The Miscommunications and Misunderstandings of Nicholas Georgescu-Roegen

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Abstract

If there is any takeaway from 1971's The Entropy Law and the Economic Process, it's this: beneath every intersection of the supply and demand curve, there's a slow, but steady, process of environmental degradation. Try as you will to recycle waste materials, the book argues—this process cannot be reversed. A formulation of economics backed with this insight was the life vision of Nicholas Georgescu-Roegen, whose work on environmental economics has recently received a new round of academic scrutiny. But one might ask, why wasn't Georgescu well received the first time around, during *his* time? This paper explores that topic.

Introduction

"7. What we cannot speak about we must pass over in silence."
Ludwig Wittgenstein, Tractatus Logico-Philosophicus

The cartoon clippings found among the papers of the late ecological economist, mathematician, and epistemologist Nicholas Georgescu-Roegen (1906-1994) are quite telling. In the first one we encounter Charlie Brown and Lucy playing checkers: it appears as though Lucy has won again, much to Charlie's dismay ("RATS!"). Lucy claims, in triumph, that the victory makes *4000* straight wins, and yet Charlie Brown insists on a rematch, saying, "I want to see if it's been skill or if you've just been lucky!"

In the second, a couple of dismayed baggage assistants at a train station behold a slew of passengers lined up at terminal A, with terminals B, C, D, and presumably down to Z, all empty. The caption reads, *bound to happen sometime, I suppose.*

In the third cartoon (one that Georgescu takes particular care in mounting to a piece of cardboard), an artist, with a blank canvas before him, frantically squeezes a host of paint tubes into a single bucket. As he begins mixing these different-colored paints together, his work environment gets progressively sloppier—the floor becomes littered with empty tubes and cans, some of the paint sloshes out of the mix-bucket. Satisfied with his concoction, he picks up the bucket and throws its contents onto the canvas before him, coating it in with a chaotic heterogeneous layer of paint. The painter is somewhat dismayed by the result—the Mona Lisa. Beneath the cartoon is Georgescu's own handwriting: *In the long run, it has to happen!*—is this a nudge and a wink towards Keynes? Hardly.

We encounter a family resemblance of themes here that gives us a glimpse into Georgescu's academic interests: an obsession with statistical improbability that marks his early mathematical career as a statistician at Paris's Institut de Statistique, as well as the philosophical and interdisciplinary struggle of his later career to wrestle those very statistical improbabilities out of thermodynamics and into the stable-state growth models of mainstream neoclassical economics. But one might wish to draw further from these innocent cartoons.

If we are to buy into the narrative that Georgescu's lifelong assaults on the auxiliary belt of neoclassical economics were by and large failures (as the secondary literature makes abundantly clear), if not outright blunders, we might begin to speculate upon the notion of accountability—*why* did Georgescu's ideas, many of them novel and intellectually stimulating, *fail to penetrate*? One wonders whether these failures were due, in part, to the very sentiment that *in the long run, it has to happen!*, a nagging persistence that Georgescu's ideas, with time, (and the materialization of a couple of competent economists) will *eventually* take hold, something of an inevitability. No procedures need to change. No rhetorical stances need to be reconsidered. No editorial concessions need to be made. In the mind of Nicholas Georgescu-Roegen, perhaps, his distance from the community of economists is a simple bout with bad luck, a case of the common cold that will go away. Charlie Brown becomes Georgescu. Lucy becomes a growing list of frustrated and alienated colleagues and publications. This very salient sense of antagonism arises over a series of exchanges, rejections, and discussions that span the decades, and we see here that checkers becomes the very future of economic thought itself.

This paper will begin with a survey of the secondary literature concerning Nicholas Georgescu-Roegen, much of it written in memoriam, and most of it, especially that contributed by his disciples, containing a marked distaste for the mainstream economic community (what Herman Daly calls the "neo-classical citadel") in failing to recognize Georgescu's genius—a narrative of miscommunication that ultimately places blame on Georgescu's colleagues for misunderstanding him and undervaluing his contributions (we'll call this *reception-side failure*). Armed with this narrative of communicative failure, we'll look closely at Georgescu's life and talk to some of his colleagues. Lastly, we will scrutinize several key exchanges in his correspondences to get a full grasp of these miscommunications, and it is hopefully in heavy light of these instances of historical contextualization and language games that the *reception-side failure* narrative of Nicholas Georgescu-Roegen will be seriously impaired.

Secondary Literature and Reception Side Failure

Much of the secondary literature on Nicholas Georgescu-Roegen paints a picture of a fierce interdisciplinary economist whose foray into thermodynamics, logical positivism, Hegelian dialectics, environmental ecology, and epistemological questions in the philosophy of science, has rendered his work, as put by De Gleria, "a mine of ideas and precious clues which has so far been exploited very little," owing to the "breadth of his interests, his extraordinary mastery of mathematical instruments, his acumen," and, of course, his "originality" (467). Philip Mirowski, in Machine Dreams, refers to Georgescu as "unusually philosophically sophisticated," stating that "economists have been blessed with a lucid expositor of [the direction of time, the nature of life, and the future of the

universe] in the person of Nicholas Georgescu-Roegen, who in a classic book [Entropy Law] sought to inform the discipline in their implications and significance for economics" (46). For all of his intellectual rigor, it was precisely Georgescu's dialectic positioning as a critic of and participant in the neoclassical economic research program that rendered him, as Paul Samuelson put it, a "scholar's scholar, an economist's economist" (Daly 150), who further proffered, "I defy any informed economist to remain complacent after meditating over [Georgescu's] essay!"

Strangely, in much of the secondary literature on Nicholas Georgescu-Roegen, it is the same "philosophical sophistication," that is said to be responsible for Roegen's unpopularity. If Georgescu's ideas failed to gain traction, this was owed precisely to the ignorance and ineptitude of his colleagues. *They* failed to understand *him*. Consider the following surveys of Georgescu's work in the secondary literature:

De Gleria asserts that Georgescu "pointed out the errors (mathematical and otherwise) of his colleagues, and they never forgave him for it. His criticisms were (and still are) largely ignored. Moreover, he was often misinterpreted, but the corrections, if and when they were made, were never made publicly" (Growth 451). Daly writes that Roegen's "contribution to revolutionary science, namely his work on the Entropy Law and the Economic Process, has not yet been victorious, and is revolutionary in the sense that it still faces opposition from the reigning paradigm, the very paradigm which Georgescu himself helped solidify" (Obituary 150, my emphasis). In Cleveland and Ruth, "it is not surprising that Georgescu-Roegen is virtually ignored by mainstream economics. By definition, adherents to a paradigm believe that all relevant phenomena are best understood through the conceptual lens of that paradigm, and that all problems

can be solved with the analytical tools used in that paradigm. The more strident and accurate the attacks, the more they are ignored or explained away by the existing paradigm" (Survey 204).

Beard and Lozada, in Economics, Entropy and the Environment unapologetically assert a sentiment of reception-side failure even more bluntly:

> Additionally, and more fundamentally, we believe that Georgescu-Roegen should be taken seriously by economists, and should be studied carefully, because, by and large, he was correct in his views. Indeed, he was often right about problems when almost everyone else was wrong…History has proven Georgescu correct. (5)

To modern economists, the arguments attributing Roegen's failures to their ignorance or intellectual inadequacy teeter somewhere between circularity and insult; indeed, the idea that scientific and academic communities have failed, at certain key moments, to recognize and embrace conceptual radicalism runs counter to the nature of almost every major scientific revolution of the twentieth century: relativity, quantum dynamics, the foundation and rejection of logical positivism name just a few. In short, revolutionary ideas aren't rejected on the grounds of being heterodox: narratives of the history of science delineate *accepted* inconvenient "revolutionary" ideas in abundance.

To restate, it is the aim of this paper to counter-balance these unmitigated assaults on the economic orthodoxy with a careful delineation of the divergence between the stuff of Georgescu's rhetoric and the multitude of communicative norms within the

communities he sought so fervently to modify. For our purposes, Blaug's reading of Georgescu (1985) begins to align with a reading of Georgescu's poor reception as mired in the obscure nature of *his* rhetoric:

> Georgescu-Roegen's career shows a steady expansion of outlook and interests from an early phase of highly technical mathematical economics, largely in utility theory and input-output analysis, to a later phase of growth modeling and the ambitious attempt to formulate the principles of 'bioeconmics', a new style of 'dialectical' economic thinking to replace more or less the whole of present-day economics with its 'mechanical' mode of reasoning… It is only fair to add that Georgescu-Roegen's later books have not been well received, or, rather, have been respectfully received and quickly put away. For various complex reasons, not to mention the difficult style in which they were written and the intimidating references which they contain to theoretical developments in physics and biology, these works have received virtually no critical discussion from economists…Today at the age of seventy-eight, he is still writing and lecturing. (Great Economists 71-2)

In 1990, Philip Mirowski, who had a year earlier dedicated his history of physics and economics, More Heat than Light, to Georgescu, was charged with the task of providing an overview of Georgescu's work in a volume entitled New Horizons in Economic Thought (Samuels, ed.). Mirowski wrote to Georgescu requesting a list of publications that would aid him in writing the paper, and in doing so inadvertently

solicited a reflection on his life's work and, crucially, (to Georgescu) his puzzlingly poor reception. On 10 February 1990, Georgescu writes Mirowski, "Naturally, when one (who does not have to be modest!) thinks about oneself only favorable judgments come into the subjective focus and none seems unworthy of considered attention." Georgescu then proceeds to tick off a few publications and accomplishments: his contemporary entry in The Life Philosophies of Eminent Economists (Szenberg, ed.), his standing as distinguished fellow in the American Economic Association, Blaug's write-up in Great Economists, and Paul Samuelson's Preface to Georgescu's Analytical Economics (in which he calls Georgescu "an economist's economist").

Georgescu continues, now directly responding to Blaug's description of his works as containing "intimidating [historical and cross-disciplinary] references":

> The reviews of my books, some of them by prominent economists, have generally been quite favorable, at times even directly eulogistic…But what I find hard to understand is that in spite of these lauds only exceptionally has an author referred to my primary contributions to the same field. It seems to me that if an author refers only to works that go back no more than a couple of years although the basic contributions are several decades old, that author seeks to present himself as belonging to an ocean tide of some new discovery. This smacks a kind of plagiarism. Issac Newton thought that failing to mention important predecessors is a crime and he accused Galileo of it for not having mentioned Kepler. As evidenced by the great number of references in my works (an aspect that met with little approval from Mark Blaug) I have been particularly sensitive to this issue.

Georgescu here describes what *he* believes to be an uncompromising ethic of intellectual honesty, but, implicitly what his colleagues see as a tendency to indulge in superfluous historical meanderings that dilute the message of his work (we'll see more of this later, but a salient example is the 60-40 split in history/scientific philosophy and economic modeling in his 1971 Entropy Law and the Economic Process).

At this junction Georgescu outlines for Mirowski his eight primary contributions to economics[1] (truncated, though preserving Georgescu's underlining):

1) in the domain of utility, the argument against the fundamental tenet of standard theory that the behavior of the individual is completely determined by just a quantitive vector of economic commodities
2) in the theory of production, the proof that the standard production function involves a crashing absurdity, the necessity of distinguishing between flows and funds, and the role of idleness in the various modes of production
3) in economic optimality, the proof that for overpopulated developing countries marginal pricing is the worst economic policy
4) in statistical theory, results concerning scatters involving random errors for all variables, a primary (but completely sidestepped) method of discovering

[1] It is particularly interesting to note that the list that Mirowski redeploys in Samuels is slightly modified: Georgescu's item 5, the "peculiar attitude of the peasants toward money," and item 7, "the formulation of a fourth law and of a third perpetual motion," are nowhere to be found.

the cyclic components of time series (to my knowledge used only by Schumpter)

5) in monetary theory, a critique of the role atributed [sic] to inflation by the standard economic theory of economic development and of the Keynsian [sic] salvation through a monetary injection from government spending, and the peculiar attitude of the peasants toward money
6) in the economics of natural resources, the relation between the entropy law and the economic process, the exposing of the ineptitude of the energy theory of economic value the Promethean destiny of our technology
7) in thermodynamics, the formulation of a fourth law and of a third perpetual motion
8) in epistemology, the argument that there are results that can be obtained with the help of numbers as there also others that cannot be obtained with their help (the latter case is the prediction of history by econometric models) with the important corollary that we need both arithmomorphism and dialectics. (3-4)

Georgescu writes (be mindful: we're still in 1990 here):

> Of course I am proud, though not vainglorious, of all these achievements. However I also am sufficiently skeptical to think that I may have erred somewhere…Excepting Paul Samuelson—to whom I dedicated a volume as a sign of my gratitude for his repeated praise—and my former students and then

> colleagues—Anthony M. Tang, Fred M. Westfield, and James S. Worley who have organized the honoring symposium for my retirement in 1976—no economist has cared to show in some literary way some appreciation of my struggles…I have had not many occasions to cross intellectual swords with some fellow economist. My fellow economists have seemed to shun from having to do with an heterodox colleague, but many have shown in other manners their lack of sympathy for my epistemological position… And I cannot say whether it is the gulf between generations or the growing void around an economist who does not swear that salvation comes only through the Walrasian system. (4-6)

This passage highlights an important point that we ought to keep in mind as we continue our journey with Georgescu. The first is that Georgescu's own conception of his reputation is the reception-side failure attitude adopted by many secondary contributors and post-commentators; Georgescu appears largely persistent and uncompromising in crafting complex historic and philosophical latticework around his economics.

This attitude might have been reinforced by Paul Samuelson, who in a 1974 letter wrote to Georgescu, "First, as Robert Merton has documented in his studies of the sociology of science, <u>all</u> good scientists have recognized the importance of receiving credit for priority and originality when that credit is due. That is the coin for which scholars work—the only coin. <u>Moreover, I have long had the opinion that you are one of those scholars of depth who has not received full measure of credit for your lasting contributions to economics</u>" (3 September, my emphasis). And as if framing Georgescu's reception as being due to a passive *lack* of "full measure of credit" isn't enough,

Samuelson readily provides further reinforcement for Georgescu to stay the course: "I feel that I have also, over the years, had something to contribute to the subject but that by the luck of circumstance I have been amply rewarded for my contributions" (my emphasis). *Bound to happen sometime, I suppose*.

It may also be useful at this juncture to also discuss the other three economists mentioned by name to Mirowski: Anthony M. Tang, Fred M. Westfield, and James S. Worley, responsible for organizing Georgescu's honoring symposium in 1976. The three were colleagues of Georgescu's at Vanderbilt, co-editing a volume entitled Entropy, Welfare and Time in Economics, Essays in Honor of Nicholas Georgescu-Roegen. Of interest here is the peculiar fact that none of the trio actually contributes an essay to the volume; in fact, nobody from Vanderbilt's department of Economics and Business Administration does. In their introduction, they struggle to articulate the specifics of Georgescu's economic contributions. We get the following:

> His scholarly output is still growing, at an increasing rate, and the growth is not merely quantitative. He continues his study of the most fundamental problems in economics: choice, production, and human welfare. This he does from an ever broader and deeper perspective…For a one-paragraph summary of his rich and varied career, we can do no better than to borrow (with permission) the citation drafted by William H. Nicholls and presented to the American Economic Association upon naming Nicholas Georgescu-Roegen Distinguished Fellow of the Association. (x)

Even *discussion* of Georgescu's work is deferred to outside the department, and no mention is made of his rigorous ecological foundation in the economic production process—in fact, mention of "human welfare" as a "fundamental problem in economics" goes a long way in illustrating the trio's unfamiliarity with the tenants of Georgescu's work, as surely any passing comprehension of Entropy Law would reinforce the codependence of human *and* environmental welfare. The Vanderbilt homage to Georgescu, upon scrutiny, emphatically reinforces Blaug's thesis that Georgescu, at best, is a figure to be appreciated but not engaged with.

As for the essays in the commemorative volume, we find many of them to be obscure, at best mathematical and philosophical exercises that fail to find acceptances in economics journals proper: Samuelson's contribution is entitled, "Speeding Up of Time with Age in Recognition of Life as Fleeting," cited in the vita is being published nowhere else, as well as Hicks's "Some Questions of Time in Economics," which mentions Georgescu as an afterthought in its final paragraph: "One final salute—to Georgescu. He has chosen a cosmic way of demonstrating the irreversibility of time. Since he was addressing himself to a science-based culture, that (I am sure) was a good way of going about it. For my part, I am very ignorant of science..." (149), as if a) the irreversibility of time hadn't been demonstrated in some fashion by Boltzmann's H-theorem, or b) Georgescu had been consciously targeting a "science-based culture" beyond which the work's import would be lost in obscurity.

In any event the foregoing analysis shows us that both pillars of support cited by Georgescu in the 1990 letter to Mirowski—Paul Samuelson and the Vanderbilt Homage

Trio—provided a deceptively optimistic frontier for Georgescu, that *in the long run, it has to happen*.

However, a truly satisfactory understanding of Georgescu's disastrous rhetorical approach needs to take more into account than these passing instances of professional courtesy. A thorough examination of Georgescu's mathematical training, political affiliation, as well as the tenants of the research programs he attempted modify needs to be undertaken before we can understand the nature of the border skirmishes we encounter as Georgescu attempts interdisciplinary collaboration.

What are the conditions that make optimal the choice to pass one over in silence?

Biographical Sketch of Nicholas Georgescu-Roegen

One of the many things that differentiates me from Einstein is that even at 80 I do not feel that these biographical notes represent my auto-obituary.

--NGR, "An Emigrant from a Developing Country"

Nicholas Georgescu-Roegen was born 4 February 1906 in the small town of Constanța, Romania, then home to a population of 25,000, comprising what Georgescu describes as "an ethnic mosaic of Romanians, Greeks, Germans, Jews, Armenians, Turks, Tartars, and a few Bulgarians," noting that "each nationality lived by its own precepts for felicity, but there never was even the smallest racial strife. I had classmates who wore the fez and turned toward the south during the morning prayer. Because the environment of my childhood was truly cosmopolitan, my ethos has remained so every since" (Autobiographical Notes 2). This sentiment of peaceful coexistence stands in stark

contrast to the interdisciplinary strife that defines Georgescu's later career, and indeed marks Romania as a curious exception to the European hotbeds for nationalism and racism to be found in Austria-Hungary, the Ottoman Empire, and Germany in the early twentieth century—tensions that preceded the outbreak of the First World War.

Son of an army captain and sewing teacher (the latter an "amazing addict to work"), Georgescu's humble origins instilled in him not only an awe-inspiring penchant for hard work, but also an innate apathy toward the production and consumption of the luxury goods that would flow through post Second World War America. We're told that "for my parents' income a bicycle meant a golden Cadillac" (AN 2). In "My Life Philosophy, he writes, "no interpersonal comparison between the upper wants of two rich people, one enjoying a motorboat, the other, a villa, makes any sense. Standard economics being a discipline of the lands-of-plenty, Lionel Robbins' famous thesis of interpersonal noncomparability fits into place there, but only there" (14).

Showing high excellence in his years of primary school, Georgescu won a highly competitive scholarship to a prestigious new military Lycée, his attendance to which was put on hold in 1916 due to Romania's forced participation in World War One. Georgescu and his family were forced into two years of refuge in German-occupied Bucharest. At the end of this period, at the age of 14, Georgescu returned to school, where the curriculum focused heavily on Latin and Mathematics. In his autobiography, Georgescu recalls fondly working with the famous probabilist Octav Onicescu as well as other PhDs who went on to become university chairs, though curiously we find among his drafts an omitted qualification: "My pride for this privilege waned as I realized later that some of these savants were the worst teachers at the secondary level." Nonetheless, Georgescu

describes his stellar secondary-school record, winning top prizes in 1922 and 1923 in a Romanian mathematics publication called GAZETA, and also placing first in his class every year until graduation.

There are a couple of salient points to be harnessed from Georgescu's secondary education that have been overlooked in other synopses of Georgescu's autobiography, the first of which will require us to embark on a slight historical detour and solidify Georgescu's attitude toward mathematics. This point centers around the overwhelming focus on a particular kind of mathematics in the Romanian curriculum. Although we don't have any of Georgescu's exams or early submissions to GAZETA that we can inspect, Georgescu tells us that "after a sustained curriculum of thirty hours of solid classes per week and my additional efforts, my general knowledge was an operational toolbox. When in 1950 I told Harvie Branscomb, the Vanderbilt Chancellor, that as much as seventy-five percent of my working knowledge came from my secondary education, he branded me as facetious" (9, my emphasis). The description of Georgescu's mathematics training as *instrumental* rather than pure or abstract appears to be consistent with the late nineteenth-century Cambridge Mathematics described by Weintraub in How Economics Became a Mathematical Science, in which "English mathematics was the antithesis of what we now think of as rigorous mathematics. To all intents and purposes, there was no pure mathematics done in England in the nineteenth century" (16). Further, synopsizing Israel (1981), "late-nineteenth century mathematics considered 'rigor' and 'axiomatization' antithetical" (Weintraub 17). Although the anachronism of nineteenth century British mathematics in 1920s Romania remains to be accounted for, a mathematical education deeply ensconced in what we would regard now as "applied"

mathematics accounts elegantly for Georgescu's ideological alignment with Alfred Marshall regarding mathematical modeling and the economy, a point that could on its own be treated in a separate text.

Georgescu gives us one more interesting tidbit regarding his education in lycée—he regrets the social isolation. "To live," he tells us, "year after year, isolated from the ordinary society, following a program on which one had no influence of any kind, sitting in class next to one and the same classmate, and sharing the same dormitory with an almost invariable group was a most inadequate conception for preparing one to meet other people and develop fruitful relations according to the opportunities of each case" (9). Both former Vanderbilt Economics department chair Frank Sloan and student of Georgescu's Herman Daly would tell me in an interview that Georgescu was a notoriously difficult person to get along with. "Just a very mercurial personality," recalls Daly. "He could end a friendship like *that*."

Georgescu tells us, "I do not doubt that because of this long extrasocietal period of my life I did not master the vital art of developing auspicious social ties with new acquaintances, or even the manners of cultivating the ties I fortuitously made " (9).

Upon graduating from lycée, Georgescu enrolled at Bucharest University to study mathematics, again a privilege that was completely state sponsored. Georgescu was already fairly acquainted with the small mathematics department from his Gazeta Matematica competitions and together with a classmate edited and lithographed several lectures on analytical mechanics. Georgescu tells us that the curriculum was "specifically classical; for example, it included a full year of elliptic functions but not a single lecture on modern algebra or topology. There was little variation from year to year" (10)—this

education is again consistent with turn of the century mathematics in England, which was very heavily ensconced in *applied* and *classical* mathematics rather than *pure* mathematics (Weintraub 2002), a temporal dissonance that can partially account for Georgescu's imperfect synchronization with mathematical attitudes in greater continental Europe, which had by the 1920s become obsessed with axiomatics and formalization, an attitude that Georgescu would later criticize for losing the empirical foundations of mathematical modeling.

Georgescu graduated from University in June 1926 with highest marks: foarte bine, and then proceeded to earn a teaching license to teach in his former lycée, ranking first in the written exam for male teachers. Simultaneously he applied for a scholarship to get a PhD in Paris, with the aim of studying statistics (as he had been convinced to study statistics by mathematician and economist Traian Lalescu, who told him, "Mr. Roegen, when you come back from France, we will have to do some great work together").

In November of 1926, Georgescu enrolled in the Institute de Statistique, where he placed first among his required courses. Now ready to complete his dissertation at the Sorbonne, Georgescu first returned to Romania for a short stint in the summer of 1929 only to learn that his scholarship had been reallocated by the new National Peasantist Party to government favorites. Georgescu was forced into several distasteful bureaucratic rounds of negotiating, pleading and bargaining before he was able to save his scholarship.

His return to Paris was rich in academic reward; he worked with the likes of Professor Lucien March, director of the General Bureau of Statistics, Alfred Barriol, professor of financial mathematics and director of a large French railway company, (who lent him a calculating machine for his dissertation) and also attended lectures by such

prominent figures in probability as Maurice Frechet, Gatson Julia, and Emile Borel. One course, however, stands out for Georgescu, and that was a course in statistics taught by French economist Albert Aftalion, who had been using statistical data to study business cycles. Georgescu gives us an idea of how Aftalion shaped his epistemology: "Aftalion endeavored to make us see what is actually obtained by the use of a statistical formula rather than to know how to manipulate it" (18). This idea, as we will see later, would entice Georgescu to reject Boltzmann's mathematical formulation of entropy as obscuring the underlying physical truth of irreversible entropic degradation of material substances.

Roughly translated, Georgescu's dissertation was called, "On the problem of finding out the cyclical components of a phenomenon" (20), which he effortlessly defended in June 1930, earning highest qualification, tres bien, but that wasn't enough for the panel of judges, who wrote on his diploma, "avec les félicitations du jury[2]" (20). Georgescu's dissertation was so well celebrated that the October 1930 issue of Journal de la Société de Statistique de Paris contained only his dissertation.

On the heels of his success, later in 1930 Georgescu applied for and obtained a scholarship to study in at University College with Karl Pearson, who "received me so naturally that I immediately felt as in heaven" (22). Georgescu describes Pearson as "a unique scholar, amazingly prolific in a vast range of interests," which include "laying single-handed the proper foundation and forging the basic tools of statistical analysis as we know it today," as well as "important contributions to applied mathematics, the theory of elasticity, anthropology, sociology, eugenics, biometry, and to philosophy through his

[2] With congratulations of the examination board

Grammar of Science" (24). Georgescu tells us that Pearson's 1892 opus, Grammar of Science, "is not properly appreciated by present tastes" (24). Pearson, for his part, tells us in Grammar of Science,

> The classification of facts and the formation of absolute judgments upon the basis of this classification—judgments independent of the idiosyncrasies of the individual mind—essentially sum up the *aim and method of modern science*. The scientific man has above all things to strive at self-elimination in his judgments, to provide an argument which is as true for each individual mind as for his own. *The classification of facts, the recognition of their sequence and relative significance is the function of science*, and the habit of forming a judgment upon these facts unbiased by personal feeling is characteristic of what may be termed the scientific frame of mind (6, Pearson's emphases).

Might not have Pearson's definition of science as merely "classification of facts" led Georgescu to believe, for example, that interdisciplinary collaboration between two scientific fields was simply a matter of *fact aggregation*, rather than a more sophisticated process of communication, clarification, and concession? I'll argue later that Georgescu's dangerously unsophisticated conception of *truth* led to a disastrous inability to collaborate with important members of other disciplines or to defend his ideas in a public forum.

After spending months on several papers with Georgescu outlining statistical methods based on Pearson's construct of "four moments," Pearson suggested to

Georgescu that he ought to attend a lecture by Arthur Bowley at the London School of Economics. Georgescu, at first reluctant to deal in economics, became enticed when offered a prestigious Rockefeller postdoctoral fellowship. Georgescu "suspected that no other but Pearson was behind that move" (27), telling us, "I already saw myself working with Warren M. Persons at the Harvard University Economic Barometer on the application of my method to their three curves system" (27). Georgescu embarked for the United States in 1934.

Arriving at Harvard, Georgescu immediately sought out the professor in charge of business cycle theory, Professor Joseph A. Schumpeter, who brought Georgescu in contact with another Professor, Wassily Leontief. Together, the trio attempted to incorporate Georgescu's statistical work into business cycle theory. As they continued to collaborate, Schumpeter brought Georgescu into a larger circle of Harvard economists, including Oskar Lange, Paul Sweezy, Gerhard Tinter, and Fritz Machlup. The informal meetings among this circle comprised the bulk of Georgescu's training in economics. No classes, just what he called "Schumpeter University."

In 1935, on the heels of another stipend from the Rockefeller grant, Georgescu went travelling around the country to meet more economists: Henry Schultz, Harold Hotelling, Irving Fisher, the Cowles Foundation, and even Albert Einstein at the Institute for Advanced Study in Princeton, New Jersey. In this period Georgescu published several economics papers, on such topics as marginal utility of income, consumer's choice behavior, and the "constancy" of the marginal utility of income. However, in 1937, Georgescu made the mistake of a lifetime. In his own words:

> Schumpeter realized that, because of what I had published before coming to Harvard and the four articles worked out during my short stay there, at the age of 30 I was a highly promising scholar. Due primarily to his judgment, Harvard wanted to keep me on. Schumpeter wanted to write an economic analysis in collaboration with me. But incredible as it must seem, I declined….That happened no more than fifty years ago and I cannot recall nor imagine why I made that inconceivable decision (42).

Filled with a patriotic ardor, Georgescu opted to return to Romania, thinking, "One reason that interfered with my vision was that all my education had been supported by the public funds of Romania and that even my Rockefeller Fellowship counted on a spot earmarked for Romania, just as the others were for each country. I ought therefore to serve in the capacity expected of me" (42). Georgescu was placed on a national Board of Trade that directed trade agreements with countries like Germany and USSR and then tried his hand in political activism by joining the anti-fascist National Peasantist Party. Ensconced in uproarious political turmoil, Georgescu narrowly escaped death several times. In 1948, Georgescu and his wife decided to escape Romania, which had just seen its elections manipulated and stolen by the Soviets, managing to obtain counterfeit travel documents and stowing away on a ship to Turkey.

Eventually returning to the United States, Georgescu regained his role at Harvard as a lecturer and research associate. However, in 1949, he was lured away by the prospect of a permanent faculty position by Vanderbilt's George Stocking. Beard and Lozada note that it has been speculated that Vanderbilt's relatively inferior standing in the economics

community with respect to Harvard and Chicago might have been partially to blame for Georgescu's poor reception. They point out, however, that Vanderbilt was "neither undistinguished nor inhospitable to him" (15), as the department boasted, at the time not only Stocking, but William Nichols, who did extensive work on industrial economics. Vanderbilt was also home to the "Agrarian Movement" among southern intellectuals and writers (Beard and Lozada 15).

It was at Vanderbilt that Georgescu began taking interest in the function of time and entropy in economics with ideas that developed through the 1950s and 1960s, in the meantime earning a series of fellowships and distinctions, such as the aforementioned Distinguished Fellowship of the American Economic Association. But it is also during his years as a professor at Vanderbilt that tales of his personality begin to emerge, where first hand accounts of Georgescu's increasingly maverick and mercurial attitudes begin to paint a more complex portrait for us than the simple, "misunderstood genius." In 1971, five years before his retirement, Georgescu published The Entropy Law and the Economic Process, likely the work for which he is best known, and we notice an increasing disillusionment in Georgescu that his life work might be going unnoticed and underappreciated.

When I asked Dr. Herman Daly, an ecological economist today at the University of Virginia about Georgescu's influence on him as a teacher, he told me:

> I was sort of an eye-witness through a lot of it. I was his student at Vanderbilt. I should say that I had a long and stormy relationship; it's been kind of a love hate relationship. As a student, I thought he was a terrific teacher…he could be a very

> kind and considerate on occasion. He could also be extremely harsh and vindictive…He could change his opinion of you on the basis of a chance remark. The students were mostly afraid of him, because you give a wrong answer in class, he doesn't just let you go and pass on to the next person, he grills you! So students didn't much like him, but some, like me, saw it as a small price to pay. The same attitude was characteristic of his colleagues, who were intellectually his students, in fact.

Georgescu made it very difficult for students to receive a dissertation under him. Daly recalls that only one student was able to do it, with he himself relying on "publishers perish" to get his dissertation accepted in spite of Georgescu's heavy opposition. "That led to a certain bitterness between us, but I thought that so much of what he was doing was correct, and we sort of became friends and allies again." But Daly says that one day he learned that the Romanian press had been writing negative articles about him and the World Bank, which is where he had taken a position after deciding to take a break from academia. He later learned that Georgescu had been giving a speech at the Romanian embassy, speaking out against Daly and the World Bank, and issuing the suggestion that Daly ought to be fired. Utterly perplexed by this seeming act of betrayal, Daly wrote Georgescu, but never heard back, and that's where the friendship dissolved. Daly told me, that Georgescu was one of those people you needed to be with 100%, not halfway."

Frank Sloan, department chair right at the juncture of Georgescu's retirement until 1993, the year before Georgescu's death, shared a similar sentiment: "He was certainly the superstar of the department when I got there, but I just couldn't help feeling that this

was a very bitter, angry man. Much of the faculty would talk about him behind his back." Asked if Georgescu ever offered so much as a "how are you," Sloan replied, "Not even a 'how are you'."

Border Skirmishes

Someone coming into a strange country will sometimes learn the language of the inhabitants from ostensive definitions that they give him; and he will often have to *guess* the meaning of these definitions; and will guess sometimes right, sometimes wrong.

--Ludwig Wittgenstein, *Philosophical Investigations*

In How Economics Became a Mathematical Science, E. Roy Weintraub presents a fascinating correspondence between a mathematician, one Cecil G. Phipps, and an economist, one Don Patinkin, that illustrates the immense difficulty in adopting a realist epistemology regarding mathematics as a language. The implications for this viewpoint are, according to Weintraub, that "1) the economy exists autonomously; 2) it can be represented by ordinary language propositions, and 3) the language of mathematics is useful in translating and operating with those propositions characterizing that autonomous existence," stating further that "an implied corollary of this position is that any disagreement between an economist and a mathematician on the nature of a mathematical proof is due to a misunderstanding of the assumptions or the logical reasoning of the proof…Consequently there is an implicit 'right way' to understand how economists and mathematicians can negotiate the more or less rigid boundary which separates their disciplines" (155-6). And yet, if the Phipps-Patinkin correspondence shows us anything, it is mutual incommensurability between the disciplines that offer

vastly differing standards regarding "the role of assumptions, the nature of proof, and the meaning of mathematical modeling—issues that challenge the belief that economics can be translated into mathematics" (156).

I would like to proffer here that Nicholas Georgescu-Roegen's highly mutated epistemology, economics, mathematics, and conception of entropy led to various communicative breakdowns that made interdisciplinary collaborations nearly impossible, a deficiency that deprived Georgescu of crucial professional backing from the other disciplines he was struggling to fish into economic thought. The first episode illustrates a damaging communicative breakdown that arises when Georgescu attempts to isolate the concept of entropy from its mathematical coinage used by thermal physicists.

In 1979 Georgescu coauthors a paper with Los Alamos physicists John C. Allred and Hillard H. Howard entitled, "Energy, Matter, and Economics, An Overview," which essentially distills several of the explicit references to thermodynamics from Georgescu's Entropy Law with a desirable seal of approval from the physics community. Allred, meanwhile, sees the paper as a viable opportunity to squeeze in some of his work on entropic accounting, "a basic schematic system which makes the accounting of entropy generation both easy and accurate" (12 Draft III)—his ideas will begin to sneak in more and more as the drafts evolve.

In the original paper, we see that very little distinguishes the certain parts of it from Georgescu's Entropy Law. Below is an excerpt from its abstract:

> Economic processes can be considered as entropic processes with much the same advantages in analysis as for thermodynamic processes. Modeling which includes

> dissipative features of systems is an important analytical tool for both thermodynamic and economic systems. Energy and matter play dual roles in the laws of thermodynamics. Often matter is not manifest in the statements of the three laws of thermodynamics. Perhaps because matter is not explicit in the common statements of the laws, the changes which matter undergoes in thermodynamic processes is not fully appreciated.

This paper, for the most part, appears to be a polemic against US energy regulation (or lack thereof) that is largely propelled by Georgescu's arguments on recyclability:

> I. No mechanical work can be obtained without some matter.
> II. The mechanical work that can be obtained with a finite amount of available matter is finite. (No complete recycling of matter.)
> III. No substance can be completely purified of its contaminates. (11-2)

They state further, "Our proposed dual laws for matter, stated as universal negatives, are not subject to formal proof. They can, of course, be disproved by a single counterexample. We have been unable to find counterexamples, but invite their submission" (12). We see already a potential shortcoming of the work—it assumes an unmathematical definition of "entropy" that deserves a modicum of scrutiny.

In Entropy Law, Georgescu goes through great lengths to specify what he means by "entropy." He tells us, "What should now give us reason for concern in meaning the term [entropy] is the fact that its meaning varies substantially, at times even within the

same domain of intellectual endeavor" (4). He tells us that the original coinage of the term owes itself to German physicist Rudolf Clausius, and that the meaning is thus "grounded in a bedrock of physical facts," stating further, "The other meanings constitute a separate category that stands in opposition to it. These [other meanings] are related in a purely formal way to a simple algebraic formulation[3] which is the cloak under which 'entropy' is now becoming familiar to an increasing number of social scientists" (4). Regrettably, this criticism of terminology lends itself poorly to academic discourse with physicists, who expect "entropy" to be accompanied with several mathematical formulations.

Our episode begins on 28 November 1979 when Allred submits "Energy, Matter, and Economics, An Overview," to Dr. Harold L. Davis, editor of Physics Today, writing, "Some physical scientists and some economists think that current economic theory is flawed because it does not take into account dissipative processes. The authors of the enclosed paper are of this persuasion. However, the mixing of economics and physics in a paper leads to difficulty in finding a suitable journal for publication."

Georgescu writes to Allred on 18 December thanking him for the submission, adding, "I would venture to say that if they reject the paper, they will establish another historical case of contemporary intellectual hardness."

While the article is being reviewed at Physics Today, Allred decides to send it to friend and economist Howard A. Cutler, Chancellor at the University of Alaska, to solicit feedback. Cutler writes Allred, rather discouragingly (but unsurprisingly):

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[3] One such algebraic expression can be thought to be:

1) Entropy = $S = \ln W$

> I have read your article, but I am incapable of making critical comment. My knowledge of thermodynamics is too small to grasp fully the implications of the analogy. This is not the first time economists have attempted to ride forward on the backs of theoretical physicists and, I might say, with considerable profit. From my meager knowledge, let me extend to you my congratulations. I found it intriguing and interesting to the extent I understood it. Thank you so much for thinking of me and sharing the article with me.

Allred also send the article to Alvin M. Weinberg, Director of the Institute for Energy Analysis, who writes:

> I have argued with Georgescue-Roegen [sic] over his point 3, page 12. Of course he is right in principle. However, if one looks at the 15 or so elements (except for CH_X and maybe P and a few other trace elements) upon which our society depends, none of them will in any reasonable time run out <u>at</u> <u>high</u> <u>concentration</u>. Remember, 7 percent of the earth's crust is aluminum!

We see that the paper is caught in something of a bind between the economics community and the physics community—the former looks upon it with due respect, but passes over it in silence due to its copious use of technical jargon from thermal physics. The latter looks upon it and fails to understand its lack of quantitative sophistication and

outdated, Claussian formulation of "entropy," which, as we read in Entropy Law, is rejected in its statistical form as formulated by Boltzmann.

Allred, perhaps somewhat perceptive of the discrepancy, rewrites the paper to make it more appealing for Physics Today, sending it to Georgescu sometime in late November 1980. The revisions disturb Georgescu, who writes back on 1 December, "It is a very good thing indeed that they are prepared to publish the paper, but I cannot hide my feeling that it would be a mistake to have it published as is." This second draft (which we don't have) is returned to Allred, presumably, with marginalia and a list of Georgescu's suggestions.

On 13 February of the following year, Georgescu receives a third draft from Allred, and again he is largely disapproving. The main point of contention is Allred's attempt to insert some mathematical work on "verticies." Georgescu writes,

> The only part of your version with which I still feel unhappy is that of pages 12-15… Let me explain my reasons. To begin with, you deal there with an idea that is entirely unconnected with the main topic of the paper. Placed in the middle of the paper it can only cloud the main argument…. The discussion of verticies as it stands now occupies not less than four pages out of twenty pages of text, that is, 25 percent…. I have been unable to follow your argument and have sought a test of this inability by asking the opinions of two other readers well prepared for the task. They have agreed with me.

Are verticies, as Georgescu puts it, "entirely unconnected with the main topic of the paper"?

In draft III, the one dated February 13, we see that Allred attempts to tie his construct, vertex, into Georgescu's scheme of entropy flows (recall that Georgescu doesn't formally model, or prove, the long-run irrecyclability of matter): "When one explores the economics in which entropic considerations strongly influence the economic process, it is helpful to have a schematic system which makes accounting of entropy generation both easy and accurate. The basic element of the system is called a vertex." If nothing else, Allred's inclusion of this section on vertices appears to be a compromise to the editors of Physics Today, an earnest attempt to bridge economics and physics in the terms of physics.

Georgescu writes Allred, "Should you decide to leave out the passage about the vertices, all you have to do is to have the new version retyped clean and send it to Physics Today. Counting that I may guess right again, I am sure that they will be delighted with it" (9 Mar 1981). Two weeks later, Allred writes back, pleading for Georgescu to submit the paper as is, including the section on vertices:

> Hillard and I have had a look at your proposed revisions. Although they are thoughtful and represent considerable effort, we think overall they move us further from our intended audience. We are trying to build a first bridge between physics and economics, to open dialog, not to produce a final definitive work. We have tried very hard to please our physicist reviewers, because they represent the bulk of our readers. PHYSICS TODAY has accepted the earlier draft…We and

> our colleagues think we have a good paper for a physics publication. We want to send it to PHYSICS TODAY in the form of the enclosed version. (23 Mar 1981)

Georgescu won't take *no* for an answer. The discussion of verticies **must** be excluded. He writes,

> I need not overemphasize the fact that it was that version [draft I] that induced Dr. Harold Davis, the Editor of Physics Today, to invite us to expand the paper so as to include 'more background on economics and how G-R's version of economics differs from the standard approaches'….[A]ll these efforts of mine (much, much greater than I had expected and also than if I had to write the article by myself), you still stick to a version which I cannot possibly accept….Instead of introducing the reader to my own work, it presents my ideas in a diluted and chopped manner.

Within a couple of days, Georgescu hears back from Allred, who writes,

> I could stop by for a visit with you and Otilia for the weekend of April 17-18. However, I cannot and will not make this stop under pressure of work. I really would love to visit you for old times' sake though. If you want me to make this visit, you must release our paper to PHYSICS TODAY as is. Then we could have a mutual celebration! With every good wish, Sincerely, John.

We don't have Georgescu's reply to this letter, but we do have a follow-up that dates several months later—"I am therefore interested to know whether or not you agree at this

late hour to send in my version of March 31, 1981…If you have no longer any interest in the original project, I plan to write a comprehensive article…and submit it to Physics Today" (14 Aug 1981).

To the very end, Georgescu is unwilling to allow Allred to publish his section on verticies alongside his ideas—the paper must be about Georgescu and Georgescu alone. Allred presumably severs his ties with Georgescu. Here the correspondence ends, and no article bearing the ideas of Nicholas Goergescu-Roegen is ever published in the journal Physics Today.

Another fascinating—and telling—border skirmish we find in the letters of Nicholas Georgescu-Roegen takes place at around the same time. In this exchange, a tripartite dispute among Georgescu, Irving Kristol, professor at New York University's Graduate School of Business, and Thomas J. Bray, associate editor of the Editorial Page of the Wall Street Journal, we again witness, perhaps to an extreme, Georgescu's inability to distinguish between academic and popular forums of discourse (and hence the accompanying modes, *rules*, or norms), much less, as we witnessed in the prior skirmish, a sense of varying modes of discourse among scientific communities in the academic realm. More importantly, this exchange illustrates the ease with which a free-market economist was able to brush aside the relevance of the second law of thermodynamics while being shielded, of all things, by Georgescu's inability to construe a rhetorically competent rebuttal that engaged the language of his opponents and audience.

On 19 September 1980 the Wall Street Journal publishes an article by Irving Kristol, entitled, "Of Economics and Eco-Mania," in which Kristol, a staunch right-wing Reagan supporter, attempts to clarify two points: the first regards the clarification of the

term "supply-side"—"Those of us who still like to think of ourselves as 'supply-siders' do not share the low estimate of the people that is so popular among our leaders. We believe in a free market because we also believe that the people are the best judges as to how much to spend, save, invest, and where or when to do so…Gov Reagan seems to have a strong, instinctive understanding of this truth" (WSJ), a point that can for our purposes situate Kristol opposite Georgescu as a believer in rational market forces.

But it is Kristol's second point that provokes Georgescu. Kristol writes, "Though religious instruction is supposedly forbidden in our public schools, an awful lot of it is nevertheless takes place…the student learns that there is a divine order and harmony in the cosmos which man, with his obscene technologies, is always threatening to disrupt. This is an old and respectable view which any student of religion will recognize as a premise of Graeco-Roman paganism (remember Prometheus?) or of most Oriental religions." Kristol is attempting to dissect an argument made by Georgescu's fellow ecological economist, Jeremy Rifkin in an article entitled, "Entropy and the New World View," one that presumably echoes Georgescu's thesis that a stable economic growth state, by the second law of thermodynamics, is unsustainable. Kristol writes,

> From this well-known law, Mr. Rifkin jumps to a radical conclusion: the conservation of energy must now become the overriding goal of the human race, so as to postpone the death of planet Earth…The second law of thermodynamics, scientists patiently pointed out, applies only to closed physical systems—i.e., to systems that do not receive new infusions of energy from outside themselves. The earth, which receives much of its energy from the sun—as enthusiasts for solar

> energy keep reminding us, is not such a closed system…. Yes, it is very possible—if you will, even likely—that one of these days both sun and the Earth may be dead. According to astronomers, that would be many millions of years from now. The idea that we must all live now so as to delay that eventuality by perhaps a couple of hundred years is obviously preposterous.

Professor Kristol, here, no doubt can be seen to have artfully, if not deceptively, crafted a simple, but compelling rebuttal to the cornerstone of Georgescu's project: linking, for example, the project of ecological economics to "Graeco-Roman paganism" and "most Oriental religions," plays on right-wing religious sensibilities, while hearkening Paul Johnson's felicitous term "eco-mania" renders it an uninformed popular movement that is bound, like all mania, to fade when emotions quell and rationality sets in. If one is persuaded by Kristol's rebuttal to the project of ecological economics as being naively premised on the second law of thermodynamics (and, given how Kristol has framed it, one undeniably would have to be), then one would expect a repartee from the likes of Rifkin or Georgescu to have something resembling the following structure:

1) Kristol's framing of our academic work is a gross oversimplification.
2) The entropic degradation of the planet is an *immediate* concern *because*…
3) The reason solar energy doesn't sufficiently stave our consumption concerns *is*…

Georgescu hurriedly crafts a 800-word rejoinder addressed "to the editor," which is, as we might have predicted, deeply preoccupied with framing a highly technical and historically sophisticated preface. He opens as follows:

> I am not alone in maintaining that in any economy in which production is not carried out by independent family or clan units but by intertwined social organizations, no computer can determine the optimal distribution of income and burden, 'optimal' being understood in Vilfredo Pareto's sense with respect not only to the pre-market distribution of productive resources but also to the prevailing economic institutions. Only the market can do that. But Professor Kristol's tenet implies that the democratic capitalism market is also optimal always regardless of the institutional framework. This tenet has been taken for granted by an economic conception that germinated in a capitalist milieu and failed to realize that in mankind's fate social evolution is not only inevitable but also far more fateful than the purely biological one. Homo sapiens has lived for a far, far longer time in non-capitalistic social frameworks. It is a common sin of Marxists and supply-siders to believe that the system they advocate is as eternal and felicitous as "life after death."

As we see, Georgescu hasn't even *framed* his rebuttal in the space that Kristol used to dismantle his program. And just as Georgescu begins to produce his argument in the third paragraph—"Economists thus seem to ignore that only harm can result for any discipline that claims to possess greater powers than it actually has. The problem of natural

resources is an ecological, not a market issue. For its solution means other than the market are necessary" (2)—he gives into the temptation to issue more historical pontification:

> But the belief in continuous economic growth has not been the privilege only of the right wing—to which Professor Kristol belongs. It is shared by all those faithful to Frederick Engles' Dialectic of Nature and to V.I. Lenin's Materialism and Empirico-Criticism. Also, quasi-religious expressions have joined this view, too (think of the great natural scientist Teilhard de Chardin), not only the opposite one.

Georgescu takes the historical sophistication of the Wall Street Journal readership for granted by assuming some kind of familiarity with *either* Dialectic of Nature or Materialism and Empirico-Criticism, effectively creating an intellectual distance rather than appealing to an estranged community of businessmen and economists by engaging Kristol's argument *in its own terms*—i.e. by addressing the temporally immediate concerns of the readership. Georgescu's *actual* point, that the communist politic is *just* as susceptible to the fallacy of unchecked growth as "the supply-siders" (thus deconstructing Kristol's false association of ecological concern with socialist agendas) is lost amidst the academic esotericism.

Georgescu then attempts to challenge Kristol's point that Earth is an open system by stating, "as it is frequent among the non-initiated, he confuses closed systems (systems that can exchange only energy with the outside, such as the Earth virtually is) with

isolated systems (systems that exchange nothing with the outside). The Entropy Law applies only to isolated systems and its verdict is extremely harsh: for everything we obtain in any part of such a system, we pay a greater price in another part…the Sun and the Earth constitute together a virtual isolated system" (2-3). If we revisit Kristol's original invocation of "closed systems," we see that Georgescu's rebuttal is again of questionable relevance: "The second law of thermodynamics, scientists patiently pointed out, applies only to closed physical systems—i.e., systems that do not receive new infusions of energy from outside themselves." If we replace "closed" with "isolated" in Kristol (thus satisfying Georgescu's demand for terminological exactitude), the implications of the sentence don't change. Georgescu only succeeds in pointing out that "the Sun and the Earth constitute together a virtual isolated system" (3).

As Georgescu winds down his article, his language retains its persistent historic character. Likely the deciding paragraph for the Wall Street Journal editors to reject Roegen's article:

> As surprising as it may appear, in the entire technical development of mankind there have been only two viable technologies, both Promethean gifts. The first Promethean gift is well-known, albeit not fully appreciated by the present temper. It was the mastery of fire, which represents a self-sustained qualitative conversion of energy—of a particular chemical energy into caloric power—with immense industrial consequences. The second Promethean gift—due to Thomas Savery and Thomas Newcomer—was the steam engine, another self-sustaining qualitative energy conversion, of the caloric power into motor power.

Only at this juncture does he manage to address Kristol's point regarding Earth as an open system, stating, "Solar technology, about which there is much din from all directions, is not yet viable: no known solar collectors can reproduce themselves with the aid of solar energy alone. All known solar recipes are parasites of the other technologies" (3). This, what we can regard as a tantalizing foundation for an independent Wall Street Journal article, instead functions, rhetorically, as a footnote.

As an afterthought, Georgescu writes, in his hand, "You may add, if you wish, 'Distinguished Fellow of the American Economic Association'," after his name—vaporizing any doubt that Georgescu's *modus-operandi* is merely the establishment of intellectual positioning rather than that of thoughtful, practical engagement with his audience. Georgescu's epistemology, as we have witnessed, heavily influenced by the strict positive empiricism of Pearson, assumes the obviousness of a truth (in this case, environmental degradation, man's historical understanding and utilization of natural energy sources) and proceeds to construct arguments that *show*, much like a picture, that truth, rather than engaging the rhetoric of his audience and *teaching* the "truth" in the language of that audience. If ever this point makes itself evident, we see it in the vastly contrasting rhetorical styles of Kristol and Georgescu.

This point is driven home as we read Georgescu's follow-ups. Publication, for Georgescu, is a *right*, and we find him concocting a series of conspiracy theories as to why his article isn't published. The truth and unassailability of his ideas, as we see, is granted. He writes on 4 December:

> On October 3, 1980, I sent you a small criticism of Irving Kristol's editorial "Of Economics and 'Eco-Mania'" (WSJ, September 19, 1980), thinking that you will print it in the same journal. By now it is clear that you decided against its publication. Since you found it proper not to write me anything, I wonder what were your reasons. Was it because my piece, unlike Kristol's, was polite and academic in content? Or was it because you did not want to expose the reasoning and the style of 'a member of the Journal's Board of Contributors?'

At this juncture, Georgescu also mails a copy to Kristol, before hearing back from WSJ on 8 December—"Thanks for your note of December 4. Your 'small criticism' of Irving Kristol, as you put it, was not logged as an article, so I assume it was treated as a letter to the editor…If you had intended your piece as an article, feel free to resubmit it and we will give you a direct yes or no as to whether we would want to publish it." Upon resubmitting it as an *article*, Georgescu is rejected.

Kristol, on the other hand, writes Georgescu back: "I'll not get into a discussion of ecology and the Law of Entropy with you. Frankly, I do not regard this as a serious issue. Regardless of how one interprets the Law of Entropy, the time scale is such as to make it irrelevant to current economic policy" (17 Dec 1980).

In an act of bitterness, Georgescu sends a parting shot to Associate Editor Thomas Bray: "Perhaps you wanted to add another case in support of the idea that one should not invest even a postage stamp in a prospectus coming from the Wall Street subculture. Because you help me learn another lesson, I must thank you" (7 Jan 1981). Georgescu slams the door in the face of another outlet, and as we see, the case can scarcely be made

that Georgescu here is rejected for bearing an inconvenient truth. Indeed, his bid for a platform severely crippled by his inability to address his readership in a clear and concise manner. Moreover, it cannot be understated how unapologetically bitter Georgescu could be, and how this lack of tact cost Georgescu key advocates.

Conclusion

Narratives of the history of science are filled with revolutionaries: the Newtons, the Einsteins, the Plancks, but rarely do we get a glimpse into the minds of the failed revolutionaries, the Georgescus. This paper attempted to explore an economist whose ideas are starting to warrant a second look. Popular literature like Friedman's Hot, Flat, and Crowded, or Al Gore's "Inconvenient Truth," or the automobile's sluggish but slowly-blossoming transition toward fuel-efficient vehicles are raising a set of economic questions that may very well incorporate some of the discarded ideas, or at the very least, sentiments, of Nicholas Georgescu-Roegen.

We've noted several important distinctions in Georgescu's work that give us pause when deciding whether to pass over him, or to give the work a second look. The first is that Georgescu isn't categorically rejected by all communities as "wrong," but as simply "obscure." One wonders whether a new age of necessitated collaboration among the disciplines will generate a new class of reader to comprehend Georgescu's difficult arguments.

The second distinction to be made addresses reading Georgescu's poor reception as being attributable to some kind of deliberate ignorance on the part of his colleagues. Georgescu ostensively had a platform in the 1980s to collaborate with the physics

community but turned the opportunity down because he wanted one-hundred percent concession form John Allred. He tried to find a route into the business community by appealing to the Wall Street Journal but instead produced a stubbornly difficult and academic article that was all but too easy to turn down. Blame runs on a two way street, as the first hand accounts have told me.

After forty minutes of reminiscing, former Vanderbilt Economics department chair Frank Sloan's demeanor became noticeably deep and ponderous. We had been talking so much about Georgescu's petulant narcissism that maybe it had only just dawned on him that behind the brooding and arrogant persona there was genius—a wealth of viable ideas, maybe, after all. He looked up at me, and said, "It's kind of tragic, isn't it, how somebody so profound can be ignored for being a difficult person?"

We want to believe that *something is there*, that *something is bound to be there*, or at least we suppose.

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