

NOTE DE RECHERCHE

From Austroliberalism to Anschluss : Oskar Morgenstern and the Viennese Economists in the 1930's

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Abstract

This paper provides an account of the economics community in interwar Vienna from the perspective of Oskar Morgenstern. Beginning at the close of the 1920's with his return to Vienna from study abroad, and ending in 1938 with the *Anschluss* of Austria, it retraces Morgenstern's activities and thinking, and explores his relationships with Karl Menger, Ludwig von Mises, Friedrich Hayek, Abraham Wald and the Rockefeller Foundation. Morgenstern emerges from the decade as something of a maverick in the Austrian School, actively promoting the use of mathematics in economics and critical of any attempt to use economics to justify political stances, including the liberalism of Mises and Hayek.

Introduction

Let us imagine the following. It is an Autumn evening in Vienna in 1927. The talk is over, the twenty or so members of the city's Economics Society have begun to leave the conference room of the offices of the National Bankers' Association. Folding his papers, the speaker, a 25-year old mathematician named Karl Menger, is being questioned by Ludwig von Mises on the work of Bernoulli. Oskar Morgenstern, also in his mid-twenties, briefly considers joining them, but decides it better to stay with his mentor, Professor Hans Mayer, who also is preparing to leave.

Morgenstern: "An excellent piece of work, don't you think, Herr Professor? Truly a model of clarity..."

Mayer: "My young man, I have the greatest respect for Menger's competence as mathematician. Yet, what we have seen this evening is but another example of the futility of trying to treat formally matters that are unsuited to it. Logarithmic formulae? Valuation functions? Is economics, the study of the complexities of subjective evaluation in time, to be reduced to these mechanistic simplicities, this pseudo-science? Is that what this man's father laboured for?"

Those gathered at Menger's lecture that evening formed the heart of the economics community of interwar Vienna. Intellectual leader Ludwig von Mises was the leader of the Austro-liberals, a staunch opponent of government interventionism, close to this mentor Böhm-Bawerk. Around him gathered a group known for their liberal views, including Friedrich Hayek, who had undertaken to resurrect the Economics Society, Fritz Machlup, Gerhard Haberler and Richard Strigl. Those standing somewhat apart from this liberal nucleus included Paul Rosenstein-Rodan, Alexander Gerschenkron and, in time, Morgenstern himself.¹ The other authoritative figure present that evening was the above-mentioned Mayer, holder of a chair at the University of Vienna. Like Mises, he was a critic of mathematical theories of economic equilibrium, but, unlike him, he did not share his radical liberal politics, being closer to his mentor, von Wieser, in his approval of a strong state. Mayer and Mises were rivals and enemies, and Hayek's effort to rehabilitate the economics seminar was part of an effort to reconcile the warring factions. Other figures we might mention include Steffie Braun, one of the few women in this male-dominated group; Richard Schüller, the

¹ On the Austro-liberals, see Klausinger (2006).

senior civil servant; Ewald Schams, one of the few economists well-trained in mathematics; and members of the business community, such as banker Karl Schlesinger, and Menger's friend Felix Kaufmann, a philosopher of law and social science who worked for a petroleum company.²

As mathematician and scion of Vienna's most distinguished economics family, Karl Menger was comfortable with the two communities.³ That evening in 1927, he was not long back in Vienna, after a somewhat acrimonious post-doctoral sojourn in Amsterdam with mathematician L.E. J. Brouwer. As protégé of Hans Hahn, Menger the student was close to the mathematicians and philosophers of the *Schlick Kreis*, or Vienna Circle, which included, in addition to Hahn himself, Moritz Schlick, Rudolf Carnap and Otto Neurath. To the liberal economists such as Mises, these figures were well-known: Hahn was the University's most academically eminent socialist, and radical Otto Neurath had long been an opponent of Mises, dating at least from Böhm-Bawerk's famous seminar at the University, which also included socialist Otto Bauer. Neurath was a key target in Mises' anti-socialist writings of the 1920's.

Menger had also grown up with economics. Only four years previously, just after his father's death in 1923, and before he had even begun his own studies, Menger had written an Introduction to the posthumous revised edition of the *Grundsätze*. Thus, even if the revision was perceived to add little to the original, all the economists were familiar with the son's editorial contribution. It was also in 1923, after reading his father on the question of uncertainty, that he started to write on the Petersburg Paradox.⁴

Morgenstern, at this point, was temporarily back in Vienna, between Rockefeller-funded stays in Harvard and Rome, completing his *Habilitation* thesis, *Wirtschaftsprognose*, a methodological treatise faithful to the tenets of the Austrian School, critical of Historicism and the American Institutionalists, and denying the possibility of economic prediction.⁵ Yet, with all the benefit of hindsight, Menger's talk that evening can be regarded as something of a watershed for the young economist. It marked the beginning of a decade during which Morgenstern would gradually distinguish himself from his Austrian mentors, actively promoting the development of mathematical economics, and, unlike the more liberal among them, learning to reject, if not quite their political stance, then certainly the idea that it could be buttressed by economic analysis. These two intellectual developments were related, with the insistence on "purity" and logical rigour being

² Schlesinger and Schams were probably the most mathematically-minded of those present. Schlesinger had arrived in Vienna in 1919, when, like the von Neumann family, he fled the Communist Revolution of Bela Kun. In his 1914 *Theorie der Geld- und Kreditwirtschaft*, his development of Walras' monetary theory, he used simple mathematics extensively, something which distinguished him in the German-speaking economics literature. (See Morgenstern (1968), Weintraub (1985)). Ewald Schams is a neglected figure, competent in mathematical economics. See, for example, Schams (?).

³ As Menger would later put it, he had "two souls reside within [his] breast" (1973, p.38). On Menger's work in mathematical economics and ethics, see Leonard (1998).

⁴ As pointed out by Borch (1979), in the first edition of the *Grundsätze* we find a paragraph in the first chapter dealing with "Time and Error" (4. Zeitirrtum), and in the revised edition of 1923 - the one introduced in detail by Menger junior - there appear in the second chapter two new parts, one dealing with the time element, (5a. Das Zeitmoment), the other with uncertainty (5b. Das Moment der Unsicherheit). Menger the son was thus quite aware, at this point, of the issue of uncertainty in economics. The time element, in turn, was the subject of Morgenstern (1934b), which was published alongside Menger's in the *ZfN*.

⁵ See Leonard (2004).

part of what was required to preserve economic analysis from political influence. In all of this, the influence of Karl Menger was decisive.

The Limits of Mathematical Representation

It is curious that Menger would later remember Mayer's finding his talk that evening to be too mathematical, for the paper was, as much as anything, about the limitations of the formal treatment of choice behaviour.⁶ It concerned the Petersburg Paradox. Player A offers player B a bet, in which a coin is tossed and B takes 2^{n-1} when heads first occur at n (i.e., if all the first $n-1$ throws show tails, and the n^{th} , heads). Although B's mathematical expectation for such a game is infinite, it is usually observed that B will not accept such a bet, something which, Menger correctly notes, is not so much a logical "paradox" as a discrepancy. Menger then considers three of the existing theoretical resolutions of the problem - based on the perception of a discrepancy between changes in utility and changes in wealth; boundedness of the utility function; and ignoring small probabilities in the calculation of expected utility - and shows why they are insufficient to resolve the paradox.⁷

The "solution" Menger himself reaches is not really a solution at all, but rather a general, qualitative description of the behaviour of a person faced with the question: "how much am I willing to pay for the probability p of winning an amount D , i.e., for the chance (p, D) ?" Several features, he says, can be regularly observed in such kinds of evaluations. First, when the possible loss associated with a bet is large, even a large gain will be undervalued relative to its expected value. Secondly, an individual will generally be willing to risk only a *part* of his total wealth in games of chance of any kind. This proportion, w , will vary from person to person, but will generally be closer to 0 than to 1. Only in cases of extreme desperation will an individual be willing to risk all his wealth on a particular bet. Finally, the behavior of individuals in buying a chance (p, D) will depend on the probability p . When p is very small, it tends to be undervalued, so that a divergence appears between observed behaviour and that conforming to expected values. In general, Menger concludes, chances are undervalued both where probabilities are very small and very large. Only in the middle range is behavior according to expected values likely to be observed. Even here, however, the existence of roulette and other games shows that chances are often overvalued. The probabilities at which the maximum overvaluation occurs for an individual, says Menger, will depend on his wealth, the potential gain, and other personal circumstances.

⁶ See Menger (1979) p. 259; also Craver (1986), p. 12.

⁷ First, as suggested by Bernoulli, it is possible that B's utility of his winnings, which are additions to wealth, is not linear in their monetary value. B's subjective evaluation (utility) function may exhibit diminishing marginal utility of wealth so that his expectation, for a given game, becomes finite. However, such a solution, says Menger, is *ad hoc*: for any given utility function, as long as it is unbounded, a Petersburg game may always be designed to yield an expectation of infinity (See Bassett (1987)). Secondly, therefore, one might impose boundedness on the utility function. But the discrepancy remains, he says, for the reasons that it is clear by introspection that B's unwillingness to bet is also due to the fact that it is very unlikely that he shall win a very large amount, and that there exist games where B would not even bet the amount corresponding to his *finite* subjective evaluation. Imposing boundedness, therefore, is not sufficient. Thirdly, B may disregard small probabilities entirely, taking account of only the first k terms in the calculation of expected earnings. This Menger dismisses as *ad hoc* and as an incomplete description of behavior.

To Menger, the value of a general analysis of this type was that it allowed one to speak with greater precision of personal characteristics. For example, given wealth and a possible gain, a person who is willing to bet at a minimum probability that is smaller than most other people, could be called a *gambler*. One who estimates the likelihood of a gain higher than most others might be called *optimistic*, etc. To the extent that this forms a general, qualitative, description of gambling behavior, said Menger, it provided an explanation of the Petersburg Game, but, again and again, he insists on the limits of what mathematical representation can achieve.

"It should be . . . stressed that in formulating general regularities in behavior with regard to games of chance we have confined ourselves to rather qualitative statements and have refrained from defining specific functions possessing those qualitative properties. This has three reasons: 1) The observed qualitative properties are shared by many, and even by many simple, functions so that there may be no reason to prefer a particular one to the others. 2) An exact description of actual evaluations is not supplied by any of these functions. 3) [T]he numbers and functions involved in the description vary from person to person. A comprehensive function, therefore, would include numerous parameters. In fact, the number of parameters would be so large, and the dependence on these parameters so complicated, that the general function would lack any transparency. And what would be gained by setting up such formulae? Could more than general remarks corresponding to the qualitative observations (asertained directly from the empirical material) be predicated about the functions?" (p. 271).

As already indicated, the reaction to his talk was mixed. Mises, as one of the few economists present familiar with Bernoulli, was quite taken by Menger's proof that the distinction between marginal utility and marginal wealth - which Bernoulli had constructed as a response to the Petersburg Paradox over 200 years ago and which had been regarded ever since as providing a solution - was in fact insufficient to resolve it. Mayer did not like the paper and, as editor of the *Zeitschrift für Nationalökonomie*, advised him against publishing it. In the version finally published, seven years later, Menger alluded to this:

"Those who believe that by writing $f(D,W)$ they introduce a mathematical formulation superior to verbal expressions, are under a misapprehension, just as are, on the other hand, those who when seeing the mere symbol $f(D,W)$ suspect the use of mathematics which, because of ignorance or on the ground of general prejudices, they reject in the social sciences. Simple special functions, however, as the example of Bernoulli's logarithmic evaluation demonstrates, are not in the theory of value borne out by experience as they are in physics. . . . [W]e therefore have refrained from setting up such formulae for $f(D,W)$ and have followed rather the method of the Austrian school of economists - hopefully without being suspected of underestimating mathematics" (p. 272).⁸

⁸ The paper first appeared in published form as a note by Menger (1934a), "Bernoullische Wertlehre und Petersburger Spiel" ("Bernoullian economics and the Petersburg game") in the fifth volume of the proceedings of the Menger's Mathematical Colloquium. The full version was published, seven years after the original talk, as "*Der Unsicherheitsmoment in der Wertlehre*", (1934c) in the *Zeitschrift für Nationalökonomie*. This was done at the behest of Morgenstern, who had become managing editor of the *Zeitschrift* in 1930, a position he held till 1937. In the English translation published in 1979, "The Role of Uncertainty in Economics" Menger says that Morgenstern had shown great interest in the paper ever since the original seminar (1979, p. 259).

The young Morgenstern, on the other hand, was quite taken by Menger's nuanced discussion. It was an encounter with the scientific attitude he had encountered in Whitehead and other recent reading, and, furthermore, it was coming from the son of Carl Menger, whose views on the place of mathematics in economic theory had been quite different and had been perpetuated in the work of Mises and Mayer.⁹ In the eyes of the young economist, the young Menger cut an intriguing figure. Morgenstern thus began to distinguish himself from his teachers, foraging in mathematics to the extent that his training allowed him, discovering the writings of the Vienna Circle. During his stay in Rome in 1928, he wrote with enthusiasm in his diary about attending a conference that featured contributions by mathematicians David Hilbert, Hermann Weyl, Emile Borel and Oswald Veblen, amongst others.¹⁰ In 1929, he could write to his friend Haberler that Moritz Schlick's *Allgemeine Erkenntnislehre* has impressed him more than any reading since Kant, and that he was now reading Carnap's "*Der logische Aufbau der Welt*", which he found to be a "first-class effort". Nothing could be done, he now felt, without a thorough knowledge of mathematical logic and epistemology.¹¹ At the same time, if only in his diary, he became increasingly critical of his mentors, registering his frustration with Mises and complaining about Hans Mayer's political intrigues with Othmar Spann.¹²

In a 1931 overview of the field of mathematical economics for the *Encyclopaedia of the Social Sciences*, he wrote that there was no reason why mathematics might not be applied to the social sciences, and to economics in particular. The objections, he wrote allusively, tended to identify mathematics with the use of the infinitesimal calculus, and to involve the claim that, in economics, one dealt with discretely varying quantities, and with relationships which were not "mechanical". This overlooked the existence of other branches of "discrete" mathematics, he said, and there was nothing inherently "mechanical" about mathematics of any kind: it was, in logical empiricist fashion, simply a machinery for drawing inferences. There were very few instances where

⁹ Members of the Austrian School emphasize the Aristotelian dimension of Menger Senior's a prioriism and his opposition to the use of mathematics. They point to his correspondence with Walras in 1884, in which he said that economists "do not simply study quantitative relationships but also the nature (das Wesen) of economic phenomena. How can we attain to the knowledge of the latter . . . by mathematical methods" (Quoted in Smith 1986, p. 3; See also Smith 1990, 1995). For later Austrian critiques of the employment of mathematics, see Mises (1934) and Mayer (1932). Menger the son saw the matter differently: "How is it that men of such high intelligence and such specific logical-analytic talent as the members of the old Austrian school did not have a better knowledge and understanding of mathematics, especially of calculus . . . even if, for whatever reasons, they decided to refrain from using it in economic theory?" (op cit, p. 44). Part of the answer, he says, lies in the fact that the Austrians came to economics from jurisprudence and government activities, and had not received instruction in mathematics in the *Gymnasien*. "Still, they might have resorted to a self-study from textbooks in later life; and in the 1890's my father indeed started such a self-study, as is clear from a three-page introduction into the elements of differential calculus in his handwriting, which he had bound into his copy of the second edition of Walras's *Eléments*. . . . But I am afraid that he did not acquire an operative knowledge, let alone a critical insight into calculus. The psychological problem is thus reduced to explaining why such eminent minds as the founders (and perhaps also several younger members) of the Austrian School were, as mature men, unsuccessful in their self-study of analysis" (pp. 44-45). He goes on to explain this in terms of the inadequacy of the textbooks, something which gave him a lifelong interest in mathematical education.

¹⁰ See Diary, OMDU, May 27, August 28 and September 4, 1928, OMDU

¹¹ OM to Haberler, March 28, 1929, OMDU, Box 4, Folder Correspondence, 1930-1932, S – Z, translated by Cornelia Brandt-Gaudry.

¹² On Mayer, see Diary, December 22, 1928 and April 19, 1929. In March, between his reading of Schlick and Carnap, he could write: "Friday was the Economics Association. Mises spoke about worn-out methodology, and his concluding talk especially was just impossible. Lots of Jews." (Diary, March 22, 1929).

mathematics was absolutely necessary, he admitted, but it facilitated the prosecution of the argument and was most useful where the problems selected were "too complicated to be tackled by ordinary means" (*ibid*). Walras, for example, had demonstrated the existence of an equilibrium set of prices and quantities by the construction of a set of simultaneous equations, representing the demand for each good and arbitrage conditions.¹³ "Another mark of progress", he concluded, "would be the achievement of a closer integration between the psychological and mathematical orientations, a development which would not be hindered by any fundamental disagreements between the exponents of the two types of economic theory" (p. 368).

Thus emerged the main tension that was to characterize Morgenstern's scientific work in the 1930's: on the one hand, upholding the Austrian conceptual orientation - the "psychological" approach to time, expectations, and equilibrium - on the other hand, in contrast to his teachers, promoting mathematics as the appropriate means of doing so. Just as he had earlier broken free of Othmar Spann's demagoguery, moving into Mayer's and Mises' orbits, so, in the 1930's, would he move on again, gradually shunning his mentors and embracing the mathematicians. Understanding this requires paying attention to Morgenstern's activities as research entrepreneur and policy advisor in an increasingly turbulent environment.

Social Planning and Contemporary Civilization

In September 1930, Hayek sent a memo to the Rockefeller Foundation describing the activities of the *Osterreichisches Institut für Konjunkturforschung*, or Austrian Trade Cycle Institute, of which he was director. He described how the affair had been set up in 1927 by Mises, with the financial help of the Austrian Chambers of Commerce, of Labor, and of Agriculture, the Austrian National Bank, various industry and banking groups, and the Federal Railroads. In the intervening period, the Institute had produced a monthly bulletin of economic conditions, carried out some special investigations, and begun producing monographs, the first of which was Hayek's own 1929 *Monetary Theory and the Explanation of the Business Cycle*. In the near future, they wanted to pursue special studies, including on the history of business cycles in Austria; on the relationship between credit and the business cycle; and the elimination of seasonal fluctuations from time series. With a staff of 5, and 2 research workers, they were stretched and needed more funds, he said, especially to hire short-term researchers: \$3,000 per year for five years would do.¹⁴

Hayek's memo was part of a campaign begun earlier that year by Mises to attract Rockefeller support. At the Foundation, the psychologist, Beardsley Rumml, had been replaced in 1929 as head of the social science division by Edmund E. Day, the business-cycle economist.¹⁵ Day had been trained at Harvard and had up to then been at the University of Michigan. His staff included Miss Sydnor Walker, who remained from the Rumml régime, and the newly appointed John Van Sickle, a Michigan colleague, who became assistant director of the Foundation's social sciences office in

¹³ "Since the number of unknowns is equal to the number of simultaneous equations it follows that the problem of general equilibrium is capable of a theoretical solution", Morgenstern wrote - quite incorrectly as Abraham Wald would soon show (p. 367).

¹⁴ Memo, Hayek to Rockefeller Institute, Sept. 23, 1930, Austrian Institute for Business Cycle Research Records, Rockefeller Foundation Archives, Rockefeller Archive Center, Pocantico Hills, New York, (hereafter AIRC), Record Group 1.1, Series 705, Sub-series S, Folder 36: "Austrian Institute for Trade Cycle Research, Vienna, 1930-1934".

¹⁵ See Craver (1986b).

Paris, before moving back to New York in 1934 and being replaced by Tracy B. Kittredge. Whilst Ruml had been interested in promoting interdisciplinary work, Day preferred to support projects in specific fields. With the collapse of Wall St. in late 1929 and ensuing Depression, he emphasised the urgent need for research on economic stabilization:

"The costs imposed by serious business depression - of demoralization, broken health, disorganized families, neglected children, lowered living standards, permanent insecurity, impaired morale, as well as financial distress - are so appalling when viewed socially as well as individually that no problem of this generation calls more clearly for solution than this of economic stabilization. It is no exaggeration to say that unless the problem can be solved or at least measurably reduced the present social order is in serious jeopardy . . . No more important contribution could be made by the Foundation to the wise development of that social planning and control which seems ultimately so necessary and inevitable if contemporary civilization is to survive"¹⁶

Thus, in the early years under Day, inspired by Charles Bullock's Harvard Economic Service, the Foundation made new grants to economic research institutes, at the University of Oslo, in Rotterdam, Kiel, Bucharest and Heidelberg.

In Vienna, one of the first to seize the Rockefeller opportunity was actually von Mises, who, in 1930, although he viewed social planning and control as antithetical to contemporary civilization, approached Van Sickle for support. The latter sought second opinions elsewhere. At Harvard, Bullock expressed reservations about the excessive Austrian emphasis on theory and deductive methods, as opposed to the empirical methods favoured by the American economists, but he spoke highly of Hayek and Morgenstern and underlined the need to preserve Viennese economics in the face of economic decline and inadequate university salaries.¹⁷ At the League of Nations, Hans Staehle regarded the Austrian institute as "the best equipped institute in German-speaking countries", adding that it was superior to the Berlin Institute, where the director Ernst Wagemann was "not equal to his task", as most of his workers were mere youngsters.¹⁸

In his professional diary, Rockefeller's Van Sickle hesitated. He was concerned that it would apparently be only a matter of time before Hayek received a call from elsewhere, and that Mises, who, because of his Jewishness, could never hope to be more than a Privatdozent in Vienna, was supposedly in negotiation with a German university. He also wondered about the wisdom of funding in light of "present dissension in the SS [social science] field, and the anti-Jewish feeling [which] would complicate future relations of the RF [Rockefeller Foundation] in Vienna".¹⁹ However, he was by and large well disposed towards the "very good men in Vienna".²⁰ A

¹⁶ Day, Edmund E. "Proposed Foundation Program in Economic Stabilization", September 1931, quoted in Craver (1986b), p. 212.

¹⁷ See Letters, Bullock to Day, May 22, 1930, and Bullock to Van Sickle, May 22, 1930, AIRC, Box 4, Folder 36.

¹⁸ John Van Sickle, Diary, May 9, 1930, AIRC, *ibid.*

¹⁹ John Van Sickle, Diary, May 21, 1930, AIRC, *ibid.* Kiel was the university with which Mises was in discussion. Note the irony of Austrian anti-semitism in 1930 sending him in the direction of Germany.

²⁰ *Ibid.*

September dinner with Mises seems to have sealed the affair, and in November 1930 the Foundation guaranteed the Institute a generous \$20,000 for the period till 1935.²¹

From 1928, Hayek and Morgenstern had been co-directors of the Institute, with its tiny staff. Then, in 1931, Hayek did indeed receive his call - from Lionel Robbins at the L.S.E. - and departed, leaving Morgenstern in charge. After the collapse of the Creditanstalt Bank that year, Morgenstern became increasingly involved in public economic debate. As Klausinger (2006) reports, for the next three years, along with Fritz Machlup, Morgenstern wrote in the *Neues Wiener Tagblatt*, advancing Austro-liberal arguments: criticizing the inflationary effects of any credit injections to save banks, opposing exchange controls as a way of defending parity, and favouring the austerity of domestic price adjustment, rather than protectionism, as a way of dealing with the trade deficit. Instead of resorting to public works as a means of countering depression, the Austro-liberals promoted *Auflockerung*, namely price flexibility and the removal of market restrictions in the spheres of both production and employment. Elsewhere in the capital, public speeches by Mises and Hayek promoted the same economic philosophy. Until 1934, when the new corporatist state became hostile to liberalism, Morgenstern was involved in these liberal circles at the intersection of academia and business. For example, his participation in the *Neues Wiener Tagblatt* grew out of discussion with Machlup, Victor Graetz (director of the *Steyermühl* company which owned the newspaper) and Julius Meinl, head of the famous coffee emporium. Others to whom Morgenstern was close included Victor Kienböck, President of the Austrian National Bank, Rost van Tonningen, of the Finance Committee of the League of Nations, and banker Karl Schlesinger. From mid-1932 to mid-1933, he was instrumental in organizing economic policy conferences, aimed at the promotion of liberal policy amongst industry leaders. He also became involved, too, to his intellectual discomfort, in a pump-priming project, advanced by certain industrialists, which aimed at subsidizing the employment of new workers. Throughout the early 1930's, Morgenstern became quite prominent and was rumoured to be favoured for positions of influence, including General Secretary of the *Hauptverband der Industrie*, something that brought him into conflict with Mises, as the post was already occupied by a friend of the latter.²²

When the time came to knock again on the Rockefeller door, in 1935, Morgenstern was able to write a glistening report of the Institute's activities in the interim, mentioning the continued monthly Bulletin, and the consulting activities to government, where, especially in the light of recent political turmoil, their impartiality was greatly respected. The turmoil in question was, of course, the rise of Austrian corporatist state, as of March 1933, a development that put the Austro-liberals – i.e., those of them that remained in the country – on the philosophical defensive. By the end of 1934, however, after a one-year gap in his personal diary, Morgenstern emerged a key advisor to the Austrian state, being a member of the team that negotiated the treaty with foreign creditors of the Creditanstalt, an advisor to that bank and to the Ministry of Commerce on matters of railroad regulation. He was also member of a governmental price control commission associated with the Institute.

²¹ Although Mises was pessimistic as to the immediate future, and believed that union with Germany would ultimately take place in one form or another, he was, according to Van Sickle, optimistic as to the long run future of Vienna as a cultural and economic centre. He regarded as Vienna's first-rate minds, philosophers Schlick, Carnap, and Wittgenstein; economists Hayek, Morgenstern, Haberler, Machlup, Schütz, and Rosenstein-Rodan; and philosophers of law Kaufmann and Schreier. John Van Sickle, *Diary*, Sept. 18, 1930.

²² On Morgenstern's activities as policy advisor, see Klausinger, *op cit*.

In his 1935 report, Morgenstern also put special emphasis on the "purely scientific work", mentioning the publication of several monographs including Hayek's 1931 *Preise und Produktion* (trans. 1937, *Prices and Production*), and his own 1934 *Die Grenzen der Wirtschaftspolitik* (trans. 1937 *Limits of Economics*), the Institute's establishment of a reading-room, and its links to the University by means of lectures and seminars, including those by Karl Menger and Franz Alt. He underlined this theoretical work:

"On the basis of the experiences of the last years I have worked out a program for research which I beg to outline briefly. This program provides for purely theoretical work as well as for empirical studies. These assume even relatively more importance than before; they are necessitated in order to examine theories of the Trade Cycle and procure a basis for new abstract thinking. It is my particular desire to harmonize more than has been done before both ways of research. I am absolutely convinced that abstract theoretical work, even making use of mathematical analysis or of the modern methods of Logic that have not yet been applied to Economics, are just as necessary as the systematic collection of facts".²³

"Economists have so far entirely neglected", he said, "the progress of mathematics and notably of logic during the last 30 years, so that it seems indispensable to subject economic theories of various kinds to the more rigorous test of these new ways of thinking and research".²⁴ He noted the availability of several excellent people from Menger's Colloquium who could work on questions in pure theory, mentioning Abraham Wald in particular.

"Betweenness" in a Cultural Space: the Case of Abraham Wald

Abraham Wald had first knocked on Karl Menger's door at the University's Mathematical Institute, in the autumn of 1927. He was a 25-year old German-speaking, Hungarian-accented, mathematician, of orthodox Jewish family, from Cluj, Rumania. He came from a large family, the father a baker. Because of the conflict between Saturday classes and the Sabbath, Wald had been educated mainly at home by his brother, Martin, an engineer.²⁵ He was particularly interested in geometry, he told Menger, and had been reading Hilbert's *Grundlagen der Geometrie (Foundations of Geometry)*, where he thought that improvements could be made by dropping some postulates and relaxing others. Menger recalls that Wald registered at the university, but was not seen for over two years, as he did not attend classes and had to serve in the Rumanian army. Early in 1930, he reappeared and Menger put him to work on the problem of "betweenness". A point q is "between" the points p and r if, and only if, $p \neq q \neq r$ and the three points satisfy the equality $d(p, q) + d(q, r) = d(p, r)$, where $d(\bullet)$ is "the distance between". Within a month, Wald had characterized "betweenness" in the ternary relations in a metric space, yielding four publishable papers.²⁶ Menger invited him to join his Mathematical Colloquium.

²³ Morgenstern "Report on the Activities of the Austrian Institute for Trade Cycle Research 1931-1935", Feb. 13, 1935, AIRC, Folder 37, Austrian Institute for Trade Cycle Research, Vienna 1935-1936, p. 11.

²⁴ Ibid, p. 14.

²⁵ See Menger (1952). Until World War I, after which the area fell to Rumania, Cluj had been Klausenberg, belonging to Hungary – hence Wald's accent - and part of the Austro-Hungarian empire. On Wald, see also Morgenstern (1951), Weintraub (1985), and various review type articles by Freeman (1967), Menger (1952), and Tintner (1952).

²⁶ Wald 1931a, b, c, 1933.

The Colloquium had been organised by Menger, beginning in 1928, and over the course of the next few years, brought together a number of mathematicians, including Kurt Gödel, Franz Alt, Georg Nöbeling, Olga Taussky, G. Bergmann and a fellow by the name of Schreiber. Among the foreign visitors were the Polish mathematicians, Knaster and Tarski, the Czech, Čech, and, on his annual trips between between Princeton and Budapest, von Neumann. Papers were formally presented and discussed, and later published in the seminar's proceedings, *Ergebnisse eines Mathematischen Kolloquiums*. A glance at that journal reveals a wide range of mathematical topics, with logic, topology, and the theories of dimension, curve, and measure dominating. Indeed, for the first five years, the *Ergebnisse* is without reference to economics or social science.

We have mentioned that many of the Viennese mathematicians at this time were Jewish, at a time of rising anti-Semitism. This traditional Viennese prejudice had been particularly strong just after the War, had declined somewhat in the latter part of the 1920's and had risen dramatically with the onset of economic depression after 1929. This time, it took the form of protests by Austrian Catholic and German nationalist student fraternities against the disproportionate number of non-Aryan professors and students at the University of Vienna, with frequent public demonstrations, class disruptions, violent outbursts and beatings. Matters were not helped by the fact that Vienna's police had no authority in the self-policing University. In anti-Semitic student diatribes, Menger himself was incorrectly labelled as Jewish on at least one occasion, and Hahn, the only member of the prestigious Academy of the University of Vienna who was both Jewish and socialist, was also targeted. To both the Austrian Catholic and pan-German nationalists, the Jewish socialists of Red Vienna were targets of opprobrium.

As one of the *Ostjuden*, or Eastern Jews, Wald stood at the lower end of the established hierarchy amongst the Jews of Vienna. It was families like his that had flooded into the Leopoldstadt, Vienna's quintessential poor Jewish ghetto. He would thus likely have been conspicuously different in accent and appearance from his assimilated counterparts, such as Mises or Schlesinger, who were of Jewish origin, but were culturally integrated. Menger, as an outsider in the Brouwer circle in Amsterdam, a gentile amongst amongst Jews, a mathematician amongst economists, and in a minority at the University in his resolute opposition to German nationalism, was sensitive to difference, to marginality. Thus, when he said that Wald "had exactly the spirit which prevailed among the young mathematicians who gathered together about every other week" at the Mathematical Colloquium, he was not simply referring to his mathematical ability (*ibid*, p. 15).²⁷ In the political climate of Vienna in the early 1930's, the Colloquium not only was a site for collective work in mathematics, but represented stability, intimacy, and shared values.

Because of straitened financial circumstances, Wald was often absent from Vienna, and it appears that, at some point, he became responsible for his ageing parents, something which added to his burdens. In late 1931, he wrote to Menger saying that he could not return to Vienna for financial reasons, but that he had been taking a university course in insurance methods, and was continuing to work on the topology of the k -dimensional interval, on which he was enclosing results. Further letters follow in 1932 with results on axiomatics and the theory of convex spaces. Then, in 1931, Wald was back in Vienna, desperately seeking some position which would allow him to remain in the city, close to Menger and his Colloquium. Given his background, however, and in the middle

²⁷ Elsewhere, Menger notes that amongst his University colleagues, his "friend Hahn was the only mathematician who knew Wald personally. No one else showed the slightest interest in his work" (*ibid*, p. 18).

of the Depression, which perhaps hit Austria harder than any other European country, Wald stood no chance whatsoever for any kind of University appointment. So Menger turned on his behalf to Schlesinger and Morgenstern.

Schlesinger was a banker with the time, leisure and inclination to engage in extra-curricular intellectual pursuits. In 1914, he had published a book on the Walrasian system, and was an active participant in the Viennese Economic Society. According to Menger, he was interested in improving his mathematical skills and therefore receptive to the offer of Wald's tuition. Out of this conjunction came Schlesinger's 1933 paper on the modified Cassel system, which introduced inequalities into the general equilibrium problem and thus dispensed with Walras' simple counting of equations and unknowns.²⁸ Wald, in turn, produced several papers dealing with systems of equations in mathematical economics, including the production and exchange variants of the Walrasian general equilibrium equation system, and the Cournot duopoly model.²⁹

Morgenstern's relationship with Wald began in earnest in 1933, as indicated by a small grant from the Rockefeller Foundation to the Institute for the employment of Wald "to undertake a methodological study of the decomposition of statistical series".³⁰ For the next few years, Wald worked as researcher at the Institute. In early 1935, Morgenstern wrote to the Foundation, praising Wald's statistical and mathematical work, which, he said, was very reassuring and indicated that there was "still very much purifying to be done".³¹ Amongst his accomplishments here, Wald constructed a procedure for seasonal decomposition, different from that of Persons' method of "link relatives", which Morgenstern presented at Louvain and Paris that year.³² When applied to Austrian unemployment data for the period 1923 - 1934, Persons' method of deseasonalization yielded a residual series which "tracked" the original series reasonably well until 1932 - in the sense that the deseasonalized data were above the annual average when the seasonal oscillation was positive, and vice versa - but then "contradicted" the original data in the remaining years, lying

²⁸ The Colloquium's developing interest in economics and social science appears in the fifth volume, which concerns the meetings of 1933-34, with reference to two notes by Menger, on "Bernoullian economics and the Petersburg game" and on the relationship between finite sets and the formalization of ethics, and to the papers of March 1934 on general equilibrium by Schlesinger and Wald. The notes by Menger (1934a,b) were essentially short communications concerning what were published subsequently as (1934c) and (1934d) respectively. On the general equilibrium papers of Schlesinger (1935) and Wald (1935), see Weintraub (1985), pp. 59-107, and Ingrao & Israel (1990), pp. 175 - 210.

²⁹ See Wald 1935, 1936a, b. The duopoly paper showed how the model's equilibrium depended on the shape of the demand function for the commodity faced by the two firms. For example, the existence of a unique, stable, equilibrium point requires that the demand function cut the price and quantity axes and have a negative first derivative and non-positive second derivative. See Tintner 1952, p. 22. A third general equilibrium paper by Wald would be lost in the flurry in 1938.

³⁰ July 24, 1933, Research Aid Grants, Paris, Rockefeller Foundation, Box 4, Folder 36, AITCR, Vienna, 1930-1934.

³¹ February 13, 1935, "Report on the Activities of the Austrian Institute for Trade Cycle Research 1931-1935", Box 4, Folder 37, AITCR, Vienna, 1935-1936.

³² See "La nature et le calcul des variations saisonnières", Memorandum per Dr. A. Wald, distribué à l'occasion de la conférence de Dr. Oskar Morgenstern, Wien, 6 mai, 1935, à l'Institut Scientifique des Recherches Économiques et Sociales, Paris, located in KMIT. A time series $\square(t)$ is assumed to be combined of trend, seasonal and accidental components: in Wald's notation, $f(t)$, $S(t)$ and $Z(t)$. Persons' decomposition method assumed, amongst other things, that the seasonal component was multiplicative, i.e., $S(t) = f(t) \cdot p(t)$, where $p(t)$ is a periodic (12-month) function of time. Wald's innovation was to assume that seasonal variation took the form $S(t) = \square(t) \cdot p(t)$, where $\square(t)$ is a non-negative function that varies "slowly" with time.

below the annual average when the oscillation was positive and vice versa. Wald's correction produced a better fit, and Morgenstern's talk included a graphic display of the results. Wald's work here culminated in a 1936 book, *Berechnung und Ausschaltung von Saisonschwankungen*.³³

Wald also provided Morgenstern with instruction in basic mathematics - algebra and differential calculus – thereby succeeding Franz Alt in that role.³⁴ Wald had a considerable impact on him, and Morgenstern wrote frequently of him in his diary.³⁵ By the end of 1935, Wald was assuring him that he would soon understand nearly everything in mathematical economics, which Morgenstern noted with delight. But these were private writings. Publicly, the imperious Morgenstern seems to have kept Wald at arm's length, with letters remaining formal even years later, and he never wrote publicly of taking lessons from him. Although this rift between private rumination and public decorum was quintessentially Viennese, it is also possible that there was a certain ambivalence in Morgenstern's relationship with the gifted *Ostjude*. After all, it had been only a decade since he wrote of the pollution of German - by foreign culture, and much less since he cast aspersions on the Jews of the Mises and Geist Kreisen.³⁶

In his relationship with Menger, however, Morgenstern's exercise of power could not take the same form. The former was a faculty member and a mathematician of international reputation. Their relationship was more equal. In many respects also, Menger's work was more directly useful to Morgenstern than was Wald's. While Morgenstern could trumpet Wald's successes to others, whether in seasonal analysis or general equilibrium theory, he could not incorporate them directly

³³ On Wald (1936) see Morgan (1990) p. 84, n. 10.

³⁴ Franz Alt (b. 1910) entered the University of Vienna as a student of mathematics in 1928, and was a participant in the Menger Colloquium and Hahn's seminar. Upon graduation, he recalled in a 1997 interview, Menger felt guilty that he could not provide him with some employment and recommended him to Morgenstern, who appointed him as private tutor in mathematics at 20 Schillings an afternoon. "Morgenstern . . . very interesting, very intelligent. . . . He was convinced that mathematics was important . . . He told me once that he had wanted to study physics, but right after World War I all the interest was in the social sciences, and so he felt he should go into that . . . He had me help him read books on mathematical economics. It helped that I knew languages. We read English mostly. There was a man named Bowley who wrote a book here on mathematical economics. It was just as interesting for me as for him. I had to prepare each meeting, read a chapter in the book, and the we discussed it. He knew as much about it as I did, but perhaps once in a while I could explain something". (From a May 1997 interview with Alt, at his New York home, conducted by Seymour Kass, Bert Schweitzer, Abe Sklar, and Mrs. Annice Alt.). Through Morgenstern, Alt met various figures, including Oskar Lange, and Paul and Alan Sweezy, and was led to publish an 193? article on utility theory in the *Zeitschrift für Nationalökonomie* (see Alt 193?). In 1938, Alt moved to the U.S., where he was introduced by Morgenstern to Harold Hotelling. The latter, in turn, introduced him to Charles Roos, formerly of the Cowles Commission, whose 193? book, *Dynamic Economics*, Alt had reviewed for the *Zeitschrift*, and who had by then left Cowles to set up a private economic forecasting consultancy, the Econometric Institute, in New York. Alt later left economics and made his career in computing. I am grateful to Professor Seymour Kass for permission to quote from this interview, the manuscript of which has been deposited in the Vienna Circle collections both at the University of Pittsburgh and in Vienna.

³⁵ "Yesterday more mathematics. I am beginning to see deeper and deeper, and through the ongoing implicit repetition it all seems to settle down" (Diary, Oct. 19, 1935, OMDU). And later: "Another mathematics lesson, very interesting. I feel as though I am making real progress. Wald told me of his new works. An amazing thing. It isn't enough, as Walras assumed, to consider only monotonically decreasing utility functions, because he [Wald] proved that with many of them, simple exchanges never lead to an equilibrium! Similar paradoxes for the addition of demand curves, which were considered before to be totally harmless! That should have far-reaching consequences . . . Wald is really intelligent. I consider these works to be very important; they throw new light on the application of mathematics to economics. One will not be able to do without these at all" (Diary, Nov. 2, 1935, OMDU).

³⁶ See Leonard (2004).

into his own personal, theoretical, work. With Menger's papers, on the other hand, he was able to make suggestions, albeit imprecise ones, as to how the mathematician's ideas might be useful in resolving questions in economics. Thus, he felt Menger's and Wald's influences in subtly different ways, each operating through the filter of power relations connecting him to them.

In Menger's Orbit

The deepening of the relationship between the economist and the mathematician coincided with the period of political tension and change in Vienna. After Hitler's rise to power in early 1933, the Austrian government under Dollfuss suspended the constitution, marking the beginning of the corporatist regime. In March of the following year, that regime cracked down on the socialist government of Red Vienna, bringing canon fire and upheaval to the city. In this context, Menger turned earnestly to the mathematics and logic of social science, and he became obsessively concerned with the demarcation between analysis and politics. In the latter regard, Menger's list of sinners included Neurath and, even if he was politically closer to him than to the socialists, Mises. It was one thing to be of liberal inclination; it was quite another to say that it could be legitimized by science. As for Morgenstern, having spent several years promoting liberalism in the policy sphere, and now become perhaps the city's most prominent economist, he found himself adviser to a regime that had no sympathies for either liberalism or socialism. He was in a difficult position. His response was to follow Menger in this insistence on the integrity of (economic) science. It was almost as if, in those critical years in the mid-Thirties, they together carved out a sacred sphere, a psychological sanctuary, impervious to the surrounding political pressures. I think it also fair to say that this exploration of the logical intricacies of economic theory, in turn, induced in Morgenstern a certain humility, something he increasingly regarded as absent in Hayek and Mises.

In March 1933, Morgenstern wrote: "Saturday I was to dinner at Menger's. He gave, in a manner of speaking, a lesson on curve and dimension theory. We talked about a math. course that he wants to give, which will probably be excellent. We plan to meet again in August; until then, he is going to read lots of books and articles which I have lent him, and we are going to construct an axiomatics of ? [illegible]. It could be of importance" (OMDU, July 11, 1933). Throughout 1934, that bond strengthened, with Morgenstern spending part of his holidays with Menger and fiancée in Ramsau and Strobl, and then with Menger alone in the Burgenland. Morgenstern read Menger's book on ethics, written in response to the civil strife of that year, and, in a seminar taught with Richard Strigl, he used Menger's paper on the Petersburg Paradox.³⁷ He attended the International Congress of Philosophy in Prague with Menger and Schlick, and in Vienna there were teas and social gatherings with Menger, Wald and Institute economists Richard Kamitz, Ernst John and Strigl. All of this worked its way into Morgenstern's writings of the mid-Thirties.³⁸

Like much of Morgenstern's work in this period, "The Time Moment in Economic Theory" ("*Die Zeitmoment in der Wertlehre*") is not always clear and offers little by way of constructive detail, the

³⁷ "We have just started a mathematical course which is given by the famous mathematician Karl Menger" (OM to Eve Burns, Mar. 6, 1934, OMDU, Box 4, Corresp. 1928-1939, Burns, Eve M.). On Prague, see Morgenstern's Diary, Nov. 4, 1934, OMDU. Then: "Monday a very good seminar, very enlightening. We started to clear up risk theory. Menger's article on the Uncertainty Moment was much help" (*ibid*, Nov. 29, 1934).

³⁸ For an account of the writings by Menger that were influential here, see Leonard (1998).

salient points being largely criticisms of existing theory.³⁹ Drawing on Schlick's 1930 *Questions of Ethics* and Menger's "Uncertainty" paper, he dismisses the criticism that, because it can be applied to explain any observed economic choice behaviour, utility theory is tautological. For individuals may well act "not according to the most dominating pleasure motive but . . . [according to] the strongest impulse of displeasure". Unlike the essentially analytical theorems of logic and mathematics, the contradiction of which would be absurd, utility theory can be contradicted by observation. This discussion comes straight from Menger, whose work has shown, says Morgenstern, that "no one is really fully aware of the relationship between 'logic and economics'" (p. 152).⁴⁰

The bulk of the paper is devoted to rather vague considerations on how time might be incorporated into utility theory, also inspired perhaps by Mayer's criticism of the Walrasian system. His main target here is H. L. Moore's (1925) "A Moving Equilibrium of Demand and Supply", which introduced time coefficients into the Walrasian system of equations to yield a moving equilibrium. Morgenstern criticizes the Walrasian system for assuming infinitely fast reaction times, presumably of prices, and suggests having different reaction times for different prices. This, he says without further elaboration, "should give results". This leads to a natural emphasis on foresight and uncertainty, issues that "might, for reasons not to be given here, make even better material for the application of mathematics to economic theory than was evident until now in an unfortunately large number of cases of mathematical economics" (p. 158).⁴¹

More successful was his 1935 "Perfect Foresight and Economic Equilibrium". Here, Walras and Pareto are criticised for failing to make explicit their assumptions about what subjects can foresee, and Hicks (1933) for assuming that perfect foresight is a precondition for equilibrium.⁴² We must ask, says Morgenstern, "the foresight of whom? of what kind of matters or events? for what local relationships? for what period of time?" (p. 171-2). Without this, the entire concept of general equilibrium is jeopardised. As it stands, the assumption of complete foresight implies that individuals have complete insight into all economic processes concerning prices, production, and income. Given the interdependence and complexity of the economic system, this implies "incredible powers on the part of the economic agent", who must not only know exactly the influence of his own transactions on prices but also the influence of every other individual, and of

³⁹ Introducing the paper to Frank Knight, Morgenstern wrote: "I put much thought and work into that article and, what's more, much breadth results, the limits of which I cannot see at the moment". OM to Knight, Sept. 12, 1934, OMDU, Box 6 Corresp. 1928-1939, Knight, Frank H..

⁴⁰ See Menger (1935), p. 278, n. 17.

⁴¹ Responding to the paper, Knight pulled no punches: "I read your article . . . and must say frankly that my reaction was not very enthusiastic. Not that I found anything to disagree with, but that it seemed to me the whole argument was rather in the domain of such a degree of refinement of conception and doctrine that I did not get the feeling of very great importance in the contribution" (Knight to OM, undated, OMDU, Box 6, Corresp.: 1928-1939, Knight).

⁴² In the opening paragraph, Morgenstern refers to the discussion of Wald's work on general equilibrium in Vol. 6 of Menger's *Ergebnisse*, 1935, which revealed that: "The mathematical economists present an especially noteworthy example [of logical carelessness]. They, indifferent to whether it is a question of a general or of some particular equilibrium, have been content to assert that there are present as many equations as there are unknowns, rather than from the start proving in an exact mathematical fashion that there is a solution at all - and a unique solution - for these equations" (p. 169). Strident tones from one who, only four years previously, had made this very claim of the Walrasian system (see 1931, p. 367).

his own future behavior on that of the others".⁴³ Such persons of perfect foresight are not mortals, he says, but "demi-gods" (p. 173). Not only does it imply that economics has posited the existence of an economic subject that perfectly knows economic science already, but it leads to a paradoxical situation of the Holmes-Moriarty type. What in his *Wirtschaftsprognose* of 1928 had provided an analogy enabling Morgenstern to refute attempts at economic prediction, was now serving to undermine the existence of a general equilibrium: "Unlimited foresight and economic equilibrium are thus irreconcilable with one another". The theoretical matters concerned "are so extremely complicated that only far-reaching employment of mathematics could help to suggest the reciprocal dependencies. The relationship between human behaviors dependent on one another, even without the assumption of foresight, is almost inconceivably complicated, and it requires cogent examination" (p. 174): "Up to the present time, the only examination of a *strictly* formal nature about social groups, even though it is carried out in another field and is limited to the co-existent individuals independent of one another, is a work by K. Menger, [*Morality, Decision and Social Organization*, 1934] which it is hoped, will become known to economists and to sociologists because of its importance in laying the foundation for further work" [p. 174-5].⁴⁴

Although he never submitted it to the *Journal of Political Economy*, as he considered doing,⁴⁵ the paper was a better success, and captured the interest of Menger and Wald.⁴⁶ Hayek, too, liked it.

⁴³ Innocenti and Zappia (2004) point out that, in the discussion of perfect foresight, Morgenstern's target here is also Hayek, following his 1933 lecture in Copenhagen, "Price expectations, monetary disturbances and malinvestment" (reprinted in Hayek 1939, *Profit, Interest and Investment and Other Essays on the Theory of Industrial Fluctuations*, London: Routledge & Kegan Paul, pp. 135-56). Hayek noted that equilibrium theory was now taking account of the time factor by making assumptions about the attitude of persons towards the future, i.e., "essentially that everybody foresees the future correctly and that this foresight includes not only the changes in objective data but also the behaviour of all other people with whom he expects to perform economic transactions" (1933, pp. 139-140, quoted in Innocenti and Zappia, p. ?). In his 1935 condemnation of the contradiction implied here, Morgenstern actually quotes Hayek verbatim, without explicit reference.

⁴⁴ He also makes a vague suggestion concerning the application of Russell's Theory of Types to questions of economic knowledge, proposing that degrees of knowledge of the economic system might be grouped into different types, ranging from low to high. Even given the same economic resources, he seems to say, different levels of knowledge and different degrees of insight might yield different kinds of equilibrium (pp. 176-77).

⁴⁵ "I have a certain interest to have [sic] this article appear in English because Mr. Keynes is preparing a book on the theory of money largely based on the element of expectation and anticipation" (OM to Knight, Dec. 18, 1935, OMDU). Morgenstern's resistance to Keynes' economics is a recurring theme in this period. In a letter to Eve Burns in 1934, he claimed to have proved that, in Keynes' theory of money, "his equations completely don't hold up" (OM to Burns, Mar. 6, 1934). Then in his diary in 1935, he wrote: "Wald finds my article on Keynes mathematically alright. Now I am going to prepare it for publication and I am going to send it to Chicago" (OMDU, Oct. 26, 1935). Writing to Haberler on the General Theory, he said: "Keynes' book is simply horrible, to the extent that I have read it" (OM to Haberler, Apr. 4, 1936, OMDU, Box 5). To Morgenstern, Knight wrote "What do you think of Keynes' book? . . . a couple of friends whom I consider pretty competent judges say outright that Keynes is losing his mind" (Knight to OM, May 1, 1936, OMDU Box 6). See also below in connection with his 1934 *The Limits of Economics*, where he attacks the General Theory's lack of rigour. I have been unable to trace any of Morgenstern's other writings on Keynes.

⁴⁶ "Yesterday I had lunch with Karl Menger. . . . we quickly discussed 2 1/2 hours. He had carefully read the article on Foresight, agrees, and wants me to deal more with these interesting questions. He is now busy with completing a large work on the calculus of variations, but then he wants to immediately return to social-scientific questions. It was, like always with him, a very stimulating meeting" (OMDU, Sept. 11, 1935). "I believe that everything is correct", wrote Wald to Morgenstern. "One can also understand by 'foresight' that the economic subject has a subjective conviction to foresee any kind of economic things, which however do not have to be congruent with reality. Foresight in this sense I want to call 'subjective foresight'. The complete subjective foresight of an individual then means the subjective conviction that

From London, he wrote: "It will interest you that we recently had an interesting discussion about your essay in our seminar . . . the results were really valuable and enlightening".⁴⁷ He had asked somebody to write down the proceedings of the discussion, which, if good, he would send for publication in the *ZfN*. No such paper appeared in the *Zeitschrift*, but when one rereads that well-known meditation on equilibrium theory of two years later, Hayek's "Economics and Knowledge" paper in *Economica*, many of the themes broached by Morgenstern surface again, and even Menger's work on ethics is cited for its promise. Like Morgenstern, Hayek emphasises the relationship between equilibrium and foresight, but while Morgenstern saw perfect foresight as a form of omniscience that became self-defeating once placed in a social setting, Hayek took a different tack:

"[T]he concept of equilibrium merely means that the foresight of the different members of society is in a special sense correct. It must be correct in the sense that every person's plan is based on the expectation of just those actions of other people which those other people intend to perform, and that all these plans are based on the expectation of the same set of external facts, so that under certain conditions nobody will have any reason to change his plans. Correct foresight is then not, as it has sometimes been understood, a precondition which must exist in order that equilibrium may be arrived at. It is rather the defining characteristic of a state of equilibrium. Nor need foresight for this purpose be perfect in the sense that it need extend into the indefinite future, or that everybody must foresee everything correctly. We should rather say that equilibrium will last so long as the anticipations prove correct, and they they need to be correct only on those points which are relevant for the decisions of the individuals" (ibid, pp. 41-42).

Thus, for Hayek, the salient questions concern knowledge - who knows what, or believes what, about the economy, and about others? These are empirical questions, ignored by mathematical equilibrium theory, the Pure Logic of Choice, and, as Hayek says allusively (p. 54), the *a priori* of older economists. But having highlighted this empirical question, Hayek subtly shifts gear. For it is clear, he says, that economics, poor specification of knowledge or not, has come closer than any other social science to answering "that central question of all social sciences, how the combination of fragments of knowledge existing in different minds can bring about results which, if

the person has the capacity to form an overview of all future economic phenomena. The full subjective foresight of two individuals need not necessarily be in agreement. The assumption that every economic subject has full subjective foresight could be free of contradiction. There are functional connections between subjective foresight and different economic phenomena. The assumption that every economic subject has full foresight in the usual sense means that every economic subject has the same full subjective foresight, and that this is congruent with the future true turn of events. Such an assumption then leads to a contradiction when situations come to pass where the economic subject wants to adjust his actions so that they are in opposition to his evaluation of the foresight of other economic subjects. This is probably the case in economics. But there are also conceivable areas where human actions foresight play an essential role, and nevertheless full foresight in the objective sense would be free of contradiction" (ibid). Letter, Wald to Morgenstern, Aug. 2, 1935, KMIT. In the same letter, Wald mentions having begun reading "the book by Weber", suggested by Morgenstern, which, he found, gave a good orientation of many problems in economics, but treated them "rather superficially and not strictly". This was probably Weber's essays on *Economy and Society*.

⁴⁷ Letter, Hayek to OM, Feb. 9, 1936, OMDU. This time, Knight too was enthusiastic: "It seems to me that in your article on perfect anticipation you have done a major piece of work". He went on to add that "the market for high grade economists in this country seems to be quite 'bullish' at the moment. Are you interested?" (Knight to OM, Mar. 12, 1936, OMDU, Box 6, Corresp.: Knight). Haberler too wrote expressing his admiration (See Haberler to OM, July 30, 1935, OMDU, Box 5, Corresp. 1928-1939, Haberler, Gottfried).

they were to be brought about deliberately, would require a knowledge on the part of the directing mind which no single person can possess" (p. 52). This allows Hayek to downplay the extent to which the knowledge question opens up any fruitful line of empirical research. It is not clear, he says, that such research would teach us anything: it is more important to be clear about the principles and, he says opaquely, about when the argument becomes subject to verification.

Here, we touch on the central, subtle, issues separating Morgenstern and Hayek. Although Hayek, emphasizing the importance of knowledge, was willing to skirt dangerously close to the suggestion that inferior knowledge placed the economic equilibrium in jeopardy, in the final analysis, given his political commitments, he retreated to the safety of oblique, invisible-hand type references to the superiority of the liberal market order. By the mid-1930's, Morgenstern was no longer willing to take such a step. As long as the logical underpinnings of the theory remained inadequate, he was ready to emphasize the possibility of disorder, disequilibrium. He did not have a blind faith in the market order. He was even ready, as we shall see, to muse about changes in people's mentality and about the desirability of governmental intervention - a far cry from Austroliberalism. At this point, Hayek and Morgenstern, who had shared so much by way of formative influences, were on separate scientific and political paths, and the distance separating them would ultimately widen to that which separated the *Road to Serfdom* from *the Theory of Games and Economic Behaviour*.

Morgenstern's "Logistics and the Social Sciences" of 1936, was essentially a rebroadcast, to an economics audience, of Menger's 1932 public talk on "The New Logic". Only by adopting a formal language, he says, can one remove the "traps and contradictions of a sentential language" (p. 398). Menger's book on ethics, fashioned as a response to the political dilemmas of 1934, was now appropriated by Morgenstern for different purposes: "It would obviously be important if one could formalize the achievement of a concrete economic policy measure in the same manner as this has been done for normative systems. . . Although space forbids [entering] upon the relation which Menger's book has to economic theory [it] may suffice to mention that his logic of wants or wishes would provide a most useful model for a logically satisfactory economic theory of wants" [p. 404].

The above papers were intended by Morgenstern as chapters in a book to be called *Time, Profit and Economic Equilibrium*.⁴⁸ With the exception, perhaps, of the "Perfect Foresight" paper, they accomplished little theoretically, but they illustrate perfectly how Morgenstern grappled with theoretical emphases inherited from the Austrian economists yet turned to formal mathematical work for clarification. He drew steadily away from his mentors.

The Limits of Liberalism

"Yesterday in the Nat. Economic Society, Menger gave an excellent presentation about the law of diminishing returns. It was an exemplary piece of work for the proof of the necessity of exact thinking in economics. It was interesting that Haberler failed totally in the discussion . . . Of all these exact things he, by far, doesn't understand the most essential. Mises talks pure nonsense"

Morgenstern, Diary, Dec. 31, 1935

⁴⁸ See OM, Diary, Dec. 9, 1934, OMDU.

"Ludwig von Mises gave stimulating lectures without, however, clearly separating the ideas of economic theory (which he presented with an idiosyncratic opposition to the use of even simple mathematics) from his idea of complete *laissez-faire* "

Menger (1994), p. 11

In Morgenstern's early years, Mises was a silent presence, never featuring explicitly in his work, but exerting an influence nonetheless on his writings and career. Morgenstern's critique of Institutionalism and prediction was very much in the spirit of Mises, and his assuming the helm at the Institute could hardly have been done without the active encouragement of its founder. Throughout the early 1930's, Morgenstern's economic commentary and policy advice was very much in the spirit of the Austrian liberal economists.

As the decade unfolded, however, resistance towards Mises became characteristic of Morgenstern, explicitly in his diary, by allusion in his writings. All of this was under the influence of Menger, who resisted the elder scholar's opposition to the use of mathematics, and was keen to point out the limitations of his style of argumentation. He also took umbrage at the way in which Mises referred to logic and mathematics when reinforcing his "scientific" liberalism, for, following his experience with Brouwer, and even Hahn and Neurath, Menger was exceedingly sensitive to any intrusion of normative or political preference into scientific work. In Menger's company, Morgenstern learned to reject much of Mises' philosophy of economics, including his a prioriism, his views on mathematics, and the way he used the discipline to justify *laissez-faire*.

A sense of Mises' priorities at the time may be found in his 1933 *Grundprobleme der Nationalökonomie*, later translated as *Epistemological Problems of Economics*. Here, Mises continued his onslaught against German historicism, arguing that the study of the unique and unrepeatable events of history could never lead to theoretical insight, that theoretical understanding was *a priori*, being rooted in the nature of human action, and constituting the prior analytical scheme by which one selected amongst the confusing mass of data presented by historical reality. The insistence by Sombart and the *Kathedersocialisten* on empirical methods, and their arguments against the possibility of a universally applicable theory, were, Mises argued, rooted in their political bias towards interventionism. Were they to concede that humans, throughout known time and space, were purposeful in their behaviour, directing it towards improvement of their situation, entering increasingly into economic exchange, and generating the economic phenomena of markets and prices, they would, said Mises, be forced to admit the universality of economic theory. They would also concede that the liberal order was the system of political organisation that best facilitated the unhindered pursuit of economic ends by individuals: "[T]he science of economics proves with cold, irrefutable logic that the ideals of those who condemn making a living on the market are quite vain, that the socialist organization of society is unrealizable, that the interventionist social order is nonsensical and contrary to the ends at which it aims, and that therefore the market economy is the only feasible system of social cooperation" (p. 196). This was Mises' message, repeated throughout the various essays of *Epistemological Problems* and later expanded in his 1949 *magnum opus Human Action*.

A subtheme was his opposition to the use of mathematical formalism in economics, his main argument being that it was not only unnecessary, being merely an embellishment of insights gained independently of mathematical reasoning, but harmful, in that it induced a simplistic, mechanical, perception of the social domain. The problems faced in the social sciences are so complex, says

Mises, that "even the most perplexing mathematical problems" appear simpler. Those who wish to resort to mathematical methods are welcome to it, he says, but "[t]hose theorists who are usually designated as the great masters of mathematical economics accomplished what they did without mathematics. Only afterwards did they seek to present their ideas in mathematical form. Thus far, the use of mathematical formulations in economics has done more harm than good" (p. 116-7). He goes on to condemn the Trojan horse of "mechanism" smuggled in with mathematics, as we have discussed earlier. He points to the natural sciences, where the role of mathematics is different from that in the social sciences, insofar as the discovery of empirically constant relationships is possible, but similar insofar as "even the mathematical sciences of nature owe their theories not to mathematical, but to nonmathematical reasoning" (p. 117).

To Menger, statements like this were naive. They suggested that Mises was unaware of the distinction between quantification and the use of mathematical symbolism, was unfamiliar with the generative role of mathematics in the development and refinement of concepts in physics, and viewed mathematics as some sort of uncontested, homogeneous, and neutral tool, to be "applied", in the natural sciences, when the occasion demanded. Little wonder that Menger, with characteristic restraint, described Mises' opposition as "idiosyncratic".

Mises also regarded economic theory, the best developed branch of the science of human action, as *a priori*, not empirical:

"Like logic and mathematics, it is not derived from experience; it is prior to experience. It is, as it were, the logic of action and deed . . . logic and the universally valid science of human action are one and the same . . . What we know about the fundamental categories of action - action, economizing, preferring, the relationship of means and ends, and everything else that, together with these, constitutes the system of human action - is not derived from experience. We conceive all this from within just as we conceive logical and mathematical truths, a priori, without any reference to experience" (pp. 13-14).

After his experience with Intuitionism, Menger was all too familiar with justifications of mathematical and logical truths "from within". Looking "within", Brouwer had found grounds to reject the axiom of choice, the law of the excluded middle, and non-constructive existence proofs. Menger was highly suspicious of appeals to intuition, the authority "within", as the basis for any kind of mathematics, as they usually translated into attitudes of intolerance. His counter-attack against Brouwer was a gesture against monotheism in mathematics (and his rejection of Neurath's campaign for Unified Science was informed by the same attitude). In that counterattack, Menger emphasised the possibility of multiple logics, and so he was especially sensitive to Mises' cavalierly appealing to "the" logic in order to undergird his conception of human action. Also, Menger's work on the Petersburg Paradox had emphasised the *empirical* nature of the question: some people accepted very favourable bets, others did not. Recourse to *a priori* reasoning here did not carry one very far in determining how individuals behaved: one would have to know much more about their particular circumstances. One can reasonably conclude that Menger learned to regard Mises with suspicion, viewing his a priorism as scientifically inadequate and rejecting the way in which he incessantly sought to put economic theory to political use.

Certainly, there was much in Mises' writing with which Menger could agree: his rejection of Spann's Universalism, his Austrian emphasis on individualism as the appropriate methodological

approach in social science, his distinction between "cold, hard" science and the consolations of metaphysics. Menger would also have completely endorsed Mises' nominal separation of the irrefutable "facts" of economic science from the domain of political or ethical choice, but, again and again, Mises himself blurred the very boundaries he proclaimed to maintain. Notwithstanding his claim that ethical choice and economic science occupied different realms - that even if economic theory pointed to the efficiency of classical liberalism, one was always free to reject it on political grounds - Mises' entire rhetoric in *Epistemological Problems* is intended to promote the politics of laissez-faire. This is presented as the reasonable political stance issuing from economic science, that collection of a priori, not empirical, truths, evident to all clear-thinking observers. This Mises reinforced with frequent reference to the natural sciences, to logic, and to mathematics, most of which would have appeared foreign, and therefore inassailable, to his economist audience, but not to Menger, who knew more than Mises about actual practice in all of these fields.

Thus, between Christmas and the New Year, in 1935, Menger explicitly challenged Mises on a question of logic and proof, presenting a paper on the law of diminishing returns to the Viennese Economic Society. He was responding to a claim by Mises, in his *Grundprobleme*, that certain propositions of economics could be proved, an example being the law of diminishing returns.⁴⁹ In the paper, later described by Schumpeter as a reading of "the logician's riot act" to economists (1954, p. 587) and by the author himself as the first instance in economics of a clear separation between the question of logical interrelations among propositions and that of empirical validity, Menger examined the existing proofs of the law of diminishing returns. Focusing on Wicksell, Böhm-Bawerk and von Mises, he took their analyses apart with a fine tooth comb, showing how they fail "to meet the requirements which logic places on a sequence of inferences intended to constitute a proof". The talk created something of a stir, and, as indicated by the diary quotation with which we opened this section, it impressed Morgenstern, adding to a rift with Mises well in the making by then.⁵⁰

In mid-1933, Morgenstern had written to Hayek that he was completing a book - "mainly a summary of discussions . . . with practitioners", "for a wider audience", that would not "go too much into methodological details". Sending a copy to Knight, in early 1934, he confirmed that his "methodological line [was] rather different from the one followed by Robbins, Mises and Hayek".⁵¹ The book in question was his 1934 *Die Grenzen der Wirtschaftspolitik*, translated in 1937 as *The Limits of Economics*. A rambling book, it is critical, rarely constructive, and targets a range of established economists, including Robbins, Mitchell, Keynes and von Mises. Indeed, it is for the latter that Morgenstern reserves his sharpest barbs.⁵²

⁴⁹ This was published as (1936). For Menger's recollection of the time, see (1979, p. 279).

⁵⁰ From London, Hayek wrote to Morgenstern: "I have heard from Schütz about a discussion between Mises and Menger about the law of diminishing returns, and according to this report there seemed to be confusion on both sides. I would very much like to talk about this if I could get more detailed information about the content of this discussion beforehand. Supposedly a longer correspondence took place and a manuscript by Menger is available. Could one see some of this?". Hayek to OM, Feb. 15, 1936, OMDU, Box 6, Corresp.: Hayek.

⁵¹ Letter OM to Hayek, July 11, 1933, OMDU. Letter, OM to Knight, February 9, 1934, OMDU.

⁵² Of the original German version, a reviewer, Henry Laufenburger, wrote in the French *Revue d'Economie Politique*: "[Morgenstern] believes in the autocratic State which, according to him, can resist the demagogic demands of the parliamentarians, form long-term economic (five-year) plans and assure a better distribution of wealth. Without doubt, Mr. Morgenstern would like to have dictatorial power subject to certain control, but this would be organized by the

"[T]here are but few sciences", writes Morgenstern, "which are in such an objectively unsatisfactory condition as economics" (p. 19). The discipline is riddled with "value judgments". In the *Foreword*, he reiterates Robbins' emphasis on the requirement of rationality of economic policy, and the need for "absolute precision of thought . . . when we are forced to be the unhappy witnesses of an almost unprecedented decay of intellectual life in so many countries" (p. vi). Widespread economic depression throughout the early 1930's pointed to the importance of directing theory towards "the mastering of practical life", without which it was but "an intellectual plaything, similar to chess, and [serving] to satisfy only a perverted desire for purely mental exercise" (p. 4). But this was not an argument for empirical studies, he says, for Whitehead (1925) has shown that it is "impossible to grasp reality without the construction of theoretical formulae", that the "utmost abstractions are the true weapons with which to control our thought of concrete facts" (p. 6). Thus Morgenstern rails against the redundant doctrines of the historical school and their disguised successors, the Institutionalists, criticizing both throughout the book, with all of which Mises would, of course, have been in agreement.

Elsewhere, however, Morgenstern challenges Mises directly:

"[T]he thoroughly empirical character of economic theory cannot be stressed too strongly. A *priori* theory would be very easy if it were possible to dispense with necessity of dealing with reality and with the flux of economic events and if it were sufficient to lock oneself in a room and invent the world of facts, adopting the attitude that if theory and reality did not agree, so much the worse for reality. 'Theory' of that kind can neither be confirmed nor refuted: nothing easier could be wished for. But, unfortunately, it has nothing to do with the real world"

*(ibid, p. 10)*⁵³

Then, citing Neville Keynes, Cairnes, Weber and Robbins on the separation of the positive and normative realms, Morgenstern lumps Mises in with the socialists in that both allow political values to enter their theorising, and both seek support for their politics in economic analysis. Liberalism is paradoxical, he writes, in that it argues against government intervention without acknowledging that it may be necessary to maintain free competition in an age of rising monopoly power. Rigid systems, in general, says Morgenstern, be it Liberalism or Socialism, also ignore changes in the

controlled themselves. Why, given this, did Mr. Morgenstern not choose a title which would allow the reader to guess the content of his book? By this means, he would have avoided wasting the time of those actually interested in "economics" (1937, p. 1085, my translation). Another brief review by E. Phelps Brown in the *Economic Journal* alluded to similar frustrations (1934). The 1937 English version, *The Limits of Economics*, on which our account is based, is claimed by Morgenstern to be considerably revised and, therefore, "not . . . simply a translation" (1937, p. vi). The attack on a priorism and liberalism was present throughout.

⁵³ Mises is named only in the Appendix, where his *Grundprobleme* is described as "an attempt to find an *a priori* basis for economics. . . one of the points where he diverges fundamentally from the view point put forward [here]" (1937, p. 154). In the same passage, Morgenstern castigates Robbins' *Nature and Significance* for presenting the Austrian economists as being more uniform in their views than was actually the case. In another oblique reference to Mises, he continues: "It is, moreover, worth noting that in practice the difference is one of method only for the few surviving apriorists are obliged in practice to make so many concessions that in the actual theorems themselves they abandon their original position, so that in the end both they and the empiricists are speaking the same language. What is really the most unfortunate result of their methodological position is their tendency to identify economic theory with a particular system of economic policy" (*ibid*, p. 10).

'economic mentality', such as the appearance of a general desire in people to have the State systematically attend to their welfare.

It is difficult to know whether Morgenstern really believed this or whether it was a gesture of deference towards the Austrian State. Between 1934 and 1937, many of his economist colleagues had left Vienna and Morgenstern found himself as policy advisor to the Dollfuss regime. In his attempt to walk a fine line between providing "neutral" advice and towing the governmental line, he appears to have been successful. Many of the allusions in this book speak to the difficulties faced by Morgenstern throughout this period.

The exclusive task of economics, he says, is to determine the effects of policy, and, alluding to the Austrian situation, where he proceeds with lengthy dissections of exogenous shifts or policy changes, of primary and secondary effects, of economic and psychological consequences. The book's two guiding metaphors are those of physical and spiritual health, with abundant references to medicine, psychological stability and pathology. Menger, Gödel and Wald are all harnessed in attacks on the imprecision of Keynes and, especially, the political biases of Mises, and, as we shall below, blows are struck in the context of the power struggles among the Viennese economists. With this volume, Morgenstern distanced himself definitively from Hayek and Mises, rejecting not so much a liberal style of economic policy as the idea that this was the *only* policy conclusion to which economic analysis could lead.

On reading the book, Hayek became testy:

"If one is supposed to be grateful for being sent a book, and one does not agree with it at all, and one knows the author too well to handle the matter in one phrase, the only way is to make the letter a counter conclusion. But for that I haven't had enough time. And you make the discussion very hard for me. To be honest, your book is a collection of, often brilliant, aphorisms, but it lacks the consistent argumentation with which one can start a discussion. Furthermore, that you were rude to some of my friends makes it even more difficult. [We] can only hope that, through the years, with many applications of the principles to specific problems, we can convince each other"⁵⁴

Hayek and Morgenstern, however, never did convince each other. Allying himself with the mathematicians and the "positivists", Morgenstern drew further and further away from Hayek. Although they had emerged from the same milieu, with shared Austrian theoretical concerns, by the mid-1930's they were on different paths. Hayek was in London, campaigning against socialism and planning, condemning the "scientistic" use of mathematical formalism in economics, pointing to the natural economic order (see Hayek 1940a, b). Morgenstern was in Vienna, criticizing liberalism and a priorism, and sceptical of the very concept of equilibrium in the absence of a logical mathematical explanation of the interaction of beliefs and opinions. It is not difficult to see why the

⁵⁴ Hayek to OM, April 2, 1934, OMDU. Neither did Knight like the book. On reading the 1937 translation, he wrote: "Frankly, I hardly know how to comment on your book. I have not read the english version in its entirety. It seems better than the German edition, but I have not made any detailed comparison. I hope it will not give offense if I say frankly that it did not seem to me, or to some colleagues whom I have heard comment, that the book represented a terribly serious effort on your part to penetrate to the more fundamental issues. We have been inclined to infer that it was written rather for a semi-popular audience than with a view to making some real contributions to the discussion, which you are certainly capable of making" (Knight to OM, July 31, 1939, OMDU).

extant Morgenstern-Hayek correspondence not only thins in the late 1930's but is devoid throughout of serious intellectual engagement.⁵⁵

Flight

Perhaps Morgenstern's repeated references to disequilibrium and psychological unease were omens of disintegration for, in a few short years, beginning in 1934, the intellectual community of which he was part would collapse entirely. A unique perspective on that slide into oblivion is provided by the surviving records of the activities of the Rockefeller Foundation.

Recall that, in 1930, the Foundation made a 5-year grant to the Institute. In July of the following year, no doubt encouraged by the Institute's success, former President of the Austrian National Bank, Richard Reisch, Hayek, Pribram and - testament to the power of lucre - enemies Mayer and Mises, sent to the Foundation a jointly signed "Memorandum on the Situation of Research in Social Sciences in Austria".⁵⁶ They were requesting money for the support of politically-independent research in the poorly-funded social sciences. "After the war", they wrote,

"these difficulties have become immense because of the general impoverishment and because the influence of party-politics, which is so particularly dangerous to social sciences, has become overwhelming. The small means which are available are mostly under the administration of more or less political organizations which, quite naturally, use it for purposes which seem most important from their respective partisan point of view and which are not in the first place guided by scientific considerations . . . There is, therefore, at present no body or organization whatever in Austria which could assist independent and unbiased research in social sciences".⁵⁷

They distinguished their project from the newly-funded business cycle Institute, which covered only a small section of economics, leaving many young men and women without support and compelled to earn a living by uncongenial means. They had in mind work in social history (the transition from monarchy); sociology (the problems arising from the "racial and national mixture of population in Central Europe"); economics (problems of changes in economic structure and others needing quantitative measurement, which did not fall into the Institute's ambit); and political science (the transition from autocracy to democracy). Without saying for how long, they requested \$15,000 per year. There is no evidence of a reply from the Foundation.

In March 1933, Rockefeller's Van Sickle met Mises in Paris and they spoke of the effect of Hitler's accession to power on the development of economics in Germany and Austria.

⁵⁵ Morgenstern's diary is replete with criticisms of Hayek, most of which are impressionistic. For example: "Hayek ... has written to Knight that he should give up economics and rather plant potatoes. He is totally crazy. Now my view is confirmed that Hayek is never going to become anything" (Jan. 9, 1935). See also Sept. 15, 1933; Sept. 14, Oct. 26, and Nov. 2 1935, OMDU.

⁵⁶ Memorandum on the Situation of Research in Social Sciences in Austria, July 27, 1931, AITCR.

⁵⁷ *Ibid.*

"[Mises] was inclined to take a very pessimistic view, and in his opinion we had probably seen the end, for at least a generation of any intelligent economic research in the German-speaking countries. He felt that the dictatorial regime in Germany and the extension of nationalistic tendencies in Austria will destroy any intellectual freedom in the field of economic studies, or will make it impossible for any properly qualified economists to obtain academic positions. He felt that the National Socialists would attempt to develop their own economic theories based on false premises with disastrous results for Germany and the almost complete suspension of the development of economic science"⁵⁸

The 1931 social science proposal was then brought up again by Pribram in October 1933, when he called to see Van Sickle in Paris. The allusion to racial issues in the original memo now came to the surface directly. Pribram suggested that the Directorship of such a social science institute might consist of:

"Aryan	Prof. Richard Reisch, representing Economics
Jew-Aryan	Prof. Mises or Prof. Hans Mayer, representing Economics
Aryan	Prof. Karl Bühler, representing Psychology
Aryan	Prof. Verdross, representing Law and Political Science
Jew	Prof. Pribram, representing Modern Social and Political History" ⁵⁹

Pribram emphasized the importance of the proposed institute being independent of the University, where the majority of the professors were frankly Nazi: "The directors of the proposed Institute would all be members of the university, but they are all Liberals and independent. There would be only one or at most two Jews in the Direction" (*ibid*).

Van Sickle indicated to Day that he supported the proposal, saying that the situation in Vienna was now so serious that the Foundation might be justified in "backing frankly the minority liberal element" (*ibid*). The group should be financed for the next two years, till 1935, he said, at which point the grants to Morgenstern's and the Psychological Institutes would have expired and the matter could be reviewed. He added, significantly: "We must reckon, of course, with the fact that institutes now receiving direct aid from the Foundation will no longer be so keen for a general institution whose Board of Directors might not treat them so generously as we have" (*ibid*). And indeed, although there is no "smoking gun", Morgenstern's actions and writings with regard to Mises, including Limits and the other attacks discussed above, are entirely coherent with his having felt stung by this bid to usurp the role of his "own" Institute. His extension of the Institute's activities beyond business cycle work, to mathematical economics, to the study of the Danube Basin, may also be seen as an attempt to thwart the funding manoeuvres of the larger group.⁶⁰

⁵⁸ TBK, Internal Rockefeller Foundation Memo, re Conversation with Prof. Mises, Paris, March 3, 1933. Mises also forecast Jewish professors having to leave Germany and the use of income tax laws to seize Jewish property in both countries. There were already cases in Austria, he said, where the entire personal capital had been confiscated through bloated tax claims.

⁵⁹ Letter, JVS to Edmund E. "Rufus" Day, October 10, 1933, AITCR.

⁶⁰ For example, in May, Van Sickle had written to Day, of Morgenstern's intention to expand the field of Institute activities beyond business cycle work. Letter, JVS to EED, May 1, 1933, AITCR. Funds for Wald followed in July, and for another price study in August. See AITCR. In December, 1934, Morgenstern wrote in his diary of his plans

Van Sickle pursued the matter. He visited the economic institute in Heidelberg, where political interference suggested that Foundation support should be reduced, and then Vienna, where more was justified: "The general opinion is that Austria will survive as an independent state with an authoritarian government, and that social science will be reasonably free. I was impressed on this visit, as on every former one, with the genuine interest in research and the surprising vitality of scholarship. There are warring factions, but there are good scholars who stand between them and who can be trusted to administer any funds we might place at their disposition".⁶¹ He had lunched with Pribram, Verdross and Degenfeld, who agreed that, to administer a grant, a Committee for Promotion of Social Science Research should be formed, independent of the university, and minus "any of the prima-donnas - notably Mayer, Mises and Spann" (*ibid*). He had later explained to the latter why they were being excluded. He continued:

"I have suggested to Pribram that in the letter of request the Social Sciences should be so defined as to exclude support of the pure Romanticism and the vituperative propaganda of Spann, yet permit support of precisely defined problems by younger scholars of the Spann School.

. . . There are distinct hazards in this proposal, which arise out of deep personal animosities. It is my hope, however, that these animosities can be reduced by a tactful and impartial committee. I am particularly desirous of drawing Spann into the circle of beneficiaries because I believe that he will then find it more difficult to continue his present destructive opposition to all objective liberal research.

Thus, if one of his men receives Committee support, it would be harder for him to characterize as 'stuff and nonsense' another piece of work accomplished under committee auspices by a man of the rival marginal utility school, and to oppose his 'habilitation' at the university. To do so would be an affront to the whole committee" (*ibid*).

In an immediate reply, Day squashed Van Sickle's proposal, citing Austrian political instability. Van Sickle fought back, with letters travelling back and forth between him and head office into early 1934.⁶² In January of that year, in a telegram to Day, he announced Dollfuss's suspension of the constitution, but insisted that the situation was not so bad as to endanger scientific work.⁶³ A few days later, having spoken to Professor Charles Rist in Paris, he elaborated further:

"Authoritarian government bids fair to spread in Europe. We shall doubtless have to learn to distinguish between good authoritarianism and bad authoritarianism. Even such an old Liberal as Professor Rist appears to be swinging around to a belief that some modified form of dictatorship may be the only way out of the present mess. The democracies seem paralyzed by the conflicting aims, aspirations and appetites of their constituents. Freedom

for the Institute for the next few years, ". . . and I am going to have reading rooms. Mises, Mayer, etc. are not going to be asked anymore" (Diary, Dec. 9, 1934, OMDU).

⁶¹ Letter, JVS to EED, Oct. 28, 1933, re "Social Sciences in Vienna", Box 4, Folder 35.

⁶² See Letter, EED to JVS, November 6, 1933; and JVS to EED, Nov. 20, 1933, and JVS to Sydnor Walker, Dec. 1, 1933.

⁶³ Cable, JVS to EED, Jan. 19, 1934, Box 4, Folder 35.

appears to be a luxury that we cannot afford after our triumphant war to make the world safe for Democracy. Unless Nazism sweeps Austria, and I don't think it will, the type of authoritarianism will be one compatible with reasonable freedom of research and expression. I hope that my proposal of October last is only postponed, not discarded".⁶⁴

In February, Dollfuss turned the troops on the Social Democrats and there was bloodshed and destruction in the streets of Vienna. Van Sickle understood the international public outrage, but said that the whole affair was "very human". The public had seen only the visible and best aspects of Social Democratic domination of Vienna; the model tenements, progressive schools, improved hygiene, etc. What they did not see was the "slow, steady expropriation of the middle classes by a variety of class taxes. Only one who has lived in Vienna can realize the bitterness and despair provoked by this policy. . . Then too the anti-religious attitude of the party . . . deeply offended the provinces with their large catholic populations. Total result: the provinces and the entire middle class against the Socialists and only waiting their chance to destroy them".⁶⁵ He admired the Socialists, but was not surprised that it ended the way it did.⁶⁶ He felt that the new regime could either swing towards the German Nazis or towards Italian fascism, but that a compromise between dictatorship and liberalism was likely. If this obtained, then social science research in Vienna could continue. He hoped to encourage the Viennese Committee to submit one or two modest proposals, and added a postscript:

"A word is perhaps in order regarding the Jewish situation in Vienna. If Nazism triumphs there will be a Jewish exodus even greater relatively than from Germany. If one or the other solutions prevails, the Jews will officially enjoy protection, but there will be little or no chance in academic life for younger men not yet in secure positions. These men will try to get out as fast as they can find openings abroad" (*ibid*).

A month later, he said that all those he had talked to were of the opinion that the Dollfuss regime was growing stronger and could hold out indefinitely against any domestic Nazi pressures. On the other hand, "Pribram was the most pessimistic", he wrote, "but his attitude is probably a function of his age, his poor health and his race. Naturally the Jews are the most uneasy".⁶⁷ That year, Pribram left for the U.S., and Mises left for Geneva.

Abraham Wald was another such uneasy Jew. At this point, he had been scraping along for three years, thanks to Schlesinger and to Morgenstern's Institute, and, like many others, began to consider

⁶⁴ Letter JVS to EED, Jan. 24, 1934, Box 4, Folder 35.

⁶⁵ Letter JVS to EED, March 10, 1934, Box 4, Folder 35.

⁶⁶ Concerning the events of March 1934, Morgenstern wrote somewhat cryptically to Eve Burns in the U.S.: "The time of the shootings was really bad, since it is really no pleasure to shoot canons in the middle of the city, and what's more to be shot at by them. One will have to wait to see what else will happen since the great task in such events is not to surmount them but rather how to liquidate them, and this process has only just started and no-one can say where it is going." Switching to his book, *Grenzen*, which Burns had read, «I am very sorry to have disappointed you with my book with its negativism, but I have the feeling that what is really necessary today is pitiless criticism, and I can tell you in confidence that I have just started with it now. My second book will also be overwhelmingly critical because only though that can the rubble of tradition be removed», OM to E. Burns, March 6, 1934, OMDU, Box 4, Folder Corresp. 1930-1932, S – Z.

⁶⁷ Rockefeller Foundation Internal Memo, JVS, re The Status of SS [Social Science] in Vienna, JVS Visit to Vienna, April 12, 1934, Box 4, Folder 35.

leaving Austria. Morgenstern, like Pribram before him, became a key person in the attribution of Rockefeller student grants and fellowships in Vienna. Throughout the 1930's, some of his underlings at the Institute were awarded travel grants to study abroad, with several of them going to Harvard, as he had done. In mid-1935, the Foundation's Tracy Kittredge interviewed Wald in Vienna, on Morgenstern's suggestion that he would benefit from some time in the U.S.A or England to work on time-series problems.⁶⁸ Nothing came of it, and Wald continued his search for stable employment.⁶⁹ With Menger's recommendation, Morgenstern secured more money to employ Wald, and continued to press the Rockefeller Foundation on the question of a fellowship. As of 1936, however, the question of Wald's background arose increasingly often in the Rockefeller correspondence. In February of that year, Kittredge interviewed Wald yet again, in Morgenstern's presence, and wrote supportively back to Van Sickle in New York.⁷⁰ Van Sickle, who had spent several years in Vienna and was close to Morgenstern, replied:

"Although Wald's work is too mathematical for me to have any opinion based upon direct examination of his publications, I have no doubt that he is one of the very ablest of the men working upon problems of statistical technique as applied to business cycle analysis. It is a pity that his nationality and race combined make his future so precarious. . .

[However, we] have given so many fellowships to Morgenstern's group that I think we should lay our emphasis elsewhere for a while after we have made an award to Dr. John. Wald should be kept under observation, but I am not inclined to recommend any early award"⁷¹

A few months later, in July, Kittredge interviewed Wald yet again. Morgenstern was still pushing to have Wald visit Princeton, at either the university's mathematics department, or the Institute for Advanced Study. In his notes, Kittredge wrote that, because of his Jewishness, Wald would be very unlikely to secure a university appointment in Vienna, or to "ever become a permanent member of the staff of the Institute". The Foundation had no provision for funding someone in Wald's position, he said, but, at least, Wald had recently invented some new device for improving radio apparatus and so was assured of at least a minimal income.⁷²

In September, Van Sickle was still holding off on Wald, who was "obviously a man of exceptional ability but, unfortunately, a man without a country": "It is impossible to foresee what the future holds in store for him. His development should be kept under observation as he may prove in time to be one of those rare individuals whom we are justified in aiding regardless of immediate

⁶⁸ See Note, undated, concerning Kittredge interview with Wald on July 9, 1935, AITCR.

⁶⁹ Later in 1935, a possibility arose in Palestine, through Jacob Fraenkel at Jerusalem, but it too fell through. Wald wrote to Menger of his intention to go to Palestine anyway, if he could get the entry permit and the money. He had been working on geometry and metrical geometry, he wrote, but it was difficult as he had to work with his brother and did not have the necessary peace. As always, he looked forward to getting back to Vienna and the Colloquium. Letter, undated, Wald to Menger, KMIT.

⁷⁰ Letter, Kittredge to Van Sickle, February 23, 1936, AITCR.

⁷¹ Memo, Van Sickle to Kittredge, 27 March 1936, AITCR. Ernest John was one of Morgenstern's economic researchers at the Institute.

⁷² Note on Interview TBK with Wald, July 11, 1936, AITCR.

prospects. It is hard on him, but I am satisfied that we should not recommend him for a fellowship in the near future".⁷³

Given the political situation, and with Pribram and Mises now gone from Vienna, the Social Science project had been shelved. The Foundation also began to worry about the Trade Cycle Institute, which seemed to be drawing too close to the government, but expressed confidence that if anyone was capable of "maintaining standards", it was Morgenstern.⁷⁴

In early 1937, Wald continued to worry. Morgenstern continued to press his case. Van Sickle continued to resist: "In spite of Morgenstern's guarantee of employment in the Institute on his return to Vienna, I doubt whether there is any real future for [Wald] there. Growing anti-semitism has closed the doors to such men throughout most of central Europe. It is a tragic situation but I don't see how we can use our fellowships to combat the trend. If an award were made to Wald to study in this country I am convinced that he would use the sojourn here to seek permanent employment".⁷⁵ He suggested that they contact other scholars, just to be sure that Wald was "really gifted". In the meantime, Morgenstern had Wald send a reprint of his *Zeitschrift* general equilibrium paper to Van Sickle. Finding it impenetrable, Van Sickle sent it onto Warren Weaver, at the Rockefeller offices in New York, explaining Wald's case: "He is one of those homeless Jews whom it is very difficult to place".⁷⁶ Weaver sent it onto Harold Davis at the Cowles Commission, saying the same thing.⁷⁷

In his letters to Menger, Wald appeared increasingly anxious. He worried about the renewal of his contract at the Institute, sent reprints to Hotelling and Schultz, and waited. Then, thanks to Morgenstern, he was invited by Hans Staehle, director of economic research at the League of Nations, to Geneva for September and October, to work on price indices as part of cost-of-living analyses being conducted by the International Labour Office. Here, building on earlier work by Haberler, Leontief and Staehle, he showed how an improved approximation to the true cost of living index could be constructed, under the assumption that the utility function could be approximated by a second-degree polynomial, and given certain other restrictions on the indifference mapping. By the same means, he showed how statistical data could be used to numerically estimate the underlying utility function and hence the demand functions.⁷⁸ Staehle

⁷³ Letter, Van Sickle to Kittredge, 16 Sept. 1936, AITCR.

⁷⁴ See Memos, JVS to TBK, Sept. 25, 1936 and TBK to JVS, Oct. 13, 1936, AITCR. A month later, Gerhard Tintner, one of the young associates of the Institute, fled Vienna. Meeting with Van Sickle, on his way to the Cowles Commission in Colorado, he said that the Italo-German agreement augured poorly for Vienna's Jews, whose lot would be serious. Freedom had already disappeared, he said, and the Institute's Monthly Bulletins, which he had been writing, "no longer reflect the views of the staff. Interpretations are consistently colored to suit the government, though the statistical data . . . have not been tampered with. Morgenstern meantime plays a larger role in Austrian public life, has secured reasonably adequate public support and . . . appears to have consoled himself for the loss of freedom by the thought that he can work freely within the government. Tintner thinks that Morgenstern's role there is thoroughly salutary. If Tintner's interpretation of the situation is correct it would seem that our relations with the Institute will have to be carefully reviewed at the time our present grant terminates" (Memo, JVS to TBK, Nov. 16, 1936, AITCR). Morgenstern quickly intervened, dismissing Tintner's pessimism as excessively gloomy. (See Letter, OM to JVS, Nov. 23, 1936).

⁷⁵ Memo, JVS to TBK, Feb. 9, 1937, AITCR.

⁷⁶ Memo, JVS to Weaver, June 16, 1937, AITCR.

⁷⁷ Letter, Weaver to H.T. Davis, June 18, 1937, AITCR.

⁷⁸ The results were published in Wald (1937), (1939) and (1940). See Tintner (1952).

was moved to write to Kittredge at the Foundation, singing Wald's praises, explicitly recommending a Fellowship, and suggesting that Frisch, Menger, Tinbergen and Haberler be consulted.⁷⁹ Kittredge remained recalcitrant, reiterating Van Sickle's argument about the risk of having Wald enter the American labour force.⁸⁰ Then, that same day, he wrote privately to Van Sickle, reporting a turn taken in the conversation with Morgenstern re Wald:

"OM of course shares Staehle's views as to AW's quite unusual abilities . . . [but] Morgenstern still feels however that if only one appointment from Vienna can be envisaged in 1938, he personally would give preference to the candidacy of Kamitz. K. has become Morgenstern's chief of staff and has been sharing increasing responsibility for the theoretical as well as for the practical investigations of the Institute. If an exceptional appointment could be made to Wald in addition to the ordinary fellowship appointment requested for Kamitz, Morgenstern would be delighted"⁸¹

Why did Morgenstern, at this moment, choose to hold back in promoting Wald? Was it because he had information about other possibilities for him, and knew that Kamitz would not face the same opposition? We shall likely never know. Why did the Rockefeller Foundation continue to create obstacles for Wald, yet look favourably upon Kamitz, whose future at the Institute was no more certain than Wald's? Might it have been because Kamitz was not Jewish, Rockefeller Foundation's commitment to scientific detachment notwithstanding?⁸²

The Foundation sought opinions on the relative merits of Wald and Kamitz. Both Haberler and Tintner rated Wald "head and shoulders" above Kamitz, whom they also rated below Ernest John, the previous Rockefeller Fellow.⁸³ On the other hand, Howard Ellis, at Berkeley, endorsed Kamitz, who, he said, was of "convincing and businesslike appearance and address [ensuring] no lost motion in awkwardness or vagueness concerning objectives".⁸⁴ Van Sickle spoke to Morgenstern, who was by now in the U.S., on a Carnegie fellowship for the first few months of 1938, visiting Vanderbilt, Princeton and elsewhere. After the conversation, the Foundation officer stuck to his guns:

"I am quite ready to believe that Wald is quite unusually gifted. I still do not see how we can give him a fellowship, in view of the fact that he would be almost certain to use the fellowship to secure a permanent position in this country. . . Morgenstern yesterday . . . said that Wald had been offered a Cowles Commission fellowship. This offers \$1,000, but

⁷⁹ Letter, Staehle to TBK, Nov. 26, 1937, AITCR.

⁸⁰ Letter, TBK to Staehle, Dec. 1, 1937, AITCR.

⁸¹ Memo, TBK to JVS, Dec. 1, 1937, AITCR.

⁸² In late 1937, precisely when it was rebuffing Wald, the Foundation was ruminating over whether to continue funding the Institute in the light of the political climate in Austria which had "greatly abridged the freedom of the individual". However, they were reassured by a prominent League of Nations official - probably Staehle - that the Institute was "a bright spot in a part of Europe not conspicuous for scientific detachment . . . The work is not subjected to political pressure and . . . the Director's views are scientific and detached as far as any human being's can be". See Comment on Foundation appropriation to Institute of 30,000 Austrian Schillings per year for 1938-1940, no date, probably late 1937, AITCR.

⁸³ See memo JVS to TBK, Jan. 6, 1938, AITCR.

⁸⁴ Letter Ellis to JVS, Feb. 21, 1938, Series 705E Austria, Folder 1214.

nothing for travel. As Wald is responsible for his parents in Rumania, he has not been able to save anything and cannot, therefore, finance the trip to Colorado. Morgenstern expressed the hope that we might be able to make a grant-in-aid to get him over here. I told him that I did not see how we could possibly do so, much as I should like to help Wald. I suggested that he attempt to interest some well-disposed American Jew in Wald with a view to getting the slight assistance that was needed".⁸⁵

Head-and-shoulders notwithstanding, the Fellowship went to Kamitz, and Wald was refused travel money. It was late January 1938. During this time, Nazi activity in Vienna rose visibly, with groups of youths roaming the streets molesting people of Jewish appearance, graffiti appearing on the walls, and petrol bombs being thrown into synagogues. Early in February, Hitler dismissed his senior generals, making himself supreme commander of the German armed forces. On February 12th, he summoned Chancellor Schuschnigg to a now-famous meeting at Berchtesgaden, his mountain retreat, where the Austrian capitulated to Hitler's demand that the Nazi von Seyss-Inquart be admitted to the Austrian cabinet as Minister of the Interior, with control of the police. On Thursday, February 24th, Schuschnigg made a radio broadcast, pleading for a unified Austria, but without defiantly challenging Hitler. Then, in early March, he threw down the gauntlet, declaring that a plebiscite would be held in which Austrians could vote for, or against, a free, German, independent, social, Christian and united Austria. Two days later, on March 11th, to the dismay of Austria's Jews, he announced in another broadcast speech that the plebiscite had been cancelled, and that Hitler had demanded that the Federal President Miklas appoint a cabinet of his, Hitler's, choosing. Otherwise, German troops would be sent into Austria. With this, Schuschnigg stepped down as Chancellor, making the way for Hitler's Seyss-Inquart. That night, as George Clare recalls, crowds of Nazis on the backs of lorries under the swastika were again roaming through the streets of Vienna.

On March 15th, Ernst Wagemann, the Director of the Berlin Institute, arrived in Vienna with instructions to liquidate the Trade Cycle Institute. He spent a week there, dismissing most of the staff, including Wald and the absent Morgenstern, and retaining only the politically acceptable Kamitz and John. The former was made acting Director, and instructed not to communicate with Morgenstern or any foreign institutions, including the Foundation. However, early in May, in an out-of-the-way café on the outskirts of Vienna, Kamitz met secretly with Kittredge.⁸⁶ He told him that he had suggested to Wagemann that the Foundation might be willing to continue support if the independence of the Vienna Institute could be assured, reporting independently on Austrian conditions and doing basic theoretical research. Wagemann had insisted, however, that economic reports and analysis would have to conform to instructions from Berlin, and that he was personally opposed to the theoretical investigations so that the monograph series would be scrapped.⁸⁷

⁸⁵ Memo JVS to TBK, Jan. 21, 1938, AITCR.

⁸⁶ See Memo, TBK to Sydnor Walker, May 19, 1938, AITCR.

⁸⁷ How the Institute could have even attempted to maintain its previous program, given its virtual dismantling by Wagemann, is not clear. Like so many Austrians, Kamitz seems to have played his cards pragmatically. At the same time as he went to the trouble, and ran the risk, of meeting Kittredge, telling him about the plight of the Institute, he was able to inform him that he had "no personal difficulties" having been asked to take over lectures at the *Hochschule für Welthandel* to replace professors who had recently been discharged. It even looked likely that he would be appointed to a dozentship so that his "prospects for an academic career . . . seemed good" (Memo, TBK to Sydnor Walker, May 19, 1938, AITCR).

On March 19th, as President of the National Economics Association, Hans Mayer wrote to all members: "In consideration of the changed situation in the German Austria I am informing you that under the respective laws now applicable also to this state, all non-Aryan members are leaving the Economic Society".⁸⁸ But, by then, many of those members, Christian, Jewish, and the "mixed group" alike, had already left or were, in one manner or another, leaving Vienna. Mises was in Geneva, and Hayek had long been in London. Menger was now at Notre Dame, Tintner in Iowa City, Haberler in Harvard, Machlup in Buffalo. Morgenstern was in the U.S., searching for a new university. In late 1937, he had broken with the Austrian regime over its unwillingness to face up to agrarian special interests in the matter of downward price adjustments. When the Nazis took over the Institute in March 1938, Morgenstern, who by then had left the city, was deemed *persona non grata*.

In the streets of Vienna, Jews were forced into demeaning acts, religious Jews were forced to commit acts of sacrilege, shops were defaced and looted, property destroyed, and apartments plundered. By April 3rd, Morgenstern, in Wisconsin, could write to Van Sickle that Schlesinger and Kunwald, another economist, had committed suicide.⁸⁹ On April 11th, the Institute's Monthly Bulletin appeared with a foreword by Wagemann:

"The vast historical development of these days, which has inspired and widened the life the German people in all its aspects, emphasizes also new ways for this publication. Out of the union of Austria with the Reich there has developed on the economic side two important issues. It will now be necessary, in general, to provide for the fusion of the economic and constitutional life of these two different State economies and, in particular, to overcome the economic distress of Austria. This has to be accomplished by the powerful and quickly-effective means and methods which National Socialism has developed and which were completely lacking in the former Austrian government with its remarkable lack of understanding . . . The close collaboration of both [the Berlin and Vienna] research organizations will make possible our fruitful collaboration in the great tasks which lie before us".⁹⁰

Excluded from this project, and fearful of the power and effect of National Socialism, Abraham Wald was still in Vienna. He wrote to Menger about the bureaucratic difficulties being created by the Rumanian government who would only issue a 3-month passport, whereas the Cowles position was for one year. He hoped Cowles would not make any difficulties for him: "It would be a great

⁸⁸ Quoted in Mises (1978), p. 99. In these *Recollections*, written in 1940 when he had just arrived in the U.S. and was bitterly upset at the turn of events, Mises condemns Mayer as a Nazi collaborator, and dismisses him as an economist. I suspect that the lack of historical interest in Mayer's economics from the outbreak of the War onward was shaped by his *Anschluss* actions, and by Mises' 1940 condemnation. Not until 1994 was some of Mayer's work translated into English, in a volume of Austrian readings, edited by Israel Kirzner (1994).

⁸⁹ On the treatment of Jews following the Anschluss, see Oxaal et al (eds.) (1987), Wistrich (ed.) (1992), and Pauley (1992). Botz (1987) reports that despair among the Jewish upper middle classes dramatically increased the number of suicides in the months following the *Anschluss*, with 220 reported in March alone; "The Jews of Vienna from the Anschluss to the Holocaust" in Oxaal et al (eds.) (1987), pp. 185 - 204.

⁹⁰ Austrian Institute for Trade Cycle Research, *Austrian Monthly Monthly Bulletin*, April 11, 1938, p. 12 (translated from German).

misfortune for me were I to lose this position. I would then be facing the abyss and would not even have the financial means to travel anywhere".⁹¹ He could not even leave Austria to go home to Cluj because the Rumanian government had forbidden reentry without the special permission of the Ministry of the Interior. Then, at the eleventh hour, he got out, making it to the U.S., and Colorado, via Cuba. With the exception of one brother, who also made it to the U.S., all of Wald's immediate family disappeared in a concentration camp.

In Vienna, for all the expression of venom and hate of early 1938, the city was struck silent. Mises, Menger, Morgenstern, Schlesinger, Machlup, Tintner, Haberler, Wald - all were gone.⁹² The Institute was but a shell. Of those present at the young Menger's talk on the Petersburg Paradox a decade ago, there remained only Mayer, now presiding over a spectral Economics Society.

Conclusion

Over the decade to 1938, Morgenstern achieved considerable power in the Viennese economic community. At the helm of the Institute, and with the confidence of the Rockefeller Foundation, he wielded influence over the type of research done. He also provided support for researchers in a difficult environment. Throughout the decade, he played the role of largely liberal policy advisor, but found himself in an increasingly difficult position, growing increasingly skeptical of dogmatic Austro-liberalism, ever more attentive to clarity in argument, and relentless in his insistence on the separation of economics and politics.

His theoretical writings, themselves a curious *mélange* of vague suggestion and harsh critique, reveal several concerns, from the emphasis on expectations, beliefs and psychological factors as the most important manifestations of time in economics; to the need to examine the logic and consistency of the field in a manner similar to that in the branches of mathematics; to the need to rid the discipline of all element of political apology.

His passage from Vienna was easier than that of Wald. Not only did he arrive in the U.S. on a fellowship from the Carnegie Endowment for International Peace, but the Rockefeller Foundation helped him settle at Princeton by paying half of his salary for a while. Although Princeton was then still a sleepy gentleman's college, and worlds away from Vienna, Morgenstern knew some members of the faculty, including Fetter, and it was close to the cultural benefits of New York city. In 1939, Morgenstern was still thinking about beliefs, interaction and the limitations of the economic viewpoint, when he wrote once more to Frank Knight: "[T]here are only a few people, if any, interested in methodological questions. Those with whom to discuss such problems are principally the mathematicians, of which we have some excellent ones in town. I have now been stimulated by these talks and proceeded to jot down notes on a further paper of what I called maxims of behavior. In this paper I shall endeavor to investigate a very curious relationship between the quantitative limits which maxims may have. I hope to be able to show you something of this in the not too distant future".⁹³ The unnamed stimulus here was John von Neumann - like Menger, a mathematician, like Wald, a Jew, although less "pure" on both counts, and like all the

⁹¹ Letter, Wald to Menger, April 18, 1938, KMIT.

⁹² On Mises' flight from Geneva to the U.S., see his (19??) *Notes and Recollections*.

⁹³ Letter, OM to Knight, Nov. 8, 1939, OMDU, Box 6, Corresp. 1928-1939, Knight.

Austro-Hungarians, affected by the upheaval of the times. Within a few months, von Neumann would plunge into the creation a new mathematics of social organisation, carrying the Austrian critic with him.

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