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One Hundred Years of Substantivist Economic Anthropology¹

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Abstract

The paper offers a perspective on the history and epistemological status of economic anthropology, ranging from the contributions of Bronislaw Malinowski in the early 1920s to the state of the field today. The focus is on the substantivist tradition, associated in the history of anthropology primarily with Karl Polanyi. The deep-rooted confusion that results from what he termed the “two meanings of ‘economic’” is already visible in the earlier oeuvre of Bronislaw Malinowski. The substantivist tradition initiated almost unwittingly by Malinowski continues to flourish and is compatible with a variety of theoretical approaches. Tensions will always exist between the relativist, empiricist thrust of substantivist economic anthropology and the allegedly universal reach of the deductive models that dominate in mainstream (neoclassical) economics. These theoretical tensions may in some places acquire a political tinge, but they can always be mitigated through meticulous ethnography. The point is illustrated in this paper with reference to work on property relations in the former socialist countries of Eastern Europe.

¹ Acknowledgements: this paper derives from an online lecture given on 11 June 2021 to mark the 30th-anniversary celebrations of the Hungarian Ethnography and Anthropology Institute in Cluj-Napoca (Kolozsvar). The text has been extensively revised; however, since the persons and materials discussed are strongly linked to this particular occasion (both the timing and the Transylvanian location), I have opted to retain the colloquial, informal style as well as the organization of the lecture. I thank Szabó Árpád Töhötöm for the invitation and members of the audience for their questions and comments. I am also grateful for the comments of three readers: James Carrier, Biao Xiang, and Lale Yalçın-Heckmann; and for the editing of Brenda Black and Ralph Orlowski.

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Introduction

It is an honour to deliver a lecture to mark the thirtieth anniversary of the Department of Hungarian Ethnography and Anthropology in Kolozsvár (Cluj-Napoca). The date of your foundation seems uncontested. Matters are not so straightforward when it comes to specifying the origins of economic anthropology. But if one is pressed to pinpoint a moment in time, a case can be made for the publication in March 1921 of an article by Bronislaw Malinowski in the *Economic Journal*. The sixteen pages titled ‘The primitive economics of the Trobriand islanders’ can be seen as a Rubicon, a tipping point, the foundation stone of what I shall term the substantivist tradition. It was the first and only occasion that an anthropologist has published in the *Economic Journal*.

Malinowski himself did not use the word “substantivist”. It can be argued that his economic anthropology was not properly substantivist at all, because he failed to clarify his theoretical position sufficiently. Clarification came a generation later, mainly thanks to the work of Karl Polanyi, who is recognized as the founder of a Substantivist school (Polanyi 1957a, b). For roughly two decades starting in the late 1950s, the followers of Polanyi argued their position against rivals known as the “Formalists” – anthropologists convinced that the theory and methods of neoclassical economic science in the Western world (in particular the Anglosphere) can be operationalized to model economic life universally. Since the 1970s, these schools have faded away. In fact, I can’t think of anyone in recent decades who would own up to being a card-carrying, unreconstructed Substantivist or Formalist. However, I shall argue that much of the valuable work undertaken in economic anthropology nowadays can be characterized as substantively substantivist (with a small s), whether or not ancestral homage is paid to Polanyi and Malinowski.

One of the last major figures to identify himself explicitly with the Substantivist school was Marshall D. Sahlins, who was a junior associate of Polanyi in New York in the late 1950s. Sahlins wrote a number of synthetic essays in the 1960s which were brought together in 1972 in a volume called *Stone Age Economics*. Whether this collection deserves to be recognized as a substantivist milestone is debatable (Hann and Hart 2011: 62–63). Sahlins presented no ethnographic analysis of his own, which had been the hallmark of Malinowski’s contribution. Yet he had a clearer view of the theoretical stakes than his Polish-British precursor (see Graeber 2017; Gregory 2021). Marshall Sahlins did not contribute very significantly to economic anthropology (or anthropological economics, as he preferred to call it) after 1972. But others have taken up the torch in recent decades, no one more brilliantly than David Graeber, probably Sahlins’s most famous student. Graeber passed away unexpectedly last autumn, and Marshal Sahlins followed this spring. So now you see how I have constructed my century, from Malinowski’s publication in spring 1921 to Sahlins’s death in spring 2021. I am not a historian of the discipline, and in any case, it would hardly be possible to outline a comprehensive history of this field in a single lecture. In the first section, I explore the fuzziness in theory and method that impeded the consolidation of economic anthropology in the early decades of the last century. In an excursus I note the little-known contribution by Felix Somló, though I conclude that his work does not meet the substantivist standard. I begin the second part by noting the vitality of economic anthropology in recent years and defending theoretical eclecticism with examples from recent Focus Groups at the Max Planck Institute for Social Anthropology. Persisting questions of theory and method are then illustrated with reference to the difficulties faced by Western anthropologists in formulating a substantivist account of socialist economy and society in East-Central Europe, notably Transylvania.

I: Beginnings

In this section I want to show how the contributions of Malinowski built upon existing literatures and reflected contradictory elements in his intellectual formation. The foundation myth of modern British social anthropology (carefully nurtured by Malinowski himself) is that he revolutionized the discipline through unprecedented methods of field research, leading to the rejection of earlier evolutionist paradigms and indeed of history (Jarvie 1964; Kuper 1973). As an Austrian citizen, Malinowski made the most of his political lockdown in Australia during the First World War to exploit the opportunity he was given to document social life, especially economic life, in the Trobriand Islands. A cornerstone of this revolution was his monograph *Argonauts of the Western Pacific*, published in 1922. But the article published in the *Economic Journal* a year earlier was a good trailer. The goal was to shift the discipline away from Frazerian speculation about the intellectual development of human capacities towards meticulous functionalist analyses of particular communities as the ethnographer experienced them through fieldwork. The institutional study of economic processes – production, distribution, consumption – had a central role to play in the new functionalism. At this point, no one was thinking of carving out a distinct sub-field called economic anthropology. Malinowski wrote of “primitive economics”.

Both in the 1921 article and in the book that followed, he described a “tribal economy”³ that functioned according to quite different principles than those of the modern economy in Europe, supposedly based on “economic man”. *Homo economicus* was therefore not universal, but the product of a specific Western history. Malinowski’s construction of *Homo economicus* turns out on closer inspection to be a straw man. The Trobriander in his portrayal is an individual agent who is scheming to maximize his satisfaction (utility) in ways that any modern economist would be able to recognize, once allowances are made for obvious institutional differences in a non-monetized economy. For example, the chief who redistributes wealth among his clan is not acting contrary to a universal rationality; he is simply following the rules of a society that is not ordered (institutionalized) like our own. Malinowski characterized the Trobriand chief as a “tribal banker” who gathered, stored, and redistributed yams and other sources of wealth. The quotation marks are Malinowski’s, and he stressed that in this context the term banker could not possess “its literal meaning” (1921: 12). He counselled against classifying *kula* valuables as money, while at the same time illuminating the different functions that money objects might have outside a market system. In this way, he sought to pave the way for comparative analysis and also for interdisciplinary conversations with economists.

The contours of the later debate between Formalists and Substantivists can already be detected in these early works. In the following decades they took firmer shape in the work of Malinowski’s student, the New Zealander Raymond Firth, who was trained in economics. In his study of the Maori and (more ethnographically) in his later investigation of the Polynesian island of Tikopia, considerable use is made of mainstream economic concepts; but the deployment of notions such as “money” and “investment” is always carefully contextualized and local perspectives highlighted (Firth 1939). For this reason, I think that both Malinowski and Firth can be claimed for the substantivist tradition. Each was inclined through his early training to assume that certain economic notions of maximizing or of “least effort” would have universal validity; but following fieldwork,

³ Malinowski presents this term as his own, but it was in fact coined by historian Gustav von Schmoller; we cannot be certain that Malinowski read the work in question (see Spittler 2008: 225–226).

each came to modify an emphasis on individual rational choice-making that lent itself to deductive formal models, and to substitute a loose, inductively worked-out functionalism that did not.

Epistemological ambiguity persisted in Malinowski's case to the end of his life. In 1940 or 1941 he drafted a critical review of a book by the US anthropologist Melville Herskovits, *The Economic Life of Primitive Peoples* (Herskovits 1940). Herskovits was a Boasian relativist who believed that Western economic theory possessed universal validity, even if the particular values maximized were culturally variable. He was chided for his empiricist approach in a review by the distinguished economist Frank Knight, for whom universal claims were predicated on deductive modelling (Hann and Hart 2011: 64–65). As for Malinowski, in a review that was apparently not completed, never published, and made available to scholars only recently, he was critical of Herskovits for the opposite reason. He judged Herskovits to be making too many concessions to economics by seeking to exclude all the specific institutional factors that characterized any empirical economy. For Malinowski, the way forward lay not in the imposition of an abstract model, but in seeking to develop theory at a secondary level *ex post facto* on the basis of empirical research (Cook and Young 2016: 667; Cook 2017a: 136).

Twelve years later, sticking to his empiricist approach, Herskovits titled the second edition of his book *Economic Anthropology* (Herskovits 1952). By now he was even more keen to engage closely with the economists and to generalize their concepts, including the idea of a maximizing *Homo economicus* (probably because, by the 1950s, it was increasingly apparent that capitalist markets were eliminating earlier forms of economy; Cook and Young 2016: 674). Karl Polanyi preferred the earlier to the later Herskovits. He argued for a more radical critique: the concepts of the modern economist might be adequate for economies in which markets dominated, but they would inevitably distort the comparative analysis of economic systems around the world. Polanyi distinguished between “two meanings of ‘economic’”: while formalists emphasized scarcity, individual choices, and means-ends rational maximization, substantivists took a broader view of the human economy as interaction with the environment to meet needs. According to Polanyi, only under capitalism did the two meanings merge, when economy was “disembedded” from socio-cultural contexts (Polanyi 1944, 1957b). (Later scholars have criticized Polanyi for failing to appreciate that even the most impersonal markets and algorithms depend on a range of institutions that ultimately depend in turn on social relations and cultural practices.)

The distinctions that Polanyi crystallized in the 1950s were not new. They had emerged gradually as political economy morphed into neoclassical economics in the closing decades of the nineteenth century. Malinowski had no training in this field, but the title of his doctoral dissertation, defended at Cracow's Jagiellonian University in 1906, was “On the Principle of the Economy of Thought” (*O zasadzie ekonomii myślenia*) (Malinowski 1993: 89–115). It was a study of limitations of positivist philosophy, particularly the ideas of Ernst Mach, who was enormously influential in those years. The same works by Mach were imbibed by Karl Polanyi almost simultaneously in Budapest. Malinowski was attracted by Mach's efforts to theorize science on the basis of concepts of minima and maxima and “least effort”, though he also expressed criticisms. The excellence of this dissertation secured Malinowski the *Habilitacja* scholarship that enabled him to move to London and begin his celebrated association with the London School of Economics.

Let us look at this dissertation more closely. “Economy of thinking”, according to the young Malinowski, can be traced back to the Greek etymology of *oekonomia*, meaning management, be it of livestock, a social group, or a physical system. More specifically, he wrote, “we understand by

economy not management in general but good management. Since the worth of management is measured by the magnitude of the objectives achieved in relation to the means used, we may call economy, in the specific sense of this word, namely thrift, a minimum outlay with the same gain, or a maximum gain achieved with the same means; both formulations come to the same thing” (1993: 91). Although the work focuses on the positivists, it also reveals the influence of Malinowski’s early reading of Nietzsche (Thornton and Skalník 1993: 16–26). He concludes that it is impossible to do away with metaphysics altogether. Economy is the basis of Mach’s philosophy of science, which posits a physiological basis in the mind of the scientist, whose job is to explain physical phenomena as efficiently as possible. Mach is a neo-positivist who privileges the senses, though he is simultaneously an idealist and a relativist who denies the reality of an empirical world “out there”. The “economy of thought” has to be pinned down and contextualized: in other words, connected to particular human beings and their communities. In this way, Thornton and Skalník forge a link between the Cracow dissertation and Malinowski’s later accomplishments as a functionalist ethnographer. From Mach, Malinowski learned that “facts” depend logically on the theory that has been empirically deployed to collect and select them. But the initial theory is subject to testing (later philosophers of science would develop the concept of falsification) and Mach’s perspective therefore opened up avenues to develop new ideas and theories promiscuously on the basis of empirical data. As Thornton and Skalník maintain (1993: 35): “In accepting Mach’s belief that ‘theory creates facts’, [Malinowski’s] openness to many theoretical perspectives led him to collect and to observe a great many facts.”

These notions of economy, thrift and efficiency must themselves be placed in their intellectual and political-ideological contexts. Ernst Mach was a contemporary of Carl Menger, one of the founders of marginalist economics from the 1870s. Mach’s deployment of *Ökonomie* in his philosophy of science has an affinity with the individualist marginalism of the neoclassicals (and also with a more specifically Austrian tradition that later included Ludwig von Mises and Friedrich August von Hayek), even though Menger’s economic science placed greater emphasis on deductive methods.

After the ceremonial awarding of his doctorate in 1908, Malinowski moved initially to Leipzig, where he was exposed to the teachings of an influential representative of a very different way of thinking about economy. The economic historian Karl Bücher formulated universal laws of development, but on the assumption that the principles of economic organization would differ in each successive stage. Bücher hypothesized an initial “pre-economic” condition. The main feature of this *Urgesellschaft* (primitive society) was the “individual search for food”. This stage was followed by the “closed household economy”, traces of which could still be observed in the European peasantry. But self-sufficient households had generally been overtaken by more complex forms of production and exchange, culminating in the *Nationalökonomie* of the modern state. Karl Bücher taught an empirical *Volkswirtschaftslehre* rather than an abstract *Ökonomie*. His interest in concrete institutions placed him closer theoretically to Gustav von Schmoller and the Berlin-based Historical School, though he was also respectful toward the general theories put forward by scholars in the Habsburg empire such as Mach and Menger (Hann and Hart 2011: 39–41).

Malinowski’s first explicit engagement with economic anthropology (the name did not yet exist) was a 1912 chapter in English in a *Festschrift* for Edward Westermarck. This paper, based on secondary literature, contains numerous echoes of Bücher (Firth 1957: 211). Malinowski draws an evolutionist distinction between economic and pre-economic labour to argue that only through magic and ritual can primitive man be mobilized to carry out productive activity efficiently in the modern

sense. But following his Trobriand fieldwork, Malinowski became critical of Bücher, even as he polemicized vigorously against mainstream economics and railed against the concept of “economic man”. The complex organization of ceremonial exchange in the Trobriand “tribal economy” contradicted the assumptions of Bücher’s first two stages. It was clear that the principle of least effort did not have universal validity, since the natives of Kiriwina on the Trobriand Islands toiled in their gardens to produce many more yams than they could consume. This surplus was transferred to their matrilineal kin in the form of *urigubu* payments, and to their chiefs. Before writing up his Trobriand analyses, Malinowski read two major works of mainstream economics in the English language: Alfred Marshall’s *Principles of Economics* and Irving Fisher’s *Purchasing Power of Money* (Young 2004: 603).

I suggest that Malinowski’s seminal publications in the early 1920s reveal a struggle to reconcile the contrasting senses of “economy” to which he had been exposed in Europe: on the one hand, rational choice-making to maximize satisfactions in conditions of scarcity; and on the other, a substantive embedding of production, exchange, and consumption in institutions that were locally rooted and regulated by custom, practices, and above all *values*. The bias of the Trobriand publications is to the latter. Thus, the principle of least effort is rejected again in *Coral Gardens and Their Magic* (1935). Here we learn more about the aesthetic values of maintaining a beautiful yam garden, which have implications for social emulation and “morals”. It is bad manners to call a gardener lazy; yet some clearly are, and their reputation suffers in consequence. On the other hand, it is also unwise to be too proficient and diligent. This can lead to accusations of vanity, greed, and even sorcery.⁴ Malinowski’s lively accounts of individual agency are consistent with his own values and what might be termed his political agenda, which is to refute the notions of collective ownership and primitive communism that were current in early twentieth-century Europe, inside anthropology as well as outside it. Malinowski showed that the “tribal economy” was made up of individual actors who interact in complex ways. The key to social organization in general lay in the property system, above all in how land was held and used by persons and kin groups.

All this is pure substantivism. Exiled in North America during the Second World War, Malinowski continued in this vein during fieldwork with local partners in peasant marketplaces in Mexico (Malinowski and de la Fuente 1982). He found an equivalent of Trobriand *kula* in the regional marketing system of the Oaxaca Valley, an institution that integrated far-flung communities, many of them economically more specialized than those of the Trobriands (Cook 2017a, b). Oaxaca commodity markets were highly monetized, yet dominated by peasant-artisans whose socio-cultural relations were as complex as those of Melanesia. In his unpublished review of Herskovits (discussed above), Malinowski insists that analysis should proceed from the empirical complexity that is uncovered by the fieldworker; theory should follow later. Yet in the same document he goes out of his way to praise work by his student David Goodfellow in South Africa (Goodfellow 1939), which represented a more “aggressively neoclassicist” approach than anything advocated by Herskovits (Cook and Young 2016: 668, 671). To judge from this evidence, influenced both by what he had observed on the ground in the monetized economy of Oaxaca as well as the neoclassical approaches with which he had become familiar in previous years in London, by the 1940s Malinowski was probably ready to grant the rational choice principles of microeconomics more general, even

⁴ Malinowski 1935, I, 175–176. Nowhere does Malinowski use the concept of a moral economy. But in the second volume of *Coral Gardens* he does write of “the moral tradition of a tribe”; he puts “economic values and morality” on a par with hunger and sex as determinants of “vital interests” (1935, II: 47; cited in Spittler 2008: 239).

universal validity. Yet he simultaneously upheld an institutionalist (in my terms substantivist) approach for understanding particular economies and the relationships between them. He thought that fieldwork should be a basis for fresh theorizing. However, the theoretical notion of the “tribal economy” that Malinowski developed in the early 1920s was quietly abandoned soon afterwards; and for all the rich detail in his notebooks from Oaxaca in 1940 and 1941, it cannot be claimed that the unfinished research in Mexico breaks new theoretical ground.⁵

Excursus

So far, I have suggested that Malinowski’s self-presentation as a revolutionist of the discipline depended to a considerable extent on his fieldwork in the domain of economy, but that he ultimately failed to synthesize the conflicting elements in his biography into a clear epistemological position. Like most serious historians of anthropology, I find the narrative of a miraculous conception in March 1921 to be inherently suspect. Before closing the first part, let me try to be a little more specific about the origins of economic anthropology. We can formulate the criteria for substantivist economic anthropology more precisely by considering the case of a scholar who was based for many years in Cluj-Napoca (in Hungarian Kolozsvár).

Félix Fleischer was born in Pozsony, today’s Bratislava, in 1873. When still a schoolboy he changed his name to Bódog Somló (which certainly sounds more Hungarian). He was appointed Professor of Legal Philosophy and International Law at the University of Kolozsvár in 1905, where in the summer of 1909 he was a doctoral examiner of Karl Polanyi.⁶ Felix Somló (internationally he was known by this hybrid form) made important contributions to legal and constitutional theory within the framework of Austrian positivism. He, too, visited Leipzig, where it is likely that he was exposed to the lectures of Karl Bücher. He spent a later sabbatical in Brussels, where he pursued his interest in the evolution of human society with reference to economy. This was very much the nineteenth-century pattern that saw comparative lawyers make use of ethnological materials and thereby contribute to early theorizing in the discipline later consolidated as anthropology. Herbert Spencer was a major influence on Somló, as he was on the young Polanyi.⁷

The work that is pertinent to this paper is *The Movement of Goods in Primitive Society*, published in Brussels and Leipzig in 1909. Contemporary readers bristle at the very concept of *Urgesellschaft*, but this was of course standard at the time.⁸ The book consists mainly of a survey of extant facts from around the world (especially recent materials from Australia) concerning the transfer of goods. Towards the end Somló constructs a typology that distinguishes between transfers in the domestic domain (*Abgabe-Leistungen*), hierarchical transfers (*Tribut-Verpflichtungen*), and gift exchange (*Geschenk-Tausch*). The last corresponds closely to the exchanges later made famous by Marcel

⁵ Scott Cook, on whose research this section draws heavily, glosses Malinowski’s concerns in the unfinished Oaxaca project as Weberian (Cook and Young 2016: 673). I think it is more accurate to state that the lasting legacy of his studies of Mach was a functionalism that was compatible with quite different traditions in social and political theory. Whereas Cook finds the contributions of Hershkovits to be confused and contradictory, it is hard to avoid reaching the same conclusion about the work of Malinowski himself.

⁶ This was necessitated by the political problems of the young Polanyi in Budapest, where he was a student activist promoting currents of radical liberalism (see Dale 2016).

⁷ Somló diverged from Spencer in his conviction that “state intervention” was compatible with individual freedom and indeed a precondition for evolutionary progress (Takács 2016: 9–10). However, he did not follow other liberal intellectuals of his generation (among them Karl Polanyi) in embracing socialism.

⁸ Decades later, Malinowski was still using “primitive” or “savage” society as English equivalents; and as late as 1972 Sahlins opted for *Stone Age Economics*.

Mauss in his *Essai sur le don*, published in 1925 (Mauss 2016). Somló is acknowledged by Mauss in an early footnote, though later commentators have questioned whether this was an adequate expression of his debt to his Hungarian contemporary (see Berthoud 1999). At least Mauss does cite Somló. You might expect Malinowski and Polanyi to do the same. But Malinowski seems never to have read the work in question. As for Polanyi, despite his personal familiarity with the Kolozsvár professor (who signed off his doctorate in the very same year that he published *The Movement of Goods in Primitive Society*), before the First World War Polanyi was a politically engaged young lawyer who took no interest in ethnological materials or in political economy. He developed his economic anthropology much later, largely on the basis of works by Malinowski and the Austrian ethnographer Richard Thurnwald. But Karl Polanyi did not discover *The Gift* until 1948. When he found the footnote citing Somló, he expressed shame that he had been ignorant of the pioneering work of a scholar who had been his teacher in a different discipline.⁹

How does the case of Somló help us to put a date on the origins of economic anthropology? There is general agreement that Malinowski offered excellent empirical accounts of how economic phenomena were embedded in wider contexts of politics, religion, morality, and kinship. Even if his theoretical position is never specified rigorously, from 1921 on we can classify him as at least a proto-substantivist, a substantivist *avant la lettre* – despite his tendency to see maximizing individuals everywhere and despite the wobbling at the end of his life when he seemed, during his Mexican work, more sympathetic to using the methods of economists, and even theories extrapolated from mainstream economics.

Somló does *not* qualify as a proto-substantivist because his work lacks ethnographic substance. Even when his interpretations are path-breaking, he fails to develop them. For example, the concluding chapter of *The Circulation of Goods in Primitive Society* contains penetrating remarks on how to supersede the nineteenth-century dichotomy between individualism and collectivism. Karl Bücher emphasized the former, while the historical materialism of Marx and Engels (in the wake of Lewis Henry Morgan) stressed the latter. Somló argued presciently on the basis of the transfers he analysed that the problem would have to be approached by recognizing more complex forms of social organization and interpersonal relations. But he did not pursue this analysis in terms of property relations and made virtually no reference to the land, despite its centrality to tribal life (Sárkány 1977). It was left to Malinowski, in his 1921 article and at much greater length in his 1935 monograph *Coral Gardens and Their Magic*, to show how a careful investigation of land tenure could provide the key to understanding the social order and to transcending the European binary of individual and collective.

I am therefore privileging *ethnography* in specifying the criteria for substantivism (small s), rather than *theory* in any conventional sense. To this extent, Malinowski's standing is reaffirmed. Yet it is not the case that every study based on field research can be classified as substantivist: some formalists and their successors (I am thinking in particular of those sometimes labelled “new institutionalists”) go to great lengths to squeeze their empirical data into an inappropriate economic framework. If the ethnographer does not respect the customs of older institutionalist schools, in which research is based on the meticulous description and analysis of the social mechanisms particular to that society, in a specific geographical location, at a specific moment in time, then the resulting work will not fall within the substantivist compass. This older institutionalism also requires digging as deep as possible

⁹ Yet it has to be added that Somló and Mauss are still missing from the references in Karl Polanyi's seminal essays of the 1950s (Polanyi 1957a, b), which are generally considered to be a charter for the Substantivist school.

into subjectivities, into the distribution of beliefs and values in the society, and notions of agency and personhood.

It equally true that not every substantivist is a brilliant ethnographer. Polanyi himself did not do fieldwork. Nor did Mauss, nor was fieldwork a forte of Sahlins. But all three based their synthetic studies on rich ethnographic monographs, including those of Malinowski himself. So, I conclude this discussion by reaffirming my date of 1921, while noting the importance of earlier contributions, including the almost unknown (at least in the Anglosphere) work of Felix Somló, who surely deserves more than just a footnote in the history of our discipline.¹⁰

II Theoretical Eclecticism and Political Challenges

In the era of neoliberal political economy, scholars throughout the social sciences have been looking again at Karl Polanyi, or discovering him for the first time. His anti-market message is also attractive to political activists of many hues. This revival has not yet had much impact on anthropology. However, in the broad sense in which I am using the term, substantivist economic anthropology has continued to flourish through to the present day. It is not necessary to read the same key essays by Polanyi that I had to digest as a graduate student back in the 1970s. Polanyi's star was by then already fading, partly replaced by a wave of neo-Marxist studies, and then by other approaches ranging from highly culturalist to more sociological approaches influenced by Science and Technology Studies. But this doesn't matter. In a sense, economic anthropologists have come to take Polanyi's teachings for granted: his relative neglect in our discipline is a sign that he won the big battles, even if he is no longer cited very often.

Of course, from a purely scholarly point of view this is not entirely satisfactory. I think that Polanyi's name could have been invoked a bit more often in David Graeber's magnum opus, *Debt: The First 5000 Years*, if only because challenging mainstream economics accounts of the origins of money, trade and markets was central to the agenda of the original Substantivist school (Graeber 2011). Be that as it may, for me Graeber's unconventional anarchist history is fully compatible with the substantivist tradition, especially when it draws on ethnographic analyses to buttress its challenges to the precepts of mainstream economics. More generally, relations of credit and debt are as central to many contemporary projects in economic anthropology as they were to Malinowski and Mauss a century ago. This is a consequence of the enhanced role of money and finance in driving global capitalism since the 1970s. Between 2015 and 2019, together with Don Kalb, I organized a postdoctoral Focus Group at the Max Planck Institute for Social Anthropology in Halle to investigate various aspects of contemporary financialization, from debt collection in Croatia to digitalized carbon markets in China (Hann and Kalb 2020).

The Marxist Don Kalb might not approve of my assimilating his perspective into a broadly conceived substantivism. But I do exactly the same with the Weberian Jonathan Parry, with whom I co-led an earlier Focus Group investigating industrial labour (Hann and Parry 2018). Parry's monograph of an Indian steel plant qualifies for me on the basis of its ethnographic excellence as an exemplary substantivist study, even though it lacks overt signposts to Polanyi and shows no respect for any demarcation line between sociological and anthropological literatures (Parry 2020). Earlier

¹⁰ Somló (like his pupil Karl Polanyi) was a German-speaking Hungarian of Jewish descent. In 1920, following military defeat and the dismemberment of the Hungarian state (which included the allocation of Transylvania to Romania), he committed suicide. His grave can be visited in the old Hungarian cemetery (Házsongárdi temető) in central Kolozsvár/Cluj.

still, Stephen Gudeman and I organized a comparative investigation of economy and ritual in six postsocialist settings (Gudeman and Hann 2015). Gudeman's own work has continued the project of "anthropological economics" by emphasizing local models of economic life, notably those of Latin American farmers (peasants) among whom he worked himself. In recent decades he has complemented this attention to local knowledge with more analytic models, and (perhaps influenced by the global financial crisis) his culturalist analysis has become more political (Gudeman 2016). In my eyes, all of these scholars fit within a broad substantivist tradition: they have very different theoretical orientations, but they share common ground in the importance they attach to investigating institutions and value systems empirically. Let me suggest that it is a sign of quality in substantivist economic anthropology if the reader is sometimes able, on the basis of the empirical evidence presented, to draw conclusions that differ from the theoretical (and perhaps also political) preferences of the author.

I have mentioned case studies ranging from China to India to Latin America, but it might be instructive at this point to reflect on theoretical eclecticism and the possible limitations of this broadly defined substantivism by zooming in on one region. East-Central Europe is the region that I know best, so let us consider briefly what economic anthropologists have had to say about it in socialist and postsocialist decades. This region raised formidable methodological and epistemological issues when I began work there during the era of the Cold War. The polemics between Formalists and Substantivists were coloured by the fact that, in those days, most anthropological research by Western scholars unfolded in communities of the "Third World" that were enmeshed in capitalist market economies. Karl Polanyi was ready to concede that formalist (neoclassical) approaches were appropriate for studying the economic organization of these societies. But how was the economic anthropologist to proceed in the case of the "Second World", which included relatively developed European industrial societies as well as remote peoples in Central Asia and Siberia? Could the concepts of Western economics, ostensibly universalist in scope, be stretched to embrace Marxist-Leninist-Maoist socialism? Or was there a socialist equivalent to neoclassical economics, such that the substantivist economic anthropologist looking to emulate a holistic Malinowskian approach in Malinowski's home region would have to be wary of two formalisms: one based in the dominant Western economic paradigm, and the other in the tenets of central planning?

East-Central Europe turns out on closer inspection to be a very diverse region. My home for more than 20 years has been a city in former German Democratic Republic. Unfortunately, like several other socialist states, this relatively advanced industrial economy was effectively closed to foreign researchers and for the decades before 1990 we lack monographs that would satisfy Malinowskian standards. I was fortunate to be able to carry out field research in Hungary in the mid-1970s. I am still paying regular visits to the village of Tázlár and the neighbouring town of Kiskunhalas more than half a century later.¹¹ The preeminent substantivist monograph on rural Hungary by a Western anthropologist is Martha Lampland's *The Object of Labor* (1995).¹² Comparable studies were published for Bulgaria by Gerald Creed (1997) and for Romania by David Kideckel (1993). What

¹¹ The economic institution in Hungary that has fascinated me the most in recent years is workfare, which I have studied with an emphasis on local understandings. Instead of condemning it as neoliberal repression or praising it for improving labour-market efficiency, I explore the *moral dimension*: most people, both recipients of workfare wages and their neighbours, agree that the value placed on work by the Fidesz government is correct and healthy for the society. For further exploration of the moral dimension of work and economy, see Hann 2018, 2021; Yalçın-Heckmann forthcoming.

¹² Two native ethnographers produced outstanding work on the Hungarian countryside prior to socialist collectivization that also fits well into the substantivist tradition as I am broadly defining it: see Fél and Hofer 1972.

these studies have in common is that each is based primarily on field research in the countryside that was undertaken under socialism, but then revised for publication after 1990, when it was possible for the ethnographer to return to the field sites and add insights into the impact of a new era, including changes in local subjectivities.

The best-known Western ethnographer of socialist and postsocialist Romania is Katherine Verdery. Her major publications concerning the socialist decades lie outside substantivist economic anthropology in the sense I am characterizing it. In her first monograph, only the opening chapter deals with socialist economic conditions. In it, she emphasizes the human costs of collectivization and the state's industrialization strategy; she is puzzled that villagers themselves do not see their relations to the state in the same terms of structural antagonism that she does (Verdery 1983: 72). The rest of the book explores such relations and mystifications over the pre-socialist centuries (the decision to focus on the past was influenced by the ethical and political difficulties of writing about contemporary conditions).

I consider Verdery's detailed analysis of the processes and consequences of decollectivization in the same Transylvanian community where she began work in 1973 as an outstanding contribution to substantivist economic anthropology (Verdery 2003). She harks back to the work of Malinowski in analysing successive changes in rural social organization (in which a class of smallholding peasants was effectively eliminated twice over) via a focus on land and property. Western scholars and consultants who alleged that Romanian society under socialism lacked any system of property are roundly criticized. On the contrary, insists Verdery, there existed a hierarchy of property forms, with the state at the top, localized collectives in the middle, and private (productive) property objects distinguished from (non-productive) personal property at the level of the individual. This "working property regime" emphasized control over persons rather than over objects (such as land). In this respect, according to Verdery, socialist Romania had more in common with non-Western systems such as of the estates of administration identified by Max Gluckman in colonial Africa than with the property regimes of Western capitalist states. She notes that some contemporary anthropologists prefer to discard the term "property" altogether, on the grounds that it is too closely bound to a particular Western tradition. Verdery opts to retain it, while insisting that differences be foregrounded; the major difference she identifies in her analysis is that, compared with the West, property institutions in Romania rest not on law but on capricious "administrative methods" (2003: 46).

Katherine Verdery's account offers rich detail on the conflictual social relations that prevailed during the postsocialist transformations of the 1990s, but it also gives rise to some ambivalences. On the one hand, she insists that the socialist property regime in Romania is historically specific and should not be seen as an inferior variant of Western systems founded on private ownership and marketability. These Western systems must not be naturalized: they too are the contingent outcomes of complex political and economic processes. Moreover, Verdery has consistently declared herself to be sympathetic to the values implied in alternative, non-capitalist ways of organizing economies and societies. *De facto*, however, her verdict on the Romanian socialist system is extremely critical. The collectivization of agriculture did not so much replace one system of property (bourgeois) with another: it "superseded" the system of evolved private property rights with a system in which a "fiat state" ruled arbitrarily through coercion (2003: 46–49). The state invoked "socialist property" for ideological purposes, rather than as an expression of or guide to actual institutional processes. Meanwhile at the micro-level, in Verdery's account, actors pursued their interests in pragmatic ways.

Farm managers acted strategically and selfishly in appropriating much of the surplus for their own benefit, while ordinary villagers took what they could for themselves whenever they could, secure in the knowledge that this was not theft, since all property was fundamentally collective. The upshot is an account of socialist and postsocialist village society that, irrespective of the values and intentions of the author, confirms common Western stereotypes and emphasizes conflict, above all conflict between villagers and managerial elites.¹³

Verdery's ethnographic account is based on deep knowledge of village life over decades combined with interviews recollecting the socialist era after its demise. It also builds upon her attempts to theorize a more general model of socialism (Verdery 1996). In this earlier work she drew heavily on Hungarian economist János Kornai, an arch-critic of socialist political economy.¹⁴ Kornai is convinced that markets and private property are the only effective guarantees of economic efficiency. He argues for minimal state interference in order to allow the individual maximal freedom to choose. This is the tradition of Carl Menger's Austrian School (mentioned in Part I), and of the formalist emphasis on choice-making in conditions of "scarcity" that was firmly rejected by Polanyi (1957b). These values are the opposite of socialist values. Kornai has elaborated them in numerous studies in the postsocialist decades, in which he has *inter alia* expressed the view that the welfare state consolidated under socialism was "premature" in view of the country's level of economic development (Kornai 1997).

In short, Katherine Verdery's adaptation of Kornai's indictment of the "shortage" economy of socialism risks imposing not only Western ideas of efficiency but also Western liberal values in a context in which they are, on the face of it, inappropriate. The politics are complicated by the fact that these values, especially those pertaining to private property, are seemingly endorsed by a significant fraction of the population of Transylvania (a larger fraction than in other socialist countries and regions). This situation differs from formalist research in the Third World that applies the assumptions of neoclassical economics, yet Kornai's model of socialism reflects a similar bias towards "scarcity" (rather than "subsistence", to use the vocabulary of Polanyi). The socialist equivalent would be a formalism that approached collective farms and other organizations in terms of socialist economic theories. Katherine Verdery pays little attention to this economic formalism, preferring to view socialist property as ideology and cultural system and to note that decrees have "illocutionary" effects (2003: 42). Her basic theory is drawn instead from the formal model (in neo-institutionalist guise) of an economist who shifted from Communist Party member to inveterate opponent of socialist political economy. The question arises: does Verdery (1996, 2003) present ethnographic material selectively to fit Kornai's abstract theory? If so, this would be the opposite of the strategy recommended by the mature Malinowski, for whom it is an error to start with the formal model.

A critique of Verdery's work along these lines was launched by Tatjana Thelen a decade ago, according to which the Kornai-Verdery model based on "scarcity" was a distortion and a "dead end"

¹³ Verdery emphasizes informal networks of reciprocity as the only way to deal with "glitches"; she also writes of "(...) legendary hoarding, dissimulation, plan bargaining, and other manipulations of state property by lower-level managers" (2003: 59).

¹⁴ Kornai is often described as an institutionalist or neo-institutionalist (e.g. by Thelen 2011). In addition to possessing considerable mathematical competence, he is genuinely interested in the particular institutions of an economic system, be it the hierarchical mechanisms of central planning or attempts to create new markets in the era of postsocialism (Kornai 1992, 2001). In this respect, irrespective of political convictions, economic anthropologists have more in common with János Kornai than with the great majority of mainstream (neoclassical) economists, who pay little or no attention to the functioning of concrete institutions.

in the study of socialist and postsocialist societies due to its economic bias (Thelen 2011). In a vigorous response to these strictures, Verdery and her student Elizabeth Cullen Dunn pointed out *inter alia* that she had also integrated a range of other theorists (including Marxists) into her analysis and that, besides, it was entirely justified to draw heavily on Kornai in view of his local experience and “indigenist perspective” (Dunn and Verdery 2011: 252). Verdery might have added that, far from imposing an inappropriate model, she adapted Kornai’s model for her own purposes only after her first immersion in Transylvanian village life in the 1970s, as the best way to make sense of her empirical data.

In any case, just as we can re-read and learn from Malinowski’s analysis of Trobriand land tenure without needing to endorse the individualist values of the author, so the substantive evidence assembled by Verdery allows the attentive reader to think outside any possible bias introduced by her model of socialism. Like the three compatriots whose works I have mentioned, she notes that the villagers with whom she spoke in the 1990s also had positive recollections of their experiences under the *ancien régime*. This is particularly surprising in Romania, given the exceptional dysfunctionality of economic institutions under Nicolae Ceaucescu. Her inspiration from Kornai leads her to portray socialist farm managers as selfishly pursuing their own interests, which diverged from those of the majority. Yet she also records instances of solidarity and “connivance” between leaders and workers.¹⁵ If socialist elites were reviled, one wonders why so many villagers in Vlaicu were keen to continue with some form of association under the leadership of former managers. For Verdery the answer is that they had no real choice in the matter and were continuously manipulated by the old elites long after the demise of the collective farms (which, she concedes, might more accurately be called cooperative farms). Readers are free to doubt assertions of endemic conflict and the author’s claim (in the same introductory chapter in which she has outlined Kornai’s theory), that stealing from the collective was the means by which ordinary members established their moral personhood as “proper human beings” (2003: 69).¹⁶

Thelen extended her critique to include the work of Elizabeth Cullen Dunn (2004). This seems an appropriate example with which to close, since the baby-food factory whose fate after socialism is charted by Dunn lies in Rzeszów in Southern Poland, in what used to be the Austrian province of Galicia, little more than a hundred miles east of Cracow, the hometown of Bronislaw Malinowski. In this study, although Dunn too pays lip service to the theoretical work of János Kornai, his concept of the “shortage economy” does not get in the way of sympathetic ethnography in which Dunn conveys the attractions of socialist working conditions compared to the new methods introduced by Western corporations. She is faithful to what she observed and heard during her field research, in and around the factory itself, from foreign businessmen aiming to shake things up according to their neoliberal model as well as from members of the indigenous population arguing for their own understanding of “social justice”. Malinowski, whose sympathies were always pro-individualist and anti-socialist, might not welcome the fact that some of those Dunn worked with (his compatriots) felt

¹⁵ For a more sympathetic understanding of such phenomena, see Konstantinov 2015. On the basis of a detailed analysis of the moral dimension of reindeer herding on a state farm in Northern Russia, the Bulgarian anthropologist develops a theoretical notion (“sovkoism”) of very general reach. Konstantinov’s theory is not based on a prior value-laden model or affected by considerations of Cold War othering.

¹⁶ Of course, the author of an ethnographic monograph is bound to be selective in the vernacular materials she chooses to include. Katherine Verdery does not provide quantitative analysis of villagers’ attitudes. Compared with Malinowski’s work in Kiriwina, her longitudinal account relies more heavily on respondents’ recollections of events in the past than on participant observation. The many quotations from her conversations are not verbatim but rather “very loose translations of what my respondents actually said” (2003: xx).

nostalgia for some basic institutions of the Polish People's Republic; but I think he would concede that in this monograph Elizabeth Dunn meets his standard by paying due attention to local categories and values.

Conclusion

Let me conclude with a few words about the relationship between anthropology and economics. Marshall Sahlins will be remembered for his aggressively "anti-economics" stance (most powerfully stated in the celebrated preface to *Stone Age Economics*). On this view, economics is an embarrassing pseudo-discipline that views itself as a science (akin to, say, physics or even biology) but is better classified as an unsavoury element of Western ideology, a "business" approach based on an impoverished vision of humanity. I do not quite share this perspective (which may have something to do with the fact that Sahlins spent most of his career in Chicago, where the most influential economists were in the aggressive vanguard of neoliberalism).

A more moderate position would be one that allows the two subjects to jostle uncomfortably alongside each other. There is scope for synergies, and not only with the increasing numbers of scholars in radical "heterodox economics" who distance themselves from the mainstream paradigm. Anthropology's focus on institutions, for example, can enable it to draw out the real consequences of pro-market policies for income distribution and well-being. It should be possible for anthropologists, sociologists and economists to work together to explicate institutional differences, e.g. between capitalism and socialism in the past, and between the varieties of capitalism that continue to mark our highly globalized present. The gap between the "two meanings of 'economic'" pinpointed by Polanyi can be bridged. The balance struck by Malinowski in 1921 and by Polanyi in 1957 remains attractive. This position differs from what might be called the "strong" substantivist position, according to which the "embeddedness" of economy collapses it into other social domains (religion, kinship, etc.) in a broad notion of social reproduction that precludes comparative economic analysis. For me (as for Polanyi himself), substantivism has to retain that basic reference to livelihoods (this need not be exclusively material). Its ultimate subject is "human economy" (a notion introduced by Polanyi [1957a: 94] and creatively elaborated in recent years by Keith Hart [e.g. 2017; see also the first two volumes in this important publication programme]). This seems more useful than seeking to dissolve the category of the economic altogether.

In conclusion: I hope to have persuaded you that today, one hundred years on, substantivist economic anthropology is both an exciting field of intellectual enquiry and a branch of anthropology that possesses practical and political importance in our neoliberal era. May the next century be no less productive.

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