

Tradition: Why Shils and Polanyi Abandoned the Action Frame of Reference¹

Richard W. Moodey

Key words: Michael Polanyi, Edward Shils, Karl Mannheim, Talcott Parsons, Action Frame of Reference, tradition, religion, science, liberal democracy, ideology, primordial ties.

ABSTRACT

Michael Polanyi began thinking and writing about tradition long before he met Edward Shils in 1946. Polanyi's religious experience in 1913 became part of the background for his thinking about tradition, and tradition entered into his thinking about spontaneous order and moral inversion. Polanyi and Shils both knew Karl Mannheim before they met one another, and had similar criticisms of Mannheim's sociology. Soon after they met, both Polanyi and Shils were briefly enthusiastic about the Action Frame of Reference, which Shils helped create. Neither of them used the Action Frame of Reference in their later work. One of the reasons was its neglect of tradition, and another was that it was simply too complex to be a useful conceptual tool. Polanyi's thinking about tradition did not change much after Personal Knowledge, but Shils continued to modify his thinking about tradition until the final years of his life.

Introduction

Michael Polanyi emphasized tradition in science, but he also asserted the importance of tradition in other domains of activity. In the words of Struan Jacobs, Polanyi regarded a good tradition as a “fiduciary object, evoking a passionate, moral dedication.”² Polanyi's friend, Edward Shils, taught seminars on tradition for twenty-five years, beginning in 1956. In 1981, he brought together the results of those seminars in *Tradition*.³ But his thinking about tradition continued to develop. By including two previously unpublished essays on “collective self-consciousness” in the book containing Shils' sociological autobiography, Steven Grosby has made it possible for us to see how Shils' thinking about tradition continued to develop even in the final years of his life.⁴ In this essay, I argue that the centrality of the notion of tradition in the philosophies and social theories of both men led to their abandonment of the “Action Frame of Reference” (AFR). The AFR is a complex set of terms, definitions, propositions, and figures presented to the social science community in 1951. Shils himself was involved in its creation. He and Talcott Parsons were the co-editors of *Toward a General Theory of Action: Theoretical Foundations for the Social Sciences*,⁵ the book in which the AFR first appeared. Parsons and Shils co-authored “Values, Motives, and Systems of Action,” the second and longest of the book's four parts. But they had little to say about tradition. In *Tradition*, after criticizing Weber for his failure to pay adequate attention to tradition, Shils says that Parsons and he were trying to provide “a more systematic reformulation of Weber's ideas,” and admits that they missed the “opportunity offered for a clarification of traditionality” (TR 8).

Polanyi and Shils focused their attention on the AFR for just a few years in their long careers. Neither of them made the AFR the explicit frame of reference for later work, but for each, thinking about the AFR left residues that were elements within the tacit background for his later thinking. Shils was drawn into the process of articulating the AFR because of his great respect for Talcott Parsons; Polanyi was attracted to the

AFR because of his friendship with Shils.

In his autobiography, Shils says that although he was enthusiastic about the AFR for the “relatively brief period of about three years split equally each side of the middle of the century,” he came to realize that “Professor Parsons’ way was not a way that I could follow” (FSA 52, 86). For a brief time, Shils sympathized with Parsons’ hopes that the AFR would unite the Harvard Department of Social Relations, and then might even unite the social sciences more generally. But he admits that the AFR did not even unite the department Parsons had created, much less the fragmented activities and products we put into the broad category labeled “the social sciences” (FSA 84-86). The lack of an adequate consideration of tradition was not the only reason why Shils and Polanyi abandoned the AFR, but it was a very important reason. Each treated tradition both as an object of inquiry, and as a conceptual tool from which he attended to other objects of inquiry. For both, tradition was an important subsidiary from which they attributed meaning to different focal objects of attention.

Both Polanyi and Shils believed that there are bad traditions as well as good ones. They treated Fascism and Communism as prototypical bad traditions. Immediately after Grosby lists things about which Polanyi and Shils agreed—“the existence of truth and the committed stance that it assumes; the nature of scientific investigation; liberty, tradition, and spontaneous order; the significance of conviviality; and much more,” he adds that they “stood shoulder to shoulder in their opposition to all forms of totalitarianism.”⁶ The main forms of totalitarianism against which they fought were Fascism and Communism, and both of these forms of totalitarian government were based upon traditions Polanyi and Shils judged to be bad.⁷ In *Personal Knowledge*, Polanyi treated the tradition of idealizing detached and impersonal knowing as bad, and sought to articulate an alternative ideal of knowing (PK vii). In *Tradition*, Shils criticized the “antitraditional traditions” generated by the suspicion of tradition fostered by the thinkers of the Enlightenment (TR 235-239). He sought to articulate a sociology that would no longer be antitraditional.

There are five parts to the story I tell. The first is an account of Polanyi’s thinking about tradition from 1913 until he met Shils in 1946. Second comes a more brief account of Shils’ thinking about tradition from 1933 to 1946. The third part is about the Polanyi-Shils friendship from 1946 until both of them abandoned the AFR, around 1952 or 1953. In this part, I describe some aspects of the AFR and explain why Polanyi was attracted to it. In the fourth, very brief part, I focus on Polanyi’s treatment of tradition in *Personal Knowledge*. In part five, I write about developments in Shils’ thinking about tradition. Changes in his notion of tradition continued until his death in 1995. The two previously unpublished essays on collective self-consciousness that Grosby has included in the book containing Shils’ autobiographical reflections make it clear that Shils’ 1981 book on tradition is best seen now as a progress report on the continuing development of his thinking about tradition.

1. Tradition in Polanyi’s Social Theory Before 1946

In 1907, Polanyi’s older brother Karl was expelled from the University of Budapest in 1907 for defending Professor Gyula Pilker, who had been accused of being too sociological, too relativistic, and insufficiently Christian.⁸ In 1908, Karl and his classmate and friend Georg Polya founded the Galileo Circle, with Pilker serving as advisor.⁹ Michael, who was a medical student at the time, participated in this group, but “objected to the ever-increasing tendency on the part of his comrades to view scientific (especially social scientific) research as a weapon in the battle for social and political reform.”¹⁰ It appears that Polanyi was dedicated to the tradition of pure science even as a young medical student. William Scott and Martin Moleski say that, in spite of those disagreements, “Polanyi’s lifelong efforts at social and economic analysis may be

seen as inspired, at least in part, by his participation in the Galileo Circle.”¹¹ In 1913, he read *The Brothers Karamozov*, and he refers to that experience three decades later in a 1944 letter to Karl Mannheim. He wrote that before reading that book he had been “a materialist and an eager disciple of H.G. Wells,” but for ten years after reading Dostoyevsky’s novel, he was “continually striving for religious understanding.” From 1915 to 1920, he was “a completely converted Christian on the lines of Tolstoy’s confession of faith.” Afterwards, he says, his faith in specific Christian doctrines weakened, but adds, “My faith in God has never failed me entirely since 1913.” Polanyi wrote this letter to “clear up one or two personal points” that had arisen in conversations with Mannheim about a collection of Polanyi’s essays that Mannheim wanted to publish in the series called the *International Library of Sociology and Social Reconstruction* (MPSP 194).¹²

Mannheim was a member of “the Sunday Afternooners,” a group led by Georg Lukács, and in 1915, Polanyi began attending meetings of this group (MPSP 41). At the time, Polanyi was serving as a physician in the Austro-Hungarian Army, but because of illness, he spent a significant amount of time on light military duty in Budapest. This was also the beginning of the five-year period during which he said he was a committed Christian in the manner of Tolstoy. Lukács was not yet a Communist, but it is unlikely that he and Polanyi agreed about religion. After the rise to power in Hungary of Nicholas Horthy, Lukács became a Communist and in 1923 published *History and Class Consciousness*, “arguably the founding text of the Western Marxist tradition.”¹³ Polanyi’s history of criticizing aspects of the Marxist tradition began early in his life.

Polanyi does not use the word “tradition” in his 1944 letter to Mannheim, but I believe that the residues of his youthful religious experiences formed an important part of the background from which he attended to tradition in science and other domains of cultural activities.¹⁴ In *Tradition*, Shils emphasizes the spiritual dimension of Polanyi’s understanding of tradition in science. He says of Polanyi’s position: “The tradition of science is external to any particular scientists. It has the ‘exteriority’ of a social fact, to use a term of Durkheim.” Shils quotes *Science, Faith, and Society* to the effect that for practicing scientists, a scientific tradition “is a spiritual reality which stands over them and compels their allegiance.” He points out that Polanyi called the intergenerational sequence of transmitters and recipients of scientific tradition an “apostolic succession” (TR 116-117; quoting SFS 38,40). Shils’ use of “exteriority,” “social fact,” and “apostolic succession” point to aspects of what *he* meant by “tradition,” as well as to his interpretation of what Polanyi meant.

The notion of tradition is an important part of the background for two of the key ideas in Polanyi’s social theory. Walter Gulick argues that Polanyi was already moving towards a formulation of these two ideas in essays he wrote in 1917 and 1919, as a young man in Budapest.¹⁵ His notion of tradition was a subsidiary from which he attributed meaning to “spontaneous order” and “moral inversion,” whenever he used one of those terms to point to the object of his focal attention.¹⁶ Tradition enters into what Polanyi meant by “spontaneous order,” because, except in the case of economic order, the success of the spontaneous adjustment of actors to one another depends upon their being guided by their commitment to the traditional ideals of the domain within which they are acting. The spontaneous adjustments of scientists to one another, for example, can create order within the scientific community because of the scientists’ commitment to the traditional ideals of science. These ideals are not, and cannot be, scientifically proven. They are transmitted by master researchers to those who are apprenticed to them, who become committed to the ideals at the same time as they learn how follow their masters’ ways of conducting research.

The validity of Polanyi’s notion of “moral inversion” is controversial.¹⁷ My point here is only that Polanyi’s notion of tradition is an important element in the background from which he attributes meaning to

the term “moral inversion.” The morally inverted person rejects traditional moral values *because* they are “merely” traditional, and cannot be proven to be true by *her* version of critical reasoning. But she is not just a skeptic about traditional morality. She rejects traditional moral ideals with passionate feelings of moral self-righteousness. Ayn Rand’s morally self-righteous rejection of altruism provides a convenient example. Moral inversion interferes with spontaneous order in those domains of activity in which commitment to traditional ideals is a prerequisite to successful mutual adjustments of the actors. Moral inversion, however, does not interfere with spontaneous order in the economic domain. Morally inverted economic actors can feel morally righteous about putting self-interested profit-seeking above “unrealistic” moral ideals, and this does not prevent the laws of the market from serving as the invisible hand that orders the economic domain.

The title of Polanyi’s 1919 essay is “New Skepticism,” and the object of the youthful Polanyi’s skepticism was politics, a kind of activity he then regarded as being infected with the “fatal disorder” he would later call “moral inversion.” He wrote the essay in Hungarian, but the words of Endre Nagy’s English translation convey the intensity of his skepticism:

[M]an in politics does not advance toward goals but aims at illusions that seize his imagination . . . Politics is a blind eruption of fear and hope. . . People of reason and imagination must not get involved in politics, that is to say, they must not add to the fatal disorder with their illusions (SEP 30).

In 1919, Polanyi’s hope was not in politics, but in a sociological search for truth:

Our job is exploring the truth; dissecting the confused images of politics and analysing the belief in political concepts. . . For such a job a sociology is needed that acknowledges only those concepts which it can lead back without remainder to actually experienced mental states of human beings, a sociology in which, accordingly, generalizations like ‘development’, ‘class interests’, ‘ideologies’, could only be problems to be analyzed and not solutions (SEP 31).

As well as pointing to the “fatal disorder” of the politically motivated disregard for truth, Polanyi also denied that “development,” “class interests,” and “ideologies” could be useful tools for sociological analysis.¹⁸ His criticism of these words and the generalizations to which they point grew out of his arguments with fellow Hungarians who were inclined to accept the analyses of Karl Marx to a greater extent than he. For Polanyi, a passionate dedication to the search for truth is not just an ideal to be embraced by the scientist or the scholar, but is also the only remedy for the “fatal disorder” of politics. His desire to ground concepts in “actually experienced mental states of human beings” informed his ideal of personal knowing, an ideal that would be more fully articulated almost forty years later in *Personal Knowledge*. Richard Gelwick describes Polanyi as attempting “to catch knowing in action,”¹⁹ and I read this as another way of saying that he attempted to ground his concepts in the actual experiences of people who believe that their job is “exploring the truth.”²⁰ Polanyi’s skepticism about politics would be significantly reduced after he moved to England, where he developed respect for the British political tradition.

Polanyi had written “New Skepticism” during the brief period when Hungary was under Communist rule in the regime of Béla Kun. In 1920, Miklós Horthy came into power in Hungary, and established an anti-Communist regime that was also repressive. Polanyi and Mannheim were among the many scientists and intellectuals who fled from the repressive Horthy regime. Both settled in Germany, Polanyi in Berlin and

Mannheim in Heidelberg, where he studied with Max and Alfred Weber. They did well in their chosen fields. In 1929, Mannheim became professor of sociology at Johann Wolfgang Goethe University in Frankfurt.²¹ Polanyi's satisfaction at his success as a chemist was enhanced by his experiences of convivial relations within a community of scientists. Mary Jo Nye says that this experience of an intellectual community was an important part of what "he later transformed into an idealized vision of the scientific research community" (MPHG 83). An important aspect of this vision of the scientific community is the order that is generated by the mutual adjustment of scientists committed to the scientific tradition. At the same time, as Scott and Moleski note, the "vicissitudes of the German economy" kept his interest in economics active (MPSP 100).

Polanyi visited the Soviet Union in 1928, 1931, and 1932, and these visits helped him to clarify his notion of corporate order as the anti-ideal type for both the organization of science and the organization of an economy. He criticized the Soviet attempts to establish centralized planning of scientific research and economic life (MPSP 160). These visits, however, were not the only basis for his criticisms of Soviet society. While still in Budapest, he had already disagreed with members of the discussion groups who were sympathetic to communism.²² His religious rejection of materialism had begun in 1913, and he had written of his skepticism about politics. He went to the Soviet Union with an existing antipathy to the political control of both scientific and economic activities. He recognized that governments were needed both to facilitate and to regulate scientific and economic activities, but believed that the scope of such regulation should be limited. In 1930, Polanyi organized a series of meetings between natural scientists and economists. These took place at Harnack House at the Kaiser Wilhelm Institute in Berlin. He hoped that the natural scientists would help the economists toward better descriptions and explanations of economic events and processes (MPSP 121-122). The Harnack House meetings illustrate a central theme in the development of Polanyi's social theory: the analogy between the ideal social organization of science and the ideal social organization of economic life. In 1933, both Polanyi and Mannheim fled again from an oppressive government, this time from Nazi Germany to England. They again settled in different places, and did not become reacquainted until 1944.²³

A letter from Polanyi to Hugh O'Neil, written in June of 1935, indicates that he was still trying to think about economic things within the context of society as a whole. He wrote:

My faith in the moral power of Humanity leads me to assume that if they could be led out of blindness, I mean literal blindness: inability to see their vital surroundings, this moral power would rise to the situation it now must fail to grasp... To find, present and develop truth in social matters is the first revelation we require, a revelation which can be gained by a technique of seeing society and cannot be found without it. This is my obsession (MPSP 162).

I interpret this in the light of my belief that the residues of Polanyi's earlier religious experiences, as well as his faith in God, which might have been at times little more than a respectful appreciation of such faith, are important background elements in his thinking about social matters. Just as in his 1919 essay, in this letter Polanyi asserts his "obsession" with truth in social matters. His use of "revelation" has religious connotations, as does his later claim that the search for truth is "the calling of man" (SM 40-71), or as Gelwick has restated it, "The Calling of Being Human."²⁴ In the last paragraph of *Personal Knowledge* (PK 405), Polanyi says that being engaged in trying to make contact with reality is analogous to being a Christian engaged in the worship of God.²⁵ Even though Polanyi was neither a traditional Jew nor a traditional Christian, I am convinced that his notion of a good tradition depended upon the analogy with religious traditions.

J.D. Bernal's *The Social Function of Science* was published in 1939,²⁶ and Polanyi was highly critical both of the scientific planning that was a theme of the book and of Bernal's expressed sympathy for Soviet Communism. Bernal's book and its popular reception convinced him that he had a duty to work out the basis for a free society. In 1941, he collaborated with John Baker and Sir Arthur Tansley in creating the Society for Freedom in Science. The expressed purpose of this society was to counter the popular movement in Britain, of which Bernal was a leader, in favor of scientific planning. Polanyi said that his first meeting at Oxford with Baker and Tansley was influential in his decision to engage in the philosophical investigation of the foundations of the liberal democracy. Scott and Moleski report, "By the spring of 1943, Polanyi had written fifty pieces in defense of the liberal tradition: five of them published, fifteen given as lectures, five incomplete manuscripts for books, and twenty-five fragments and short essays" (MPSP 176-190).

One of these fifty pieces was the 1941 essay, "The Growth of Thought in Society."²⁷ Struan Jacobs²⁸ and Phil Mullins²⁹ treat this essay as a key document in the development of Polanyi's social thought. In it, Polanyi gives a detailed articulation of his two types of social order, spontaneous and corporate. Stephen Turner, however, cautions against attributing too much originality to Polanyi's notion of spontaneous order in science. He says, "the idea of science as a paradigm of spontaneous co-ordination ... was a commonplace of late-nineteenth century continental liberalism that in turn derived from earlier theories of the marketplace."³⁰ Nevertheless, for Polanyi the contrast between spontaneous and "planned" corporate order was central to his sociology of economic life, his sociology of science, and his contrast between "individual liberty" and "public liberty."³¹ Both Mullins and Jacobs point to the difference between the economic domain and other domains. Spontaneous order in the economic domain does not depend upon the actors' commitment to traditional ideals.³²

Polanyi's use of an organic metaphor in writing about the "growth" of science is suggestive in connection with his notion of a tradition. Mullins argues that early in his philosophical career, Polanyi characterized science as a "growing organism of ideas." He liked the Gestalt psychologist Wolfgang Kohler's treatment of embryonic development as a spontaneous order, drawing an analogy between this kind of organic growth and the growth of science and growth in other cultural domains.³³ I connect Polanyi's use of this organic metaphor to what Shils says about Polanyi's emphasis upon the "exteriority" of scientific tradition, "a spiritual reality which stands over" the individual scientists "and compels their allegiance." I interpret Shils as interpreting Polanyi as believing that the spiritual reality of the scientific tradition is something like a non-material "organism" that somehow "stands over" the individual scientists, and grows of its own accord. In his two posthumously published essays on collective self-consciousness, Shils returned, not to his interpretation of Polanyi, but to a struggle with the issues associated with his own conviction that traditions in general are somehow exterior to individuals and stand over them.³⁴

In 1942, Polanyi addressed the Manchester Branch of the Trades Advisory Council of the Board of Deputies of British Jews. "Jewish Problems" was published in 1943,³⁵ and provides an illustration of Polanyi's treating a tradition as bad. It is, however, not the tradition of anti-Semitism that comes in for his harshest criticism, but the Jewish tradition itself. He says that the achievements of Spinoza, Heine, Mendelssohn-Bartholdy, Ricardo, Marx, and Disraeli "prove the presence of genius among Jews."

But since all these were baptized Jews, their achievements may also indicate that Jewish talent is most effectively released by baptism. Or at least that Jewish triumphs are bound up with the neglect of distinctive Jewish traditions (SEP 37).

In this essay, he never quite gets around to calling anti-Semitism a “tradition.” He treats it, rather, as the result of a failure of democracy.

Assimilation was sponsored by the modern idea of equal national citizenship. It flourished during the forward march of the new democratic nationalism in the latter half of the 19th century. But as opposition to democracy gathered force, there arose—first in Germany during the economic crisis of the seventies—a new political form of Jew-baiting which became known by the scholarly name of Anti-Semitism (SEP 39).

The name might have been new, but the tradition was not. Polanyi was well aware of the long history of prejudice and discrimination directed toward Jews, but seems to have been reluctant to call it a “tradition.”

Another 1943 essay, “The English and the Continent,” provides evidence for the way in which Polanyi’s appreciation of religion served as background or context for this notion of traditions. In this essay he argued that the difference between politics in England—and probably the United States—and politics on the European continent “is connected with the fact that in England social progress was not on the whole associated with enlightenment and anti-clericalism, but was, on the contrary, very often prompted by religious sentiment.”³⁶ Gulick says that although Michael disagreed with Karl Polanyi’s idealization of Russian Communism, he seems to have agreed with Karl’s idealization of the beliefs and values that generated English governance.³⁷

“Jewish Problems” was one of the essays Mannheim hoped to publish in a collection of Polanyi’s essays.³⁸ In 1944, Polanyi and Mannheim became reacquainted, both by talking and corresponding about this proposed book, and by participating together in Joseph Oldham’s group called “the Moot.” Mannheim was a very active member of the Moot, and he persuaded Oldham to invite Polanyi. At the very first meeting he attended, Polanyi engaged in what one participant called a “ding-dong battle” with Mannheim. He said that Mannheim was “taken by surprise at Polanyi’s demonstration of the intuitive and traditional element of all vital scientific discovery.”³⁹ Mullins and Jacobs describe the relations between Mannheim and Polanyi as basically friendly, even though they disagreed about some things. In contrast, Jefferson Pooley implies that their relations were unfriendly. He includes Polanyi with Friedrich Hayek and Karl Popper as the three central Europeans whose criticisms of Mannheim were important in transforming Shils from being a follower of Mannheim to being a harsh critic.⁴⁰ I don’t doubt that Polanyi’s criticisms contributed to changing Shils’ views, but I do doubt that Polanyi was personally hostile to Mannheim. Pooley says that the crucial years for Shils’ turn against Mannheim were 1942 to 1944, but Shils did not meet Polanyi until 1946.

In this account of Polanyi’s early thinking about tradition, I have contrasted what he said about the economic domain with what he said about other social domains. Polanyi wrote a good bit about economics, especially in the decade between 1935 and 1945. His first publication on economics was a short booklet entitled *USSR Economics—Fundamental Data, System and Spirit* that appeared in 1935 (MPSP 160). His most significant economic work was published in 1945, *Full Employment and Fair Trade*. Paul Roberts and Norman Van Cott say that by integrating Keynesian economics with the economics of the monetary school, Polanyi made a contribution to economic theory that was way ahead of its time.⁴¹ But Polanyi does not say much explicitly about tradition in his economic writings. Capitalism, socialism, and various schools of thought in economics all are traditions, and Polanyi’s notion of tradition is part of the background for his thinking and writing about these. But tradition remains in the deep background in his economic writings.

2. Tradition in Shils' Theory Before 1946

The year 1933, the year Polanyi and Mannheim fled from Germany to England, was the year that the young Edward Shils moved from his undergraduate work at the University of Pennsylvania to Chicago, where he became a graduate assistant to sociologist Louis Wirth. He soon began translating some of Mannheim's German writings into English. Shils admits to having been "exhilarated" by Mannheim's *Ideologie und Utopia*, but in his autobiography he says that he later "came to be at a loss to account for that enchantment" (FSA 33). *Ideology and Utopia: An Introduction to the Sociology of Knowledge* was published in 1936.⁴² Wirth and Shils are listed as the translators, but it is likely that Shils did most of the actual translation. Shils was the sole translator of Mannheim's *Mensch und Gesellschaft im Zeitalter des Umbaus; Man and Society in an Age of Reconstruction* was published in 1940.⁴³

In 1936, Shils met Talcott Parsons when Parsons was teaching a summer course at the University of Chicago. Parsons asked Shils to read the manuscript of *The Structure of Social Action*.⁴⁴ Shils says that he "stood in awe" of what Parsons had accomplished in that book (FSA 52, 81). In it, Parsons argued that the positivist and idealist traditions in social science were converging toward a unified frame of reference he called "analytical realism." Shils says that Frank Knight, Robert Park, and Parsons were the only men "who moved in the direction in which I began to move very vaguely in the 1930s" (FSA 81). In his sociological autobiography, written many years later and published posthumously, Shils says that his best work in the 1930s "was hidden in Parsons's essay on social stratification."⁴⁵ The editors of the *American Journal of Sociology* (published at the University of Chicago) had asked Shils to be a pre-publication referee for this essay. Shils' comments ran to about thirty pages, and Shils claims that Parsons modified his article substantially on the basis of those comments. Parsons knew that Shils had written the comments because he recognized the handwriting (FSA 53-4).

When Shils went to London in 1942, he was still enthusiastic about Mannheim's sociology. I think that it is possible that in 1942, he might have said that his translations of Mannheim, rather than his comments on Parsons' article on social stratification, had been his best work in the 1930s. Soon after he arrived in London he visited Mannheim, and for the next two years was a frequent dinner guest of Mannheim and his wife, Julia. During those two years, however, Shils was changing from being a supporter of Mannheim's ideas to being a harsh critic of them.⁴⁶ In his autobiography, he says that in his visits with Mannheim, they did not discuss the sociology of knowledge, but focused more on Mannheim's troubles at the London School of Economics (LSE). He says that Mannheim was always kind and generous towards him and showed him great respect, in spite of his comparative youth, his lack of significant accomplishments in sociology, and his more frequent disagreements with some of Mannheim's ideas (FSA 35-36). At the LSE, Mannheim was very popular with students, but several of his colleagues sought to force him out. F.A. Hayek and Karl Popper (who would later join the faculty) were very critical of Mannheim's ideas in their writings. Morris Ginsberg, the professor of sociology at the LSE, was hostile to him. Mannheim was an obstacle to Ginsberg's efforts to move British sociology away from prescriptions for scientific and economic planning. Mannheim left the LSE in 1945 for a professorship in the sociology of education at the Institute for Education. In 1946, Shils was appointed lecturer in sociology at the LSE, and went from a full-time to a half-time position at the University of Chicago. In 1947, Mannheim died.⁴⁷ Shils reports that when he visited Julia soon after Karl's death, she embraced him and said "Ginsberg killed him!"⁴⁸ In 1948, Shils showed Julia the draft of an article he had written about Mannheim, and she called it "blind violence to somebody to whom you owe so much." She later wrote to her sister that she had been successful in preventing Shils from publishing "an extremely nasty paper on Karl."⁴⁹

When Shils went to London in 1942, he was an employee of the U.S. Office of Strategic Services. In 1943, Morris Janowitz was drafted and then assigned to the London office of the OSS. Janowitz was soon sent to France, where he supervised the interrogation of German prisoners of war. After the war, Janowitz became a graduate student at the University of Chicago, and in 1948, Shils and Janowitz published “Solidarity and Disintegration in the Wehrmacht in World War II.”⁵⁰ This essay is widely considered to be a landmark in the study of the dynamics of small groups and in the study of propaganda. In his autobiography, Shils spends three pages discussing this article, but never mentions Janowitz (FSA 63-65). He says that he returned to the University of Chicago in the 1945-1946 academic year. “It was then,” he says, “that I wrote the paper on the *Wehrmacht*” (FSA 63). He does write about Janowitz later in the autobiography, where he says that Janowitz had been his “protégé,” but says of him: “his mind was distorted by jealousy and rivalry and he had nothing fine in his make up, moral or intellectual” (FSA 155). This is Shils’ harshest pronouncement about any of the sociologists at the University of Chicago, but of them all he says, “I had no sense of intellectual or personal affinity with the members of the department” (FSA 155).

Although Shils and Janowitz do not use the word “tradition” in their essay on the *Wehrmacht*, they made a distinction between two bases for solidarity that is similar to a distinction a number of thinkers in England, including Polanyi and Shils, were making in the middle years of the twentieth century. Shils and Janowitz argued that ideology contributed much less to the solidarity of German soldiers than did primary group ties. A set of people Turner describes as being committed to articulating the foundations of liberal democracy argued that ideology contributed little to these foundations. Turner says:

[T]hey rejected the ideological cast of mind, and sought to identify and defend something valuable at the basis of liberal democracy that could not be understood in the Marxian way as an ideology. They did not so much find this thing—which they most frequently called tradition—as find arguments for its ineffability, its irreducibility to explicit doctrines or creeds; for the inadequacy of such notions as norms and values, or for that matter, principles, as a means of characterizing it, and for the peculiar qualities of tacitness and commitment that it possessed.⁵¹

Primary group ties are not, of course, identical to traditions, but the notions are similar in being contrasted with ideology. Primary group ties, what Shils liked to call “primordial ties,” are also important for the transmission of the content of traditions, especially in the master-apprentice relationship. The strong emotions associated with primordial ties make what people learn in them especially significant.⁵²

In 1943, Parsons gave some lectures at the London School of Economics, and Shils arranged a party for him and some of Shils’ friends from the Tavistock Institute. Shils says that Parsons later asked him to collaborate on the project that eventually became TGTA (FSA 81). Shils does not specify just how much later that invitation was, but it was not until the fall of 1949 that he spent a semester at Harvard, working with Parsons and others on developing the AFR. My guess is that Shils did not do much on the project until 1948 or 1949, because he says that he did not follow Parsons’ manner of theorizing “except for that relatively brief period of about three years split equally each side of the middle of the century” (FSA 86).

3. Friendship and the AFR: 1946 to 1953

Louis Swartz,⁵³ Grosby,⁵⁴ Turner,⁵⁵ and Shils himself⁵⁶ comment on the importance to both

Polanyi and Shils of their friendship. In 1946, before Shils left Chicago to take up his duties as a half-time lecturer at the London School of Economics, the nuclear scientist Leo Szilard gave him either Polanyi's phone number or his address.⁵⁷ Shils remembered telephoning Polanyi from a call box in London, but he could not recall just where he first met Polanyi face-to-face. He says that Polanyi was not interested in talking about the international control of atomic weapons, secrecy, Karl Popper, or Karl Mannheim (FSA 78-79).⁵⁸ Polanyi was then in his fifties and Shils in his thirties. Shils respected Polanyi for his scientific accomplishments, and Polanyi respected Shils for his knowledge of social theory. Their early correspondence indicates that each quickly decided that it would be good for him to collaborate with the other.

Before they met personally, Polanyi and Shils were both members of the loose network of people Turner describes as carrying on conversations about liberal democracy and something they usually called "tradition." Turner says that this "was not a group so much as a current of thought within which were a large number of personal relationships." He puts Polanyi close to the center of this network, along with Michael Oakeshott and T.S. Eliot. Somewhat further from the center were Friedrich Hayek, Karl Popper, Isaiah Berlin, J.P. Mayer, Christopher Dawson, and Karl Mannheim.⁵⁹ Mannheim's membership seems to have been especially marginal. Turner says that several of the other members "used Mannheim as a foil for their own arguments." Pooley goes much further, emphasizing the intensity of criticisms of Mannheim by Hayek, Popper, and Polanyi, and how these criticisms caused Shils to turn against Mannheim.⁶⁰

Polanyi invited Shils to lecture at the University of Manchester in the winter of 1946 and 1947, and Shils spoke about the tendency of literary figures to dislike their own societies. At that time, Polanyi courteously disagreed with him, but Shils says that within a few years "the intellectuals' rebellion of moral indignation, of excessive claims and exaggerated hopes, became a major theme of Polanyi's view of the world." After this, when Polanyi disagreed with him, Shils would often say, "Don't be too strong in your disagreement, Michael. In two years, you will say exactly what I am saying having forgotten that you first heard it from me" (FSA 109-110).

Shils says that he and Polanyi were riding in a London taxi in 1947 when he told Polanyi that Mannheim had died. After noting that Polanyi and Mannheim had known one another as young men in Budapest, Shils says of Polanyi:

He was a liberal and a severe critic of social planning. He had very little sympathy with Mannheim—to the point of indifference—while Mannheim was very sensitive to "Michi's" lack of sympathy. When I said to Polanyi, as we were nearing the station, that Karl Mannheim had died, Polanyi made no reply at all. He was probably thinking one of his philosophical conundrums and there was no clear category in his mind for Mannheim.⁶¹

I consider Shils' conjecture as to what Polanyi's was "probably thinking" to be probably mistaken. Polanyi was indeed critical of some of Mannheim's ideas, but I cannot believe that his failure to reply to Shils' announcement was a sign of his personal indifference to Mannheim. I do not know why Polanyi did not reply, but Shils' explanation contradicts my beliefs about Polanyi's moral character. Even after his death, Mannheim remained important to both Polanyi and Shils. Nye says, "Mannheim's sociology of knowledge is the shark cruising beneath the waters of Polanyi's argument" (MPHG 280), and Pooley points to ways that Mannheim remained "a haunting presence" in Shils' thought.⁶²

In 1947, Polanyi was invited to give the Gifford Lectures, which he would eventually transform

into *Personal Knowledge*. In 1948, his position at the University of Manchester was officially changed from Professor of Chemistry to Professor of Social Studies (MPSP 204). In the fall of 1949, Polanyi was struggling to complete his Gifford Lectures, and Shils was on leave from the University of Chicago, spending the semester at Harvard working with Parsons, and other members of the Department of Social Relations, on creating the AFR. The three-year period during which Shils thought he might be able to follow Parsons' way of developing sociological theory was a time in which he was especially close to Polanyi, as well as to Parsons. He had been successful in getting the University of Chicago to offer Polanyi a faculty position. Polanyi accepted the position, and was to begin his duties in Chicago in 1951, but was not able to do so because of difficulties in getting a visa.

In the spring of 1950, Polanyi visited the University of Chicago to give a series of lectures that Shils had arranged, and it is likely that before he returned to England, Shils gave him a copy of the manuscript of "Values, Motives, and Systems of Action." This long essay, written by Parsons and Shils, would become Part Two of TGTA, and it is by far the longest of the four parts. It is an exposition of the terms and propositions Parsons and Shils believed to be at the heart of the AFR.

Shils gave Polanyi the manuscript of "Values, Motives, and Systems of Action," when he was in those years in which he thought he could follow Parsons' way. In "Backstage with the Parsons Circle: Dialogue and Dissent in the Formation of a Theory School,"⁶³ Lawrence Nichols reports on his examination of the records, preserved in the archives at Harvard, of the meetings that took place in the fall of 1949. He points out that there were two weekly seminars, a larger one held on Mondays, and a smaller one on Tuesdays. He calls the members of the Tuesday seminar the "inner circle." All but one of the members of this inner circle contributed essays to TGTA. But even within the inner circle, Nichols says, there were "loyal critics" and "dissenters," as well as "true believers." He considers Parsons, Shils, Edward Tolman, a psychologist from the University of California, and possibly Robert Bales, a Harvard sociologist, to be the true believers. Samuel Stouffer and anthropologists Clyde Kluckhohn and Richard Sheldon were the loyal critics.⁶⁴ They supported the AFR, but were critical of some of its content. The psychologists Henry Murray, Robert Sears, and Gordon Allport were the dissenters. Murray and Sears were reluctant to contribute essays to TGTA, but were finally persuaded to do so.⁶⁵ Shils calls the fall of 1949 as "a happy time for me." He describes his collaboration with Parsons:

We usually met at 9AM in his room in Emerson Hall where we sat at the corner of a rectangular table. We talked and talked, taking as our point of departure, the topic of the previous day's discussion...Then we went back to discussion, going forward from where we had left off before lunch. At about four o'clock, we wrote out the main conclusions of our discussion...After 5:30 we broke off, taking the written results home to ponder on in the evening...There were times when he telephoned at 7:00 AM to tell me delightedly of some good idea which had occurred to him since we had parted.

Once a week or once a fortnight, we conducted a seminar for the professors of the department of social relations (FSA 80-82).

In this account, written many years later, Shils captures some of the excitement he felt during his participation in a project he later came to believe had been an impossible dream.

It is not hard to explain Shils' initial enthusiasm for the AFR. He respected Parsons, both for his

accomplishments and for his commitment to the search for truth. He was Parsons' closest collaborator in articulating the AFR. He must have conveyed some of his enthusiasm to Polanyi, but Polanyi had difficulty with the manuscript Shils had given him. In a letter dated July 20, 1950, Polanyi asked Shils for some readings that would help him understand the manuscript he referred to as "Values, Motives, and Action Systems." Later letters to Shils indicate that Polanyi had overcome whatever difficulties he had been having. In a letter of March 19, 1952, he said that he had picked up the Parsons-Shils book, and that his colleague, anthropologist Max Gluckman wanted to establish the Parsons-Shils type of sociology at the University of Manchester. In a letter of April 30, 1952, he wrote, "I feel very much attracted by the Parsons-Shils outlook of which I believe I could make good use within my own at first sight somewhat incommensurable preoccupations."⁶⁶ Shils would have received this letter after the three years, "split equally each side of the middle of the century," during which he had tried to follow Parsons' way.⁶⁷

Polanyi felt attracted to the AFR because of some of its intrinsic properties. I will comment on four of these: (i) the AFR focused explicitly on frames of reference; (ii) it is realist, rather than positivist or idealist; (iii) the relations among its components summarize much of the accumulated wisdom of the social sciences; and (iv) it treats the economy as a subsystem within the larger social system.

(i) Frames of reference. As a "frame of reference," the AFR is an attempt to articulate the key elements in what Polanyi would later call the *from* side and the *to* side of the *from-to* structure of knowing and doing. Parsons and Shils did not refer to this as a *from-to* relation, but called it the relation between the orientations of "actor-subjects" and the structure of the "objects" to which they are oriented. Centrally important to this frame of reference is what they called the "pattern variable scheme." In their introduction to "Values, Motives, and Systems of Action," they say, "the scheme of five 'pattern variables' of value orientations will be developed as a tool for analysis" for the "interrelations" among "certain elements of the orientation of action and certain elements of the structure of the situation." They say that this scheme is "the most important thread of continuity running through Part II" (TGTA 47-48). It has five dimensions, with two alternatives on each dimension. Parsons and Shils claim that actor-subject's orientation is the result of five selections, in which the actor-subject has selected one alternative on each dimension (TGTA 48). The dimensions and their alternatives are:

1. Universalism or Particularism,
2. Ascription or Achievement,
3. Affectivity or Affective Neutrality,
4. Specificity or Diffuseness, and
5. Self-Orientation or Collectivity-Orientation

The text of "Values, Motives, and Systems of Action" is followed by fifteen figures that summarize the terms, propositions, and relations that had been presented discursively (TGTA 247-275). Thirteen of the figures are elaborations of the pattern variable scheme. Figures 10, 10a, 10b, 10c, and 10d display all thirty-two combinations of the alternatives on the five dimensions (TGTA 258-262). Each combination is a five-dimensional type. An example of one type is the combination: (1) universalism + (2) achievement + (3) affectivity + (4) specificity + (5) self-orientation. Changing the selection on any one of the five dimensions results in a different type of orientation.

The complexity of this typology is one of the main reasons for its failure. A typology consisting of

thirty-two types, each one of which has five dimensions, is too complex and unwieldy to be a useful tool for analysis. When it is combined with the other components of the structure of action, it becomes even more complex. Samuel Stouffer, one of the contributors to *Toward a General Theory of Action*,⁶⁸ brought this up in a meeting of the contributors on January 7, 1950:

[A]s soon as we get into elaborations with 15 or 20 variables interrelated to each other which means thousands of millions of possible combinations theoretically, then I don't think we're really getting very far ... then I wonder whether it serves any orienting function. ... when it comes to breaking it down, Talcott, and getting hundreds of subcells, all on a theoretical level with no empirical referent except illustrative things that you pull out of the hat, I'm not sure how useful that is. That's my bias. I think you have to stay closer to data. (Discussion Group 1, 1950a.)⁶⁹

In his autobiography, Shils says that even Parsons dropped the AFR as articulated in *Toward a General Theory of Action* in favor of his four-function schema:

His heart was in the “boxes,” the “four-fold tables”—an expository device developed by Samuel Stouffer for teaching elementary statistics but which Professor Parsons adopted as a basis of portraying the fundamental features of society (FSA 85).⁷⁰

Shils continued to respect Parsons, but calls his sociological theories “titanic failures” (FSA 86).⁷¹ The AFR did become an *object* of study for a generation of sociologists—and a few other social scientists, but it is far too unwieldy to be a useful conceptual tool.

(ii) The Realism of the AFR. The epistemological and ontological realism of the Parsons-Shils outlook also appealed to Polanyi. The followers of Polanyi disagree about many of the details of his philosophy, but they generally agree that he held (1) that the world exists independently of what we know or say about it, and (2) that we can truly know at least some aspects of that real world.⁷² This is similar to the “analytical realism” Parsons articulated in *The Structure of Social Action*, the book that so impressed the young Shils when he read it in manuscript form in the summer of 1936. Parsons and Shils say that the AFR is a further elaboration of the perspective that informed *The Structure of Social Action* (TGTA 53).

The anthropologist Richard Sheldon dissented from the epistemological realism of Parsons and Shils, who honored his dissent by making his essay, “Some Observations on Theory in the Social Sciences,” the second and final chapter to Part I of TGTA. The occasion for his anti-realistic essay was his objection to the denial that cultural systems are systems of action. The first chapter of Part I, for which all the contributors are listed as co-authors, contains the following sentences:

Apart from embodiment in the orientation systems of concrete actors, culture, though existing as a body of artifacts and as a system of symbols, is not in itself organized as a system of action. Therefore, culture as a system is on a different plane from personalities and social systems.

A footnote states: “Mr. Sheldon dissents from this view. His grounds are stated in Chapter II” (TGTA 7). In his chapter, Sheldon argues that sentences such as these are assertions that about the *natures* of personalities,

social systems, and cultural systems. He denies that scientists ought to make such assertions, because the “general principles that make up a body of science” are “the free creations of the human intellect.” They are “free” because they are unconstrained either by the nature of the phenomena or by the nature of the human mind (TGTA 31-32). He asserts that there is nothing about the phenomena to which the word “culture” refers that provides any basis for saying that culture “is not in itself organized as a system of action” (TGTA 41).

I make just two comments about Sheldon’s argument. The first is that it is self-referentially inconsistent. He asserts, as true, propositions about the general principles of science. But he himself is asserting a general principle of science. The content of what he asserts should deny him the right to assert anything about the nature of the general principles of science every bit as much as they deny the right of Parsons and Shils to assert anything about the nature of culture. The way he asserts *his* propositions indicates that he believes that they are exceptions to what he asserts about *all* the general principles of science. In this comment, I have used “retortion,” the argument from the self-referential inconsistency of an intellectual opponent. Polanyi often used this kind of argument.⁷³

My second comment is that Sheldon does not really come to grips with what Parsons and Shils say is the crucial distinction between social and non-social objects:

The social object, the alter, is seen by ego to have expectations which are complementary to ego’s own. The distinction between those objects which do and those objects which do not have expectations complementary to ego’s is fundamental to the theory of action (TGTA 65).

Sheldon’s objection to the denial that culture is a system of action is an objection to the denial that subject actors (ego) generally see artifacts or symbols as being the kind of objects that have expectations that are complementary to their own. But Sheldon has too much common sense to say explicitly that he usually sees artifacts and symbols as the kind of objects that have expectations complementary to his own. Nowhere in his essay do I find him engaging in the kind of personification of artifacts or symbols that would make them into participants in social interactions.

(iii) The AFR as a Summary of Social Science Wisdom. Although Polanyi read, conversed, and wrote about social things throughout his career, he was aware of his lack of formal education in social science. Shils, in contrast, had that kind of education, and had read broadly in the social sciences. He was also a link between Polanyi and the eminent social scientists who contributed to TGTA. These men brought their diverse educations and experiences to the task of articulating the AFR, and were explicit about trying to create a general theory that would “aid in the codification of our existing concrete knowledge” (TGTA 3). What Polanyi called “the Parsons-Shils outlook” involved defining and relating the main components of the AFR in a way that was informed by a substantial body of social scientific theory and research. Parsons and Shils describe these components in their introduction to “Values, Motives, and Systems of Action” (TGTA 53-60), and summarize their text in Figure 1.⁷⁴

Parsons and Shils underlined some of the terms in Figure 1 to show how the pattern variables are the “the most important thread of continuity.” Even though the words “pattern variable” do not occur in Figure 1, they are tacitly present as the scheme by which they show the identity of the different patterns present in the value aspects of roles and in the patterns present in cultural objects. The thirty-two types of possible patterns are thirty-two metaphorical “threads” that “stitch together” personalities, social systems, and cultural systems.

Understanding Figure 1 (p. 20) requires knowing how Parsons and Shils distinguish between the two kinds of non-social objects:

[C]ultural objects can be internalized and thereby transmitted from one actor to another, while only *possession* of claims to physical objects can be transmitted. This difference rests on the fact that the cultural object is a pattern which is reproducible in the action of another person while it leaves the original actor unaffected. Only in a figurative sense does an actor *have* patterns of value-orientation. In a strict sense he *is*, among other things, a system of such patterns (TGTA 66).

They say that a cultural object is a *pattern* that is present in both symbols and in the value aspects of roles. Because roles are within both personalities and social systems, the same thirty-two patterns are elements in personalities, social systems, and cultural systems. Parsons and Shils say that the box in the center of Figure 1 “shows how social systems and personalities interpenetrate one another whether they are subjects or objects” (TGTA 60). They “interpenetrate” one another because the value aspects of roles are patterns that are within both social systems and personalities.

I interpret what Parsons and Shils meant by their use of “pattern” in terms of my understanding of what Aristotle meant by the Greek words we translate as “form” (*eidōs* and *morph-*). Just as, for Aristotle, the same *form* can be in the mind of a sculptor, in the marble statue he has created, and in the mind of the viewer of the completed statue, similarly, in the AFR the same *pattern* can be in the symbols and/or artifacts of the cultural system and in the value aspects of the roles in personalities and social systems.

(iv) The Economy as a Subsystem. The AFR gave Polanyi a possible way of integrating his extensive work in economics with his general social theory. At the end of the “Introduction,” Chapter I of Part One, there is “A Note on the Place of Economic Theory and Political Theory in the General Theory of Action.” The authors acknowledge that economics and political science are “well-established social science disciplines.” Both the economy and the polity are subsystems within the social system, but the authors say that the AFR will have to become more elaborately differentiated for it to be able to integrate economic or political theory. What they say of economic theory, in what follows, they also say of political theory:

Economic theory, then, is the theory of a particular set of processes or of a subsystem within a class of highly differentiated social systems. This subsystem is of very great strategic significance in these societies. Economic theory has its conceptual foundations in the categories of action theory here set forth, but only becomes a distinctive subtheory of the general theory on a considerably more elaborate level of differentiation than that reached here (TGTA 28).

The reason I say that the AFR is only a “possible way” for Polanyi to integrate his work in economics with his general social theory is that economic theory could only become a subtheory of the general theory of action after the general theory would become considerably more elaborate than it was in 1951. By the time Parsons and Smelser published *Economy and Society* in 1956, the frame of reference that informed their work was the “four box” schema, rather than the AFR.⁷⁵ In *Personal Knowledge*, Polanyi treats “Co-operation for a joint material advantage” as “the predominant feature of society as an *economic system*.” He says that co-operation is one of the “four coefficients of societal organization,” along with “the sharing of convictions,” “the sharing of a fellowship,” and “the exercise of authority or coercion” (PK 212; italics omitted). But he does not refer to the AFR or to any of Parsons’ later four boxes analyses.

Fig. 1. COMPONENTS OF THE ACTION FRAME OF REFERENCE (TGTA 247)

THE SUBJECT	THE OBJECT
<p>1. An actor-subject: the actor whose orientation of action is being analyzed. (In an interaction situation, this actor is called “ego.”)</p> <p>The actor-subject is sometimes called simply the “actor” and is always an “action system.” Thus the actor-subject is either:</p> <ul style="list-style-type: none"> a. a personality b. a social system 	<p>2. Objects: those objects to which the actor-subject is oriented. These are (i) social objects and (ii) nonsocial objects.</p> <ul style="list-style-type: none"> i. Social objects are actors (i.e., action systems) but here they are objects rather than subjects in a given analysis. (In an interaction situation, these actors are called “alters.”) Social objects are: <ul style="list-style-type: none"> a. Personalities. b. Social systems.

Personalities and Social systems fit together in the following fashion whether they are subjects or objects:

	Personality A	Personality B	Personality C
Social System 1	Role 1-A* Motivational aspects <u>Value aspects</u>	Role 1-B Motivational aspects <u>Value aspects</u>	Role 1-C Motivational aspects <u>Value aspects</u>
Social System 2	Role 2-A Motivational aspects <u>Value aspects</u>	Role 2-B Motivational aspects <u>Value aspects</u>	Role 2-C Motivational aspects <u>Value aspects</u>
Social System 3	Role 3-A Motivational aspects <u>Value aspects</u>	Role 3-B Motivational aspects <u>Value aspects</u>	Role 3-C Motivational aspects <u>Value aspects</u>

	<ul style="list-style-type: none"> ii. Nonsocial objects may be: <ul style="list-style-type: none"> a. Physical objects b. <u>Cultural objects</u> (i.e., <u>symbols or symbol systems</u>).
--	--

Cultural Systems

Cultural systems are the common values, beliefs, and tastes of the actors (as either subjects or objects) interacting with symbol systems (as objects). Thus the underlined components above show the abstraction of cultural systems from the action frame of reference.

*Each of these roles is a subsystem of orientations. This subsystem can be analyzed with respect to either (i) the personality’s motives, of which the orientations are a function, or (ii) the values which the personality respects in the specific social system. Thus roles are divided into motivational aspects and value aspects.

4. Tradition in *Personal Knowledge*

In section 3, “Tradition,” of Chapter 4, “Skills,” Polanyi begins by saying that crafts must be passed on from master to apprentice, and treats the passing on of the art of scientific research as an example of the transmission of a craft. He then uses the craft of making violins as his next example, and says, “A society which wants to preserve a fund of personal knowledge must submit to tradition” (PK 53). His next example is that of precedents in Common Law, and his final example in this section is the “unspecifiable art” that embodies the “exercise of public liberties” (PK 54). This goes back to the central concern of the network of thinkers in mid-twentieth century England, a network that included both Polanyi and Shils. Tradition is a key concept in Polanyi’s social theory, and, as I have said before, it is a necessary component of spontaneous order in every social and cultural domain except for the spontaneous order created by the “invisible hand” of economic markets.

What Shils said about Polanyi’s notion of tradition is far too narrow. Polanyi emphasized the importance of tradition in every domain of social and cultural life, but Shils repeatedly implied that Polanyi was interested only in scientific traditions. In *Tradition*, the only reference to Polanyi is in connection with tradition in science, and the only work of Polanyi’s Shils cites is *Science, Faith, and Society* (TR 116-117). This is despite the fact that Shils read the pre-publication manuscript of *Personal Knowledge* (PK ix). In his presentation in 1991 at the Centennial Celebration of the Polanyi’s birth, Shils quotes *Science, Faith, and Society* frequently, but never once refers either to *Personal Knowledge* or to *The Study of Man*.⁷⁶ In his autobiography, Shils complains that although Polanyi “stressed the importance of tradition in the growth of scientific knowledge,” he “could not be drawn, at least by me, into any conversation about tradition in general” (FSA 99). I do not know why Shils says that he was unable talk with Polanyi about tradition outside of the domain of science. The comment is especially surprising because they were both involved in the conversations that took place in the network of people who stressed the importance something that they usually called “tradition” as the necessary foundation for liberal democracy.

5. Tradition in Shils’ Later Social Theory

When I first read Shils 1981 book, *Tradition*, it angered me, but my anger had nothing to with what he had written about Polanyi. I felt that Shils had failed to acknowledge the work of men and women with whom I had studied, and whose work on traditions I admired. Robert Redfield had died before I became a student at the University of Chicago in the 1960s, but I had written about him in my master’s thesis.⁷⁷ I studied with McKimm Marriott, and Milton Singer, both of whom used Redfield’s distinction between great traditions and little traditions in their anthropological studies of India. I also studied with the political scientists Lloyd and Suzanne Rudolph, who wrote *The Modernity of Tradition*, also about India.⁷⁸ They were all, as was Shils, on the faculty of the South Asian Studies program at the University of Chicago, and I was a graduate student in sociology in that program. I had taken Shils’ seminar on the Indian intellectual in the spring quarter before I left to do my dissertation research in India. During the course of that seminar, Shils and I often disagreed.⁷⁹ When I read *Tradition*, over fifteen years later, I judged Shils to have violated an important tradition in science and scholarship, the tradition of citing the relevant literature. I believed that it was especially “anti-traditional” for Shils to have failed to cite the work of his colleagues, not only at the University, but also within one of the programs with which he was affiliated. I could not believe that Shils, who is celebrated for his omnivorous reading, did not know about their work.

I have gotten over my anger. For a number of years I have made tradition an important theme in my teaching, and I regularly use Shils' notion of tradition. He writes:

Traditionality is compatible with almost any substantive content. All accomplished patterns of the human mind, all patterns of belief or modes of thinking, all achieved patterns of social relationships, all technical practices, and all physical artifacts or natural objects are susceptible to become objects in a process of transmission; each is capable of becoming a tradition (TR 16).

Of course, Shils had much more to say about tradition—three hundred and six more pages worth. But he never backed away from his treatment of traditions as “objects” that are transmitted from older to younger generations. Because of this, I always criticize his notion of tradition, and turn to Alisdair MacIntyre's notion of tradition as arguments that continue from generation to generation,⁸⁰ and A.N. Whitehead's notion of tradition as a temporally linked series of events.⁸¹ Some contents really are objects that are handed down from generation to generation, but the patterns of which Shils writes are “objects” only metaphorically. Arguments (MacIntyre) and events (Whitehead) are not literally objects, even though it is appropriate to say that an argument or an event can become an “object” of inquiry or of thought.

Polanyi and Shils both say that a tradition is a kind of “apostolic succession.” Whitehead's notion of tradition is especially useful for thinking and writing about “apostolic succession.” The Catholic Church teaches that apostolic succession began with Jesus' commissioning of the twelve apostles. Since then, the Church teaches that there has been an *unbroken* series of consecrations of bishops continuing to the present. These ritual ceremonies are sacramental events, although not listed as one of the traditional seven sacraments. The ritual events are the “actual occasions” upon which God confers supernatural powers to each newly consecrated bishop. One of those powers is the power to ordain priests (ordination is one of the traditional seven sacraments). The ordination of a priest is the actual occasion upon which God gives to the newly ordained priest the power to transform bread and wine into the body and blood of Christ (the sacrament of the Eucharist). The *contents* of the tradition of apostolic succession consist *both* of the series of humanly performed ritual acts of consecration of bishops, *and* of the supernatural powers God is said to bestow upon the new bishops. Apostolic succession was, for both Polanyi and Shils, an important central prototype for their thinking about tradition.⁸² This prototype enters into Shils' interpretation of Polanyi's notion of scientific traditions, their “exteriority” and their way of being, to individual scientists, “a spiritual reality which stands over them and compels their allegiance” (TR 116-117).⁸³

I am not saying that Polanyi and Shils personally believed in the Catholic doctrine of apostolic succession, but they did believe that at least some, if not many, Catholics really do believe that this doctrine is true.⁸⁴ In his essays on collective self-consciousness, Shils wrestled with the significance of what he believed about the beliefs of the participants in a tradition. In these essays, he both draws upon Durkheim's notion of “collective consciousness” and criticizes it. I regard this criticism as especially significant:

It was wrong of Durkheim to fail to see that human beings have really believed in the existence of deities who enunciate or exemplify commandments or norms. He was right in seeing that it is the power of society which gives rise to the attribution of sacredness to it. But he was wrong to be unwilling to accredit the belief among human beings that there are powers which transcend human existence, which are beyond society but which also enter into it, and into the rules which they lay down or which human minds derive from those

powers (i.e., from the powers located in the transcendental realm) (FSA 185-186).

This is just one of several passages in these essays in which Shils goes back and forth between his appreciation for Durkheim's notion of collective consciousness and his criticism of some of the things Durkheim asserts about it.

Shils recognizes that "collective self-consciousness" is "not a notion that is easy to state very exactly." He says that the notion of "group mind" was "laid to rest many years ago." But he also says, "it seems to me that it is indispensable" (FSA 171). "In collective self-consciousness," he asserts "the collectivity is both the knowing subject and the collectively known object" (FSA 173). He cannot refrain from saying that the collectivity is a knowing subject, even as he explains why it cannot really be one:

There is no physical locus in the collectivity, such as a brain or neural system is in the individual, in which such collective consciousness can reside. When I say that collective self-consciousness exists, I also emphasize that it exists in the brains and neural systems of the individuals who make up the collectivity. If this is so, then in what sense can we speak of the collectivity as a knowing subject knowing itself? The collective self-consciousness is an inter-individual structure of consciousness commonly aware of the membership of the collectivity (FSA 173).

The final sentence of that quotation is typical of Shils' attempts to hang on to some "indispensable" remnant of group mind even after he has stated why it is impossible. Even though he says that he "emphasizes" that collective self-consciousness "exists in the brains and neural systems of the individuals," he holds back from saying that it exists *only* in the brains and neural systems of individuals. For if he were to have said "only," he would not have been able to say, as he does two sentences later, that collective self-consciousness is an "inter-individual structure." Shils wanted to say both that it is in the brains of individuals, and that it is somehow "inter-individual," transcending individual brains.⁸⁵

Conclusion

In 1981, Shils' *Tradition* was a progress report on his thinking about the topic, and his two essays on collective self-consciousness are a progress report on his thinking about tradition over the last years of his life. If he were alive and mentally active today, I am sure that he would have still further things to say, things that would result from working out implications of things he and Polanyi had said before. Polanyi's religious awakening in 1913 remained, throughout his life, part of the background from which he thought and wrote about tradition. For Polanyi, and then later for Shils, religious commitment became the prototype for the commitment of a person who genuinely participates in a tradition. Shils comments on what he means by "participation" in ways that I believe support my argument that commitment to a *religious* tradition is the prototype for his thinking about commitment to any tradition (FSA 173). Polanyi appealed to St. Augustine's maxim "*nisi credideris non intelligitis*" (unless you believe, you will not understand) (PK 267). I interpret this in the strong sense of the necessity of believing in something *sacred*, even if the sacred object of that belief is not formally religious. He said that the alternative to the interconnected horrors of the "scientific Minotaur," "nihilism," and "moral inversion" must be "to restore to us once more the power for the deliberate holding of unproven beliefs" (PK 268).

For both Polanyi and Shils, it was the scientific tradition to which they personally attributed this sacred quality. I believe that a major contribution Polanyi made to Shils' thinking about the scientific tradition, and tradition more generally, was making commitment to a religious tradition the prototype for commitment to any tradition. This does not mean that Shils never thought about religion prior to meeting Polanyi in 1946. But an important basis for Shils' respect for Polanyi consisted in Polanyi's achievements as a chemist. I believe that their friendship consolidated Shils' understanding of, and affirmation of, commitment to a religious tradition as the prototype for commitment to any tradition.

Endnotes

¹This is a substantial revision of "Polanyi, Shils, and the Action Frame of Reference," a paper presented at "Connections/Disconnections: Polanyi and Contemporary Concerns and Domains of Inquiry," held at Loyola University, June 8-10, 2012. Thanks to Martin Moleski, S.J. for reading my paper in my absence and to Phil Mullins, Richard Schmitt, and Walt Gulick for helpful comments and suggestions. When I became a graduate student in sociology at the University of Chicago in 1964, I had been a disciple of Polanyi since 1959. As a student at Chicago, I had several conversations with Shils in his office and participated in his seminar on Indian intellectuals. But I never worked closely with him. He brought Polanyi to the University for a series of lectures, and scheduled a two-hour session for graduate students to meet with Polanyi. Only two of us showed up, and the other student did little more than listen as Polanyi and I discussed relations between his work and that of Jean Piaget and the Jesuit philosopher and theologian Bernard Lonergan. After I left Chicago in 1968, I had no further personal contacts with either Shils or Polanyi.

²Struan Jacobs, "Polanyi on Tradition in Liberal Modernity," in Struan Jacobs and Richard Allen (eds.), *Emotion, Reason, and Tradition: Essays in the Social, Political and Economic Thought of Michael Polanyi* (Burlington, VT: Ashgate, 2005), p. 77. Hereafter ERT.

³*Tradition* (Chicago: University of Chicago Press, 1981), p. vii. Hereafter TR

⁴Edward Shils, *A Fragment of a Sociological Autobiography*, edited by Steven Grosby (New Brunswick, NJ: Transaction, 2006), pp. 52, 86. The essays Grosby included are: "Society, Collective Self-Consciousness and Collective Self-Consciousness" and "Collective Self-Consciousness and Rational Choice." Hereafter I refer to the two essays as well as the autobiography simply as FSA.

⁵Cambridge, MA: Harvard University Press, 1951; New York; Harper and Row, 1951. Hereinafter TGTA.

⁶"Further Reflections," p. 13. Karl Polanyi was also an important influence on Michael's opposition to these bad traditions. See Lee Congdon, "Between Brothers: Karl and Michael Polanyi on Fascism and Communism" *TAD* 24, 2 (1997-1998): 7-13.

⁷To say that Fascism and Communism were based on ideologies rather than traditions is to use "ideology" as if it were a good theoretical term, and I will argue that both Polanyi and Shils denied that "ideology" is a good theoretical term.

⁸Mary Jo Nye, *Michael Polanyi and His Generation: Origins of the Social Construction of Science* (Chicago: University of Chicago Press, 2010), p. 7. Hereafter MPHG.

⁹In "Notes Toward Understanding the Hungarian Roots of Polanyi's Heuristic Philosophy of Religion," *TAD* 32, 3 (2005-2006): 29-30, Richard Gelwick discusses the relation between the thought of Polanyi and George Polya.

¹⁰Lee Congdon, "Polanyi and the Treason of the Intellectuals," *Canadian-American Review of Hungarian Studies* 2 (1975): 81-82, cited in (MPSP 22).

¹¹William Taussig Scott and Martin X. Moleski, *Michael Polanyi: Scientist and Philosopher* (New York: Oxford University Press, 2005). p 23. Hereafter MPSP.

¹²The book was never published. Mannheim died in 1947, and Polanyi had not completed the introductory essay that was supposed to tie the diverse essays together.

¹³Jefferson Pooley, "Edward Shils' Turn Against Karl Mannheim: The Central European Connection," *American Sociologist* 38 (2007): 369.

¹⁴I must confess that the residues of my youthful religious experiences are part of the background from which I attend to what Polanyi and Shils wrote about tradition, and from which I attribute meaning to the word "tradition." When I was twenty, I joined the Jesuits, and remained in the order until I was almost 30. I first read *Personal Knowledge* in 1959, when I was studying philosophy in a Jesuit seminary.

¹⁵"Michael and Karl Polanyi: Conflict and Convergence," *The Political Science Reviewer: An Annual Review of Scholarship* 37: 17-19, 25-26. Endre Nagy translated both "To the Peacemakers: Views on the Prerequisites of War and Peace in Europe" (1917) and "New Skepticism" (1919), and both essays are in Richard Allen (ed.), *Society, Economics, & Philosophy: Selected Papers, Michael Polanyi* (New Brunswick: Transaction, 1997), pp. 15-28. Hereafter SEP.

¹⁶By "attributed meaning" I mean something very similar to what I believe Polanyi meant by "sense-giving" in his "Sense-Giving and Sense-Reading" (KB 181-207).

¹⁷See, for example, Zdzislaw Najder's criticism of the notion in "'Moral Inversion' or Moral Revaluation?" in Thomas Langford and William Poteat (eds.) *Intellect and Hope* (Durham, NC: Duke University Press, 1968), pp. 364-385, and D.M. Yeager's rebuttal of Najder's arguments in "Confronting the Minotaur: Moral Inversion and Polanyi's Moral Philosophy," *TAD* 29, 1 (2002-2003): 22-48.

¹⁸Polanyi's rejection of "ideology" as a useful conceptual tool will persist. A similar rejection will be a unifying theme for the members of a network of thinkers in England in the middle years of the twentieth century. See Steven Turner, *Brains/Practices/Relativism: Social Theory after Cognitive Science* (Chicago: University of Chicago Press, 2002), p. 176. Chapter 9 of this book, "The Significance of Shils," is a revised version of a paper of the same name published in *Sociological Theory* 17 (1999): 125-145.

¹⁹Richard Gelwick, *The Way of Discovery: An Introduction to the Thought of Michael Polanyi* (New York: Oxford University Press, 1977), p. 135.

²⁰Polanyi's skepticism about politics was similar to that expressed by Harold Lasswell's in *Politics: Who Gets What, When, How* (McGraw-Hill: NY, 1936).

²¹Pooley, p. 369.

²²Gulick, "Michael and Karl Polanyi," pp. 16-17.

²³Phil Mullins and Struan Jacobs, "Michael Polanyi and Karl Mannheim," *TAD* 32, 1 (2005-2006): 29.

²⁴*Polanyiana* 5, 1 (1996): 63-75.

²⁵Mary Jo Nye calls attention to this in "A Response to Theodore L. Brown and Richard Henry Schmitt," *TAD* 38,2 (2011-2012): 33. One of the reasons I became a disciple of Polanyi upon my first reading of *Personal Knowledge* was that when, in 1953, I made a thirty-day Ignatian retreat as a Jesuit novice, the personal resolution I made was always to try to stay in close contact with reality. The last sentences of *Personal Knowledge*, as well as the reference to the Jesuit Teilhard de Chardin (PK 388), whose works I had read, had deep personal significance for me.

²⁶London: Routledge.

²⁷*Economica* 8 (November, 1941) 421-456. Hereafter GT.

²⁸"Michael Polanyi and Spontaneous Order, 1941 to 1951," *TAD* 24, 2 (1997-1998): 14-28; "Polanyi's Analysis of Social Orders" (ERT 100).

²⁹"The Growth of Thought in Society as a Major Motif in Polanyi's Philosophy," presented at "Connections/Disconnections: Polanyi and Contemporary Concerns and Domains of Inquiry," held at Loyola

University, June 8-10, 2012.

³⁰“Polanyi’s Political Theory of Science” (ERT 84-85).

³¹In *Beyond Liberalism* (New Brunswick, NJ: Transaction, 1998), Richard Allen discusses these two notions of liberty at some length. See also his essay, “Some Implications of the Political Aspects of *Personal Knowledge*,” *TAD* 39, 3 (2007-2008): 8-16.

³²Mullins, “Growth of Thought,” p. 4; Jacobs, “Polanyi’s Analysis,” pp. 104-5.

³³“Growth ... as a Major Motif,” pp. 2-3.

³⁴I refer here to the two essays Grosby appended to *A Fragment of a Sociological Autobiography*.

³⁵*The Political Quarterly* 19 (Jan.-Mar., 1943): 33-45.

³⁶*The Political Quarterly* 14 (1943): 372.

³⁷Personal communication. Gulick also reports that, after moving to England, Karl was dismayed at the degree to which English life was still dominated by class distinctions. See also “Michael and Karl Polanyi,” p. 16.

³⁸Phil Mullins and Struan Jacobs, “Michael Polanyi and Karl Mannheim,” *TAD* 32, 1 (2005-2006): 29-30.

³⁹Polanyi said of his participation in the Moot, “These things changed our lives” (MPSP 197).

⁴⁰“Edward Shils’ Turn Against Karl Mannheim.”

⁴¹“Polanyi’s Economics,” *TAD* 25, 3 (1998-99): 26.

⁴²International Library of Psychology, Philosophy and Scientific Method.

⁴³London: Routledge & Kegan Paul.

⁴⁴New York: McGraw-Hill, 1937.

⁴⁵Talcott Parsons, “Analytical Approach to the Theory of Social Stratification,” *American Journal of Sociology* 45 (1940): 841-862.

⁴⁶Pooley, pp. 371-374.

⁴⁷Pooley, pp. 366, 371-374.

⁴⁸Shils, “Karl Mannheim,” *American Scholar* 64 (1995): 233.

⁴⁹Pooley, p. 375, citing D. Kettler and V. Meja, *Karl Mannheim and the Crisis of Liberalism* (New Brunswick, NJ: Transaction, 1995), p. 246.

⁵⁰*Public Opinion Quarterly* 12 (1948): 280-315; republished in Edward Shils, *Center and Periphery* (Chicago: University of Chicago Press, 1975), pp. 345-383.

⁵¹*Brains/Practices/Relativism*, pp. 176-177.

⁵²See Richard Allen, “Polanyi’s Rehabilitation of Emotion” (ERT 41-54).

⁵³“Michael Polanyi and the Sociology of a Free Society.” *The American Sociologist* 29 (1998):59-70; “Reflections on Shils, Sacred and Civil Ties, and Universities” *TAD* 27, no. 1 (2000-2001), pp. 7-12.

⁵⁴“Further Reflections on Shils and Polanyi,” *TAD* 27, 1 (2000-2001): 13-15.

⁵⁵“Edward Shils 1910-1995.” *TAD* 22, 2 (1995-1996): 5-9.

⁵⁶“On the Tradition of Intellectuals: Authority and Antinomianism According to Michael Polanyi,” *TAD* 22, 2 (1995-1996):10-26. (Based on a presentation at the Centennial Celebration of the birth of Michael Polanyi, April 11-13, 1991, Kent State University.)

⁵⁷Szilard and Polanyi had known one another as young men in Budapest. Tibor Frank reports that in 1922, Polanyi had written a letter of recommendation for Szilard regarding his application for a position at the Institute of Physical Chemistry at the University of Frankfurt. “Cohorting, Networking, Bonding: Michael Polanyi in Exile,” *TAD* 28, 2 (2001-2002): 7.

⁵⁸Phil Mullins, in a personal communication, says that in other contexts Polanyi did discuss these topics. Perhaps he just did not want to discuss them with Shils.

⁵⁹*Brains/Practices/Relativism*, p. 176. In “From Salon to Institute: Convivial Spaces in the Intellectual Life of Michael Polanyi,” *TAD* 32, 3 (2005-2006): 19-22. Ruel Tyson argues that conversations within convivial groups or networks were integral to the development of Polanyi’s thought.

⁶⁰“Edward Shils’ Turn Against Karl Mannheim.”

⁶¹“Karl Mannheim,” *The American Scholar*, Spring 1995, 234.

⁶²Pooley, p. 372.

⁶³Paper presented at the annual meeting of the American Sociological Association, Montreal Convention Center, Montreal, Quebec, *Canada*, Aug 10, 2006. Available at http://citation.allacademic.com/meta/p_mla_apa_research_citation/1/0/2/9/9/pages102990/p102990-1.php

⁶⁴Because of Sheldon’s radical criticism of the realistic assumptions of the AFR, I would categorize him as a “dissenter” rather than as a “loyal critic.” See “The Realism of the AFR,” below.

⁶⁵Nichols, p. 4.

⁶⁶All three of these letters are in Box 4 of the Shils archive in the Regenstein Library at the University of Chicago. I am indebted to Phil Mullins for sharing his notes on these letters.

⁶⁷In his autobiography, written four decades later, Shils might have shortened the time after the turn of the century during which he still believed that he could follow Parsons’ way. He co-authored, along with Parsons and Bales, *Working Papers in the Theory of Action* (NY: Free Press), published in 1953.

⁶⁸His essay is “An Empirical Study of Technical Problems in Analysis of Role Obligations,” (TGTA 479-496).

⁶⁹Nichols, “Backstage,” p. 6.

⁷⁰Richard Schmitt has pointed out Stouffer was not the first to use two-by-two contingency tables in statistics. I believe Shils was suggesting that it was Stouffer who influenced Parsons to use the “four boxes” approach in his typological analyses. Because of my familiarity with Parsons’ division of the social system into four functionally defined subsystems, I initially thought it would be interesting and fruitful to dig into the possible influence of the AFR on the social theory Polanyi presents in *Personal Knowledge* and *The Study of Man*. I was struck by the similarity between Polanyi’s description of “four coefficients of societal organization” (PK 212) and Parsons’ four subsystems of the social system. I have since concluded that this comparison between Parsons and Polanyi is a dead end. There is not a single reference in *Personal Knowledge* or *The Study of Man* to anything written by Parsons (or Shils, for that matter). Polanyi acknowledges Shils’ reading of the pre-publication manuscript of *Personal Knowledge* (PK ix), but that is the only occurrence of Shils’ name in the book. If Polanyi had believed that either the AFR or the four-function schema had shaped his social theory, he would have acknowledged it. This does not mean, however, that his study of the AFR and his brief enthusiasm for it did not become part of the tacit background for things he said in his later writings.

⁷¹Turner, who often conversed with Shils after 1982, says that too many people think of Shils only as Parsons’ junior partner. “But,” he says, “Shils was never as narrow as this relationship might have suggested, and in any case he soon moved on with respect to the central concerns he shared with Parsons, and moved on in a direction that has proven to be more durable and significant than the failed effort of the early fifties to create a scientific theoretical sociology around a scheme of definitions.” “The Significance of Shils,” p. 170.

⁷²Andy Sanders, “Polanyians on Realism: an Introduction,” *TAD* 26, 3 (1999-2000): 7.

⁷³See Martin Moleski, “The Role of Retortion in the Cognitive Analyses of Loneragan and Polanyi,” in Bartlett, S.J. and Suber, P. (eds.), *Self-Reference: Reflections on Reflexivity* (Amsterdam: Martinus Nijhoff, 1987), pp. 218-38.

⁷⁴My Figure 1 includes all of the elements of Parsons and Shils’ Figure 1 (TGTA 247), but with slightly different formatting of the cells in the tables.

⁷⁵NY: Free Press, 1956. Parsons and Smelser discuss the relations between the four box schema and

the AFR in a “Technical Note” (pp. 33-38), but this is the only place in the book that the pattern variable scheme is mentioned.

⁷⁶“On the Tradition of Intellectuals.”

⁷⁷*A Heuristic Structure for the Study of Sociocultural Integration* (Chicago: Unpublished Master’s Thesis, Loyola University, 1961).

⁷⁸Chicago: University of Chicago Press, 1967.

⁷⁹In spite of our arguments, Shils spent time with me before I left for India on the design of a possible research project on the Jesuit colleges in India. As it turned out, I had no time to pursue the leads he had given me during my summer in India.

⁸⁰*Whose Justice? Which Rationality?* (Notre Dame: University of Notre Dame Press, 1988), p. 12. See John Flett, “Alisdair MacIntyre’s Tradition-Constituted Enquiry in Polanyian Perspective,” *TAD* 26, 2 (1999-2000): 6-20.

⁸¹*Science and the Modern World* (N.Y.: MacMillan, 1925).

⁸²See George Lakoff, *Women, Fire, and Dangerous Things: What Categories Reveal about the Mind* (Chicago: University of Chicago Press, 1987) for an explanation of central models or prototypes in our use of words to signify cognitive categories.

⁸³In *Steps to an Ecology of Mind* (Chicago: University of Chicago Press, 1972), Gregory Bateson laments the failure of so many modern people to understand what he meant by a “sacrament.” I believe that what Polanyi and Shils meant by a (good) “tradition” is very close to what Bateson meant by “sacrament.” I have put this in terms of what Bateson, Polanyi, and Shils meant, rather than in terms of “the meaning” of “sacrament” and “tradition” because I agree with Polanyi that it is people who mean things by words, and that words mean nothing by themselves (“The Personal Mode of Meaning,” PK 252-253).

⁸⁴Very few of the Catholics among my students have been able, in response to my questions, to explain their Church’s doctrine of apostolic succession. When I explain it to them, however, they are not disposed to deny it. This might be a good example of a tacitly held belief.

⁸⁵It is possible that Shils’ conversations with Steven Turner led him to a clearer realization of the impossibility of both “group mind” and of “collective mental objects.” Turner reports that he had a number of conversations with Shils from 1982 until the end of Shils’ life (“The Significance of Shils,” p. 168). When Shils was working on his autobiography and his essays on collective self-consciousness, Turner was working on *The Social Theory of Practices* (Chicago: University of Chicago Press, 1994), in which he argued against the notions of group mind and shared mental objects.