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Towards a “Prodigious Revival of French Economics”?

Allais, Debreu and the Dead Loss Controversy (1943-1951)

Raphaël Fèvre * & Thomas M. Mueller **

Abstract: This article aims at reconstructing the hitherto unknown controversy that involved Maurice Allais, François Divisia, Harold Hotelling and Gérard Debreu in the early postwar period. The crux of this controversy focused upon the “dead loss”, a measure of the maximum value of available surplus working as a gauge of economic efficiency and social welfare. The protagonists of the controversy argued about its mathematical expression, the hypothesis behind it as well as its general significance. By drawing heavily on unpublished materials from the protagonists (letters and notes), the article will unfold the different steps of the dead loss controversy and shows that it was driven by an intricate interlacing of technical advance of welfare economics with Allais’s own ambition to embody the revival of French economics. Eventually, this controversy (and the tense personal exchange between Allais and Debreu that came with it) proved a remarkable—though tacit—driving force behind Debreu’s contributions of the early 1950s.

Keywords: Maurice Allais, Gérard Debreu, Deadweight loss, Theoretical practice, Welfare economics

JEL Codes: A11, B21, B41, C02, D60

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Introduction

In the first years of German Occupation of France, Maurice Allais spent significant amount of his time frantically covering page after page with words and mathematical formulas. This material eventually turned into a nearly thousand-page long book entitled *À la recherche d'une discipline économique* (Allais 1943). Allais was then a young but already highly accomplished engineer in charge of strategic national industries (mining and attending railway system). Why should he sit for hours on end, trying to master the mathematical economics drawn by Walras, Pareto and Fisher? Was he trying to find some kind of retreat from this troubled world and its most pressing issues? Quite the contrary. Although Allais had been deeply affected by the economic and social turmoil of the interwar period, it was France's early defeat by Axe forces in June 1940 and the need to "contribute to prepare for the post-war period" that triggered his re-orientation (Allais 1997, 3).¹

In these dark years, Allais entered the profession of economist having formed a formidable project: to set forth the conditions of the maximum achievable economic efficiency, and to outline the politics necessary to achieve it. With war coming to an end, he published various studies in theoretical economics (1945a; 1947) and politico-social organisation (1945b; 1946a; 1946b). In this process, Allais (1945a, 10) aimed at no less than re-establishing the credentials of the "French Economic School", thus reviving the analytical heights that Cournot and Walras had reached in the past century. In a short time, Allais

¹ A pupil of the nation coming from an extremely popular background, Allais specialized in mathematics and entered in 1931 the *École Polytechnique*, one of the tops republican higher schools. He graduated first of his class in 1933. Allais then embarked on an elite engineer career for about ten years (1937-1948). After having briefly served as an artillery lieutenant in 1940, Allais was restored to his former position as chief engineer. In parallel to his administrative duty as an engineer, he became Professor of economics at the *École des mines* from 1944 on, and a director of research at the *Centre national de recherche scientifique* (CNRS) from 1946 on.

established himself as the figurehead of French marginalism (Drèze 1989; Arena 2000). Allais's ambition can be fleshed out by focusing on a single episode of the early post-war period: a cross-correspondence debate that he engaged with François Divisia (1889-1964) and then with Harold Hotelling (1895-1973). The crux of this controversy focused upon the evaluation of optimal welfare in general, and of the "dead loss" in particular. The dead loss measures the maximum value of available surplus, hence working as a gauge of economic efficiency.² The protagonists of the controversy argued about its mathematical expression, the hypothesis behind it as well as its general significance. The controversy—hitherto unknown in the current literature—lasted only a couple of years, from 1944 to 1946, but had far-reaching consequences and eventually included yet another participant.³

In May 1949, Allais had news from one of his most promising disciples, Gérard Debreu. Debreu was then twenty-seven, and currently visiting the U.S. thanks to a Rockefeller Fellowship Allais got him. In his letter, Debreu expressed his intent to come back to France and embrace a career there, and declared his willingness to support and continue Allais's research program:

I hope anyway to help you in the exalting task that you have already carried so far alone. I am convinced that a *prodigious revival of French economics* is possible: the

² What is today called "deadweight loss" in economics was initially labelled "dead loss" by Hotelling. Allais and the French-speaking authors often called it "perte sèche" and Debreu translated it with "dead loss". In the paper we consistently used the historical expression.

³ This paper deals extensively with unpublished letters, notes and draft articles coming from the Maurice Allais private archive in Saint-Cloud (France). Allais's archive is not open publicly, and we had a unique chance of access due to the kindness of Christine Allais, his daughter. We also had access to Hotelling's Archive thanks to Antoine Missemer and Marion Gaspard who digitalized a large part of it. Unfortunately, there is no trace of Debreu's correspondence before 1952 in his papers kept in Berkeley, and we could not find trace of Divisia's papers. All translations from the original French are ours.

remaining effort is of course gigantic (Debreu to Allais, letter 13.05.1949, emphasis added).⁴

Debreu's commitment is likely to have greatly pleased Allais.⁵ Debreu's return was imminent, and he was willing to help the master to develop—and hopefully to disseminate—scientific contributions that had been highly neglected by the profession so far. Yet in June 1950, Debreu joined the Cowles Commission in Chicago. He left France behind—and so Allais—to embrace an American career. While in some of his first articles Debreu acknowledged his intellectual debt to Allais, he nonetheless embarked on his own program dealing mainly with the proof of the existence of a general economic equilibrium by using highly advanced mathematics, and most notably topology (Weintraub and Mirowski 1994; Weintraub 2002, Chap. 4; Düppe and Weintraub 2014b).

Allais's imprint on the young Debreu is apparent—and yet, their relationship has not been documented in any detail thus far. This paper will provide some elements by examining the private discussions Allais and Debreu engaged on the dead loss by the beginning of 1947, after Allais failed to convince Hotelling of the general relevance of his “dead loss” formula. While in the first place Debreu sided with Allais' views, he grew more and more frustrated with them, eventually turning his critical fire back at Allais, in the second half of 1949. How should we understand this remarkable turn of events? The paper will suggest that the rising

⁴ “J’espère de toutes façons pouvoir vous aider dans la réalisation d’une tâche exaltante que vous avez déjà menée si loin seul. Je suis convaincu qu’une prodigieuse renaissance de l’économie française est possible : l’effort qui reste à accomplir est bien entendu immense.”

⁵ What is certain is that Allais highlighted this passage by both underlining it in the original letter and had it neatly re-type in a selection of passages coming from Debreu's handwritten letter.

conflict between Allais and Debreu was fuelled by methodological and technical differences, complicated by more personal issues.⁶

Only a month after Debreu's enthusiastic letter to Allais (quoted above), the former attended the conference of the Cowles Commission in June 1949, an event that contributed to set the canons standards of mathematical economics for decades. Technical rigor, the uses of convexity analysis rather than differential calculus but also the general call for a-political research agendas: all of these criteria were in tune with Debreu's deepest aspirations (Düppe 2012; Düppe and Weintraub 2014a). Clearly, these were crucial factors in both Debreu's way of conceiving economics and choices of career. However, the American side of the story framing Debreu's early works (in particular Debreu 1951; 1954) should be completed by the discussions Allais raised in Paris. As a matter of fact, Debreu acknowledged in his paper "A Classical Tax-Subsidy Problem" (1954), written in 1951, that he did greatly benefit from the Cowles Commission environment, yet "the conception of this study goes back to [his] reading correspondence of 1946 between M. Allais and H. Hotelling" (Debreu 1954, 14).

The following pages will attempt to reconstruct step by step the history of the dead loss controversy. Initiated by Allais and Divisia as early as 1944, the controversy extended to Hotelling in 1946 before tearing Allais and Debreu apart in 1949, and culminated somehow in Debreu's articles from the early 1950s including the proof of the optimality theorem. In following a chronological narrative, we also aim at unfolding three different—but interrelated—levels of reading of the controversy: where the technical discussion about the way to measure and calculate the dead loss has to be related to a global ambition (restoring the credentials of French economics) coupled with more personal matters (the Allais-Debreu

⁶ The relationship between Debreu and Allais was a complex one, that evolved through time. We only cover here the period 1943-1951. Whatever their personal relation though, Debreu always publicly emphasized the importance of Allais' contributions to mathematical economics in general, and to the formulation of the dead loss in particular, even decades latter (Debreu 1984).

connexion). These inter-personal issues are not only precious for what they tell us on the relationships between the economists involved in the controversy, but proved to be an integral part of the blur and intricate process by which economic theory is actually worked out.

1. The Allais – Divisia Dialogue (1943-1945)

In 1943 issued the first outcome of Allais’s ambition to renew French economics. Allais dedicated his *magnum opus* to his former teacher François Divisia (along with Walras, Pareto and Fisher). Just like Allais but on a somewhat older generation, Divisia belonged to the long-established tradition of economist-engineers (Etner 1987; Ekelund and Hébert 1999). He is known for having developed an index measuring changes in prices and quantities of goods as functions of continuous time, a.k.a. the Divisia index (Balk 2005). He was also at the origin, with Ragnar Frisch and Jan Tinbergen, of the creation of the *Econometric Society* and its related journal *Econometrica* (see Bjerkholt 2017).

Among many teaching occupations (see Armatte 1994), from 1929 Divisia taught economics to the students of *Polytechnique*—among them was Allais. Later, Allais recalled he owed Divisia’s teaching his own interest for “economic phenomena” (EP, §5. p. 30). Hence Allais situated his interest for the subject-matter economy, not for the scientific discipline of economics *per se*.⁷ Certainly, economic education within the *grandes écoles* occupied only a very small part of the cursus (by comparison to physics and mathematics). If it has its specificities, slightly more technical and apply-oriented, it was nonetheless largely devoid of mathematical formalism of any kind; hence not so different from the teaching of

⁷ Yet as Alain Béraud (2011) emphasized, Divisia’s *Économique rationnelle* (1928) was probably an important milestone in Allais’s theoretical path (he read the book in 1937). Divisia had offered a new exposition of the Walrasian general equilibrium where he raised issues—intertemporal choice and monetary pricing—that became central in Allais’ model.

political economy in university law schools (Le Van-Lemesle 2004). All of this may explain why, despite Divisia's lessons, Allais considered himself a "self-taught" economist.

However, one of the major drawbacks of being an enlightened "amateur" was precisely Allais's limited knowledge of the most recent international literature—not to mention that the war made access to this literature very difficult. While Divisia had himself his own shortcomings from that regard, he—and his letters—came as a friendly reminder of the contributions that Allais's 1943 book was challenging without knowing it, such as was the case with Hotelling's work.

Welfare Economics à la française

On March 12, 1944, as France was still under Occupation, Divisia wrote to Allais about a matter they were discussing since at least one year. The object of the discussion was the mathematical expression of the dead loss Hotelling gave in "The general welfare in relation to problems of taxation and of railway and utility rates" (1938) published in *Econometrica*. In this article, Hotelling argued that, in order to maximize the general welfare, every goods and services should be sold at their marginal cost. More precisely, Hotelling emphasized that bridge tolls, railway rates and similar—above marginal—costs, are essentially the same nature as an excise tax, i.e. a tax proportional to the quantity of sold good. He thus showed that these taxes always imply a welfare loss, compared to taxes that are not proportional to sold goods, such as inheritance and taxes on the site value of land. Therefore, the optimal solution would be to set prices equal to marginal costs, and then to cover the resulting economic losses with this second kind of taxes.

Drawing on the work of engineer Jules Dupuit, Hotelling generalized the concept of consumer surplus in a strict ordinalist framework, arriving at a formula for the dead loss, i.e. the amount of consumer surplus that is lost with a price maintained above the marginal cost fixing:

$$\delta r = \frac{1}{2} \sum \delta p_i \delta q_i$$

where δr is the dead loss of individual i , δp_i is the difference in price (between marginal and non-marginal pricing), and δq_i the change in quantity of consumed commodity (under those two same price vectors), that is summed over commodities.

In his *À la recherche*, Allais (1943) reached independently a somehow similar conclusion, but with quite different premises and only for quite particular goods. Indeed, Allais introduced two sectors of an economy: the “differentiated sector” and the “undifferentiated sector”. On the one hand, the differentiated sector corresponded to industries where “the optimal production technique is realized by a juxtaposition of different firms, usually of the same size”. On the other hand, the undifferentiated sector corresponded to industries where “the optimal production technique is realized by a standalone firm”. Only the undifferentiated sector calls for marginal cost pricing, while competition provides it de facto in the differentiated sector. Yet, as Allais himself stressed, whether we should sell or not at marginal cost depends on the industrial organization. It is unrelated to monopolistic, oligopolistic or competitive market structures. The distinction has to do with production techniques, not with selling conditions. This proves a crucial difference between Allais and Hotelling.

The mathematical expression that Allais provided to evaluate the dead loss also diverged from Hotelling’s simple formula. For two economic states (an economic state being a vector of production and consumption associated to every individual and every firm), Allais evaluates the satisfaction $S_i(A^1, B^1, \dots)$ and $S_i(A^2, B^2, \dots)$ for each individual i owning goods A, B, \dots in states 1 and 2. He further asked how much of good A one should take or give to every individual in state 1 to equalize his satisfaction to the one he would have in state 2, i.e. the amount of dA_i such that $S_i(A_i^1 - dA_i, B_i^1, \dots) = S_i(A_i^2, B_i^2, \dots)$. The total amount of

good $dA = \sum_i dA_i$, that Allais termed “distributable surplus” would measure—if it has a positive value—how better state 1 is compared to state 2, in terms of good A. Allais intended this measure to somehow evaluate how much is gained by a certain Pareto improvement.

Contrary to Hotelling, Allais aimed for general results. The latter attempted to evaluate the dead loss between any two states (and not only between a state of marginal cost pricing and another). He considered marginal cost pricing as the exception, not the standard. His model also included production: when consumption varies, productions varies too and so marginal costs. By contrast, Hotelling’s case was simpler for he did not take production into account. When consumption varies, marginal costs remain fixed. Allais’s formula for the dead loss included a second order term that was not part of Hotelling’s formula: ⁸

$$\delta r = \frac{1}{2} \sum \delta p_i \delta q_i + p_i \delta^2 q_i$$

From the beginning of his dialogue with Divisia, Allais claimed the superiority of his result both in rigor and generality (letters of 13.03.1945 and 20.03.1945). However, Divisia was more skeptical about it: according to him, the two formulations by Allais and Hotelling were equivalent. In a letter from March 9, 1944, Divisia pointed out to Allais that “a second differential is meaningless without specifying what the independent variables are”. Accordingly, Divisia wondered if Allais’s $\delta^2 q_i$ formula that would represent a “physical quantity of a reference commodity” was not in last analysis “completely formal and empty”. It was unclear for Divisia how to interpret those second order differentials. More importantly

⁸ Allais employed a notation that proved difficult to translate into Hotelling’s. The formula that we give here never appeared neither in the public nor in their private communications. We rather use Divisia’s own rendition of Allais in Hotelling’s notation (with simplifications, letter of the 09.03.1944). Yet none of the authors, Allais, Hotelling, Divisia and later Debreu ever agreed on how to correctly interpret—the conceptual signification behind—their notations, and therefore this formula is our best try to get the French and the English mathematical notations closer.

to Divisia's eyes, it was unclear whether Allais and Hotelling interpreted the first order term in the same way since their working hypothesis were somehow different.

Discussing Hotelling

About ten months after Divisia's initial letter, Allais wrote back on January 15, 1945. Allais had not changed his mind: the "relation indicated by Dupuit and confirmed by Hotelling was false" for the latter's simplification would be exact only in "exceptional cases", thus by no account on the general one. Two weeks later, Divisia said "not being surprised" that Allais's got different results from Hotelling as the latter "seems having considered the interdependency of commodities but not of individuals" (29.01.1945). And yet that would not prove that Hotelling's formula was wrong altogether. On deeper consideration, Divisia had pinpointed an important point of dissent, namely that Hotelling's mathematical formula was derived following a different set of hypotheses than Allais's.

By March 12, 1945, Divisia engaged deeper into the discussion, and provided a dense, fifteen-page long note entitled "Calculation of the dead loss resulting from the replacement of a direct tax by an indirect tax".⁹ In it, he meticulously reviewed and compared Hotelling's and Allais's formulae. Once again, Divisia proved critical of Allais's approach, since the latter would have provided "formal calculations without physical meaning" (p. 7). Moreover, in the precise example stated by Hotelling, Divisia claimed that Allais's formula reduced to Hotelling's, up to "negligible terms".¹⁰ Divisia arrived at formulating the following "Theorem: If indirect taxes are balanced, so that the new prices are proportional to the old ones, the dead loss is null." (p. 11)—Divisia underlined that the condition of proportionality of the prices was also invoked by Frisch (1939) in his response to Hotelling's paper.

⁹ "Calcul de la perte sèche résultant du remplacement d'un impôt direct par un impôt indirect".

¹⁰ In fact, Divisia specified: "as long as tax reliefs (*dégrèvements*) are made frequently enough"; cf. "Calcul de la perte sèche ..." (p. 14).

Nonetheless, Divisia also claimed that Allais's more complete formula would incline "in favor of the practical application of balanced indirect taxes".¹¹

Throughout his note, Divisia's calculations also displayed a certain discomfort in dealing with the technicalities of infinitesimal calculus. He relied heavily on Humbert's *Le calcul symbolique* (Humbert 1934), yet Divisia expressed several doubts on whether his calculation were rigorous, correct, and acknowledged some troubles in performing them, what he recognized openly in both his note and letters. Hence the Allais-Divisia dialogue had reached the limits of Divisia's mathematical knowledge, but Allais was ready for more. For a year, Divisia tried to convince Allais of the equivalency of the latter's formula to that of Hotelling. Based on various testimonies, Allais was known to be a man of character.¹² At this stage in March 1945, it might be interesting to underline that Allais did not directly consult Hotelling's 1938 paper. It is not for lack of having asked Divisia for the original version (or even better the French translation that the latter claimed having completed). Divisia regularly promised to send him one of these versions, but apparently without actually doing it.

On March 20, 1945, Allais wrote back to Divisia with striking news. He did provide a mathematical counterexample of the Hotelling formula, thus disproving its generality. Allais's counterexample was in fact a special case inspired by—but slightly different from—the appendix of his new book *Économie pure et rendement social* (1945a). This unpublished note Allais sent to Divisia was entitled "Note on the social loss in the case studied by Hotelling", and provided an actual example of n identical producers and consumers of two

¹¹ In this context, indirect taxes are equivalent to excise taxes, i.e. proportional to the quantity of goods sold.

¹² Allais was certainly not the kind of person to change his mind easily, and he was more inclined to fuel dissent and controversy rather than adopting a consensual position (Drèze 1989; Fischesser 1989). The following pages on Allais's correspondence with Debreu will provide illustrations of Allais's strong temper.

goods. The mathematics behind the example were quite sophisticated (and plagued with a miscalculation, as we will see later). Allais proved both enthusiastic and overconfident. A year exactly after Divisia's initial note, he announced:

The personal study I have been working at, allowed me to reach absolute certainty that in the Hotelling's case [...] the dead loss does not have the simple form corresponding to [Hotelling's formulae]. (Allais to Divisia, 20.03.1945)

Confident in the counter-proof he provided, Allais was already drawing general methodological lessons. According to him, his work demonstrated that "abstract and simplified" reasoning would have a "high methodological value" for they can display the "inexactitude of certain general deductions", as in the case of Hotelling's formula (letter to Divisia 20.03.1945). Both Divisia and Allais agreed on the urgent need for French economists-engineer to "wash the dishes" to make economics progress, i.e. to undertake "mathematical cleanups" of the work performed by "intuitive" economists and to provide "simple and highly abstract models [...] of high methodological value [...] as in mechanics and physics". That Keynes was part of these intuitive economists is one thing, but that Allais considered Hotelling belonging to the same group is far more surprising.¹³ As we will see, soon enough Allais was led to reconsider his rash judgment on Hotelling.

¹³ Both Divisia and Allais shared this view on Keynes (Divisia's letter to Allais 29.01.1945; Allais's letter to Divisia 07.02.1945). But there is no evidence that Divisia felt the same way about Hotelling. Nonetheless, Divisia did express some reservations about Hotelling, "wondering whether [he] had made some miscalculation". Hotelling was a well-trained mathematician, owning a PhD thesis in mathematics (under Oswald Veblen's supervision); on his training as a mathematician see Gaspard and Mueller (2020).

2. The Hotelling – Allais Debate (1946)

On the one hand, Hotelling's 1938 article had sparked lively debates in the international community of economists. While on the other hand, the French side (the Allais-Divisia dialogue) was only one—small and hidden—part of a broader discussion among the economic profession. Indeed, Ragnar Frisch, Abba Lerner, Paul Samuelson or James Meade, among others, fueled a debate that Ronald Coase (1946) then termed the “marginal cost controversy” in his eponym paper.¹⁴ French protagonists of the debate probably did not know the extent of this international discussion, with the exception of Frisch's contribution as stressed above.¹⁵ Hence Allais and Divisia provided a rather autonomous reception and discussion of Hotelling, mainly framed by the French theoretical and political context.

Contesting the Generality of Hotelling's Formula

About a year after having worked his counter-proof out, Allais directly wrote to Harold Hotelling on May 20, 1946. He wanted to discuss the “note” he had previously sent to Divisia and that was now in Hotelling's hands. According to Allais, his note showed that the mathematical expression Hotelling had found in both his *Econometrica* papers was “inexact, at least in the general case”.

In a letter dated June 4, 1946, Hotelling replied with a detailed argument. He confessed having found Allais's note “quite disturbing”, but not for the good reasons. Indeed, Hotelling underlined some errors in calculations in Allais' note (apparently the mathematics was not as “easy” as Allais himself had claimed). Hotelling corrected miscalculations by Allais, offered

¹⁴ On this discussion of pricing and optimal welfare, see (Ruggles 1949; Medema 1994, Chap. 3; Frischmann and Hogendorn 2015).

¹⁵ Allais mentioned to Divisia (letter 13.03.1945) the importance of Meade's book *Politique économique et économie politique* (pp. 394-403) where Meade would arrive at the “fundamental results of the theory of social productivity” in a different and independent way from that of Allais.

the right result, and then suggested how his differentials may be expressed in terms of Allais's problem. He finally showed that the two results were matching to one another. However, Hotelling brought up a more troublesome issue to Allais's attention:

The differential which you denote by da and db are not the same as the differences δp_i between price and marginal cost of my paper, which with reference to taxation mean the tax rate per unit of the commodity. The tax rate is not the same as the difference between the price of a commodity before the tax and after its imposition. (Hotelling to Allais, 4.6.1946)

Hotelling pinpointed that Allais's definitions and therefore results were not equivalent to his own (Allais wrote A and B for goods and a and b for prices). Yet, when the difference was considered, the two results did match. On July 10, 1946, Allais wrote back to Hotelling. He had no alternative but to acknowledge his "gross" mathematical miscalculations that he subsequently corrected by hand on the original manuscript. He also recognized that if the differentials in Hotelling's formula accounted for differences between "new" marginal costs and new prices, then Hotelling's formula was indeed identical to his own. Yet Allais was not ready to surrender: "I had the impression that the differences you used [in the 1938 paper] were differences between the ancient and the new prices"—and thus his original argument would stand.

On Allais's account, one may notice that the entire issue in Hotelling's 1938 paper, followed by Frisch (1939) criticisms and a re-joinder by Hotelling (1939), was not explicit on this subtle distinction. One has to carefully read Hotelling to notice that he set-out the supply curve equal to the marginal cost curve (under the hypothesis of free competition), "in the sense that each would regard the price as fixed beyond his control" (Hotelling 1938 in Hotelling 1990, 142). However, one may also conjecture about Allais' and Hotelling's mastering of each other language as a source of misunderstanding. Indeed, they both wrote to each other in their native language.

In any case, Allais insisted in his letter that the issue of marginal cost pricing was a major one that deserved attention and promised a careful studying of it “since a coupling has still to be done” (Allais to Hotelling 10.07.46). Tactfully, Hotelling answered that he was “looking forward to seeing the outcomes of . . . the relation between the results” for it would undeniably be “a matter of great interest . . . for our science in general” (Hotelling to Allais 22.07.1946). Yet nothing came out of this wishful thinking in the following years, each party standing firmly by its own convictions.

Technical or Political Disagreement?

Apparently, the short exchanges between Allais and Hotelling centered on a (minor) technical disagreement. Yet one might wonder whether their disagreement would not be rooted in deeper social and political concerns. As a matter of fact, the first letter Allais wrote to Hotelling (20.04.1946) came not only with the aforementioned “Note” on the dead loss, but also with a copy of books Allais recently published: his theoretical study *Économie pure et rendement social* (1945a) but also the more applied- and politically-orientated book *Prolégomènes à la reconstruction économique du monde* (1945b). Hence, everything happened as if Allais was as much interested in communicating about pure economics as about practical wisdom, and more essentially about the link between the two aspects.

Clearly, the calculations of the dead loss by Hotelling and Allais took shape within quite different perspectives, motivations and approaches. In his 1938 paper, Hotelling’s main point was to prove that the replacement of income taxes (such as inheritance, taxes on site value, and similar taxes that are not proportional to sold commodities) with excise taxes would always imply a dead loss. Therefore, Hotelling implicitly suggested that any rent—be it a land rent, a monopoly or oligopoly rent, a toll or a charge—would diminish the social optimal welfare. Virtually everything should be sold at marginal cost.

Politically, Hotelling was committed to Georgism, and had embraced its main policy of taxing inheritance and the site value of land. Georgism, or Single Tax, was a political philosophy and party that mainly favored a 100% tax on the site value of land. Despite some political support, Georgism suffered a poor reputation among professional economists (England 2015). Hence, Hotelling's 1938 paper can be read in part as an attempt at showing that Georgist policy recommendations were tantamount to optimal welfare policies. Allais most certainly never realized the implicit political endorsement in Hotelling's paper, since Georgism never spread outside the USA. Yet, when one considers Hotelling's political background, his previous work on taxation and on natural resource economics, Georgism was a persistent theme indeed (see also Mueller 2019; Gaspard and Mueller 2020).

Allais endorsed a rather different political outlook from that of Hotelling, not directly linked to any established economic reforms in France—even if he campaigned for a federalist Atlantic Union (Diemer 2010a). On the one hand, Allais's *économie pure* advocated a form of liberalism, confident in the superiority of free prices and market allocation in the general case.¹⁶ Yet on the other hand, Allais suggested marginal cost pricing by authorities for the specific case of the undifferentiated sector; the market mechanism would have granted marginal pricing for the differentiated sector without state regulation.

Jointly with his letter of April 20, 1946, Allais sent to Hotelling a note “On the application of the theory of social rent to the problem of collectivization of firms” where he clarified in further details what kind of marginal cost pricing he thought to be necessary. Allais considered that the distribution of water, gas, electricity or the railway system (i.e. public monopolies) belonged to the undifferentiated sector, while metallurgy and coal mines,

¹⁶ As is well known, Allais participated in the first meeting of the Mont-Pèlerin Society but refused to sign its “Statement of Aims”. In the post-war period, Allais outlined his own model of “*planification concurrentielle*” (see Diemer 2010b).

for instance, belonged to the differentiated sector. Regarding the differentiated sector “there is no theoretical argument in favor of collectivization” (p. 1), while for the undifferentiated sector an optimal management would require collectivization.

The optimal use of exhaustible resources (such as mining) was also a subject Hotelling (1931) had dealt with. But instead of considering collectivization, he was in favor of a tax on rent (mine rents eventually), following the famous Single Tax policy of Georgists (eventually rising up to a 100% tax). Allais underlined that if collectivization would imply that the government owns public monopolies, it would not mean central planning and public management on a daily basis. On the contrary, the managerial board of public monopolies should be as independent as possible, with the sole restriction of selling at marginal cost.

Hotelling and Allais undeniably shared a certain taste for mathematical rigor, and the belief that ordinal analysis and a massive use of mathematics should replace intuitive—purely literary and intuitive—reasoning in economics. Proper economic calculus and technical argument ought to become the bread and butter of public decision, if it aimed at economic efficiency and social justice. From this perspective, Allais and Hotelling evolved amid an unfriendly environment, where only a handful of fellow-specialist shared their methodological outlook.¹⁷ While Allais and Hotelling understood—at least implicitly—this common ground between them, they nonetheless never explicitly engaged in applied- or politically- orientated discussion that probably would have helped clarify their technical argument.

¹⁷ In a letter dated April 22, 1947, Allais solicited Hotelling’s views on his article on “Physical productivity of indirect production processes”, asking for relevant Anglo-Saxon references, and for mentoring on where to publish such a piece. The latter was indeed a tricky question, and Hotelling (28.04.1947) warned Allais not only about the “length” of his paper (four times the standard) but about the fact that “many economics journals tend to refuse mathematical papers on the ground that these are unintelligent to their readers”—indicating *Econometrica* as virtually the only possible place for publication.

All along 1947 and 1948, Allais and Hotelling exchanged letters—the former regularly sending his work and soliciting responses from the latter.¹⁸ By the end of 1948 (day and month missing), Allais asked for help in welcoming to the U.S. the arrival of his protégé, recipient of a Rockefeller Fellowship, that happened to have “especially worked during those months on the question of the influence of taxes from the point of view of social return”. Hotelling’s unpublished papers show that he took Allais’s criticism very seriously, and had most probably a high esteem of his fellow French. He certainly continued to analyze the problem, especially its potential applications. One may find several handwritten notes dealing with “Allais criticism” (for instance 5.8.1951 Box #46, Folder ‘Lectures (I)’) and even an unpublished Manuscript entitled “Marginal cost pricing theory and the new French electricity rates” (17.11.1951 #46, Folder ‘Lectures (2)’). Hotelling also came to Paris on Allais’s invitation in 1951. As for 1948, he kindly accepted to help Allais’s “first order mind” student to find his place in the US academic context.

3. Debreu Comes into Play (1947-1948)

The brief exchange of letters between Hotelling and Allais on summer 1946 was insightful but inconclusive. Yet this episode did not coincide with the end of dead loss controversy. On the contrary, it opened a new step of the controversy marked by a long run discussion between Allais and his younger college Gérard Debreu.

Since 1946, Debreu was research associate at CNRS under Allais’s tutelage. Unlike Divisia and Allais, Debreu was no engineer. He was an highly trained mathematician that studied during the war at the *École normale supérieure* (ENS)—the only other place teaching

¹⁸ The Allais-Hotelling correspondence did continue beyond that date, but apparently without further discussing the dead loss.

science able to compete with the *École polytechnique*, according to Allais (1989a, 3). During his ENS years, Debreu had embraced the credo of the Bourbaki collective of mathematicians, in search for high levels of abstraction and “mathematical purism” (Düppe 2012, 416). Yet at the same time, Debreu (1991, 4) would have not be “ready for a total commitment to an activity so detached from the real world” and eventually came to economics for he was concerned with the post-war reconstruction, much like Allais. Debreu was first disappointed by the theoretical weakness of economics teaching in France, until he came to read Allais’s *À la recherche...* current Spring 1944: he then “discovered the theory of general economic equilibrium, and found a scientific vocation” (1991, 4). While Allais was never formally Debreu’s teacher, he did embody the role of the master, at least for a few years. Their exchanges gave a new turn to the dead loss controversy and help us documenting how Debreu gradually emancipated himself from Allais.

Supporting Allais’s Formula

We do not know precisely whether it was Allais who openly solicited Debreu’s help to overcome his predicaments, or if the latter forced his way into the discussion. What is clear, however, is that on May 30, 1947, Debreu wrote to Allais asking for Hotelling’s papers, and that he mentioned having read Hotelling’s letter of June 4, 1946. A fourteen-page long note untitled “Evaluation of the social loss” followed, in which Debreu compared Allais and Hotelling’s formulae and discussed extensively the meaning of differentials used, and provided a series of criticisms addressed to Hotelling’s 1938 paper. In particular, Debreu noted that Hotelling differentials of prices (dp) had changed of definition from the 1938 paper to the 1939 (answer to Frisch) and the letter addressed to Allais (04.06.1946). The final interpretation outlined in the letter, where the differentials were differences between price and marginal costs, was for Debreu the only one that stands.

Yet just like Allais, he also spotted an “imprecision of the same kind in the definition of dq ” (Debreu’s note, p. 7). Hotelling would use q and dq to denote quantities of goods. Those quantities would be produced during the economic process and—following Hotelling—consumed by individuals. Debreu suggested a more general model accounting for the fact that some goods may be consumed in the production of other goods. Thus, in that case, they are part of the production and consumption process of the economy, but they should not be part of the satisfaction of individuals. In doing so, Debreu pinpointed an imprecision that both Allais and Hotelling had missed, i.e. that the amount of goods in the economy and the amount of goods in the satisfaction functions are not necessarily the same.

Concerning prices, Debreu was more directly influenced by Allais. Yet, despite what both Allais and Debreu claimed, Hotelling did have a coherent definition of price (he set initial prices equal to marginal costs) differentials and did not change his interpretations, even though he stated it explicitly only in his letters to Allais.

On August 23, 1947, Debreu wrote another letter to Allais (but some in-between must be missing) mentioning some further calculations on Hotelling’s note. This letter reached Allais while he was in the U.S., attending the international meeting of the Econometric Society held in Washington (September 6-18)—together with a rather dense French delegation (including economist-engineers as Divisia, René Roy and Jacques Rueff). There, Allais gave a talk titled “*Rendement social et productivité sociale*” in one of the two sessions on the “Theory of choice and utilization of resources” chaired by Abraham Bergson. During his stay in Washington, Allais had the chance to meet Hotelling in person (as well as Oscar Lange), and gave him a copy of Debreu’s booklet.

It was only two years later, on September 21, 1949, that Allais wrote again to Hotelling sending yet another copy of Debreu’s 1947 note. At that time, Debreu had completed a second article (that he started writing as soon as summer 1948), with substantial technical

improvements but also more interested in developing his own perspective (and no more in proving Allais's or Hotelling's right). By the end of 1948, Debreu had arrived in the U.S. (first in Harvard) thanks to a Rockefeller Fellowship Allais got him, with the aim of studying "the application of the most advanced economic theory to practical problems, chiefly in the field of planning".¹⁹ In a long letter dated May 13, 1949, from Cambridge (Mass.), Debreu informed Allais about how he would spend the coming months of his Rockefeller Fellowship, including short visits he planned to various universities.²⁰ More critically, Debreu mentioned his projects for the near future. No doubt, he felt his U.S. experience was truly positive: even though he confessed having been "disappointed" by Schumpeter for instance, Debreu felt having "benefited greatly" from vivid academic discussions and the "melting of ideas so vital to our discipline".

However, after these five months spent in the U.S., Debreu was now seriously considering a career within French institutions, hopefully "with part of my time free for my own work, in contact with *reality and action*" (emphasis added); although he was longing for a job with a "less indecent salary than at CNRS". More significantly, Debreu's return to France would also be associated to his eagerness to assist and continue Allais's research program.

Early Signs of Emancipation

On second thoughts, other passages of Debreu's letter (13.5.1949) may have irritated Allais as well. Indeed, Debreu claimed that he "felt it necessary to pick up on several points that [Allais] have already covered" on the issue of the dead loss, but by outlining the same content

¹⁹ Gerard Debreu RF Fellowship Card Record Group 10.2 Box 4, Rockefeller Archive Center.

²⁰ We know that Debreu officially toured Harvard, Berkeley, Chicago and by the beginning of 1950 Uppsala and Oslo.

in a “perhaps slightly simpler and more rigorous way”.²¹ He also insisted that this new note (the upcoming 1949-paper) would eventually result in a standalone article, hence without that many references to “the *Discipline*”. Discipline proved to be a remarkable choice of word. Debreu was clearly referring to the title of Allais’s 1943 book, but this word also revealed his intention to free himself from both the way Allais approached the subject-matter (the economic *discipline*) and the overwhelming personal *discipline* Allais was imposing on his followers.²²

Debreu claimed having “no scruples” in eluding direct references to Allais, since economists would be known for “repeating each other’s” without bothering about the issue of priority. On the one hand, Debreu was clearly trying to free himself from the influence of his (past) master. On the other hand, Debreu probably felt that structuring his paper on the basis of some internationally unknown French work—Allais’s *oeuvre*—would undermine his credibility in the eyes of Anglo-Saxon economists.

Looking back at these past couple of years, the relationship between Allais and Debreu has already evolved a lot. In 1947, when Debreu wrote his first article, it was merely intended as “additional calculations” of Allais’s note on Hotelling, and not even signed with Debreu’s own name. Hardly surprising, since he was still acting as Allais’s assistant, candidly looking back at a mistake he made during Allais’s last exam; or asking for permission to claim Allais’s “paternity for the introduction” in economics of this idea or that tool (letter

²¹ Weintraub and Mirowski (1994, 261) suggested that Allais’s 1943 book enclosed “very primitive mathematics from a Bourbaki point of view, though it was more sophisticated than most neoclassical texts”. Whether Debreu did share this viewpoint is open for debate. Allais most probably did not know the Bourbaki credo.

²² Debreu explained that he would not mention Allais’s 1943 book for it is “out of print, exists only in very small numbers, and that Alas! most of those who own it have not read it” (13.05.1949).

23.08.1947).²³ In a letter from May 13, 1949, Debreu sent a far more mixed message, as we saw: the process of emancipation was already underway. Essentially, Debreu was trying to inform Allais that thanks to his fresh U.S. experience, he would now think “rather differently” from him on some accounts, while being optimistic on how Allais would accept changes: “but I am sure you will enjoy it as much as I do”. Yet as Debreu had partly anticipated, Allais proved anything but happy about the new orientation his *protégé* was taking.

4. The Allais - Debreu Argument (1949)

The second article Debreu sent to Allais current summer 1949 presented a series of breaking novelties, both in its technical form and final results. In particular, Debreu’s article substantially diverged from the approaches of both Hotelling and Allais by introducing topological tools in the discussion. He aimed at reaching general, sophisticated and “clean” mathematical results on the dead loss controversy. Debreu’s way of “washing the dishes”—in Divisia’s expression—required a combination of new mathematical tools with abstract reasoning. Yet Debreu’s muscular washing was consuming the “dishes”, losing the connection of theoretical tools with applied policy making and their power to reform society that was crucial to Allais’ intentions.

In a letter dated September 10, 1949, Allais wrote a severe and lengthy (nine-page) criticism of Debreu’s second article. While Allais first acknowledged Debreu’s “usual qualities of clarity and concision”, he then provided a long and detailed series of comments expressing a clear frustration. To put it bluntly: Allais either stressed that Debreu’s mathematical expressions were his, or denounced any deviation from it as mistaken. Debreu

²³ It is not clear from the correspondence in which capacity Debreu took this examination: was he an assistant who had to mark it, or was he a free auditor following Allais’s classes to bring himself up to speed on economic issues? The second option is more likely.

answered on December 16, with an almost as long (six-page) letter. He first informed Allais that the original article already changed a lot, and that in this new—more concise—English version of the text, most of Allais’ previous comments would “disappear on their own”.²⁴ However, Debreu provided a detailed response of Allais’s remaining comments, where most of the answers worked as counter-attacks.

This exchange—Allais’s criticism (10.09.1949) and Debreu’s response (16.12.1949)—was as tense as it showed intellectually meaningful. Indeed, their discussions can be re-organized around three mains—interrelated—points of contention charting the epistemological break-up that was happening between them. This break was obviously concerning Debreu’s revised appreciation of the Allais-Hotelling controversy, but it also spread to questions about the place of French science among the international community, and to the purpose of economic science as a whole.

Overcoming the Allais-Hotelling Debate

The main conceptual innovation Debreu provided in his second article was a definition of what he termed the “powerfulness” between economic states (see Appendix 1 for precise definitions). Debreu—following Pareto and Allais—defined an economic state E^0 as *superior* to another E when at least one individual in E^0 has a higher satisfaction than in E , and no one has a lower satisfaction (today, we would say that E^0 is a Pareto improvement over E , and we would call superiority “Pareto improvement”). “Powerfulness”—Debreu’s novelty—was a more general concept than “superiority” for it allows to rank economic states that are not superior one to another. One may nevertheless define superiority in term of powerfulness

²⁴ Debreu insisted that in the new version of the second booklet worked on in contact with the editor of *Econometrica* (that would be Ragnar Frisch). Hence this paper was very likely to be published in this journal eventually, as it did in a quite revised form as we will discuss later (see Gerard Debreu 1951; see also the working papers for the Cowles Commission 1949; 1950a; 1950b).

(therefore powerfulness is a more general concept), and optimality is also definable in terms of powerfulness: “The necessary and sufficient condition for an economic state to be an optimum is that there is not a more powerful state than it” (p. 13). Thus, powerfulness would allow economists to “measure” how far an economic state is from optimality, or alternatively “how much” is lost (in a dead loss). Thinking in term of powerfulness implies thinking in terms of partial orders, a topological concept, and naturally introduces topology as a suitable instrument.

Debreu devoted the remaining part of the article to improve the concept of powerfulness, and to restate Hotelling’s results within this new framework. Debreu’s main conclusions were now that Hotelling’s results were compatible with his more general case under appropriate approximations, although—he claimed—*only* as an approximation.

Regarding Allais’ “pending controversy” with Hotelling, the former could not miss that in this second article, Debreu was now much less straightforward about who was right (indeed this question was now irrelevant from Debreu’s perspective). More upsetting from Allais’s viewpoint, Debreu was now giving more space to Hotelling’s contributions—in particular what Debreu termed Hotelling’s “famous” article (p. 25)—rather than Allais’. Allais deplored this uneven treatment that he attributed to Debreu’s presumed youth conformism and lack of autonomous judgement—Allais teased him: “No doubt you were impressed by the ‘celebrity’ status of the Hotelling article you highlighted”.

Certainly, Allais got very upset about both these aspects, and attacked the general relevance of Debreu’s second article in a vexing way. Indeed, Allais claimed that Debreu’s first article of 1947 was in fact much more interesting than this second opus, hence reducing Debreu’s work as mere illustrations: “I think the American audience would have been interested in a debate between Hotelling and myself on the issue of loss”, what would be the real role of Debreu’s contribution according to Allais (Allais to Debreu, 10.09.49). Allais’

preference for the first note “astounded” Debreu for he felt he “had not reach the heart of the issue” by comparison to his second opus (p. 5). So, when Allais urged Debreu to “show more precisely that Hotelling’s formula is wrong” (p. 5), Debreu seized the occasion to push right where it hurts in his reply:

The most amusing is, naturally, that the classical Dupuit-Hotelling formula is correct, as I have been striving to prove. It was rather the reasonings to prove it that were wrong, and I don’t recall hearing you defending that thesis. (Debreu’s letter to Allais 16.12.1949, p. 4)

Yet this is not exactly what Debreu stated in his article, where he was much more cautious. No doubt that Allais’s admonitions did not invite Debreu to more nuance in his response letter.²⁵

French Economics in the International Context

The crux of Allais’s criticism to Debreu rested on the way the latter related—or rather did not relate—to the Allais-Hotelling controversy, but it eventually extended to other aspects. Indeed, Allais’s main, pervading and repeating concern with Debreu’s new 1949-article was its lack of explicit references to Allais’s publications.²⁶ Through and through a nine-page long letter, Allais insisted more than thirty times about missing quotations and links, or insufficient appreciation of both his theoretical books and the private correspondence he had with Divisia and Hotelling—after all, Allais claimed, “your study originates from this correspondence” (p. 5). Hence Allais urged Debreu not to “suppress every trace of paternity”

²⁵ A couple of years later, Debreu (1951, 283) stressed that Allais, and Lange, provided more “general” and “synthetic” approach to the dead loss than rival definitions, including Hotelling’s.

²⁶ Therefore, Debreu’s previous warnings that he did not wish to refer extensively to the work of Allais proved self-defeating (Letter to Allais, 13.05.1949).

towards him: how else could the Americans discern the links between “your work and mine”? In that way, Allais stressed in conclusion, “you will only do me justice and just be honest”.

Debreu far less referenced Allais’s work nor acknowledged his intellectual debt to him as much as he did in his first 1947-article—or even in his first published article (see Debreu 1949). Allais outlined such a list *à la Prévert* also to address to Debreu a vigorous reminder of how much his own research owed him, and that he should not forget so quickly under the excitement of his new American surroundings.

There is certainly a hint of pride in Allais’s complaints: “I hardly see how you could compare the work of Bergson, Barone or Lange with mine. I may lack some modesty, but there is no common measure” (p. 2). But he was also fighting for more than personal recognition: the revival of French science—and in particular that of the economist-engineer—was at stake. The question of precedence and the place of Allais’ research program within the broader picture of international economics was indeed a very sensitive subject:

You know how some American economists resist my work by trying to underestimate it . . . I am therefore compelled to point out the paternity of certain ideas whenever I have the opportunity to do so. (Allais to Debreu, 10.09.49)

As an illustration of the suspected U.S. resistance to his work, Allais mentioned the recent book-review of his *Économie et intérêt* in the *American Economic Review*. The “American economist” who wrote the review was in fact Andreas G. Papandreou, a Greek *émigré* who had obtained his Ph.D. from Harvard University in 1943, and was at the time Professor at the University of Minnesota.²⁷ The comment by Papandreou (1949, 751, 754) was not exactly diplomatic: while emphasizing that Allais’ “abstract and mathematical” analysis was “elegant and interesting”, his unacquaintedness with the “Anglo-Saxon and Scandinavian literature”

²⁷ Papandreou returned to Greece in 1959 where he eventually founded the Greek social-democrat party. He served several terms as a Prime Minister (in the 1980s and 1990s).

would have led him to “arrive independently at some conclusions already well established”.²⁸ Therefore, when Debreu had replaced Allais’s contributions within a long list of foreign economists, as just a name among others, he was not only turning by anticipation Allais’s call for paternity against him, but also very clearly cutting loose his—intellectual as well as personal—ties with the master.

In his response letter, Debreu claimed he would have now correctly referenced Allais’s works (he more or less did) and had “explicitly” stated that the definition of σ_A (i.e. the value of the dead loss measured in terms of a good A) was Allais’s.²⁹ Nonetheless, Debreu also dismissed the half-veiled charge of plagiarism by Allais. A very noteworthy example of this is the way Debreu responded Allais’s claims of being the first one to have demonstrated the “existence of infinite maxima states” (so he should be cited accordingly). Debreu opposed a three-step response. First, Hicks (1939) and especially Lange (1942)—and probably others if one looked closer—would have explicitly indicated this argument before Allais did. Second, Debreu underlined that, in any case, he did not enter economics to engage in a vain “searches

²⁸ Did Papandreou deliberately undervalue the novelty of Allais’s contribution? It is somehow unlikely. Since Allais did not master the most recent international literature, he was leaving the reader—here Papandreou—lost in an eight hundred-page volume without any roadmap. Indeed as George Schakle (1949, 88) stressed more tactfully: if Allais had managed to “write a shorter book”, then his “original contributions” could have “stand out more clearly”. In hindsight, several economists stressed the importance and precocity of Allais’s theoretical contributions, such as the overlapping-generations model, among other things (Samuelson 1982; Malinvaud 1986; Grandmont 1989; Munier 1991). Yet the question of scientific priority is extremely difficult. Indeed as Roy Weintraub (1991, 76–87) emphasized, one could claim Allais’ priority in proving stability of general equilibrium as early as 1943 only by reading “the past from the present in a thoroughly Whiggish manner”.

²⁹ More generally, Debreu reassured Allais that “justice is done to you for the origin of my study”. And indeed, the final version of the text did state: “To nobody is my debt . . . greater than to M. Allais, whose interest in this kind of question has been the origin of mine” (Debreu 1951, 273).

of priority”.³⁰ The two argument seems almost contradictory (*either* one shows that an idea was previously stated elsewhere, *or* there is no point in looking for origins at all). Yet this apparent contradiction clears up with Debreu’s third argument about his willingness to fit in his new international *milieu*, and thus to specify within it the place of Allais’ contributions:

I am by no means trying to diminish the value of your work, but it is absolutely necessary to fit it in the context of a developed Anglo-Saxon current of thought if I am not to make a fool of you and of myself . . . (Debreu’s letter to Allais, 16.12.1949, p. 2)

On deeper consider, Debreu was starting to look at the whole issue of welfare economics as a consistent international research program, were relevant contributions came from various perspective. To some extent he wanted to acknowledge his intellectual debt toward Allais. Yet clearly his new U.S. intellectual environment and the reading he must have had access to were starting to change his way of thinking and writing.

Applicability or Sophistication?

Allais’s ambition to embody the restored credentials of French science was central in his criticism, but he also engaged Debreu’s work from the methodological perspective, especially regarding what would be the true purpose of the economic science. One of the clearest departing Debreu made from Allais is the methodologically twist in the mathematical tools. In the first part of his 1949 article, Debreu relied heavily on tools familiar to Allais, such as differential analysis, Lagrangian multipliers, and more generally infinitesimal calculus. However, when Debreu introduced his new concept of “powerfulness”, he did so by using a new branch of mathematics, namely topology. Hence, he started to replace Lagrangians and

³⁰ The issue of priority and scientific credit turned to be almost as important for Debreu as it was for Allais, but in a different way. In particular, Debreu’s path reveals a “tension” reinforcing his “strong need for driving personal credit” on the one hand, and his Bourbakian ambition to develop “impersonal knowledge in science” on the other hand (Düppe and Weintraub 2014b, 238–39).

differential equations by partial orders, supremum and infimum, as well as other topological notions. The comparisons of states in term of a given good using σ_A was already present in *À la recherche...* (p. 595). However, Allais always thought in terms of differential analysis, and never used any topology.³¹

This methodological shift echoed the different training of the two men. On the one hand, Allais, the engineer-physicist, thought in term of trajectories and geometrical representation. He also stressed the importance of tangible interpretation of abstract models, and of empirical testability. On the other hand, the (Bourbakian-orientated) mathematician Debreu cared about the generality of results, higher abstraction level and getting rid of working hypothesis—such as continuity, derivability, local maxima—as far as possible. Allais opposed Debreu’s approach by stressing that his proof was “gaining in simplicity what it loses in resemblance with reality” on the account that “a) the firms are clustered together” and “b) the second order is neglected” (p. 2).

On several occasions in his letter, Allais also blamed Debreu’s lack of interest for rooting his theoretical work in reality. He complained about missing “tangible examples” (p. 2) where the issue of the dead loss applies; examples that Debreu promised in the introduction of his paper but hardly provided afterwards. As we stressed, one of Allais’s reasons in dealing with the marginal price controversy was to act as a Prince’s adviser. He wanted to enlighten and counsel policy makers. This was to a large extent what he thought economics was about. Debreu’s taste for mathematical abstractions was conflicting with his views, and this quarrel about concreteness is an important one in their dispute and in the larger aim of economic science itself.

³¹ In his introduction to the third edition of *À la recherche* Allais wrote “I have often given general properties of a market model independently of the restrictive hypothesis of continuity, derivability and convexity, unfortunately without realizing the importance of this implicit model” (p. 95).

Finally, Allais reprimanded Debreu for neglecting a lot of (peripheral) theoretical elements in his article, and in particular to “overlook the issue of the stability of the competitive regime” (p. 2)—a central question according to Allais.³² By contrast, Debreu was developing a growing interest in the question of the existence of the general economic equilibrium, as is well known. But there is also an obvious difference of approach of style between the two men. Debreu retorted that he “*don’t need to*” talk about all the elements Allais mentioned for he has a “perfectly limited subject to treat in a reduced space”. Hence remarkably, Debreu envisaged writing his article just like an (argumentative) maximization under (words) constraint. From that regard, Allais happened to be the Debreu’s antithesis. He was a prolix writer beyond every accepted canon, and his style often displayed a convoluted language, both discursively and mathematically—an obvious reason, in addition to the French language, for the limited reception of his work.

5. Debreu Embarks on a New Road (1950-1951)

In face of the vigor of Allais’ letter (10.09.1949), Debreu’s response was slow to come. When he eventually took up his pen (16.12.1949) he was in New York, about boarding the ferry for France. Debreu’s late answer was due to two reasons. First, Allais’s original letter never reached him—indeed Debreu was touring U.S. universities as we noted, hence often changing places—and he received copies only several weeks later. Second, and more importantly, Debreu was trying to get through what he felt were purposely “hurting” comments by Allais. That is why he would have waited to write a more reasonable answer, though with mixed results as he himself recognized: “I don’t think being completely successful [in avoiding

³² On Allais’s 1943 demonstration of stability, see Lenfant (2005).

inflexibility], it is probably humanly impossible”. As we saw, Debreu did answer with more than a sight of bitterness.

This tense exchange Allais and Debreu fueled all along the second half of 1949 had certainly deep consequences on their personal relations. Although Debreu ended his response letter being confident that both of them will surely “overcome their misunderstandings”, he nonetheless wondered whether their “friendship would resist a long time under such shocks”. According to Allais, these shocks were truly “beneficial”, as he scribbled on the margin of this passage. Allais was largely immune to this tense intellectual climate, and perhaps he even thrived in this kind of adversity—that was certainly not the case for Debreu.³³ In any case, their “friendship” did survive, at least seemingly. And Debreu did not forget to take with him back to France the “electric razor” he had promised Allais. What Allais might not have known at that time was that Debreu has already been offered by Tjalling Koopmans a short-term position at the Cowles Commission. Debreu likely informed Allais about it during his stay in Paris in the last days of December 1949.

Putting a Price on the Dead Loss

The next letter we have by Debreu is from June 8, 1950, when he announced having just took up his new functions at the Cowles Commission in Chicago. Attached to this letter came a draft copy of *The coefficient of resource utilization* (eventually published in 1951) he had just presented at the Cowles, which was in fact a revised version of his second—1949—article. Debreu was very enthusiastic about the results he obtained with an expression of the maximization of the “standard of living” (\vec{S}) under the conditions of limited technical knowledge and physical resources that would be “crystal-clear”, “concise” and of “general”

³³ On Debreu’s aspiration to hermetically separate the mathematical analysis of “abstract structures” from “contestable intellectual activities” he despised, see Düppe (2012, 421).

validity. As D ppe and Weintraub (2014b, 138) stressed, the first version of this paper was instrumental in setting the proof of optimality (the fundamental theorems of welfare economics); a proof worked out through convexity analysis (not calculus) that Debreu presented at the meeting of the Econometric Society in Harvard in August 1950.

In this new version, Debreu was following Allais in including production on his model, yet using the topological tools he had freshly introduced. Debreu succeeded in providing an accurate measure of “how much” is lost in a dead loss, that he evaluated on a money rod, thus avoiding Allais’s dependence on a particular commodity. In order to evaluate more generally the dead loss, Debreu developed a coefficient that would provide a “money metric measure” of the minimum distance separating an economic state characterized by a non-optimal set of resource allocation from an optimal economic state (Pastor, Lovell, and Aparicio 2012, 109).

Despite Debreu’s course towards abstract reasoning, he wanted to let Allais know that his new results also carried possible applications, such as “the comparison of economic organizations, including classical taxation problems [...] the increasing of available capital for a given economy [and] the gain associated to technical progress” (Debreu to Allais, 8. 6. 1950). In the published version of the paper, Debreu (1951, 285–86) clarified further that his coefficient of resource utilization was accounting for a (social) dead loss that would originates from “one or several of the following sources”: first, an “underemployment of physical resources” (labor, machinery, lands etc.); second, an “inefficiency in production”; third, the “imperfection of economic organization” (as in the case of monopoly or indirect taxation for instance). Hence Debreu provided an approach more general and that was enclosing economic issues stressed by both Hotelling and Allais. However, in contrast to them (and to his own initial draft), claims about possible applications virtually disappeared from the published paper by Debreu.

By contrast, Allais was still concerned by the potential applicability of theory to political issues. Indeed, his commitment with tangible action and policy making remained unbroken, which appeared to be much less the case for Debreu. In his previous letter, Debreu (08.06.1950) stressed that eventually he may provide “some tangible evaluations for the American economy” based on his new coefficient—but apparently nothing would come of it. The published version of Debreu’s article provided no calculation of the sort, which is far from surprising if one considers that he not only disregarded scientific confrontation with the actual world (Düppe 2012, 417).

The theoretical leap Debreu made was rooted in the introduction of new mathematical tools to economic analyses. Indeed, Debreu’s new formulation provided several advantages, as he himself underlined: the extensive use of topological tools allowed to consider non-continuous functions, global instead of local maximization and more general results. In his letter to Allais, Debreu stressed that the concept of hypersurface (3) he used would be “not only extremely intuitive” but also has “not yet appeared in economics” (8.06.1950). Interestingly, Debreu’s thirst for finding the most adequate mathematical instrument to treat a specific economic problem (here the evaluation of the dead loss) was then insatiable, and he mentioned being working in depth on “linear models”, a new “literature that would be proliferating though nothing was published yet”.

Closing the Controversy?

The near-article Debreu sent on June 1950 was accompanied by a brief sum-up note in French, probably written for the sole use of Allais. The space between them was increasingly widening—the mathematical technique, the language of writing—but Debreu was still trying to bridge this gap. The tone of Debreu’s letter (08.06.1950) was now far calmer compared to the tense words he shared with Allais some six months ago. Debreu was convinced of his technical superiority and the importance of the result obtained. He even reassured Allais that

their disagreement—and in particular his criticism of Allais’s concept of “distributable surplus”—would remain private, i.e. absent from the final version of the text.

Moreover, Debreu insisted that, although Allais’s formula was a “plausible numerical measure of social return”, his own concept of “coefficient of resource utilization” would be far superior: “you will no doubt agree with me that giving an arbitrarily chosen good such a special role to play was a great methodological disadvantage”. Debreu (1951, 287) outlined very clearly this difference in the published version of the article, stressing that Allais’s “exposition and *its results* rely entirely on the asymmetrical role played by a particular commodity” chosen “*arbitrarily*” (see also Debreu 1954, 14).

In his last answer on July 13, 1950, Allais was done fighting a technical battle he could not possibly win. Just as in the case of Hotelling, Allais had no choice than to back-up and acknowledge the authority of his now former disciple. If he agreed to do so, it was nonetheless accompanied by a last concern. Indeed, Allais doubted the actual applicability of Debreu’s results: “What formulas do you get? he asked, *If they contain only calculable terms*, your work represents certainly a considerable progress” (13.07.1950, emphasis added).³⁴

Incidentally in this last letter, Allais also asked Debreu if the “project to build in France an econometric Center, partially financed by the Rockefeller Foundation” was still on tracks? Was Allais still hoping Debreu’s return to Paris? Did he believe that Debreu would keep his promise to renew the French economic science with him? Or more simply had Allais envisioned the progress the two of them could achieve by working closely together? Unfortunately, their correspondence gives no indication whatsoever to answer in one way, or the other. In any case, this project for a new Center went unheeded, and as we know Debreu

³⁴ Allais’s concerns somehow anticipated the legacy of Debreu’s coefficient: if the concept has been often cited in the economic literature it only had a “mere tangential role” (Ahlheim 1988, 21).

embarked on a live-long Professor career in leading U.S. universities such as Yale and then Berkeley.

Concluding Remarks: Success, Failure or Something Else?

In the aftermath of World War Two, the United States established their ascendancy over the West in military, political, economic as well as scientific terms. In this context, Maurice Allais came as an outsider pursuing the ambition of restoring the place of French economic theory on the international scene. For Allais, one way to assert his authority was to prove that he could actively contribute to the technical discussion of the most recent economic literature on the international scene. Therefore, when Divisia suggested to him that he had come up—in his 1943 book—with a different calculation of the dead loss from the famous formula obtained by Hotelling before the war, Allais seized this opportunity.

After sharpening his arguments by challenging remarks and criticisms from his former teacher Divisia, Allais thought he had formulated a counter-evidence invalidating the generality of Hotelling's formula. But the American economist only had to point out some errors of calculation to disengage himself altogether from some legitimate questions Allais had pointed out. However, Allais remained convinced of his technical superiority and called one of his best assistants to the rescue. After promising initial results in 1947, a couple of years later Debreu was more interested in developing his own analysis of the dead loss than in proving right one of the protagonists of the initial debate—much to Allais's frustration. Perhaps even more disturbingly for Allais, Debreu was moving further and further away from his principles: formally by making his debt to Allais's work less visible; methodologically by introducing new mathematical tools to economic analysis and neglecting the issue of the application.

Eventually, both Debreu (in 1983) and Allais (in 1988) were awarded the Nobel Prize in Economics, the highest recognition in the profession. From that regard, their contributions to economics can hardly be deemed unimportant or failed. However, the tricky question might be: did they contribute to the “prodigious revival of French economics” they were aiming at in the aftermath of World War Two? That Debreu received the Nobel Prize came as no surprise to anybody (if not to everyone’s delight), which was far from being the case for Allais, who remained an outsider even within the French profession (Giraud 2011; Sterdyniak 2011). If one cannot doubt the depth of his analyses, these were hardly incorporated to the mainstream: the “Allais Paradox” being the exception that proves the rule (Mongin 2014, 743). Indeed, following the episode of the dead loss controversy, Allais (1953) embarked on a different kind of criticism of the “American school”, this time directed toward the expected utility hypothesis set forth by von Neumann and Morgenstern. In any case, Allais continued to develop his surplus analysis for decades until he came up to his *Théorie générale des surplus* (Allais 1978/1989b). On that occasion, Allais (1989b, 258–95) came back on the dead loss controversy and reaffirmed the superiority of his initial formula over that of Hotelling (that had passed away in 1973).

With insights, Marcel Boiteux (2010, 215) suggested that Allais’s “main contribution to microeconomics was perhaps having trained to economic calculus a whole generation of students”, including notorious economists he formed—such as Boiteux himself, but also Jacques Lesourne and Edmond Malinvaud.³⁵ As the paper underlined, Debreu’s professional and personal relationships with Allais was pivotal in his early career. Thus, reconstructing the dead loss controversy was also about documenting the early steps of Debreu’s intellectual and

³⁵ Young minds around Allais were instrumental in settling the tariffication (marginal cost pricing) of French public services, such as in the case of electricity (see Yon 2020).

professional path before—or as he was—becoming the economist of the axiomatic analysis of the General Equilibrium.

Would there have been some place in postwar France for the kind of mathematical economics that Debreu was setting-up, and that the *Theory of Value* (1959) would establish as an undisputable benchmark for decades? This paper offered no element to answer adequately this question.³⁶ But by pushing the Bourbakian rational to its logical conclusion, one might argue that, in the long run, Debreu could not truly adhere to the ambitious project of reviving the French economics tradition. Indeed, if economics had to be expressed as far as possible in mathematical terms, and if mathematics was truly a language on its own (the best substitute to discursive language), hence the very idea of “French economics” could only appear as a contradiction in terms. It was only a matter of time before Debreu realized it, and his engagement in the dead loss controversy proved instrumental from that regard.

Appendix

1. Debreu’s 1949 unpublished article (extract)

Following Debreu, let us define the satisfaction of an individual i , owning goods $A_i^0, B_i^0, C_i^0, \dots$ and in economic state E^0 as $S_i^0(A_i^0, B_i^0, C_i^0, \dots)$, and analogously, $S_i(A_i, B_i, C_i, \dots)$ for the satisfaction of the same individual in a state E . Those satisfactions are usually unequal but there must be a quantity σ_i^A of good A such that

$$S_i(A_i - \sigma_i^A, B_i, C_i, \dots) = S_i^0(A_i^0, B_i^0, C_i^0, \dots)$$

³⁶ In a sense, Till D ppe (2017) suggested that Belgium (rather than France) established itself as the French-speaking place for mathematical economics compatible with Debreu’s standards, with the creation by Jacques Dr ze in the 1960s of the Center for Operations Research and Econometrics (CORE), the European version of Cowles.

Let us now define:

$$\sigma_A = \sum_i \sigma_i^A$$

i.e. the sum over every individual of the quantity of good A that we either have to subtract or add to each individual to reach the satisfaction he had in the economic state E^0 .

Debreu states that if this quantity $\sigma_A > 0$ than the economic state E is more powerful than economic state E^0 . He explains the intuition as follows:

We can, in this case take from individuals $1, 2, \dots, i, \dots, n$ respectively the quantities $\sigma_A^1, \sigma_A^2, \dots, \sigma_A^n$ of good A , and equalize every satisfaction with the first state, yet having at our disposal a positive quantity of good A that we may redistribute thus increasing every satisfaction. (p.11)

Note that $\sigma_A^1, \sigma_A^2, \dots, \sigma_A^n$ may either be positive or negative, and only the sum of all of them has to be positive. Of course, if every σ_A^i is positive (or null) the economic state E is a Pareto improvement of economic state E^0 . More generally if a state is superior to another it is also more powerful, superiority being a much stringent condition. Debreu immediately also notes that if $\sigma_A \leq 0$ then “we cannot imply that E^0 is more powerful than E , because if we switch E and E^0 [...] we may find a σ_A' either positive, negative or null” (p. 11). This is due to the fact that satisfaction curves do not linearly increase with quantity of goods.

Take a simple illustration: let's imagine two states of satisfaction for a given standalone individual, in the first state he has an apple, and in the second he has an apple and a pear. Now consider that to reach a satisfaction as high as the second state he needs two extra apples—i.e. $S(3 \text{ apples}) = S(1 \text{ apple}, 1 \text{ pear})$. Yet, if I want to reverse the states, and ask how many apples should I take from the second state to reach the satisfaction of the first,

nothing insures that $S(-1 \text{ apple}, 1 \text{ pear}) = S(1 \text{ apple})$. This is due to the fact that satisfaction does not increase linearly with the number of goods.

Finally, Powerfulness is not a transitive relation, as Debreu noted. Moreover, we may define it on any other good, yet if $\sigma_A > 0$ it may be the case that $\sigma_B \leq 0$, i.e. powerfulness is defined up to a given good.

2. Debreu's 1950 article (extract)

Debreu defined an economic system by l commodities (goods), m consumption units (agents) with consumption vectors $x_i \in \mathbb{R}_l$ associated to each consumption unit i . Production is similarly defined by vectors $y \in \mathbb{R}_l$ (consumed quantities are positive and produced quantities are negatives components of y). The total consumption of the economic system $z = x + y$ is constrained by an upper bound of utilizable physical resources, $z \leq z^0$ and by a technological constraint, $y \in \mathfrak{Y}$ that neither depends on physical resources nor consumption.

Every individual i has a satisfaction associated to his consumption vector $S_i(x_i)$. As usual each individual can rank his satisfactions, interpersonal comparisons are not allowed, and satisfactions are ordinal rankings. Debreu called “*standard of living*” the m -vector of satisfactions of every consumption unit $\mathfrak{S} = (S_1, \dots, S_n)$. Standard of living $\mathfrak{S}^1 \geq \mathfrak{S}^2$ if every individual's satisfaction is equal or larger in \mathfrak{S}^1 than \mathfrak{S}^2 . Standards of livings are therefore partial orders, and maximal elements are called “optimal” by Debreu. A dead loss is associated with each and every standard of living that is not optimal, yet distances between standard of livings have no “meaningful content” (p. 278) since satisfactions are only ordinal rankings.

Debreu restated the problem in a different form. For every possible standard of living, he defines $\mathfrak{Z}(s)$ the set of z utilized physical resources ($z = x + y$) “that enables the economy to

achieve at least s ” (p. 279) under the constraint $y \in \mathfrak{Y}$. This is equivalent to consider the standard of living as a constraint and to minimize z .³⁷

Every optimal standard of living has a minimal set of utilized physical resources that is required for its achievement. Those minimal sets form a hypersurface $\mathfrak{Z}^{min}(s)$, one may therefore “define the magnitude of the loss as the distance from z^0 to \mathfrak{Z}^{min} ” (p. 284), where z^0 is the minimal set of utilized resources associated with a given standard of living. Thus defined, the distance would be a vector over commodity space. Debreu shows that a price vector can be associated to every optimum of satisfaction and therefore employed to give a numerical value (a money value) to this vector. When conveniently normalized, this gives a coefficient, $\rho \in (0,1)$ that he named the coefficient of resource utilization. This evaluation is possible because we are now dealing with sets of commodities instead of satisfactions, thus allowing for interpersonal comparisons on a meaningful scale.

³⁷ We slightly adapted Debreu’s notation and quotations accordingly

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