought of social theorist Edward Shils. The article includes preliminary observations about Shils' life and work, brief summaries of the essays included in the collection, and several suggestions aimed at encouraging additional study of Shils' writings.*

Introduction

Edward Shils (1910-1995) is the distinguished scholar whose life and writings are discussed in the recent collection of essays edited by Christopher Adair-Totef and Stephen Turner and titled *The Calling of Social Thought* (2019). Unless otherwise noted, parenthetical citations in this review are to this work. Shils was chiefly affiliated with the University of Chicago and its Committee on Social Thought which John Ulric Nef, Robert Redfield, and Robert M. Hutchins founded in 1941. Parallel with his Chicago position, Shils held appointments at the London School of Economics (1946-1950) and the University of Cambridge (1961-1978), as well as visiting other countries such as India (1955-1956) and The Netherlands (1976-1977).

In the time I was waiting to receive a copy of the book from the publisher, I jotted down some of my main impressions of Shils' work based on my reading of it over the years. He reminds one of the great German sociologist Max Weber, except he writes more clearly than did Weber. As with Weber, his reading and learning were prodigious, and his research is characterized by detail, rigour, and integrity. Assisted by Henry Finch, Shils translated Weber's methodological writings into English (their translation of Weber's *The Methodology of the Social Sciences* first appeared in 1949), and he made important use in his own work of ideal types and other methodological offerings of Weber.

Shils' writing covers vast tracts of the social landscape, ranging from characteristic features of intellectuals to issues surrounding atomic science, from Indian social life to the fundamental constitution of society (including primary groups, sacredness and social bonds), from civil ties and civility to universities and science, etc. Thomas Kuhn attested to the catholicity of Shils' erudition in remarking in his foreword to

the English language edition of Ludwik Fleck's *Genesis and Development of a Scientific Fact* (1979) that Shils was one of only two people known to him who "had read [Fleck's] book independent of my intervention." Unsurprised at this, Kuhn commented Shils "has apparently read everything" (vii).

Shils was officially recognized among the leading scholars in the humanities and social sciences in the second half of the 20th century. He gave the Jefferson Lectures for the National Endowment for the Humanities in 1979 and was awarded a Balzan Prize in 1983 (a co-winner that year with the biologist Ernst Mayr and the orientalist Francesco Gabrieli). Given the undoubted high quality of his work, why is it we hear so little of Shils and his work these days? Why doesn't his work command anything like the attention devoted to the writings of social theorists such as Ulrich Beck, Michel Foucault, Jurgen Habermas, Pierre Bourdieu, Anthony Giddens, Erving Goffman, and Talcott Parsons? A Google search easily confirms Shils' relative neglect and, as further evidence of it, after 1,000 or so pages of fine scholarship in the body of the book, the highly detailed index of Roger Smith's major study, *The Fontana History of the Human Sciences* (1997), has no mention of Shils' name.

Part of the explanation of his neglect may lie with Shils' willingness to tread on the toes of his colleagues. He also ended up on the "losing" side of several arguments, supporting ideas and causes that became unpopular in the universities. For example, for a time he supported Talcott Parsons' functionalist approach to sociological explanation, a position many social scientists excoriated as rationalizing the *status quo*. To explain institutions and practices in terms of their maintaining the structure of society is innately conservative, complained critics of functionalism. Further, Shils' involvement with the CIA-backed Congress for Cultural Freedom and its journal *Encounter* was out of step with rank and file sociologists and social scientists who viewed him as a "Cold War warrior." He made himself unpopular in certain quarters by abjuring quantitative social research methods in favour of qualitative methods, and by favouring a consensual perspective on society instead of one that emphasises conflict. However, the common description of Shils as a sociologist *simpliciter* obscures the reality of his polymathic, protean mind. In explaining facts about societies and cultures, he draws on the knowledge provided not only by sociology, but also by history, philosophy, psychology, politics, literature, and anthropology. His eclecticism made him an "outsider" (48, 212).

Shils' erudition is more evident in his book *Tradition* (1981) than it is in any of his other writings. His decision to write on tradition was an unusual one for a scholar to take in the 20th century; here we have another example of him following his own course rather than conforming to academic fashion. Since the Age of Reason of the 18th century, "progressive" thinkers have disparaged tradition as procrustean, prejudiced and non-irrational, a hangover from the Middle Ages and incompatible with the rational-empiricist spirit of the (modern) age. Only musty conservatives, it was widely assumed, would bother studying tradition, and it was on account of this assumption, for example, that John Stuart Mill puzzled members of his circle of "philosophical radicals" with his sympathetic study, "Coleridge" (1840). Shils considered his book to be the first book ever devoted to analysing the subject of tradition *per se*. *Tradition* is a fascinating book: rich in ideas, generous with examples, and criss-crossing many fields of scholarship. Cogently arguing there can be neither social life nor culture without traditions, Shils' book has helped in stimulating scholarly interest in tradition.

Shils writes about subjects that are often complex and nuanced. Among his salient ideas are some with which I have struggled to affix definite meanings. Of none of them is this more true than "collective self-consciousness," an idea that leaves me wondering whether he hypostatized or reified it as if it were an

existing thing. “Civility,” “ideology,” and “nationality” are among other concepts of his that have taxed my powers of comprehension.

The Essays

Given the intellectual orientation of *Tradition & Discovery*, readers of this review will want to know whether Shils is relevant to the study of Polanyi and, if so, in what way(s). The “terms of engagement” between Shils and Polanyi is the subject of Phil Mullins’ contribution to *The Calling*. Written with his characteristic verve and clarity, and based on his vast knowledge of the Polanyi manuscripts, Mullins details a friendship that commenced in 1946 and continued for 30 years. It was an intellectually supportive and productive friendship, Shils citing Polanyi as one of three “elders” he knew personally who “left an imprint on me,” the others being the economist and philosopher Frank H. Knight and the sociologist Robert E. Park (79). Shils was probably second only to the philosopher Marjorie Grene among Polanyi’s “more intimate intellectual friendship[s]” (80). Shils’ 1945 writings for the *Bulletin of Atomic Scientists*, the journal he helped establish at Chicago, show how quickly he absorbed Polanyi’s broad understanding of pure science as animated by agents with commitments to discovering more of the spiritual ideal of truth and—as the totalitarian experience attests—ceasing to function when governments impose an agenda of planning on science. *The Bulletin of Atomic Scientists* would provide Polanyi with a convenient vehicle for a number of his essays beginning in 1946.

Mullins also throws light on relations between Shils, Polanyi, and Karl Mannheim, referring to Shils’ major role in translating from German to English Mannheim’s exposition of the sociology of knowledge, *Ideology and Utopia* (1936), and Mannheim’s planning tract *Man and Society in an Age of Reconstruction* (1940). Polanyi and Mannheim knew each other from their adolescent years in Budapest, and they resumed their acquaintance in England in the 1930s. In the next decade Mannheim arranged for Polanyi to attend meetings of “The Moot,” the group of eminent English intellectuals that included T.S. Eliot, Geoffrey Vickers, A.R. Vidler, and H.A. Hodges that met informally under the auspices of the ecumenical church leader, J.H. Oldham, to discuss how Christianity might be used to revivify Britain once the allies had won World War II. Mannheim generously encouraged Polanyi to produce a book of essays on the autonomy of science for inclusion in the International Library of Sociology and Social Reconstruction series that Routledge & Kegan Paul launched in 1942, with Mannheim the founding editor. Polanyi’s attention got diverted by other projects, particularly his Keynesian tract for the times, *Full Employment and Free Trade* (1945), delaying the completion of his essay-collection on autonomous science and the free society—*The Logic of Liberty*—until 1951. In his intellectual autobiography, Shils suggests Polanyi had a low regard for Mannheim’s thought, which is hardly surprising given that Mannheim’s doctrines of social determination of belief and large-scale dirigiste social planning resembled themes associated with the “Freedom of Science” movement that Polanyi repudiated as contrary to the requirements of pure science and as destructive of civil society.

Relations between Shils and Mannheim and their respective depictions of ideology form the subject of Christopher Adair-Totef’s contribution to *The Calling*. Including Mannheim’s lesser known writings, as well as the books—most notably *Ideology and Utopia*—that secured his reputation, Adair-Totef does a fine job of disentangling Mannheim’s understandings of ideology. He discusses how Shils’ thinking on ideology diverged from Mannheim’s, prompting one to ponder whether some of Polanyi’s dissatisfaction with Mannheim’s thought may have rubbed on to Shils. Even so, Shils never ceased regarding ideology as

an inevitable feature of modern social life, rejecting the “end of ideology” thesis promoted by the Harvard sociologist Daniel Bell in the 1960s. Stephen Turner describes Shils’ account of ideology as a “universal, flowing,” and necessary condition for social life to be rationally ordered, coming to the fore in “times of crisis” (27).

A couple of Adair-Totef’s claims look to be disputable. He suggests as a priority claim that Shils regarded Mannheim as likely “the first person to draw attention to the notion of ideology” (111). Either Adair-Totef and/or Shils is mistaken on this: the Lockean *philosophe* Destutt de Tracy coined the term *idéologie* no later than 1796. Adair-Totef also suggests Shils looked on *tradition* as “withering away” in modern society. Shils would disagree with such a generalization. He acknowledges that certain traditions—including, for example, Christianity, the nuclear family, and the tradition of pure scientific research—have weakened in late modernity, while other traditions—practical science and technology, for instance—have never been more vibrant than they are today.

Michael Oakeshott, whose ideas exist toward the opposite end of the social-political spectrum to Mannheim’s, is juxtaposed with Shils in a chapter contributed by the distinguished Oakeshott scholar Efraim Podoksik, who displays their intellectual similarities and differences. Nothing in Podoksik’s text suggests Shils and Oakeshott had dealings with each other, that they were mutually influential, or that they were given to citing each other’s work. In light of such facts, the reader may wonder about the editors’ grounds for deciding to include a chapter on Shils and Oakeshott in the book. Presumably they based their decision on the intrinsic intellectual interest of the topic. But then, shouldn’t they also have included chapters on Shils in relation to Karl Popper and to T.S. Eliot, who are thinkers no less interesting than Oakeshott and who, as Podoksik recognizes, made “a significant impact on Shils’ thinking” (123)? In the preface of *Tradition*, Shils acknowledged Eliot’s writing as having “done so much to arouse and nourish my mind” on the subject (Shils 1981, vii). Shils, one notes, joined with Popper to lead a seminar on substantive social topics and sociological methods at LSE in the late 1940s. He cited Popper on a number of occasions, and he noted that his important idea “of the autonomy of objectivated symbolic configurations [what an ugly name is that!] was greatly aided by the appearance of Karl Popper’s ‘Epistemology without a Knowing Subject’” (Shils 2006, 126). Podoksik offers a discussion of correlations between Shils and Oakeshott rather than of demonstrated causes and effects between them. He describes Shils and Oakeshott as “anti-totalitarian” intellectuals and opponents of social planning (124-126). They reject “ideological politics” at the same time as they agree that ideology is inherent to modern politics (126, also 27). They support, says Podoksik, the politics of “plural values,” grounded in “tradition, consensus, and hierarchy,” as defining the liberal order (126, 128). Philip Altbach’s essay in *The Calling* depicts Shils as a conservative on certain issues but not as a “traditional conservative,” and Podoksik would surely agree with this description (209).

Bryan Turner’s essay, “Edward Shils and his *Portraits*,” describes and contextualizes the content of Shils’ 1997 book *Portraits: a Gallery of Intellectuals*, which includes profiles of eminent subjects such as Raymond Aron (philosopher and sociologist), Nirad Chaudhuri (historian of India), Sidney Hook (philosopher), Robert Maynard Hutchins (President and later Chancellor of the University of Chicago), Arnaldo Momigliano (historian), and Leo Szilard (nuclear physicist). Turner stresses how Shils is no different than other human agents in being “constitutionally” and “systemically” contradictory (191). Turner cites by way of illustration the disparity between Shils’ love of America and his immersion in English culture, particularly the academic life of Cambridge University. Shils’ interest in science and science policy Turner sees as contradicting his studies of tradition, and these studies as contradicting his work on contemporary society. Turner’s

meaning of “*contradiction*” is figurative: he uses the term to signify offbeat or unusual conjunctions, not the logician’s notion of incompatibility or mutual exclusion. What I find most interesting in Turner’s chapter is his interpretation of Robert Maynard Hutchins: Hutchins is well known for his involvement in the “Great Books” program of the Committee on Social Thought, but I suspect I am not alone in having been oblivious to Hutchins’ negativity. His friendship with Shils notwithstanding, Hutchins had a low regard for sociology, he doubted whether the empirical social sciences would ever have utility, and he cared little for “the physical and biological sciences.” Judging from Turner’s discussion, Hutchins’ intellectual enthusiasms were limited to the “Great Books” program and Mortimer Adler’s Aristotelian-Thomistic “philosophical framework for the analysis of American society” (200).

The theme of twists and turns and “contradictions” in Shils’ thought provides a useful peg on which to hang discussion of Stephen Turner’s introduction to *The Calling*. Like Bryan Turner, he uncovers a number of “paradoxes” and qualifications in Shils’ writings. For example, Shils took his first degree in literature rather than sociology, and his first job was in social work. He was sanguine about sociology’s prospects in the aftermath of World War II, but “disillusion soon set in” (5). Turning away from Robert Merton, Paul Lazarsfeld, and other “conventional” quantitative sociologists and disagreeing with sociologists on the political left, Shils came to regard sociology as “a form of the self-understanding of society” and as a discipline able to enhance “human autonomy” (5). His interests and commitments were too diverse for him to be properly classified as a sociologist; nevertheless, a remarkable coherence and continuity is evident in the fact that he explored only a handful of ideas “throughout his long career” (7). He was averse to “the engaged scholarship that” became popular in the final decades of his life yet “was himself engaged” (as of course was Polanyi) as an anti-Communist, writing for *Encounter* magazine and working for its host organization, the Congress for Cultural Freedom (7). Shils worked alongside Talcott Parsons for a time, and proceeded then to ignore “the theorizing that occupied the rest of Parsons’ career” (8). Eventually Shils joined Chicago’s sociology department where his writings remained remote from “the professional literature of sociology” and from “sociological theory” (Ibid). He thought and wrote at the margins of sociology and philosophy, being more of a “literary intellectual” at home in the company of novelists and possessing a fine prose style (11). Turner rightly sees Shils’ intellectual twists and turns as the manifestations of a singular, complex, and restless mind, a mind it has to be said that some critics regarded as obstinate and difficult. Turner notes Shils never produced “the great work which pulled it all together,” but still he managed to provide his readers with a coherent view of society, including themes of charisma, tradition, civil and civility, pluralism, centre and periphery, “collective self-consciousness[,] and the sacralised character of society” (8-9, 12).

Steven Grosby’s essay on Shils’ philosophical anthropology piques the reader’s interest as a text that might shed light on Shils’ important but nebulous idea, “collective self-consciousness.” Philosophical anthropology is an area, Grosby says, to which Shils felt “called,” where he could put aside the conflict-based model of society employed by Marxists and critical theorists, and instead devote himself to exploring society from the viewpoint of consensus (32). In Shils’ philosophical anthropology, the social agent appears as a utilitarian, rational calculator who is able to distinguish himself as the “I” of self-awareness from the “we” of his social context, and who prefers to compromise with his fellow citizens than to behave in an inflexible and mean-spirited way towards them. Grosby explains how Shils developed and deepened Max Weber’s four-fold distinction of types of social action with, I would submit, a discernibly different emphasis to Weber’s (46). The principal difference is Shils is adamant that tradition remains important in modernity, whereas Weber saw rationality in its social form—rationalisation—as a many-headed hydra in modern society, ubiquitous

and squeezing life from traditions (46). Moving beyond Weber's typology of actions, Grosby traces out Shils' identification of four "orientations of attachment," being the primordial, personal, sacred, and civil (33). Grosby, it has to be said, is dealing with deeper dimensions of Shils' thought, and it is this that makes Grosby's essay the most demanding of all those in *The Calling*.

Agents in a society of ordered liberty, Shils explains, bring the prevailing image of their society as just and free into correspondence with changes in the social landscape, this process being a part of his concept of "collective self-consciousness." (Here Shils reminds one of Thomas Kuhn's image of members of scientific communities practicing "normal" research, aiming to bring their "paradigm" theory of the world into closer alignment with the facts that inquiry has disclosed.) Much of the difficulty facing the reader wanting to grasp the meaning of Shils' idea of "collective self-consciousness" lies in trying to work out which part of the expression he mostly wants to emphasise. If, on the one hand, the *collective* side is emphasised, the idea recalls (intentionally or otherwise) what Ludwik Fleck's *Genesis and Development of a Scientific Fact* described as "thought collectives" (organic social groupings). If, on the other hand, the *self* is emphasised, Shils would seem to be affirming a social *aggregate* of individual agents (comparable to what Margaret Thatcher meant when she said, "And, you know, there's no such thing as society"). Groups of the first sort are "wholes" (irreducible *Gestalts*) and those of the second are "heaps" (reducible without remainder). Surprisingly, neither Fleck's name nor Kuhn's rates a mention in the index of *The Calling*.

Richard Boyd's essay on "pluralism and civility" is particularly valuable for its clear separation of Shils' ideas of civility. Shils, he finds, envisages civility in two main forms: *private* and *public*. When agents are respectful, cordial, well-mannered, and polite toward one another, they are enjoying relations of *private* civility (143). *Public* civility, the more fundamental dimension of civility for Shils, is essentially conduct that supports the common good. Metaphorically speaking, *private* civility lubricates the moving parts of pluralistic society, helping them to glide smoothly, whereas *public* civility ("public spiritedness") is a glue holding the liberal-democratic polity and its various "substantive values" together (143-144). Shils associates public civility with a style of politics that is respectful of people's beliefs and practices, and which acknowledges "a plurality of standards of judgement" (142). Politics of this sort contrasts with a politics of *ideology* which, embedded in a monistic view of the world, encourages fanaticism, followers aiming to remake society using violent revolution. Followers of an ideology live beyond the bounds of civil society, denying its validity and the legitimacy of its government. Boyd's interpretation of civility as practices promoting the common good of society raises questions about the constitution of the common good: is it one good or a blend of goods, is there a *summum bonum*, who is to mediate between conflicting views of the common good, and on what will any such mediators base their decisions?

There is no gainsaying the importance of notions of civility in Shils' thought; this is underscored by the number of contributors to *The Calling* who refer to them. Stephen Turner, for example, finds Shils conceiving of civility as a social art form that agents learn through trial and error practice. The emphasis in this case is on civility as politeness being a virtue necessary to liberal society. For Adair-Toteff, Shils primarily uses "civility" to signify "the virtue of the citizen' who believes in the common good," a conception that seems to combine the two ideas of civility that Boyd sees Shils as separating (117). An agent who possesses the virtue of civility is, by Adair-Toteff and Shils' reckoning, the citizen of a civil society, accepting the principle of reasoned argument-with-compromise as conducing to society's common good. Peter Mentzel's erudite chapter on "Nations, Nationality, and Civil Society in the Work of Edward Shils," renders Shils' idea of civility as a view of the world that assigns responsibility to individual agents "for the smooth functioning of

human society. In this sense, it is the foundation both for what is usually thought of as ‘civil society’ as well as for a liberal democratic state and free economic system” (156). Grosby highlights Shils’ use of “civility” to underscore the “artful adjudication of...tensions” resulting in compromise settlements (42). These various notions all depict civility as a causal activity or *process*. Shils, we note in passing, also uses “civility” to refer to *civil society* as the *product* of such causal activity.

Lenore Ealy writes of Shils’ recovery of tradition from two centuries of modern neglect. A tradition for Shils is a “pattern of actions,” both physical and mental (73). A number of people think and act in similar ways, being re-enactments that have withstood the test of experience and which later generations of agents will re-enact again. Ealy notes Shils’ view of tradition in modern society differs from that of Max Weber. As their major difference, Shils insists tradition remains influential in modernity, whereas Weber sees it being diminished by the trend of rationalisation whereby social agents require that ends be sought after by way of actions that have been found to be optimally efficient. The majority of sociologists accept Weber’s rationalisation thesis with its roots going back to the 18th century Enlightenment, the protagonists of which called for personal autonomy and free thought to supersede traditional institutions and beliefs which they deemed to be anachronistic. Weber bases his sociology on a fourfold distinction of unit acts: instrumentally-rational, value-rational, affective, and traditional. From these atomic concepts he builds models of complex social institutions and social processes. He appreciated that sociologists need conceptual models for the purpose of imposing order on what William James in another context described as the “blooming buzzing confusion” that would otherwise characterize the experience of social life. Salient in Weber’s depiction of the unit act of *tradition* is the idea of *habit*. Located at the opposite end of the spectrum to deliberative, instrumentally rational action, traditional action is devoid of meaning, Weber says, involving no agential purpose or intent. This, as Ealy notes, is a further disagreement Shils has with Weber, Shils describing tradition as a transfer of “*meaning* from the past to the present” (66, italics added). Moreover, tradition for Shils is strictly speaking not a type of action but a “*pattern*” of the actions (mental and or physical) of the members of a group (66 ff.). In Weber’s account of tradition, conduct of a particular sort continues being re-enacted because this is how the forebears of these people have behaved in the past; the conduct has no goal and is in this sense meaningless. By contrast, Shils argues members of a tradition act for a *reason*, which is to say they act *meaningfully*, whether their goal be intellectual (as with scientists trying to discover some aspect of reality), moral (as when people commit themselves to act justly), or pragmatic (as when an entrepreneur invests his capital in a factory with the aim of selling its manufactures profitably). Shils is correct to say Weber exaggerated the extent of rationalisation (critical reason) in modernity, and that this prevented Weber from appreciating how influential tradition remains. Shils followed Polanyi in depicting, as the means by which the tradition of science gets transmitted, the apprenticeship of the PhD student to a respected master of the craft of research from whom the student learns the traditional skills, leading to his accreditation as a practitioner of scientific research. Weber would have us believe rationalization has attacked tradition in all its forms like an acid corroding metals, whereas Shils paints a complex picture in which traditions such as reason, technology, and languages have gone from strength to strength, while other traditions (e.g., Christianity, family, education) have been weakened by rationalisation and by “progressive” ideals, expansionist legislation, and “scientific and romantic critiques of tradition” (71). Weber seems never to have clearly recognized that the agents of the trend of rationalization—reason, science, and technology—are themselves traditions.

The Weber-Shils relation appears in different guises in chapters of *The Calling*, including Peter Mentzel’s “Nations, Nationality, and Civil Society,” a painstaking analysis of Shils on civil society as permeated with

the “ethic of responsibility” (155). Weber’s celebrated essay “Politics as a Vocation” took the ethic of responsibility to be the vital element of the politician’s role in liberal democracy. Shils agrees with Weber that voluntary association is the hallmark of a modern civil society, the social form that emerged historically among the sectaries of the Protestant Reformation. Civil society is the arena, Mentzel observes, on which most human activity proceeds in liberal-democracy, “self-governing individuals” constituting the liberal order and performing a “watchdog role” over the economy and the activities of politicians (156). Directing our attention to images of nation and nationality in Shils’ writings, Mentzel identifies their essential feature as “a primordial attachment to a bounded” territory together with an appreciation of kinship (165). Shils looks on nations both as mental constructs (or, as he prefers to call them, “self-conscious collectivities”) *and* as objective facts. The idea of such collectivities, Mentzel considers, is imbued with Durkheim’s understanding of the *conscience collective*, the consciousness of common beliefs, sentiments, values, and memories, notwithstanding Shils’ claim he owed Durkheim no intellectual debt.

Philip Altbach discusses Shils as a supporter of the traditional university, the research university that Wilhelm von Humboldt took to be the centre of intellectual life in modern society. An exemplar of von Humboldt’s university, the University of Chicago, provided Shils with a guild environment in which to hone his skills as a scholar and as a teacher. In the 1960s and 70s he offered informed commentary on the development across the United States of the world’s first system of *mass* university education. “The American university assumed world leadership in science and scholarship during this period, with Europe’s pre-eminent role fading” (205). Shils designed the journal *Minerva* to serve as a vehicle for discussing developments in university education and science, and for putting the case for universities to go on receiving government support while maintaining their self-governing autonomy. Shils’ support of the ideal of the meritocratic university, in the tradition of Humboldt and Weber, was at odds with students and academics who push the barrow of affirmative action. So much of what Shils stood for in the university has been eroded in the 21st century, as Altbach appreciates. Universities have been subjected to downsizing and made “accountable,” and—“in the long run more damaging”—the share of funding allocated to *applied* research and the achievement of useful knowledge has risen compared with the proportion of funding for basic research (211).

Whereas Stephen Turner reflects on Shils as being a “widely recognized but misunderstood thinker,” Thomas Schneider believes Shils has been neglected by scholars (48). Some of the factors adduced by Schneider to explain Shils’ neglect are similar to ones I posited earlier to explain why the level of interest in his work is nothing like that in scholars such as Giddens, Beck, and Foucault. Schneider points out that, unlike Talcott Parsons and Robert Merton, Shils established no school, never wanting students to become his disciples. Shils looked on sociology as being part of a scientific programme of human self-understanding. His “Prospect of Sociological Theory” essay of 1961 advocated intensive as well as extensive studies of Western and non-Western societies (the duality of centre and periphery), temporal change (class, power, values), and tradition (56). It was high time, Shils believed, that sociologists returned to the exemplary figures of Weber and Durkheim, tracing out the implications of “man’s being in contact with the sacred or charismatic things in politics, in the legal system, in education and learning, as well as in the churches” (57).

Further Comments

Shils was a great supporter of, and a most erudite and articulate contributor to, the liberal-conservative tradition of understanding Western society in the second half of the 20th century. In his intellectual qualities

of controlled passion, rigour, independence, and commitment to uncovering the truth, Shils reminds this reviewer of Julien Benda, famed author of *The Treason of the Intellectuals* (1927). The editors of *The Calling*—both Adair-Toteff and the indefatigable Turner—must be congratulated on having conceived of the idea of this book, and the authors are to be applauded for the impressive scholarly chapters they have produced for it. The essayists of *The Calling* are warmly accepting of Shils as a person and admiring of his scholarly work. The overall tone of the work is affirmatory rather than critical, which is no bad thing since analysis, exposition, and comprehension constitute a necessary prelude before informed criticism can get underway. It is a first-rate collection of essays, helping give an outstanding mind its due and providing other scholars with a filip to improve further the understanding and assessment of his work.

As is only to be expected, the book leaves hiatuses for future scholars to fill. T.S. Eliot and Karl Popper are cited often, but (as I suggest above in my comments on Podoksik's essay) the book provides no sustained discussion of their relations with Shils. I would particularly like to see a comparative analysis undertaken of Popper's theory of objective knowledge as a part of his three-worlds ontology and Shils' study of individual and collective forms of self-consciousness. Also, how did Shils manage his relations with Polanyi and Popper after these two formidable, independent thinkers formed an intense disliking of each other in the early 1950s? Did Shils' developing friendship with Polanyi come at the expense of what appears to have been his good working relationship with Popper? Did he throw in his lot with Polanyi? Shils, we recall, acknowledged Eliot's writings as having informed his thinking on tradition. But what aspects of his theory of tradition did he owe to Eliot? In what respects did his and Eliot's understandings of tradition differ? Did they have direct personal dealings with each other? Was Eliot's influence on Shils limited to the subject of tradition or did it extend to other aspects of his thought? "Primary groups" are a feature of Shils' social ontology, being viewed by him as strong social cement. Polanyi's social ontology turns on his distinction of "spontaneous" ("dynamic") orders and bureaucratic organizations. A discussion of the similarities and differences of Shils' and Polanyi's types of social entities might prove to be illuminating. More substantive and less exegetical, one asks whether Shils' depiction of science remains valid today. Bryan Turner points out that Shils' essay-collection *Portraits* includes a wistful vein of reflection "on the decline of the university as an autonomous community of scholars and the" bureaucratisation of the modern university (193). This salutary reminder readily transfers across to science and its evolution since World War II. Polanyi was decisively influential on Shils' metascientific thought, and Polanyi's writings on (rather than in) science illuminate a form of science which—as Jerome Ravetz and a number of other scholars argue—was practiced in autonomous, specialist communities until the end of World War II, a form whose days were numbered with the advent of the Manhattan Project ushering in large-scale, capital intensive developments, described by Alvin Weinberg as "big science." Polanyi epitomized the earlier form of science in "The "Republic of Science," the celebrated essay he wrote for the first issue of Shils' journal *Minerva*. "Republican" science, of which Polanyi *qua* scientific researcher had been a part, was explicated in very different ways by the likes of Popper in *The Logic of Scientific Discovery* and by Kuhn in *The Structure of Scientific Revolutions*. Shils' obituary for Polanyi in *Minerva* included a spirited defence of the republic of science along with stern criticism of "the utilitarian attitude towards science" as some sort of munificent cargo cult. The bias toward utilitarian scientific research is stronger these days than it ever has been.

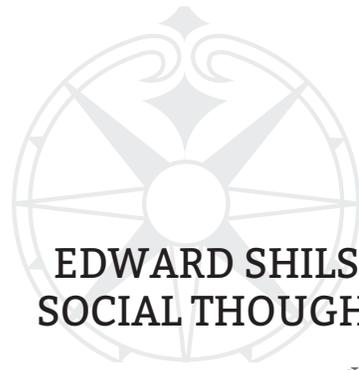
Most of the contributors to *The Calling* respect the rule of good scholarship that requires authors to include page numbers as well as years of publication in their citations. The less detailed method of citing, providing the year of publication without page numbers, has no evidential value. How is the conscientious

reader, wishing to check an author's assertions or interpretations in a book or an article, to proceed if she has no page numbers to guide her? *The Calling* has been well edited: I noticed only two typographical errors ("flowing from" should appear on page 27 instead of "flowing for," and "world" rather than "word" is required on page 131). The index of the book is helpfully detailed.

I understand *The Calling* to be priced at \$120, which puts it well and truly beyond the financial reach of students and of most interested academics. This is a pity, given the intrinsic and historical interest of the essays and their first-rate scholarship. Manchester University Press is to be congratulated on publishing such a fine collection of essays. The press would do the cause of Shils scholarship a further great favour were it able to see its way clear to publish an affordable paperback version in the near future.

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EDWARD SHILS AS STRANGER, SOCIAL THOUGHT AS VOCATION



Peter C. Blum

Keywords: Edward Shils, Georg Simmel, Peter L. Berger, Christian Smith, sociology, social thought, consensus, collective self-consciousness

ABSTRACT

*This essay is a response to Struan Jacobs, “Recovering the Thought of Edward Shils,” which is an extended review of Adair-Totef and Turner’s *The Calling of Social Thought*. It considers Edward Shils as a “stranger,” in the sense defined by Georg Simmel, relative to contemporary sociology. Christian Smith’s claim that American sociology is implicitly pursuing a “sacred project” is invoked, in contrast with Shils’ vision for consensual sociology. The expansion by CST to “*Social Thought*” as a calling (vocation), and its ties to science as understood by Polanyi, are strongly affirmed.*

“Edward Shils is often referred to as a sociologist. This description of Shils and his work can be misleading because of what sociology has become” (Adair-Totef & Turner 2019, 32). There is a deep sense of familiarity, for me, in reading these words by Steven Grosby and in seeing the many ways in which *The Calling of Social Thought* (hereafter *CST*) reminds us of Edward Shils’ status as a *stranger* vis-à-vis contemporary sociology. Having as a basis Struan Jacobs’ thorough and stimulating review essay on the book, instead of presenting an alternative review as such, I will offer some brief reflections, from the point of view of someone who is also “often referred to as a sociologist,” on this sense of familiarity.

What does it mean to be a sociologist? I hope that the reader might hear how this question resonates rather differently from the related question, “What is sociology?” It is what is popularly called an “existential” question for me, as I have traveled a somewhat non-standard route to becoming one.¹ I am often inclined to think that the only thing that makes me one is that I earned a PhD in sociology. But when I am somewhat confident of my identity in this regard, it is most often because I identify with sociological thinkers who are strangers not only in society, but also (to some degree) to sociology as an institutionalized discipline. I feel, in other words, like the passionate lover of religion who is never quite at home with “organized” religion. I use the term ‘stranger’ (*Fremde*) in the sense given to it by the German thinker, Georg Simmel (1908). Simmel understood, because he was a stranger himself.

Edward Shils also understood, and he is one of a few figures in sociology that have long populated my awareness as, in one way or another, fellow strangers. The more decisive “stranger” figure for me as a student was Peter L. Berger. Stumbling into an introductory sociology course in the early nineteen eighties, I soon found Berger’s treatment of sociology as “an individual pastime,” and as “a form of consciousness” (Berger 1963) with a phenomenological and sociology of knowledge bent, and was smitten. But it was not long before I also encountered the respectful antipathy (if I may use that phrase) that was widely aimed at Berger among sociologists informally, as his admittedly conservative sensibilities had become more widely perceived. I know (though the memory is vague) that I also encountered Shils in the early eighties, in relation to interests in the sacred and charisma. It was probably at this point that I first became aware of Shils’ essay, “The Calling of Sociology.” Even then, I connected the word ‘calling’ with the German word *Beruf* (calling or vocation) in Weber, both in *The Protestant Ethic and the Spirit of Capitalism*, and in “Science as a Vocation.” Sociology as a calling would be seen as a vocation in a weighty sense, a discipline implying a sort of asceticism.

To contextualize this more fully for *TAD* readers, it would have been near this time when I also first encountered Michael Polanyi, in a course on the sociology of science. The professor was a self-identified relativist, who was most interested in Thomas Kuhn and laboratory ethnographies, but he also had us read *The Tacit Dimension*. A book from those days by Jerry Gill (1981) helped me to connect Polanyi with Maurice Merleau-Ponty and Wittgenstein (both formative thinkers for me). At some point before graduate school, I learned that Polanyi was a friend of Shils, so the two were already tenuously joined in my consciousness when I later became more fully aware of the deep significance of their relationship (admirably documented by Phil Mullins in *CST*). It is thus no surprise that my return to consideration of Polanyi in recent years has also brought Shils back into my consciousness. I now think, in retrospect, that it was no accident that both Shils and Polanyi struck a chord to my ear, since both are strangers in Simmel’s sense (to sociology and philosophy, respectively). But we must recall that Simmel’s sense of strangeness centrally involves the possibility of a kind of objectivity, not simply as detachment, but as “a particular structure composed of distance and nearness, indifference and *involvement*” (Simmel 1971, emphasis added).

As Grosby’s contribution to *CST* reminds us, Shils’ vision for sociology as a vocation is a vision for a consensual sociology pursued in service of a consensual society:

Modern society is, despite all its conflicts and disorders, more of a consensual society than its predecessors were. It is also a society in which personal attachments, for better or for worse, play a greater part than in most societies in the past, one in which the individual person is appreciated, in which there is a concern for his well-being—not just in a veterinary sense but as a moral personality (Shils 1980, 13).

As Shils typified it, consensual sociology (as opposed to technological or oppositional sociology) is one in which the sociologist is alive, in a particular way, to the social relationship that exists between herself and the *persons* (deliberately using this word) that she studies. Shils clearly sees this implied in the idea of *Verstehen*, associated with Weber, but it is also clear that he intends much more than a “technique of research” (as *Verstehen* has so often been presented). The connection is shared fundamental identity, a *moral* connection of “moral personalities,” where the word “moral” has the profound weight that it still carried, for example, in Durkheim’s thought:

A sociologist who takes seriously his own intellectual undertaking, who thinks himself capable of appreciating and of acting in accordance with criteria of cognitive validity but who thinks that the persons who are his subject-matter are incapable, at least minimally, of doing the same, is committing himself to error (Shils 1980, 87).

With regard to the institutionalized discipline of sociology, the final paragraphs of “The Calling of Sociology” resonate now at least as profoundly as they would have forty years ago. Both the technological and oppositional visions help to make sociology more attractive and apparently relevant to those outside the discipline. The consensual vision is inescapably modest in the strength of its claims; it “can at best add a tincture to opinion” (Shils 1980, 92), always requiring interpretation and free judgment rather than packing decisively demonstrative punch or authoritative ideological weight. While technological vision is by no means dead, oppositional vision has arguably gained significant ground. Sociologist of religion Christian Smith has recently penned an especially striking characterization of this (Smith 2014), arguing that American sociology has developed a “sacred project,” with the word ‘sacred’ deliberately chosen in light of its Durkheimian articulation. Significant in relation to our concerns here is that Smith also refers to the project as “visionary.” The project, according to Smith, is

realizing the emancipation, equality, and moral affirmation^[2] of all human beings as autonomous, self-directing, individual agents (who should be) out to live their lives as they personally so desire, by constructing their own favored identities, entering and exiting relationships as they choose, and equally enjoying the gratification of experiential, material, and bodily pleasures (Smith 2014, 7-8, emphasis his).

Those familiar with Smith’s other work will already know that he finds fault with various elements of the project, as summarized here, but he insists that the primary aim of his book is not so much to negate it as to call attention to it, to make it manifest. His contention is that the project is treated as sacred (with implications for the character of transgressions), but that it also remains mostly latent in its operation, covered up by a manifest façade of commitment to an ideal of objective science. The point is to make the project an explicit theme of discussion, and to raise openly the question: Is this what we sociologists actually want to be committed to? At the very least, in Smith’s view, such commitment should not be covered up by false claims that he finds in popular introductory textbooks.

Smith’s argument is a controversial one in sociology, and it is not my intent here simply to endorse it *in toto*, though I am very sympathetic. My point here, rather, is that Smith strikingly articulates hesitations regarding current institutionalized sociology that are informally echoed widely, at least in broad outline, by many contemporary sociologists as well as by external critics. Tellingly, in comparison with the modesty and lack of luster of Shils’ consensual vision, Smith writes: “Without this Durkheimian sacred project powerfully animating the soul of American sociology, the discipline would be a far smaller, drabber, less significant endeavor—perhaps it would not even have survived as an academic venture to this day” (Smith 2014, 8).

But this brings us back, not only to how academic sociology may be far from Shils’ idea of its calling, but also to the fact that the collection of essays now under discussion is titled *The Calling of Social Thought*, where ‘social thought’ is inherently and emphatically interdisciplinary, drawing in (as Chicago’s famous program has) not only sociology and the other “social sciences,” but the humanities and the natural sciences as well. One might hope, in reassessing the work of Shils, that his consensual vision is perhaps very much

alive, a vital faith that is not readily contained in a particular organized church, yet rooted in a tradition of moral inquiry that, while not limited to it, is by no means a stranger to empirical science. And to be clear, my sense is that Shils' friend Michael Polanyi has provided an especially compelling thematization of what "empirical science" would/should amount to.

If we think about this hope (this *faith*) in light of Shils, and of the efforts presented in *CST* to rediscover it, then the problem of understanding "collective self-consciousness," which Jacobs considers an "important but nebulous idea," is indeed crucial. It is not terribly surprising that the idea would seem nebulous to Jacobs, given his apparent assumption that one must decide whether society is a "whole" (*Gestalt*) or a "heap." I take it that Durkheim, Simmel, Berger, and Shils (among others), though they have been read as choosing either an individualist or a collectivist understanding of social order, have been at great pains to work against this as a dichotomy (though perhaps with varying degrees of success). I suspect that it is possible to read them in a way that emphasizes this effort rather than allowing the typologies we use to introduce them to solidify into something like *a priori* categories. Fighting that tendency, and also fighting the insidious remainder of solipsism inherited from Cartesian thought, seem much more likely elements of the difficulty in question than failure to decide on which part of the phrase ("collective" or "self") bears greater emphasis.

Yet none of this is to say that Shils provides us with all of the answers to questions that arise in this regard. There is much more to do. Still, if the phrase "social thought" continues to have resonance, as I believe it does, I would tend to assume that "collective self-consciousness" is caught up in that ongoing resonance as well. It is the great merit of *CST* to call our attention both to Shils' vision, and to the possibility of seeing this vision not just in terms of sociology in the sense of sociology departments in academic settings. Shils and others have heeded the calling of *social thought*, and pursued a consensual vision, and I hope that it is still possible for us to do so. It is certainly no less difficult.

I conclude with Shils' own concluding words regarding consensual sociology (read social thought), which are thoroughly Polanyian in spirit:

It leaves to the human beings to whom it is addressed the freedom of interpretation and judgment which is needed in the public life of a reasonably decent society. It recognizes its own limitations and the limitations in human powers more generally. These are important virtues in an age which is tempted by scientific aspirations and beliefs in the total transformation of societies (Shils 1980, 92).

ENDNOTES

¹The story is much longer than needs to be told here. The summary I often give is that, as an undergraduate, I completed an "interdisciplinary" major in philosophy, religion, and sociology. I completed graduate degrees in both philosophy and sociology (and took a bit of graduate-level theology), but I have never really been able to make the scope of my major concerns any narrower than it was when I completed that undergraduate major. I am very fortunate to be employed at a school that values this inability.

²It is important in this context to note the rather different sense carried by the word "moral" in the phrase "moral affirmation," from that carried by the phrase "moral connection" above. In Smith's summary, it means something like "approving acceptance," and is consistent with the assumption of autonomy in the sense of giving the law (norm) to oneself.

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THE HUMAN FACE OF KNOWLEDGE: A RESPONSE TO JACOBS AND BLUM



Stephen Turner

Keywords: Edward Shils, Robert Park, Michael Oakeshott, Michael Polanyi, tacit knowledge, social theory

ABSTRACT

This is a brief response to comments by Struan Jacobs and Peter Blum on The Calling of Social Thought, Rediscovering the Work of Edward Shils, a recent collection of essays edited by Christopher Adair-Totef and Stephen Turner. It identifies a distinctive contribution of Shils to the larger problem of the tacit.

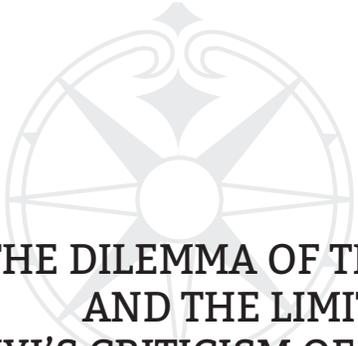
Struan Jacobs is right to point out that there are several chapters that should and perhaps could have been included relating Shils to other thinkers, such as T.S. Eliot. The “should” in this case would be an editor’s wish; the “could” part, finding the right person to do it, is always the hard part. It would have also been nice to relate him to Frank Knight, who was a formative influence, and to Robert Park, whose sensibility he took a great deal from, and admired. Like Park, Shils had a personal interest in social worlds, in workers, and in ordinary people, and Chicago provided him with a rich variety of them. The workings of a family restaurant in Chinatown in Chicago fascinated him, and inspired his loyalty. So did a young priest from Europe who came to Chicago to take his classes, and whom he admired for his parish work. Academics, especially those who were too concerned with climbing the prestige ladder, were, for him, somewhat amusing. He loved academic gossip, however, and retailed it. It is too bad that this part of his character was not preserved.

A reviewer pointed out the failure to deal with India—something I would have also liked to do, as well as Africa, which interested him especially for the contrast between the post-Colonial intellectuals and leaders in the British and French Empires whom he saw as oriented to their respective centers—London and Paris. Our expected chapter on the relation to Parsons was sadly lost because of the tragic illness of the contributor, Uta Gehrhardt, which we learned of too late to replace her. *Minerva*, to which he devoted so much of his life and effort, deserved its own history: in this case there is at least an excellent article. Shils as a pedagogue and contributor to the Soc Sci sequence at Chicago deserved treatment. His relation to Saul Bellow has been told, from the point of view of Bellow’s biographers, but there are other relations to historians and writers that could have been explored. So could his relation with RAND. One hopes all these themes will get the treatment they deserve.

It would have been nice to deal with Popper and Aron, as well as Polanyi—these tremendously prolific writers were important to Shils. But Shils was not a philosopher by temperament, and although he taught Hegel and other such thinkers, he did not engage them as a philosopher would have. Similarly for science, which deserved more discussion: for Shils it was the scientists as a breed that intrigued, and he had plenty of experience with them, as individuals, and this informed his view of science. The philosophical ideas, such as Popper's late discovery of World III, interested him as persuasive observations, not as matters that he was interested in contesting or engaging with on philosophical grounds. In this respect, the comparison with Oakeshott is important. Shils certainly thought of him as intellectually similar, perhaps even as the most similar of all intellectuals. He had tried to recruit him to the Committee on Social Thought. He joked about the bad food at Caius College, where Oakeshott had been. There is something important in their shared interest in antinomies. But Shils sociologized this topic into the puzzle of intellectuals' rejection of their own societies—a rejection that now takes the form of “wokeness” and has become a challenge to civility in the name of civility. And this was characteristic: for Oakeshott it was the intellectual interdependence of the politics of faith and the politics of skepticism that was interesting; for Shils it was the same, and they saw the problems of liberalism as coming out of its internal ambiguities. But the differences also were apparent: for Shils, *who* was on each side mattered to understanding, and to the long history of these antinomic traditions. These loomed larger in Shils' later thinking: he always recognized dissensus, and studied it. But after the Nixon affair, he saw what he called the antinomies of liberalism as the key to the political conflicts of the present. And if anything these have become more important to our polarized politics. But the more fundamental relationship, as I at least think of it, is this: Shils, Oakeshott, and Polanyi as a kind of triangle of theorists of the tacit and tradition. They each deal with different aspects of it, but in a way that does not conflict.

Peter Blum raises the question of what it means to be a sociologist, and the distinctive contribution of Shils to these issues provides part of the answer to the question of what Shils added to this triangular relation. To be a sociologist, for Shils, was to put a human face on ideas, on notions of morality, tradition, and the like, to see what they mean in the lives of people living in the real world of attachments, face to face interaction, and practical affairs. The topic both Jacobs and Blum focus on, and Shils continually referred to, is this concern seen from the side of what Shils took to be, for understanding “society,” a particularly crucial “idea”: the mysterious and largely tacit “collective self-consciousness” which gets manifested in our mutual relations.

Shils knew that he would be subjected to the kind of reductive analysis Jacobs alludes to—as a Cold war ideologue, cheerleader for “development,” Parsons acolyte, and so forth. All of these caricatures are wrong. That his subtlety, sympathy for his subjects, and his ambivalence about the larger social processes at stake would be lost on hostile readers is something he would have expected, despite the fact that they are plain to see in his actual writings. This well-grounded expectation perhaps explains the embargo he placed on his papers, which are still not organized. In any case, subtlety, sympathy, and ambivalence are the first things lost when a thinker is reconstructed and pigeonholed, and also lost by the passage of time and the disappearance of the contexts in which works were composed. Shils often said that a great text was inexhaustible—that there was always more to be discovered. With all his flaws and failures, that is true for Shils as well as for Polanyi, and for some of the same reasons: they were thinkers who thought systematically but never finished the system. They left us with good things to think about.



THE DILEMMA OF THE MODERN MIND AND THE LIMITS OF RULES: POLANYI'S CRITICISM OF POSITIVISM (1946-1952)



Alessio Tartaro

Keywords: empiricism, falsification, modern mind, positivism, scientific discovery, skepticism, verification

ABSTRACT

Starting in 1946, Polanyi begins to criticize a comprehensive system of ideas that he names positivism. His criticism is twofold. On the one hand, it has the narrow aim of pointing out the inconsistencies of a positivist account of science, according to which the essence of scientific objectivity lies in establishing rigorous mathematical relations between measured variables employing fixed rules. On the other hand, it examines the broad assumptions underlying this view, namely radical empiricism and skeptical doubt. The present paper analyzes both aspects of this criticism, stressing its crucial role in the development of Polanyi's philosophy.

Introduction

In a paper titled, “The Roots of Tacit Knowledge: Intuitive and Personal Judgment in Polanyi's Early Writings (1939-1946)”, I proposed a historical reconstruction of the idea of tacit knowledge by analyzing the concepts of “intuitive judgment” and “personal judgment”.¹ The present paper continues that historical reconstruction by examining another crucial step in the development of Polanyi's philosophy, namely his criticism of positivism between 1946 and 1952.

Starting with a criticism of planned science, Polanyi reflects on the functioning of science, focusing on its institutional arrangement and the factors leading to its growth. In particular, Polanyi draws attention to scientific discovery, emphasizing the crucial role of “intuitive judgment” and “personal judgment”. Indeed, scientists rely on undisclosed abilities to make discoveries, abilities that allow them to integrate scattered particulars in a coherent solution. According to Polanyi, the crucial steps leading to scientific discovery are a matter of intuition, creativity, instinct, and personal commitment. For these reasons, I concluded that making discoveries relying on an intuitive and personal judgment is something analogous to knowing “tacitly”.

This account of scientific discovery has an important consequence. Indeed, since intuitive and personal judgment cannot be formulated in exact terms, we cannot provide a set of definite rules for replacing their use. Thus, in view of the fact that these abilities are crucial in the process leading to new discoveries, we can conclude that we cannot formulate rules for making discoveries. As we shall see in the present paper, this is one of the main aspects of Polanyi's criticism of positivism.

The second relevant aspect of this criticism lies in the fact that it involves the broad assumptions underlying the positivist view of science, namely radical empiricism and skeptical doubt. Polanyi names the philosophical framework established on these principles "the modern mind". According to Polanyi, the modern mind creates a self-contradictory ideal of knowledge because it leaves no room for the personal participation that makes knowledge meaningful. This false idea of knowledge, which Polanyi will call "objectivism" from *PK* onward, has moral and political repercussions as well. Against this false idea of knowledge and its consequences, Polanyi elaborates his fiduciary program, according to which the process of knowing is the realization of the conjunction between understanding, believing and belonging (see Polanyi 1947b, 9). Although the criticism of the modern mind is only sketchy in the period under consideration (1946-1952), it is still important because it shows the interplay between critical and constructive thinking in Polanyi's philosophy.

The criticism of positivism had a crucial role in the development of Polanyi's thought. As I showed in my earlier paper (Tartaro 2021), Polanyi didn't develop the concept of tacit knowledge by starting from the maxim "we can know more than we can tell". Rather, his reform of the very idea of knowledge originated with an effort to show that there is something more than what we can tell and that this has to be regarded as knowledge. Polanyi's criticism of positivism had exactly this role. Indeed, the faulty logic of positivist accounts of science demonstrates that scientific knowledge cannot be established by applying explicit rules to primarily given observation. As Polanyi (1947b, 9) states, "science is not based on the mere evidence of our senses". On the contrary, it always involves beliefs that we hold in virtue of our belonging to a group sharing these beliefs. In other words, the value of this criticism lies in demonstrating that not all knowledge is always explicit. Only once this point has been established is it possible to inquire into the tacit dimension of knowledge. This inquiry, which is Polanyi's most important contribution to the epistemology and philosophy of science, thus has its roots in the problems I'm going to consider.

Radical Empiricism and Skeptical Doubt: The Principles of the Modern Mind

The analysis of the modern mind is a first attempt to disclose the origins of the 20th-century crisis. According to Polanyi, the reasons for this crisis and its tragic implications "can be traced back to the very beginnings of modern civilization as it emerged from the Middle Ages" (1945, 8). Polanyi points out how the rise of the modern age is characterized by a struggle against the medieval worldview, in particular against Aristotle's philosophy and the authority of the Church. Radical empiricism and skeptical doubt were the two principal means of the modern mind in this struggle against authority:

Cartesian doubt and Locke's empiricism became then the two powerful levers of further liberation from established authority. These philosophies and those of their disciples had the purpose of demonstrating that truth could be established and a rich and satisfying doctrine of man and the universe built up on the foundations of critical reason alone.

Self-evident propositions or the testimony of the senses, or else a combination of the two, would suffice (*SFS*, 61).

Here Polanyi clearly states the twofold function of the two principles. On the one hand, they are employed as tools against established authority. On the other hand, they lay the groundwork for a critical reason. In a later section, we shall see how the combination of these two principles not only establishes the critical reason but also determines its failure and, consequently, how it motivates the quest for a *post-critical* reason. For the time being, however, we can just focus on the first aspect.

The role of skeptical doubt is to provide a systematic elimination of unwarranted assumptions implied in our way of thinking. As Polanyi states: “To assert any belief uncritically has come to be regarded as an offense against reason... We feel in it the danger of obscurantism and the menace of an arbitrary restriction of free thought. Against these evils of dogmatism we protect ourselves by upholding the principle of doubt that rejects any open affirmation of faith. For the past three centuries the principle of doubt has been continuously at work on the elimination of all uncritical affirmations of faith” (1952b, 217).

Once the unwarranted affirmations of faith are abolished through skeptical doubt, only propositions based on the testimony of the senses are justified. According to the modern mind, the truth can be established on the basis of unquestionable hard facts because empirical evidence is cogent and has the power to compel the assent from any rational human being (cf. Polanyi 1947a, 10). The role of radical empiricism, thus, is to provide a method allowing to formulate propositions that can resist the skeptical doubt. In this way, it is possible to reach a solid foundation for knowledge. The consequences of these principles are far-reaching. Since only self-evident propositions (e.g., logical tautologies) and the testimony of the senses can guarantee reliable knowledge, science is considered to be a mere organization of experience through the application of logical rules.

From the Modern Mind to a Positivist View of Science

As shown in the previous section, both skeptical doubt and radical empiricism played a leading part in the establishment of modern science. Polanyi’s reconstruction of the beginning of modern science can be summarized as follow: (1) modern science is founded on a critical struggle against all authority; (2) the exercise of radical doubt is a means to demolish any a-critically accepted authority; (3) only auto-evident proposition and hard facts gathered with empirical methods can stand up to this radical doubt; (4) thus, science, being the refusal of all authority and the rational activity par excellence, is a mere collecting and organization of these hard facts (Polanyi 1947a 10-11 and 1949, 14).

The positivist account of science pushes further this interpretation, because “the movement set out not only to liberate reason from enslavement by authority, but also to dispose of all traditionally guiding ideas, so far as they are not demonstrable by science. Thus, in the positivist sense truths become identified with scientific truth and the latter tend—by a positivist critique of science—to be defined as a mere ordering of experience” (Polanyi 1949:14). The interrelation between the principles of doubt and empiricism and the view of science as “ordering of experience” or a “calculating machine” is thus quite plain. Polanyi provides a grotesque picture of this conception in the following passage:

A passionate affirmation of what some scientists believe science to be was given in recent years by the distinguished American psychologist, Clark L. Hull, in his *Principles of*

Behaviour. The essence of scientific objectivity lies, he says, in establishing rigorous mathematical relations between measured variables. Given the values of one set of variables, science predicts exactly the value of another set. A genuine scientific theory must operate like a calculating machine, which, once the keys representing the dividend and the divisor have been depressed, determines the result automatically (Polanyi 1950, 2).

Thus, from this perspective, scientific knowledge is conceived as objective, impersonal, verifiable, and unambiguous.

Polanyi criticizes the positivist account of science from various perspectives. History of science, for instance, supplies evidence that the essential features of scientific discovery do not reside only in establishing rigorous mathematical relations between measured variables. For example, the cases of Copernicus, Kepler, and Newton, show how scientific discovery is a process in which “each new phase re-states that which was known before. Each reveals that its predecessor was the embryo of a truth wider and deeper than itself” (Polanyi 1945, 9). Thus, scientific progress is not a process of accumulation, as the positivists claim. Furthermore, capital studies in heuristics, such as the ones of Poincaré and Polya, reveal how the essential phase of discovery represents rather a process of “spontaneous emergence” than a formation of a mathematical relation between sets of values. Moreover, mathematical relations empirically established are not by itself a mark of scientific discovery, as Polanyi makes clear when he refers to the alleged link between the period of gestation of rodents and multiples of π (cf. Polanyi 1947a, 11). Eventually, Polanyi’s criticism of the positivist account of science is also based on a reflection on the limitations of the rules and the procedures of scientific practice. We shall focus mainly on this last criticism in what follows.

What Rules Cannot Do: Polanyi’s Arguments Against Positivism

As stated in the introduction, a consequence of the crucial role of personal judgment in scientific practice is that the rules alone are not able to lead to discovery, verification, or falsification. Polanyi’s criticism aims to demonstrate that the positivist account of science is false and, *a fortiori*, the assumptions on which this conception is based are faulty as well. Nevertheless, Polanyi doesn’t regard the rules framing scientific practice as dispensable. Nor does he claim that procedures and formalization are of no use. On the contrary, this critical analysis leads Polanyi to a deeper understanding of the meaning of scientific rules in light of personal judgment. Rules prove to be “rules of art,” which are such only when embedded in practice. They are not strict codifications prescribing unambiguous application, but rather vague maxims leaving a significant margin to personal judgment.

Rules and Discovery

The main target of Polanyi’s criticism is the idea that scientists can make discoveries simply by applying fixed rules to given empirical observations. In *SFS*, Polanyi sets the problem with the following question: “Given any amount of experience, can scientific propositions be derived from it by the application of some explicit rules of procedure?” (*SFS*, 7). For the sake of argument, Polanyi concedes both that the relevant experience is provided in the form of numerical measurements and that we already know which sets of values are in mutual connection. So, for example, we now face two sets of numerical values, and we know that one can be represented in the terms of the other. Despite this significant concession, however, we

cannot obtain a scientific law through the application of rules and procedures intended as a mechanical activity. Indeed, as Polanyi states,

There are many forms of mathematical series—such as power series, harmonic series, etc.—each of which can be used in an infinite variety of fashions to approximate the existing relationship between any given set of numerical data to any desired degree. Never yet has a definite rule been laid down by which any particular mathematical function can be recognized, among the infinite number of those offering themselves for choice, as the one which expresses a natural law (*SFS*, 7).

We can restate this in mathematical terms. Starting from a series of couples $(a_1, b_1), (a_2, b_2), \dots, (a_n, b_n)$, with $a_i \in A$, $b_i \in B$, we are looking for a rule or a procedure so that we can determine a single function connecting values of the set A to values of the set B . Yet there are many of these functions. For instance, if our couples were $(0,0), (1,1), (2,2), (3,3), (4,4), (5,5)$, the function $f(x)=x$ would be appropriate, but the function $g(x)=x \sin(\pi x)$ would be suitable as well. Nor can we overcome this problem by adding new values. Indeed, although new observations, i.e., new values, could allow us to discern between $f(x)$ and $g(x)$, because they imply different predictions in, let's say, $x=1.5$, many other functions would still be available. That is due to the fact that we are just adding new couples to our series. Now we have $(a_1, b_1), (a_2, b_2), \dots, (a_n, b_n), (a_{n+1}, b_{n+1}), \dots, (a_{n+m}, b_{n+m})$, but the situation is unaltered, because we have only extra m couples. An infinite number of functions are still available, and no rule (e.g., “make new observations and select the one which predicts rightly”) allows us to select a particular one. Polanyi explains this point as follows:

It is true that each of the infinite number of available functions will, in general, lead to a different prediction when applied to new observations, but this does not provide the requisite test for making a selection among them. If we pick out those which predict rightly, we still have an infinite number on our hands. The situation is in fact only changed by the addition of a few more data – namely, the “predicted” data—to those from which we had originally started. We are not brought appreciably nearer towards definitely selecting any particular function from the infinite number of those available (*SFS*, 7-8).

At first sight, Polanyi's argument seems to state simply the thesis of the underdetermination of theory by data. In a certain sense, this is true. Polanyi acknowledges that different mathematical functions, i.e., different scientific laws or theories, can account for the same empirical evidence. At the same time, however, Polanyi does not share the conventional anti-realistic and skeptical interpretations and implications of the thesis. When providing this argument, Polanyi doesn't mean to prove that scientific discovery is unachievable, or that scientific laws don't describe anything “real”, or that they are only conventional accounts of reality. Instead, the argument aims to show that scientific discovery is not the result of the application of rules and procedures as the positivist account holds. Polanyi summarizes his point as follow: “I am not suggesting that it is impossible to find natural laws; but only that this is not done, and cannot be done, by applying some explicitly known operation to the given evidence of measurements” (*SFS*, 9). As we will show in the next section, according to Polanyi, scientific theories concern reality, and they are susceptible to genuine verification, too. However, there are no rules that underlie the process of verification.

Rules for Verification

Even though positivism is unable to provide explicit rules for discovery, the argument sketched above doesn't amount to a complete refutation of this conception. Indeed, making recourse to the distinction between the context of discovery and the context of justification, advocates of a positivist account of science could argue that the criticism misses the point. The generation of a new idea or hypothesis and the validation of that hypothesis are two different processes. While the former is not subject to an analysis in terms of rules and procedures, the latter is precisely the circumstances where rules come into play to answer questions of justification and validity. Thus, the rules cannot lead us to discovery but can guarantee that the outcomes of the process of discovery are sound and justifiable.

According to Polanyi, positivism is wrong even on this point. As far as rules of verification are concerned, Polanyi provides at least three arguments. The first one is just a consequence of what we said in the previous section. Indeed, having proved that scientific propositions are not strictly derivable from experience, *a fortiori*, further observations cannot give any conclusive confirmation about the validity of these propositions. Let us consider again, for example, the rule of successful prediction. Even though it is often conceived as an *experimentum crucis*, the fulfillment of a prediction does not lead automatically to the confirmation of a scientific proposition. This is due to the fact that adding new observations is not enough for determining which function holds between the measured values. New evidence can discriminate between competing functions involving different predictions, but an infinite number of functions would still be available (See Polanyi 1950, 28).

As a second argument, Polanyi provides a case study proving that correspondence with observations does not always imply the validity of a scientific proposition or theory. I will not go into detail about this argument, but will simply show the conclusions drawn by Polanyi. In this case, both the fulfillment of predictions and the reproducibility of experiments are concerned. According to Polanyi, even the most rigorous criteria of verification may be fulfilled, and nevertheless, they may lead to apparent confirmation of a false proposition in science. The case proposed by Polanyi (*SFS*, 78-79) confirms this conclusion. Moreover, a further example makes manifest that "our reliance on reproducibility suffers from a fundamental weakness. It is always conceivable that reproducibility depends on the presence of an unknown and uncontrollable factor which comes and goes in periods of months or years and may vary from one place to another" (*SFS*, 79). Different criteria of verification, such as the reproducibility of the experiments or the fulfillment of prediction, always leave room for conceivable doubts on the reliability of the results. Thus, the question about how we can validate a scientific proposition remains open because we do not know how to determine the reasonableness of these doubts through strict and explicit rules.

Polanyi's last argument is the most compelling one because it clearly shows that verification is not reducible to the application of explicit rules and fixed procedures. To prove this point, Polanyi proposes the following example:

Suppose a player of roulette observes the numbers of reds and blacks that turned up in a hundred consecutive throws. He may plot them in a graph and derive a function in the light of which he will make a prediction. He may try it out and win. He may try it again and win. And win a third time. Would that prove his generalisation? No; in our view, it would only prove that some roulette players are very lucky—i.e., we would consider the fulfillment of his predictions as mere coincidences (Polanyi 1950, 28).

In this case, the player of roulette behaves as the scientist does according to the positivist point of view. She gathers observations, establishes correlations, and represents them in a mathematical form. Thus, starting from a series of couples (throw1, color1), (throw2, color2)...(thrown, colorn), he determines a function allowing him to predict the value of colorn+1 corresponding to the thrown+1. At this point, his law needs to pass some test to be confirmed. The results have to be reproducible; different methods have to lead to the same results; and, above all, predictions have to be fulfilled. Now, let us suppose that the law discovered by the player of roulette succeeds in meeting these criteria. Despite the evidence, we cannot conclude in favor of the validity of the law. We would prefer to say that the fulfillment of the predictions is only a coincidence, and the player is merely lucky.

Discussing the deeper reasons why we reach this conclusion is beyond the scope of this article. However, the main point is that this example allows us to show that the fulfillment of predictions in terms of observations is not in itself capable of validating a scientific statement. In general, any rule for verification cannot definitively validate a law or a theory. Consequently, given that there are valid scientific statements, we have to admit that verification is based on a different ground. Before displaying on what this process is grounded, we shall show that falsification as well is not a process consisting of the application of rules and procedures.

Rules for Falsification

Polanyi's considerations on verification are only a part of an alternative understanding of the relationship between theory and experience. The arguments above demonstrate that a scientific theory cannot be considered a mere ordering of experience, because observations alone, and a mathematical articulation of the measurements derived from them, can not lead to discovery. Nor, as argued in the previous paragraph, can experience alone validate a scientific statement. Now we can point out that the opposite holds too, namely that adverse observations don't lead automatically to the disposal of the theory. As Polanyi writes in "Scientific Beliefs," "the current positivist story that a scientist immediately drops a hypothesis the moment it conflicts with experience is a pure myth. No true scientist acts in this clumsy manner" (Polanyi 1950, 28).

Against this idea, Polanyi argues that, as in the case of verification, rules of falsification are not applied mechanically and thus always leave room for conceivable doubts about the reliability of the outcomes. In particular, Polanyi points out how some potential falsifications are explained away, for instance, through the addition of an *ad hoc* hypothesis or the reduction of the adverse observations to anomalies, i.e., an unsolved and partially unimportant problem of the theory.

Before going into details, consider a possible positivist objection to Polanyi's argument. An advocate of the positivist view of science could argue that even though it is true that scientists explain away potential falsifications in their daily practice, this is what they do but not what they should or ought to do. In other words, she could support the idea that any account of science, and in particular accounts of falsification, should be normative and not just descriptive. Thus, scientists explaining away anomalies behave incorrectly, because they do not follow the rule prescribing to reject a theory or a scientific statement each time these are refuted by experience. In this respect, the compliance with the rule according to which every time predictions fail the theory should be rejected is a sign of good science, in opposition to the bad practice of scientists explaining away potential refutation of their theory.

Without going into details about the alternative between a normative and a descriptive account of science, Polanyi's reply to this criticism aims to show that the process of explaining away adverse observations

is not in and of itself an inappropriate practice in scientific research. Indeed, the positivist objection relies precisely on this assumption. As Polanyi states:

The process of explaining away deviations is in fact quite indispensable to the daily routine of research. In my laboratory I find the laws of nature formally contradicted at every hour, but I explain this away by the assumption of experimental error. I know that this may cause me one day to explain away a fundamentally new phenomenon and to miss a great discovery. Such things have often happened in the history of science. Yet I shall continue to explain away my odd results, for if every anomaly observed in my laboratory were taken at its face value, research would instantly degenerate into a wild-goose chase after imaginary fundamental novelties (*SFS*, 17).

Moreover, explaining away anomalies also has a much more valuable role than this. Indeed, when scientists dispose of contradictions to their theory by calling them anomalies, they pave the way to a further expansion of the theory itself. Indeed, if theories were abandoned each time they face adverse experiences, these theories could never advance. Scientists would behave in a clumsy manner if they did so. As Polanyi writes: "It is true enough that the scientist must be prepared to submit at any moment to the adverse verdict of observational evidence. But not blindly" (*SFS*, 17). A strict and mechanical application of the rules of falsification, such as the one that prescribes abandoning the theory when it does not fulfill the prediction, would paralyze scientific research. This is the reason why the abandonment of a theory cannot be decided exclusively by the application of some rules. As Polanyi summarizes this argument: "Scientists will often tolerate such contradictions to their theory, regarding them as anomalies which may be eliminated in the course of time by an amplification of the theory. Whether they should abandon a theory or not in any particular case can be determined by no fixed rule" (Polanyi 1950, 30).

Rules as Rules of Art

Polanyi's argument definitively demonstrates that scientific processes such as discovery, verification, or falsification cannot be carried on through the application of fixed rules and explicit procedures. At the same time, given that they are actual processes commonly found in scientific practice, the other goal of this criticism is to show that there has to be a different ground, other than the explicit one of rules and procedure, on which discovery, verification, and falsification are based. It is my conviction that this critical debate with positivism that occurred around the forties is a crucial step in the development of Polanyi's thought. Indeed, it is exactly the reevaluation of the function of rules that allows Polanyi to develop the first insights about the "intuitive judgment" into the concept of "personal judgment".

Before closing this section, we need to make clear the meaning of Polanyi's conclusions on the function of rules. Indeed, we have to avoid a possible misunderstanding which could follow from the considerations presented so far, namely that Polanyi's criticism amounts to disposal of any kind of rules in favor of a complete intuition-based account of scientific discovery, verification and falsification. This is not the case. Even though these reflections foreshadow the preeminence of the tacit over the explicit, the goal is not to get rid of the explicit dimension as something pointless in scientific practice. On the contrary, Polanyi aims to show rules in a different light. Rules of scientific research are not unambiguous prescriptions to be followed mechanically, as rules for multiplication are, for instance (*SFS*, 44). On the contrary, they are rules of art,

namely “vague rules embodied in the art of scientific research” (*SFS*, 44). This kind of rule is not subject to an unambiguous interpretation. As Polanyi states: “How can we ever interpret a rule? By another rule? There can be only a finite number of tiers of rules so that such a regression would soon be exhausted. Let us assume then that all existing rules were united into one single code. Such a code of rules could obviously not contain prescriptions for its own reinterpretation.” (*SFS*, 44).² Thus, because they are not incapable of precise formulation, they leave room for personal and intuitive interpretation at each new application. If so, this also implies that “every process of reinterpretation introduces elements which are wholly novel” (*SFS*, 44), i.e., each new application and interpretation modifies the very meaning of the rules themselves.³ It is exactly in this context that personal judgment comes into play. In order to use and apply the vague rules embodied in the art of scientific research, scientists have to resort, in various degrees, to their own personal judgment. As Polanyi concludes:

We may conclude that just as there is no proof of a proposition in natural science which cannot conceivably turn out to be incomplete, so also there is no refutation which cannot conceivably turn out to have been unfounded. There is a residue of personal judgement required in deciding as the scientist eventually must what weight to attach to any particular set of evidence in regard to the validity of a particular proposition (*SFS*, 17).

The Dilemma of the Modern Mind

The failure of the positivist account of science makes clear that the modern mind establishes science on the wrong basis. The dilemma of the modern mind, as I call it, lies in the contradiction between a “false theory” and a “right practice”. Indeed, the modern mind expects science to be based on radical empiricism and skeptical doubt. If science were actually founded on these principles, any scientific discovery would not be possible. At the same time, however, the progress of science in the last three centuries clearly shows that scientific progress is possible. Thus, the practice of science is based on different premises than the one assumed by positivism.

Polanyi’s reflection on the real foundation of science is beyond the scope of this article. However, it is worth noticing how this criticism of positivism disposes of both radical empiricism and the skeptical doubt employing the idea of personal judgment. Indeed, the twofold function of personal judgment is, on the one hand, to integrate observations in a coherent theory and, on the other hand, to establish the boundary between reasonable and unreasonable doubt. The first function reduces the role of the principle of empiricism because a scientific theory is no longer a matter of gathering indiscriminately new observations. Instead, the second function restrains the application of skeptical doubt. Indeed, a conceivable doubt is always possible. Positivist accounts of science miss exactly this point. In the struggle against any authority, they adopt a kind of doubtful attitude toward any a-critically accepted statement. At the same time, however, they try to avoid the skeptical consequences of this approach by relying on empiricism, namely on observations organized through explicit rules. This line of thought aims to establish the truth of some statements beyond any conceivable doubt. Nevertheless, Polanyi’s criticism shows that this position is untenable because of the very nature of the rules. Indeed, since each rule needs to be interpreted before its application, rules alone can never determine by themselves their own application, because every application can be made out to accord with the rule employing a specific interpretation. That is the reason why doubt is always conceivable. Since it is up to the scientist to decide if doubt is reasonable on any occasion, it is only personal

judgment—not rules and procedures—which allows us to explain how these processes are really fulfilled in the scientific practice.

Conclusion

Polanyi's criticism of positivism paves the way for his constructive postcritical philosophy. Indeed, the rejection of the objectivist tendency of positivism doesn't culminate in an untenable subjectivism. On the contrary, Polanyi's proposal aims to overcome the duality between objective and subjective. Indeed, scientific knowledge is neither absolutely valid nor is it relative to the subject. Instead, it is a *belief* we are *committed to* and that we retain with *universal intent*. In this respect, science has a *fiduciary* foundation. Although Polanyi further develops these concepts afterward, his reflection during the forties is a crucial step in their development. In conclusion, I propose that the following quotation shows the intricacies between the criticism of positivism and its dangerous consequences, the reflection on personal judgment and the constructive project of a post-critical philosophy:

We can now discern the fundamental fallacy of the positivist model of science. It tries to construct a machine which will produce universally valid results. But universal validity is a conception which does not apply outside the commitment situation. Any reference to it is merely a manner of expressing our submission to an ultimate obligation and can appear only as part of a fiducial declaration. The attempt to construct something universally valid, prior to any belief, is logically nonsensical. Science can never be more than an affirmation of certain things we believe in. These beliefs must be adopted responsibly, with due consideration of the evidence and with a view to universal validity. But eventually they are ultimate commitments, issued under the seal of our personal judgment. At some point we shall find ourselves with no other answer to queries than to say, "because I believe so." That is what no set of rules, or any model of science based on a system of rules, can do; it cannot say, "because I believe so". Only a person can believe something, and only I myself can hold my own beliefs. For the holding of these I must bear the ultimate responsibility; it is futile, and I think also ignoble, to hunt for systems and machines which will take that burden from me. And we, as a community, must also face the fact that there is no system of necessary rules which will relieve us from the responsibility of holding the constitutive beliefs of our group or of teaching them to the next generation and defending their continued profession against those who would suppress them (Polanyi 1950, 34-35).

ENDNOTES

¹Editor's note: The article was published in the last issue of *Tradition and Discovery*. See the References for full bibliographic information.

²This is a straightforward formulation of a part of the Wittgensteinian "rule-following paradox", namely the regress of interpretation.

³The understanding of the rules for discovery, verification, and falsification as rules of art has further implications for the question "what is science?" First of all, it implies that science is a kind of art. Being an art, its rules "only can be transmitted only by teaching the practice which embodies them" (*SFS*, 44). But this kind of transmission is typical of traditions, such as artistic or workshop traditions. In this respect, thus, science itself is a tradition. In particular, given the constant process of reinterpretation

of its rules, science is a tradition that constantly renews itself at every stage of transmission. This subject, and the theory of tradition that it implies, however, is beyond the scope of this article.

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MICHAEL POLANYI'S SOCIAL THEORY AND ECONOMIC THOUGHT



Eduardo Beira

Keywords: economics, Michael Polanyi, Keynes, Hayek, “Unemployment and Money” (economics film), science and technology studies, *Full Employment and Free Trade*

ABSTRACT

This review article continues the forum from Tradition and Discovery 47/1 (February 2021) on Gábor Bíró's book, The Economic Thought of Michael Polanyi (London: Routledge, 2019; 178 pp. Hardback: 9780367245634, £120.00; eBook: 9780429283178, £22.50).

Introduction

Twice in *The Economic Thought of Michael Polanyi*, Gábor Bíró makes kind comments about my scholarly work. I thank him for his acknowledgements and am pleased to have contributed to his academic interest in Polanyi's early phase (outside chemistry) working on economic ideas. This book is a new contribution to Polanyi literature relative to a phase which has not been very much explored and discussed until now. My comments are intended both to stimulate an interesting conversation about *The Economic Thought of Michael Polanyi* and to introduce a broader discussion of Polanyi's economics. I begin with global comments on the book and proceed to more particular criticisms.

There is some confusion in Bíró's book about (1) what Polanyi actually thought and did and (2) what he *could* have said or written—considering his later personal and post-critical philosophy—but *did not*. That is, the book purports to be about the period from 1933 (when Polanyi emigrated to the UK to chair physical chemistry at the University of Manchester) to 1948 (when Polanyi began his activities in his new chair of social studies, which allowed him to prepare his Gifford Lectures [1951 and 1952] and then *PK* [1958]). However, Bíró's book often seems to be primarily an account of Polanyi's “personal economics” (110)¹ that is based on an application of Polanyi's late thought about personal knowledge and its philosophical implications to Polanyi's early thinking about economic issues and particularly his film. This, of course, may be an interesting and perhaps even a promising approach to economics, but it does not accurately reflect Polanyi's way into economics. It is a revisionist approach and this colors most of *The Economic Thought of Michael Polanyi*. I thus think that the account of Polanyi's “economic thought” outlined in the book is misleading

and partial. Also Bíró primarily covers Polanyi's writings about Keynesian public policies (and not Keynesian theory per se) that Polanyi articulates in his film and his subsequent book *Full Employment and Free Trade* (1945, hereafter *FEFT*). Polanyi's writings about Keynesian public policy were primarily a carryover from Polanyi's liberal agenda, which was the primary driver for his social thought.

Polanyi's Economics

Polanyi's economics was always a component of his social theory and was not developed as an objective per se, as I will attempt to show. Perhaps the best account of Polanyi's economic thought is his "Economic Lectures"² delivered in 1948 and 1949 after Polanyi moved from chemistry to his new chair in social studies at the University of Manchester. I suspect Bíró regards these lectures as falling later than the period covered in his book and therefore he does not discuss them. But they cannot be overlooked if one hopes to give a well-rounded and sensible account of Polanyi's economics. In these lectures, he tries to combine his spontaneous order approach with classical equilibrium theory ("spontaneous order is a state of equilibrium"). He does not manage to avoid the mathematical complexity of the equilibrium models and their numerical solutions.³ These economic lectures suggest that Polanyi is attempting to frame an alternative construction of economic theory based on the organizational principles arising from his earlier discussion of "two types of orders" in society. Keynes and Keynesian policies, which had been the main subject of *FEFT*, are never mentioned in these economics lectures.

In his lectures, Polanyi also explores a reformulation of economic science.⁴ He suggests that economic science is more than "the theory of choice between scarce resources" (the classical definition). But otherwise he defines the bearing of economic science on his social theory: "economic science arises from problems of polycentric mutual adjustment and increases in complexity with the variety of adjustments to be made." This concept gives a central role to Polanyi's polycentricity: "economic science studies the manner in which these adjustments are related to the achievement of the overall task, and seeks to improve this relationship." Polanyi also includes management in economic science and discusses the rational purpose of managers (to maximise profit) and the functions of a manager. He even attempts a "mathematical formulation of overall economic purpose" and he aspired to produce a mathematical formulation of "the task of a manager in terms of economic theory."⁵

Postmodern Economics?

Bíró suggests that Polanyi was developing views that might be identified as "postmodern economics." He claims that "perhaps, this account [is] the first book on postmodern economics" (12, n.2); Polanyi's work is an "untold story of giving birth to a *postmodern economics*" (1). There is, of course, an abundant earlier literature about postmodernism (philosophical and cultural) and postmodern economics.⁶ I believe to claim Polanyi offers a "postmodern economics" is a mistake since it implicitly brings into the picture a great many large issues about economics as a social science.

Bíró says that "postmodern" denotes what should come "after 'modern' in Polanyi's view" (12, n.2). He associates this new period with recognition that science, economics and society have gone too far on the road of scepticism and utilitarianism which lead[s] to the moral and economic crisis of his [Polanyi's] time." But there seems to be a certain confusion here: "postmodernism" is most often discussed in the literature as "after modernism" where "modernism" has at least a somewhat clear historical definition linked to the

Enlightenment period. But Polanyi does not think of himself and his philosophical ideas in terms of such a historical shift. It is true that Polanyi sharply criticizes scepticism and utilitarianism and the way in which some Enlightenment values developed, leading to the problematic development of modern science and culture. But Polanyi also seeks to recover and reaffirm some Enlightenment values and sometimes suggests there is continuity between his work and ideas and values in some Enlightenment thinkers.⁷

Polanyi argued for a “post critical” philosophy and justified his meaning for this term very carefully.⁸ “Postmodern” and “post-critical” are not synonymous, although both seek to express dissatisfaction with the consequences of the worldview commonly found in modernity and its arrogance and limitations.⁹ Perhaps Polanyi was a constructivist postmodern philosopher, as Jerry Gill suggests,¹⁰ and his postmodern philosophy of personal knowledge (in my view, a late Polanyi achievement, outside the period discussed in Bíró’s book) may contribute to the formulation of an economic theory based on personal knowledge. Polanyi could have done it, but he did not. I don’t, in sum, think “postmodernism”—even “constructive postmodernism” with its interesting allure—is very useful for understanding Polanyi’s economic thought in its context.

The “What to Believe” Talk and Published Excerpts (1947)

Bíró attributes great significance to “What to Believe,” a little-known, brief talk about scientific, religious and civic knowledge that Polanyi delivered in the spring of 1947, excerpts of which were published a little later.¹¹ He argues this talk is the “missing piece” in Polanyi’s “earlier concept of knowing” (142).¹² Bíró makes it a cornerstone of his argument for a postmodern interpretation of Polanyi’s economics. This reading does not, however, seem warranted.

Polanyi contends in this lecture that “understanding, believing and belonging are in fact three aspects of the same state of mind: of the mental process of knowing; they are its theoretical aspect, confessional aspect and its social aspect” (24). But this linkage was not altogether new in Polanyi: when Polanyi delivered his “What to Believe” talk, he had already published, in 1946, *Science, Faith and Society*.¹³ Here understanding, believing and belonging were extensively discussed.¹⁴ More importantly, in “What to Believe,” Polanyi also notes that “only when we realise the perfect conjunction of these aspects in all forms of knowledge, can we hope to judge rightly whether to accept or reject any particular form of knowledge.” He emphasizes that “all knowledge...relies for its truth on all three [aspects]” (24-25). So this “perfect conjunction” of the three aspects suggests a criterion to evaluate and judge knowledge, both at the personal and collective levels. But Polanyi does not say in “What to Believe” how to realize or operationalize this “perfect conjunction.” That in fact seems to be something Polanyi worked on in later ideas about “subsidiary and focal” attention and his later broader account of tacit knowing.

Bíró justifies his postmodern classification of Polanyi’s economics by arguing, somewhat oddly, that “what makes Polanyi economics postmodern lies with the other two aspects” [of knowing]: believing and belonging” (142). In the closing paragraph of the book, Bíró again says that “Polanyi based his postmodern economics on the three interrelated aspects of personal knowing”(159). But he does not explain why this qualifies Polanyi as “postmodern.” Interestingly, many postmodern thinkers emphasize that they are not rooted in tradition which I link to believing and belonging; they de-emphasize the importance of tradition and this seems at odds with Polanyi.

Bíró suggests that the ensemble of the “three interrelated aspects of personal knowing” (159) in fact “offers new opportunities for the interpretation of economics” (142). Bíró tries to fashion such an interpretation, but Polanyi did not do it. Certainly the link between economics and knowledge was not new

in the literature and it seems to me helpful to acknowledge this. I was surprised that Bíró did not cite here the famous “economics and knowledge” paper by Hayek, delivered in November of 1936, ten years before “What to Believe.”¹⁵ This paper changed the path of Hayek’s scholarly work and was a Eureka moment, as Hayek recognized.¹⁶ Polanyi very likely was aware of this paper.

Bíró claims that Keynesian economics ignores two aspects of knowing, believing and belonging, and treats only understanding (111). He contends that Polanyi’s “postmodern economics was not limited to understanding the economy, an aspect of knowing which, for him, mostly mirrored Keynesian ideas” (111).¹⁷ He adds: “According to Polanyi, knowing the economy always comes with two other aspects of knowing: believing and belonging—matters which Keynesian economics ignored, and matters having a rather qualitative and more artistic nature.” I acknowledge that I simply do not follow what Bíró here means. Is he suggesting that Keynes’s economic ideas undervalue belief while Polanyi’s economic ideas highly value belief? I am not sure this follows. Is tradition (belonging) not important to Keynes? Did Keynes ignore the tradition both of economics and his community, while Polanyi did not ignore these factors? Applying Polanyi’s linked three aspects to economists and their theories seems to me very problematic.

Bíró’s framework emphasizing understanding, believing and belonging seems not to be very operational. The three aspects are interdependent processes embedded and imbricated in the process of knowing. According to Polanyi’s concept of personal knowing, you necessarily rely on a background of belief (also later called the subsidiaries, some of which are not conscious) when you understand the meaning of something. And your believing reflects your belonging, your dwelling in certain traditions in a particular historical-cultural community. There is a kind of hierarchy here: understanding depends on believing (but not necessarily only on believing) and believing is influenced by belonging (but not necessarily only on belonging). This is true both for tacit and explicit knowing. This means that the three aspects are three non-independent tacit processes that can not be made fully explicit at the personal level of the knower. It is quite odd to use these aspects, as Bíró seems to do, as something like descriptors of knowledge.

In my view, Polanyi’s central point in “What to Believe” concerns the links or parallels between scientific, religious, and civic knowing and is not the “three aspects of knowledge.” The general relevance of this lecture is due to the continuing thread of Polanyi’s discussion of the “threat of nihilism” associated with moral scepticism. Polanyi’s account of scepticism later leads to a more comprehensive and deep discussion of moral inversion which is an important and *sui generis* topic in Polanyi thought during the fifties and sixties. I believe that “What to Believe” presents the three aspects of knowing as a tentative idea. He proposes that understanding, believing and belonging need to be seen as a counter to the corrosive powers of doubt undermining science, religion and society. But he does not here provide a detailed and careful analysis. Polanyi was a very intuitive thinker as seems apparent in this talk. He wrote later, “one must shoot first and ask questions afterwards, as I have always done. For better or worse.”¹⁸

Polanyi perhaps was somewhat aware of problems in his framework identifying the three aspects of knowing as described in “What to Believe.” These “aspects” do not re-appear in *PK*, *SM* or *TD*, although Polanyi does develop related ideas about such matters and different kinds of awareness and their roots in personal life in a community.¹⁹ Possibly, we should take more seriously what the editor of *Credere Aude* wrote, in a note at the end of the published excerpts from the talk: “Professor Polanyi asked me to state that the views expressed in it do not represent his final thought on the subject” (page 10). Polanyi apparently wished to qualify his brief statement and this leads me to believe that Bíró’s strong emphasis on “What to Believe” is disproportionate.

There is another more general problem with Bíró's strong emphasis on "What to Believe": it, as well as *SFS*, were published during a transitional period (i.e., 1946 and 1947) for Polanyi. During 1946, Polanyi was entering his transition from chemistry (and the economic parts of his social theory) to philosophy and some of the fundamental ideas later discussed in *PK* began to emerge. During the previous year, 1945, Polanyi began changing the direction of his work, focusing less on social theory and more on philosophy. A kind of tipping point was the preparation of the Riddell Lectures delivered during 1946.²⁰ "What to Believe" comes not long after that and it is an early manifestation of ideas developed more richly later by Polanyi. It seems to have things backward to use this transitional matter as a justification for early Polanyi ideas. For example, nothing in Polanyi's film deals with the three aspects of understanding, believing and belonging, although the film was a primary early achievement of Polanyi in economics. Also, nothing in *FEFT* deals with the "aspects of knowledge" in the economic system.

The three aspects of knowing appearing in "What to Believe" are also the basis for Bíró's argument about "personal economics" (110, 150, 159). His final chapter of the book is even entitled "Towards a Polanyian Personal Economics." However, Bíró does not indicate where and when Polanyi used the expression "personal economics" based on the "personal aspects of knowing the economy" (158). The only reference of Polanyi to "personal knowledge" in social theory or an economics context that I am aware of is in an unfinished article from August 1948 about dynamic order.²¹ Polanyi discusses the impossibility of replacing the dynamic order of science with a centrally planned system and, more generally, he argues that dynamic order can never be replaced by corporate order. Polanyi's argument in this article is based on three points: (1) personal knowledge, (2) speed of self adjustment and (3) the number of relations adjusted. About the first point, personal knowledge, Polanyi wrote: "Mutual adjustment of men to a system of dynamic order is often based on the intimate knowledge of the situation of each man; some of that knowledge may be subjective—as the scientist's consciousness of his own special abilities and the consumer's knowledge of his own tastes—or else largely unconscious, like the knowledge involved in any practical mastery of one's personal affairs. It is not possible to report adequately the contents of such subjective or unconscious knowledge to a superior." But this seems to be far from the context of "personal economics" which Bíró proposes.

Bíró's Threefold Mission and Polanyi and the Liberal Party

In his Introduction, Bíró summarizes his account by suggesting Polanyi's threefold early mission was "to craft a heart for economics, to revitalize liberalism and to save the West from the growing shadow of totalitarian regimes" (1). At the least, I believe that it is important to reverse the order of these elements in order to accurately represent Polanyi from 1933 to 1948. The project of revitalizing liberalism is intimately tied up with avoiding moves toward a totalitarian state. The objective referenced in the phrase "to craft a heart for economics" remains quite ambiguous, although for Polanyi establishing a sensible economic order is bound up with establishing the political or social order that will avoid a slide into a totalitarian state. But Bíró seems to use the phrase to point to a new core based on a new epistemological approach (a "personal," human friendly economics)²² which I have suggested above is a revisionist reading. I don't wish to deny the imagination shown in the discussion of key claims in Bíró's *a posteriori* reading and understanding of Polanyi's economic thought. But in a book intended for scholars interested in the history of economics (as claimed by the author and editor in the book's forematter), I would have preferred a more steadfastly historical perspective.

I acknowledge that I presupposed that a book titled *The Economic Thought of Michael Polanyi* would provide a relatively objective overview of the economic ideas, social theories and political movements of the UK (and other western countries) in the analysed period. However, neglecting this discussion means the much needed broader context is missing and this would have been very helpful to most readers. For the most part, *The Economic Thought of Michael Polanyi* is a collection of essays about individual and bilateral views related to Polanyi's interesting letters found in the Michael Polanyi Papers. The chapters seem at times to be independent essays and their relation is not always clear. The title for the book was perhaps chosen by the publisher. Nevertheless, the present title is misleading. A title such as *Essays Around the Social Theory of Michael Polanyi* would have more accurately advertised the book's content. Bíró does suggest that "this book is intended to shed new light on the history of economics in the 1930's and 1940's and a later historiography of this period" (p. 155). But this "new light" is quite dim, in my view, since he also candidly admits the book "does not provide an objective overview of the economic ideas, social theories and political movements of the UK in the analysed period, nor claims to do so" (1).

According to my reading, Polanyi never pretended to be an economist and his contributions to economics are much more diverse than discussed by Bíró in this book. His concern was politically driven: he wished to understand what had happened to his cherished, free and liberal (central) European pre-World War I society and to understand and counter the subsequent rise of totalitarianism (which includes both fascism and communism). His agenda was not economics per se, but what he called "social theory" (which included economic components of a new, regenerated liberalism with social concerns).

In November 1940, Polanyi wrote a note (to himself) about preparing a talk with his Manchester colleague the economist John Jewkes²³ about an eventual liberal manifesto "to set out the framework of a Good Society." Polanyi noted: "I think it is imperative that Liberal economic thought should summarize its fundamental principles now and announce them without further delay. The statement should be positive and emphatic." Polanyi emphasized the manifesto must "proclaim our conception of the purpose of society and of the technique for pursuing that purpose." Polanyi justifies here his interest in economics as a natural consequence of his fight for a new liberalism: "If, with this broader aim in view, I address myself to economic thought and not to political philosophy, my reason is that the most urgent task and the most decisive one lies in this part of social theory." This statement locates the economic interests of Polanyi as part of his broader social theory and justifies these economic interests along the same lines he did earlier in earlier foundational lectures such as "Visual Presentation of Social Matters" and "On Popular Education in Economics."²⁴ In the aforementioned 1940 letter to J. Jewkes, Polanyi emphasizes these points: "the disruption of political democracy was due to the clashes of opposing economic ideas, arising in a medium of profound confusion regarding economic matters. The source poisoning the social body lies in the sphere of economic thought." The driver for Polanyi's economic thought was not his academic life nor his epistemic views about knowledge but was, instead, his own political practice.

Polanyi's agenda in social theory was very much driven by his relationships with the Liberal Party in the UK during the period discussed in this book. Polanyi not only lectured in many liberal clubs and societies around (but not only around) the Manchester area, especially during the 30's and 40's, but Polanyi's circle of "economist friends" were active in Liberal Party activities. This includes, for instance, Beveridge, Lionel Robbins, Harrod, Hayek, Hicks, Colin Clark, Dodds, Dennison and especially Jewkes (who was Polanyi's early economics mentor at the University of Manchester) and Ernest Simon.²⁵ Later, Sir Ernest Simon (Lord Simon of Wythenshawe, of Manchester), who was wealthy and deeply involved in the affairs of Manchester

University, would be pivotal for the scholarly future of Polanyi's move to become professor of social studies in Manchester. Polanyi's move to a new chair in social studies in 1948 was supported by the Simon Fund.

The involvement of Polanyi with the Liberal Party and its community has not been considered seriously in the scholarship on Polanyi: there is no listing for the Liberal Party in the index of B  r  's new book, but there are also no listings in the indices of the Scott and Moleski Polanyi biography (2005) and MaryJo Nye's *Michael Polanyi and His Generation* (2011). Polanyi was a member of the important "structure of industry" subcommittee of the Liberal Party Reconstruction Committee led by Sir Percy Harris from 1941 to 1943. Some planning was then emerging as acceptable within certain sectors of the Liberal Party. Harris and other members clearly favoured an interventionist form of Keynesianism, although the committee found it difficult to reach an agreement in its final report.²⁶ Polanyi, of course, warned that planning would take the government beyond its legitimate role of establishing and enforcing the rule of law.²⁷

I suggest that this discussion of policy within the Liberal Party was one of the key drivers for Polanyi to write *FEFT* during 1943-1944. His son George's insistence also likely was important. Young George Polanyi was then very active in the Liberal Party, editing a liberal journal (*The Liberal Review*).²⁸ In general, it is surprising how aligned the writings of Polanyi about social theory are with the internal Liberal Party ideological and policy discussions.²⁹ Polanyi's "neutrality principle" for the Keynesian policy offered in *FEFT*³⁰ is his own maneuver to align Keynesian policies (management of aggregate demand through control of the monetary circulation in order to get a full employment economy) with his liberal principles. It saves his liberalism from the left-leaning policies of economic Keynesian control of investments (like public works). There is no economic justification at all for the neutrality principle; there are only ideological reasons (i.e., his political philosophy) for this option in public policy.

I thus suggest that *FEFT* was a book that was targeted (and this was an important motive) for the inside of the Liberal Party as a reaction to the final Harris report; it was not a book written for purely scholarly reasons. During 1943 and 1944, Polanyi interrupted his writing program (begun in 1939 concerning social orders) in order to write *FEFT*, a book intended, as B  r   notes, to make Keynesian economics "much simpler and clearer before they can become the common property of all thinking citizens," to put matters as Polanyi wrote in the Preface of the book (v). And Polanyi also acknowledges there that "this book can not claim substantial originality" (v). But the Liberal Party context of *FEFT* is of importance. Polanyi seems to believe a better understanding of Keynes' ideas will temper some of the left leaning extensions of Keynesianism and the general acceptance of planning in economics.

In general, *FEFT* continues to be a forgotten book in the economics literature; it is the only published book by Polanyi dealing specifically with economics and bearing on his experience with the production of the economics film, but contrary to public perception it is neither Polanyi's only nor his major contribution to economics. Because *FEFT* argues for Keynesian policies, the popular image is that Polanyi was essentially a Keynesian-centered scholar in economics. But this is at least in part a misunderstanding and, unfortunately, B  r   promotes this view (perhaps inadvertently) when he claims that *FEFT* is Polanyi's "economic *magnus opus*" (9). This overstates matters. B  r   covers over the complexity and subtlety of Polanyi's ideas when he emphasizes that Keynesian economics was "one of the most essential sources of the Polyanian understanding of the economy" (111) and that "Polanyi usually framed his economics as Keynesian" (142).

I suggest that Polanyi's social theory was much richer than most economic thought. Social orders theory, not Keynesian theory, is the essential kernel of Polanyi's social thought and his ideas about the economic organization of society need to be discussed under this broader rubric. In a word, B  r  's book does not make

clear that Polanyi's most lasting contribution to economic thought needs to be situated in this broader context as at least some others have recognized. Elinor Ostrom, the first woman to win a Nobel prize in economics in 2009,³¹ and her husband Vincent Ostrom, for example, pioneered the "polycentric analysis" of public governance; they have made important contributions to the economics of institutions for decades. Their inspiration was Polanyi's ideas about polycentricity and social orders, as they acknowledge in several of their papers.³²

Nye and the Sociology of Science and Technology

Bíró emphasizes the importance of Mary Jo Nye's book on Polanyi and his milieu in UK science and society discussions. He also sees his own book as a contribution to studies in the sociology of science and technology (SST). Nye's book likely has somewhat contributed to greater visibility of Polanyi's thought in the SST domain. And Nye's book is a valuable contribution to Polanyi scholarship, but it is a book that contextualizes Polanyi in the broad framework of the history of ideas which includes the history of philosophy of science as well as science and technology studies but not so much the history of economics. *Michael Polanyi and His Generation* is primarily relevant to SST insofar as it shows the influence of Polanyi on the discussions of social constructionist views of science. But it should be clear that Polanyi is not a social constructionist, although he certainly is keenly attuned to (and discusses) changes in scientific ideas in the history of science. Bíró's enthusiasm for SST seems a bit uncritical for one who understands Polanyi's later "post-critical" perspective.

It is also not quite accurate to suggest that Nye's book is a pioneering book which "put Polanyi on the radar in the sociology of scientific knowledge and social constructivism" (1). Polanyi was already on the SST radar before the publication of Nye's book because he articulated ideas about tacit knowledge. Polanyi was influential for early writers in the SST movement, perhaps especially Harry Collins and Trevor Pinch. Collins, foundational 1974 SST paper³³ "rediscovered" Polanyi and his tacit knowledge as the key to understanding that practice is central to science and this is the reason why it is very difficult to articulate what a scientist is doing. Later, Collins developed his own path around tacit knowledge that is influential in SST. Trevor Pinch³⁴ has recently recalled the influence of Polanyi's tacit knowledge on the Collins works and also on the popular 1971 book by Ravetz³⁵ about social processes of science. Pinch argues that the tacit-based notion of craft practices and the actual practices of scientists made an important contribution to the initial SST methodologies. Later in Bíró's book (135), he calls the popular 1998 book by Collins and Pinch "a masterpiece of philosophy of technology" (135). This may be an interesting and influential book, but I cannot agree it is a book about philosophy of technology rather than sociology of technology.

Processes, States and the Fluid Hydraulics Background

Bíró argues that Polanyi's "visualizations of economic realms mostly focused on processes rather than discrete states" (99). He seems to suggest that "processes" are dynamic entities, as opposed to discrete states. There is here a technical confusion between processes and states: a process can be continuous or discrete (in time), and both continuous or discrete processes can be modeled in transition mode (dynamic models) or stationary mode (sometimes also called equilibrium state).

Later (99) Bíró rejects the notion that Polanyi's model of economics is based on fluid dynamics. But, of course, Polanyi's film is a nice and early example of visualization of "hydraulic Keynesianism,"³⁶ following

up his previous efforts to create a model of macroeconomics in his lab devising an analog model using glass devices and connections with a fluid (water) flowing along the circuit.³⁷ Bíró's arguments about the incompatibility of fluid dynamics with some of the initial visualizations in the film (inspired by Neurath's isotypes) only shows the limitations that Polanyi found in this kind of visualization when applied to the dynamic of continuous processes. His "abstract" visualization³⁸ is itself very much inspired and similar to the traditional diagrams of the flow of fluids used in hydraulics and chemical engineering.³⁹

Finally, as a closing note, let me comment on the odd bibliography at the end of Bíró's book. The publisher may have required the rather strange organization here. Polanyi gets credit for writings that have been authored by others. The usual practices of referencing archive materials (mainly unpublished) versus published books, reports and papers are not followed. It requires work to make Bíró's rich bibliography useful.

ENDNOTES

¹Bíró considers Polanyi's economics traditional (154), although he also claims that Polanyi's economics mirrors the Keynesian account (111). If there is something less than orthodox in Polanyi's economics, I contend it is Polanyi's interest in social orders and polycentricity rather than his Keynesian views. See the discussion below.

²Regenstein Library, University of Chicago, Polanyi Collection (henceforth RPC), folder 31, item 14, Economic Lectures, 1948. Polanyi had serious health problems (exhaustion and insomnia) and "he attributed his health problems to the strain of lecturing in an area which he was not qualified" and this led the University to release him from his lecture course obligations in order to concentrate on the preparation of the Gifford Lectures (Scott and Moleski, 2005, 211-212).

³In an elaborate argument, Polanyi discusses suboptimal solutions of these equations and its implications: to produce a dynamic order as a relative optimum of "aggregate advantages" ("2nd lecture").

⁴See his "3rd lecture" in the initial version of the economic lectures, RPC (31,14).

⁵See the "7th lecture."

⁶For instance, see Ruccio and Amariglio (2003); Cullenberg, Amariglio and Ruccio, eds. (2001). See also Amariglio (1990).

⁷This was a hot topic of discussion after Polanyi presented his "Beyond Nihilism" at the 1960 conference in Berlin organized by the Congress of Cultural Freedom. See part I of Jelenski (ed.) (1962) and also Polanyi's postscript at the end of this book (185-196).

⁸*PK*, 265-266, 322. See also the unpublished version of "The Scientific Outlook: Its Sickness and Cure," a lecture delivered in Austin in 1958 after the release of *PK* (RPC [33,11]). Polanyi published a paper in *Science* (1957) with the same title before *PK* was published, but it has a different content.

⁹Jerry Gill's book *The Tacit Mode. Michael Polanyi's Postmodern Philosophy* (2000), suggests Polanyi is a "constructive" postmodernist. David Griffin, the editor of the SUNY series on "postmodernism," distinguishes "constructive" and "deconstructive" postmodern thought (*op. cit.*, ix-xxiii).

¹⁰Gill's book, not cited by Bíró, would be helpful if Bíró's objective is to offer a view of economics based on Polanyi's postmodern philosophy. See also Prosch, H., *Michael Polanyi. A Critical Exposition*, (1986), chapter 13 ("The Free Society") and Prosch's (2015) 1991 Kent State address recently reprinted as a TAD essay about Polanyian economics, free society and society of explorers, also not cited by Bíró.

¹¹"What to Believe" was recently published in 2020 in *TAD* 46:2 (21-28) accompanied by a discussion of the brief talk's general context by Phil Mullins as well as comments by Bíró and Marty Folsom. Page numbers of this Polanyi talk are cited internally.

¹²Bíró claims that the lecture "has remained quite unnoticed in the depths of his archival materials" (142), but this is not exactly the case. A two-page summary was published in 1947 and there is a bibliographic reference in the Polanyi bibliography included in Scott and Moleski (2005, 342, reference 1947). A copy is also included in Richard Gelwick's collected articles and

papers of Michael Polanyi, available on the Polanyi Society website (polanysisociety.org). There is also an alternative one page typed summary of the lecture with a different organization and text in RPC (31,10).

¹³There is no citation of *SFS* in the Bíró book, although his book includes a chapter entitled “Correspondence on the spirituality of science and economics” (chapter 5). The title of the chapter may be misleading: it may suggest economics is not a (social) science.

¹⁴“Beliefs” and related words appear 63 times on 26 different pages of *SFS*. “Tradition” (the usual Polanyian term for belonging) appears 65 times on 24 pages.

¹⁵Hayek, F., “Economics and knowledge,” *Economica* (New Series) vol. 4, 1937, 33-54.

¹⁶“I had suddenly the one enlightening idea which made me see the whole character of economic theory in what to me was an entirely new light”, *The Collected Works of F. A. Hayek*, Volume 9: *Contra Keynes and Cambridge* (University of Chicago Press, 1995), 62.

¹⁷Polanyi’s economics includes much more than his Keynesian book (*FEFT*). Bíró’s statement does not make much sense: how can economics not be limited to the understanding of the economy? Is he suggesting a metaphysical based economic science? Is such a thing possible? Is this a new definition of economics, including politics or religion, for instance?

¹⁸RPC (38,8), Contribution to “Midcentury Authors”, 20 July 1966. The manuscript has the comment handwritten by Polanyi: “Autobiography by M. Polanyi”. It was published almost a decade later: Polanyi (1975).

¹⁹Two other “aspects” of knowledge are discussed in *TD* (7 and 11) but with completely different meanings.

²⁰On 28 May 1945, Polanyi hand wrote a note in an unfinished essay (RPC [30,2]). “I am abandoning this work today in order to turn to the preparation of the Riddell Lectures of 1946.” And later he would write that 1946 was the year when he had discovered his true vocation: “the pursuit of a new philosophy to meet the need of our age” (Polanyi [1975], 1152). In a 1946 letter to Mausi, Polanyi wrote that “the crisis of my life is over” and he suggested that his task was now clear (Scott and Moleski, 200).

²¹RPC (31,11) Dynamic Order, 23 April 1948.

²²Bíró seems to suggest this in the last chapter of the book.

²³RPC (26,5) Notes for talk with prof. Jewkes, Nov. 1940.

²⁴See “On Popular Education in Economics” (1937) and also “Visual Presentation of Social Matters” (1936), published by *TAD*: Polanyi (2014) and Polanyi (2016).

²⁵In his preface to *FEFT*, Polanyi acknowledges T. Ashton, J. Jewkes, J. Hicks (future Nobel prize, 1972), S. Dennison and Lionel Robbins for their help and advice (vi).

²⁶See P. Salmon, *The Liberal Party and the Economy, 1929-1964* (Oxford University Press, 2015), 144, 145.

²⁷Polanyi’s contribution to discussion of the Harris report is available in RPC (28,3), in his comments on the “draft memorandum on a planned economy,” 22 January 1943.

²⁸Scott and Moleski, 187

²⁹For instance, the controversial policy issues about monopolies and patents, or the inheritance laws, are themes that may surprise but are found in the Polanyi agenda and writings of the 40’s.

³⁰*FEFT*, 29. Polanyi’s neutrality principle states that policies for management of monetary circulation should “be carried out in a neutral form, i.e., in a way requiring no materially significant economic or social action to accompany it.”

³¹Her Nobel Lecture was titled “Beyond markets and states: polycentric governance of complex economic systems” (8th December, 2009).

³²For instance, in V. Ostrom, “Polycentricity (part1)”, chapter 2 in M. McGinnis, *Polycentricity and Local Public Economies. Readings from the workshop in Political Theory and Policy Analysis* (University of Michigan Press, 1999): “I shall draw upon the work of Michael Polanyi.” She cites the relevance of Polanyi’s concept of polycentricity for “understanding patterns of behavior in

market organizations and judicial decision making” (56). A good review is P. Aligica and V. Tarko, “Polycentricity: from Polanyi to Ostrom, and Beyond,” *Governance: an International Journal of Policy, Administration and Institutions* 25 (2012), 237-262.

³³H. Collins, “The TEA set: tacit knowledge and scientific networks,” *Social Studies of Science* 4:2 (1974), 165-185.

³⁴S. Tosoni and Trevor Pinch, *Entanglements. Conversations on the Human Traces of Science, Technology and Sound* (MIT Press, 2017), 9-10.

³⁵J. Ravetz, *Scientific Knowledge and Its Problems* (Harvard University Press, 1971).

³⁶The term was coined forty years later, in the seventies by Alan Coddington (1976). Coddington dates its origins in the 40’s and 50’s with the Hicks IS/LM diagram, but not with diagrams of money circulation (Beira, 2013). See also Morgan, M. and M. Boumans (1998).

³⁷See Scott and Moleski, 163-164. Polanyi’s dissatisfaction with the results of his analog models (including an electromechanical one) was a main driver and inspiration for the alternative “diagrammatic film” model.

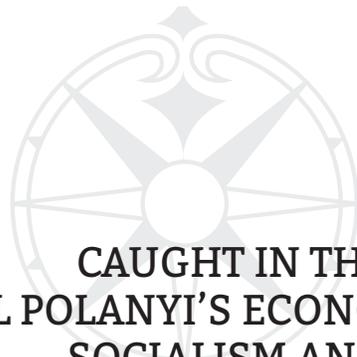
³⁸In the film, Polanyi spends around four minutes introducing progressively and very carefully the transition from the first pictorial scenes of the film (reel #1) to the abstract model of continuous flows in the monetary circulation (reel #2: from figures to symbols). For a transcription of the audio track of the film, see Beira (WP121b). Translations are available in Hungarian (Bíró, 2013), Spanish (Rota, 2018) and French (Beira, Fernandes and Festré, 2017).

³⁹In an unpublished note from 1941, Polanyi extensively uses an hydraulic system as an analogy to the flows of savings and investments: “take a bathtub with a partly open outlet. Open the tap wide. To begin with, more water flows in than out. The water rises. ... “ RPC (27,2) Money and unemployment, notebook no. 2, 1941. In *FEFT*, Polanyi extensively uses the idea of two pumps, one for “sucking” and another for “squirting” money from and to the money belt (monetary circulation). This terminology is from Polanyi himself, not from Keynes, and it is clearly the terminology of a chemist intimately familiar with hydraulics.

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- (*) available https://sites.google.com/site/ebeira/mp_wps



CAUGHT IN THE CROSSFIRE: MICHAEL POLANYI'S ECONOMIC THOUGHT BETWEEN SOCIALISM AND LIBERALISM¹



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Keywords: Thomas Balogh, William Beveridge, Michael Polanyi, John Maynard Keynes, Friedrich August von Hayek, “Unemployment and Money” (economics film), *Full Employment and Free Trade*, Beatrice and Sidney Webb

ABSTRACT

This response addresses some points raised by Eduardo Beira's review article found in this issue of TAD and suggests new directions for future studies focusing on the economic thought of Michael Polanyi.

Introduction

When one reads a 14-page review article on his book that does not include a single positive comment, one has the impression that the reviewer is not fond of the author, his work, or both. But one must chase away such an impression if one is to write a compelling response. Readers are much more interested in stimulating new discussions that might be helpful for promoting their own understanding and scholarly pursuits than in the ding-dong of rival commentators debating matters. So, instead of responding in detail to each point made by Eduardo Beira, I am going to respond only rather briefly to two crucial points in his review and then broaden the topic to explore new directions in the main part of this text. This strategy also avoids redundancy as some of the points raised by Beira were raised by others that I have previously responded to (Bíró 2020 and 2021 and elsewhere).

The first of Beira's points that I cannot leave unanswered is the generic labeling of my account as revisionist, misleading, partial, unwarranted, and perhaps by implication, non-Polanyian. I categorically reject this claim. The book is based on years of thorough archival research and uses more quotations from Polanyi's published and unpublished materials from the period in question than any before. I did not read anything into Polanyi that is not there. I let Polanyi speak for himself even though it came at the expense of smoothness and consistency. I did not cover up times when Polanyi was inconsistent, or used *topoi* that are uncommon in contemporary economic discourse. The primary aim was to show what Polanyi thought

and not to show what others (including me) think about what he thought. The quotations I used may seem underwhelming, peculiar, or simply unrelated, but I cannot understand how they might be seen as revisionist or unwarranted.

The second point from Beira's review that needs a brief response concerns the purported missing parts. Again, I need to emphasize that the book does not intend to be a comprehensive account of the economic thought of Michael Polanyi. It is primarily concerned with Polanyi's vision for his economics film: "democracy by Enlightenment through film" (Polanyi 1935a, 1), the reception of the two versions of his film and the 1945 textbook, *Full Employment and Free Trade* (hereafter *FEFT*) that he hoped would re-direct public attention to his film. Of course, Polanyi's concepts of spontaneous order and tacit knowing are very important, but they are not strictly related to his film project. Moreover, the fully-fledged versions of these concepts only emerged in the late 1940s, after Polanyi's economics film. A detailed discussion of these concepts is outside the scope of this book. What follows is now a few interesting topics and directions related to Polanyi's economic thought that will hopefully stimulate further studies.

Polanyi, Keynes' Hayekian Follower

Polanyi considered that his film portrayed Keynesian economics with a twist and explicitly stated that *Full Employment and Free Trade* (1945) is a piece of Keynes made easier. He exchanged a few letters with Keynes while he was trying to interest him in his film project, but unfortunately, the Bloomsbury businessman did not pay much attention to this self-appointed pupil, since he had "much else to do" (Keynes 1940, 1). Apparently, some thought that Polanyi was not portraying Keynesian economics, but instead the economics of Keynes' scholarly nemesis. Joan Robinson, a member of Keynes' inner circle, the Cambridge Circus, considered Polanyi to be a supporter of laissez-faire liberalism (Robinson 1944). Also, an anonymous reviewer of the *Church Times* described Polanyi as the "bouyant economist" (Unknown 1946) and Hayek as the "warning prophet" (ibid) of liberal capitalism implying that they were rowing in the same boat.

His fierce opposition to socialism perhaps made Polanyi seem even more Hayekian than Keynesian in the eye of the British reds. Maurice Dobb, a Marxist economist from the University of Cambridge, wrote a heated 1936 review of Polanyi's 1935 article, "U.S.S.R. Economics," several years before the 1938 premiere of the first version of the film and the publication of *FEFT*. Perhaps, this early incident set the tone for readers about what to expect in Polanyi's economic writings: proposals of anti-socialist, anti-authoritarian policies.²

Beveridge, Balogh, and the Boy Who Cried Socialism

The British public in the thirties and forties was not at all hostile to state intervention into the economy. Proposals suggesting that there was a need, perhaps a moral obligation, to defend people from the cruelties of economic life fit into a general trend of demanding a more humane economy. Several scholars, writers, and other intellectuals gave utterance to the hardship of the poor from the mid-nineteenth century forward. One might recall here how Charles Dickens portrayed the insensitivity and the hypocrisy of laissez-faire liberalism with the fictional figures of Thomas Gradgrind and Josiah Bounderby in *Hard Times* (1854). There, he more directly blamed laissez-faire liberalism than in 1843's *A Christmas Carol*, wherein he criticized instead the selfishness and ignorance of the rich by teaching Ebenezer Scrooge a lesson. The increasingly

romanticized *zeitgeist* of the Victorian era (1837-1901) led to the establishment of social movements and political parties in the first part of the twentieth century that spearheaded issues of the poor.

One of the key figures of this transformation might help us better understand the unfavourable reception of Polanyi's anti-socialist, anti-authoritarian economic policies. William Beveridge, in the government report entitled, *Social Insurance and Allied Services* (aka "the Beveridge Report") suggested wide-scale welfare reforms for a mid-war Britain. He proposed confronting the "five giants" of "Want...Disease, Ignorance, Squalor and Idleness" with the help of the government (Beveridge 1942, 8). Polanyi argued against the Beveridge Report in *FEFT* and elsewhere. For him, the kind of state intervention Beveridge proposed was too much and too vulnerable to partiality and corruption. Basically, the Beveridge Report established the welfare state in the UK and apparently Polanyi was against it.

It is worth noting that Beveridge was a researcher for Beatrice Webb, who authored *The Minority Report of the Poor Law Commission*. In that report, she sketched the principles of the welfare state that would "secure a national minimum of civilised life...open to all alike, of both sexes and all classes, by which we meant sufficient nourishment and training when young, a living wage when able-bodied, treatment when sick, and modest but secure livelihood when disabled or aged" (Beatrice Webb 1948, 481-2). Beatrice and her husband, Sidney were immensely wealthy and influential, thanks to Beatrice's family. The couple established institutions such as The London School of Economics and The Fabian Society, as well as the newspaper *New Statesman* and regularly wrote on social issues for a wide readership. They were staunch supporters of the Labour party from 1914 (Sidney even contributed to the Labour Party constitution). But then, disappointed with the first Labour government's political deals and moderate politics (1924, 1929-31, 1931-35), the Webbs began to idolize Soviet Russia. They then published *Soviet Communism: A New Civilisation?* (1935) and *The Truth About Soviet Russia* (1942) in which they celebrated the economic benefits of central planning and portrayed the Soviet way as the most desirable way towards social progress.

After their publication, Polanyi started to correspond with them. On February 25, 1937, he sent a copy of his critique of the Webbs' *Soviet Communism: A New Civilisation?* (1935) to Sidney Webb. In his response, Webb stated that the aim of their book was to show "a picture of the whole social organism" (Webb 1937, 1) to the British and American audience and that only time can tell which aspects of this picture were actually "inaccurate" or "defective" (ibid, 2). He commented how accounts of social issues necessarily are flawed in one way or another and are never "precisely accurate in detail" as an "architect's drawing" (ibid). In Webb's view, focusing on certain aspects, choosing the details, and arranging them were still part of telling the truth. He further explained that, despite what Polanyi's critique suggests, there is a truthful kind of propaganda and he and his wife had been writing this kind of propaganda for 50 years. Polanyi's response to the letter was brief, but he was apparently not convinced. Polanyi acknowledged that science cannot limit itself to "facts and facts only" because this would lead to "boreness" and to "the end of science" (Polanyi 1937, 1). He still thought it important to differentiate between "propaganda which disrupts civilization and truth which is its only hope of resurrection" and emphasized that the standards of truth cannot be relaxed even if one is captivated by a "vision of a more generous society" (Polanyi 1937, 1).³

The Webbs were not the only public intellectuals in Britain whose socialist or authoritarian leanings Polanyi considered to be threatening to democracy. Another was a fellow Hungarian, Thomas Balogh. Balogh became an influential economist in Britain in the 1940s. When Polanyi asked John Hicks to write a Preface to *FEFT*, Hicks refused to do this because he did not want to interfere publicly with the influential Balogh school (Hicks 1943). Hicks wrote to Polanyi that, in his view, Polanyi went against the Balogh

school in two important respects: he aimed for full employment and did not support “thoroughgoing exchange control” (ibid, 1). Polanyi’s anti-Baloghism did not remain unnoticed by Balogh himself, who published an unusually hostile review of *FEFT* in *The New Statesman and Nation*, a newspaper established by the Webbs that was still dominated by influential British socialists. In the 1930s, under the editorship of Kingsley Martin, the newspaper moved even more to the left. Keynes noted that Martin was “a little too full, perhaps, of good will” towards the Soviet Union and Stalin, and that the newspaper mirrored a stance that any doubts about Stalin’s Soviet Union had “been swallowed down if possible” (Beasley-Bullock 2013). When Orwell submitted his scribbles about the Spanish Civil War, Martin did not publish them because they struck a critical note against communism. When H. G. Wells did an interview with Stalin in the newspaper and made a few critical remarks, G. B. Shaw accused him of being disrespectful to the Soviet leader (see Beasley-Bullock 2013). *The New Statesman and Nation* defended Soviet economic collectivisation and gave space for those who wanted to popularize communism. Not surprisingly, Balogh’s review of Polanyi’s economics textbook was full of ideological statements favouring the Soviet Union.

Balogh described Polanyi as someone “engaged on a crusade for *laissez[ic]-faire* economics” and whose “prejudices prevent a logical development of his reasoning” (Balogh 1946, 252-253). Balogh also noted that Polanyi was not being “grateful for the heroic sacrifice of the Russian people” because he dared to criticize the Soviet economic performance (ibid, 252). This last statement shows the deep ideological entanglements of this piece.⁴ Polanyi was seen by left-wing progressivists as the boy who cried socialism, a fellow who worried too much (and too often) about the socialism of certain proposals. But how did he fit into the liberal mainstream?

The Lone Wolf Liberal and His Unexplored Ties to the *Methodenstreit*

While Polanyi was certainly part of the liberal team (as Beira notes), he did not completely conform to any of the mainstream liberal ideas. While he shared several points with Oscar Jaszi, he rejected the idea of a global liberal world democracy as imagined in *The City of Man* to which Jaszi contributed (Polanyi 1941). For Polanyi, neither a top-down transformation, nor an implication that people are exactly the same all round the world, was acceptable. Instead, Polanyi promoted a bottom-up transformation based on small communities of people of different traditions and practices. Polanyi considered nations important because they develop from and develop into traditions which are pivotal for our knowing and being. This does not mean that Polanyi was a nationalist. Instead it means that nations, as instances of traditions, were important for his liberal scheme. But Polanyi was not a radical relativist either. His liberalism did not affirm “anything goes” (Feyerabend 1975). It was about individuals being always already embedded in communities and trying to improve their knowing and being based on what they perceive as objective standards. For Polanyi, there are signposts embodied in traditions that demarcate patterns and a way forward. All “goings” are directed toward, but not controlled by, an objective ideal.

Polanyi participated in the most important liberal gatherings of his time. He attended the 1938 Paris Colloque Walter Lippmann (hereafter CWL) and was also a founding member of the Mont Pelerin Society (hereafter MPS) in 1947. However, he did not simply join forces with other liberal participants. Instead, he attempted to carve out his own way of reforming liberalism. That is why it is problematic to call Polanyi a “neoliberal,” unless we consider this an umbrella term. It is true that many liberals who attended the CWL and the MPS wanted to reform and revitalize liberalism, but they had very different ideas about what to do and how to do it. While a detailed comparison lies outside the scope of this brief response, the controversial

reception of Polanyi's economic ideas (too laissez-faire for Keynesian liberals and not laissez-faire enough for anti-Keynesian liberals) is perhaps enough to hint at the uniqueness of the road proposed by Polanyi.

Another interesting aspect of Polanyi's economic thought is his focus on the role of traditions. One cannot help but recall here the so-called *Methodenstreit* (Methods Dispute) between the Austrian and Historical Schools about the preferable method for use in economics. While theoreticians of the Austrian School studied human action as an individual phenomenon based on the universal operation of atomistic subjective factors, scholars of the Historical School studied human action as a specific and context-dependent social phenomenon. The Austrian School promoted using logical methods (deduction) on a carefully crafted set of statements in order to be able to arrive at novel insights of universal validity about human behavior. The Historical School promoted using empirical methods (statistics and historical records) to interpret a specific human behavior in the context of the cultural and social niche in which it is embedded.

Polanyi's liberal leanings and the central place of the individual in his thinking made his approach akin to that of Austrian economics. His film portrayed an abstract economy that consists of abstract agents who make decisions based on universal principles. Pumping more money into economic circulation is called following a "principle of neutrality" precisely because it is presumed to affect the whole economy and everyone involved in a uniform manner. On the other hand, Polanyi's emphasis on the pivotal role of traditions and communities resembles the central tenet of the Historical School that the meaning of the "idea of justice" is different at different times and places. Polanyi wrote that the economic machinery "can be operated in conformity to any standards of economic justice, provided that these are widely enough accepted by society as a whole" (Polanyi 1948, 146).

Are these two entanglements, one toward the Austrian School (universal principles of operation) and one toward the Historical School (fluidity of social standards) inconsistent? Not necessarily. One can imagine a theoretical approach that presumes the principles of human behavior to be universal and also presumes the social framework within which these principles operate to be contractual. However, defending a claim that Polanyi clearly took this approach would require a much longer analysis. It would be a fascinating scholarly project to analyze the minutia of Polanyi's economic thought by comparing it in detail to the ideas of both the Austrian and the Historical School. Perhaps such an inquiry would help to build some bridges, explicate many inconsistencies in Polanyi's economic ideas, or both. Either way, his economic thought is an intellectual treasure trove waiting to be explored.

ENDNOTES

¹This paper contributes to the research programme of the MTA Lendület Morals and Science Research Group. I am very grateful to Phil Mullins and Paul Lewis for their generous help in editing the paper. All remaining errors are mine.

²Agnès Festré (2017), Charles Lowney (2020), Martin Beddeleem (2017), and others are working on finding Polanyi's place in various aspects of the Keynes-Hayek debate, so interesting new studies are forthcoming related to this topic.

³This is a sober warning that might offer a useful lesson for those who tried to stay sensible in an increasingly radicalizing political atmosphere not only in the 1930s, but also in the 2020s.

⁴Every free thinker will likely be shocked in reading this outrageous instance of political loyalty overwriting facts and logic. One might ask if political servility has decreased since Balogh's day. Seeing the contemporary polarization of politics and the increasingly radical rejection and punishment of those who do not join either of the two choirs singing fancy, but oversimplified nonsense, one is tempted to claim that it has not decreased at all. The Polanyian approach favouring truth over propaganda is needed today as it was needed in the 1930s.

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