Chapter 6

Kinks, tools, spurts, and substitutes: Gerschenkron's rhetoric of relative backwardness

Donald N. McCloskey

Alexander Gerschenkron, one of the thin, bright stream of historical economists before the name, was educated as an economist and then used his education on historical questions. Economists believe in numbers. Gerschenkron the economist became a master of numbers in the 1930s and 1940s, when national income accounting and index numbers were on the frontier of statistical economics.

What made Gerschenkron and his work peculiar is that unlike most economists he also believed in words. He believed in them not merely the way any good writer does, or any widely read person who went to school in Mitteleuropa before its fall. He believed in words the way a literary critic does. His theory of relative backwardness rested on the theory of index numbers, true, with all its scientific authority, but it also rested on wordlore from novels and plays and poetry.

This peculiarity shows up best, perhaps, in comparison with another great student of European industrialization, Walt Rostow. Both started as statistical experts, Gerschenkron more penetrating; both wrote well, Rostow better; both thought in terms of grand theories of economic growth. It is no disrespect to Rostow, however, to observe that he uses a phrase like 'take-off into self-sustained growth' without irony or self-reflection, the way most economists use their words. A literary analysis of Rostow's work would show the master using metaphors and storylines without recognizing that he was using metaphors and storylines. Gerschenkron, by contrast, was obsessively interested in watching himself use words. Both men were wordspinners, of course, but Gerschenkron was also a critic.

His work invites rhetorical criticism. One cannot look at Gerschenkron's theory innocently, as a thing in itself. One places some sort of grid over it, and measures it along the notches cut into the grid.

The usual grid is a folk philosophy of science. Historians and especially economists share a grid marked by the positivism of the 1930s. Gerschenkron might be said to have offered a 'hypothesis', itself properly part of a 'hypothetico-deductive system', which can be 'falsified' by 'empirical testing' against 'observable implications'. His own words sometimes evoked these popular misunderstandings of scientific method, as near the beginning of 'Economic

Backwardness in Historical Perspective' (1952, when he was 48 years old (reprinted 1962)):

> Historical research consists essentially in application to *empirical material* of various *sets of empirically derived hypothetical generalizations* and in *testing the closeness of the resulting fit*, in the hope that in this way certain *uniformities*, certain typical situations, and certain *typical relationships among individual factors* in these situations can be ascertained.[1]
>
> (Gerschenkron 1962: 6)

And elsewhere he would say repeatedly that the concept of relative backwardness ('concept' and 'process' were favourite placeholders in Gerschenkron's prose) is 'an operationally usable concept' (Gerschenkron 1962: 354).

The words did not, in his practice, carry the freight they usually do in economics. He used Russian novels as a source for economic history and he recognized that his statistical work had normative content. In using the resources of language to argue his case he did not suggest that verbal evidence is necessarily less conclusive than statistical evidence. He wrote on the Soviet novel as historical evidence and contributed to literary journals. He used the absence of accounting terms in the Russian language of the eighteenth century to indicate the absence of commercial attitudes (Gerschenkron 1968: 449; 1970: 81). This early exponent of historical statistics, Paul Gregory notes, used mainly literary evidence to claim that Russian agriculture stagnated after emancipation.

The grid of folk philosophy does not recognize much subtlety. Its demarcations go some way towards classifying science: scientific/non-scientific, objective/subjective, positive/normative, observable/non-observable, justification/discovery, but they do not go very far. They cannot show science in detail. Furthermore the received demarcations are normative rather than positive but give no guidance on how to reason about norms. An observer of science using them cannot see, for instance, why scientists disagree, since every scientist claims stoutly to operate on the good side of each demarcation – scientific, objective, positive, observable, and justificatory. The grid therefore worsens disagreements. It leaves scientists to conclude that if another scientist disagrees with him the other must be incompetent. The grid of folk philosophy is itself unscientific, a bad description, and a bad moral theory of scholarship.

More often Gerschenkron applied another grid, the practical philosophy of the scholar, an older ethic than the positivism of the mid-twentieth century. Gerschenkron exhibited to a high degree the values of Continental scholarship. In a way that contrasts with British and some American traditions, he practised, as he affirmed, 'a program of research' – not a method of scholarship but a plan of life. His range as a scholar – at ease with mathematics, history, statistics and a dozen languages – was subordinated to the programme.

The programme was to yield not sharp or mechanical 'tests of the theory', but mature judgements, satisfying, as he frequently put it, 'the sense of reasoned adequacy'. This phrase itself is not without mystery, but he did not intend the

historical reasoning to be kept mysterious. Doubtless some matters of scholarship would need to remain tacit, but Gerschenkron had no patience with the unfootnoted *argumentum ex cathedra*, even from eminent scholars. Repeatedly he advocated and exhibited explicitness in argument. His enthusiastic review in 1953 of Franco Venturi's *Il Populismo russo* (Gerschenkron 1968: 455) congratulates Venturi for a 'mature understanding': 'And yet one cannot but wish that the author had decided to share his thinking more fully with his readers'. Twelve years later he warmly praised his student Albert Fishlow for 'the statistical appendixes in which the author offers a full insight into his laboratory and without which no real appreciation of the importance of the study and of the validity of its interpretative results is possible' (Gerschenkron 1965: viii).

The scholarly ethos of care is prominently commended in Gerschenkron's reviews and in his footnote polemics. It has two parts: avoidance of error in the details and modesty in the putting forward of conclusions. We stop believing someone who makes little errors ('Bad spelling, bad reasoning') or who draws conclusions hastily ('How can she say such a thing?'). Gerschenkron here does not inhabit the world of modern economics, in which theory provides a check on facts and in which a blackboard exercise is said to have 'policy implications'. He detested theories of history that can in their rigidity supply bridges across evidential voids – Marxism most notably, of course – and favoured theories (such as Arnold Toynbee's) that provided merely a way to shape the facts into a story. The good theories, he said, were 'fruitful' in the sense of suggesting new enquiries; they did not close the conversation. In this he was a modern professional historian, whose discipline is not economic or historical theory but the shaping of tales constrained by fact.

But neither the folk's philosophy of science nor the scholar's credo of virtue is much of a grid for measuring Gerschenkron. They cannot register his peculiarly literary bent. A better grid is the ancient discipline of rhetoric, that is to say, the study since the Greeks of the available means of persuasion. As Paul Hohenberg has put it, we need a way of understanding 'how we experience Gerschenkron's writing', to better understand the meeting of number and word in the mid-Atlantic.

For instance, *ethos*, character, the implied author figures heavily in Gerschenkron's writing. Gerschenkron wrote judgementally about scholarly discourses, raising up or breaking down the *ethos* of other scholars. Most scientific prose suppressed such judgements. In a few pages early in *Europe in the Russian Mirror* Gerschenkron admired Marc Bloch and Max Weber and Tugan-Baranovskii – of the last, 'valuable contribution', 'probably the most original Russian economist, ... amazingly broad in his interests', 'a serious scholar', his greatest compliment (Gerschenkron 1970: 6ff). Such compliments serve more to honour the author than the subject. The author exhibits the good taste to admire good work. And the old Russian economist's 'amazingly broad interests' turn out to be various subjects within economics; whereas the author himself, also a

Russian economist, ranges over statistical theory, Greek poetry, and the Boston Celtics basketball team.

Part of his *ethos*, of course, was his style of writing, the most obviously 'rhetorical' part of anyone's rhetoric. Gerschenkron delighted in obscure but fine words in English, such as 'flummox', a special favourite. He spoke mainly in the idiom of cultivated Europe, which made some sentences pure Latin, especially in earlier writings. Most of the prose in his most famous and earliest article on the subject, 'Economic backwardness in historical perspective', though lucid, is undistinguished. He appeared to be playing the sober scientist, and waxed eloquent only when the subject turns to ideology ('Ricardo is not known to have inspired anyone to change "God Save the King" into "God Save Industry"' (Gerschenkron 1952 (1962), 24f)).

Beyond *ethos* and style, Gerschenkron recognized that social theories are metaphors. He recognized that words are not mere tags for things behind them but have an intent to persuade in the scholarly forum. When Gerschenkron speaks of the 'tensions' produced by economic backwardness he was aware that this was a manner of human speaking constrained by the facts of the world, not a raw fact in itself. If the Russian aristocracy used the condition of its serfs as a symbol of Russia's shameful backwardness, as Olga Crisp argues so persuasively, then the words have political and then economic consequences. The ideology of economic development resides in the metaphors used, whether of 'nation-building' or 'development' or 'backwardness'. One might ask whether Gerschenkron thought of the state as a Good Tsar or as an Ivan the Terrible; it is not clear. The nation itself is a metaphor; 'Germany' does not literally do anything, individual Germans do, as Gerschenkron pointed out, when they export bullion or banking to Italy. The very word 'growth' embodies a biological metaphor, which could be specialized, as Paul Hohenberg has observed, to tropical growth (a capitalist view that growth is hard to kill) or sub-arctic growth (a socialist view that growth is hard to sustain, and must therefore have the attention of the state).

Gerschenkron's chief scientific accomplishment was to undermine the metaphor of social stages which had dominated nineteenth-century and much twentieth-century thought. Henry Maine, Auguste Comte, Friedrich List, Karl Marx, Werner Sombart, Bruno Hildebrand, and latterly Walt Rostow thought of a nation as a person, with predictable stages of development from birth to maturity. If the stage theorists viewed the child as the father of the man, Gerschenkron was a new Freud, noting the pathologies arising from retarded growth.

He favoured his own metaphors of 'spurt' and 'relative backwardness' as against 'take-off' or 'absolute prerequisites', but not because he believed his to be less metaphorical. Wordlore was no ornamental appendage to the real work. European philology knew that the work was the word. Gerschenkron was aware in particular of the economistic character of his metaphors, especially the notion of 'substitutes' for prerequisites. 'The German investment bank was a substitute for the missing or inadequate available prerequisite' (Gerschenkron 1970: 103).

The word might be interpreted in the illuminating jargon of neoclassical economics as the speed of industrialization arising from a 'production function' with 'sustainable inputs' of demand, finance, entrepreneurship, disciplined labour, and so forth. Or it might be interpreted, as Paul David argues persuasively, as the substitute paths in a branching process of choice. In any case it is a metaphor with consequences. The size of the spurt, for example, has consequences: 'along with differences in the vehemence of the process were the differences in its character' (Gerschenkron 1970: 72). The argument persuades economists in part because it relies on their master metaphors. In the neoclassical interpretation, at greater speed of industrialization certain inputs would rise in marginal product, and the Soviets would naturally build giant factories and crush a complaining peasantry. In the branching interpretation, greater speed would require a choice of quick-but-centralized branches.

Gerschenkron justified his economistic metaphors in a mainly Kantian rather than a Baconian way. Like many scholars as they get older, he appears to have become more Kantian and less naively Baconian, becoming more convinced, as Kant said, that concepts without perceptions are empty and perceptions without concepts are blind. The classic essay on 'Economic backwardness' was first published in a volume edited by Bert Hoselitz in 1952. It is thoroughly Baconian and British. Things were as they were. The 'story of European industrialization' (Gerschenkron 1952 (1962): 26) is a 'story' only incidentally; really it is a scientific compound 'synthesized from the available historical information' (Gerschenkron 1952 (1962: 7). There is no trace here of 'story' as a shaping fiction, disciplined by the facts of matter but underdetermined by them. By 1962, however, at the age of 58, Gerschenkron is speaking of his argument in a different way. The 'Postscript' to the first collection of essays, in 1962, speaks of '*viewing* European history as patterns of substitution' (Gerschenkron 1962: 359). Failures of such a view 'may be such as to require and perhaps suggest an *organizing principle* very different from the variations in the degree of backwardness' (Gerschenkron 1962: 364). And likewise in similar words.

These are points of view, weights so to speak in a statistical index of industrial output. That is, they are human choices, not things-in-themselves. As William Parker has argued, Gerschenkron's experience as a double refugee from Russia and then Austria appears to have led him to thinking hard about points of view, relative backwardness, and translation.

Typologies or categories are points of view. In 'The typology of industrial development as a tool of analysis' (Gerschenkron 1968: 77–97), which was first published in 1962, Gerschenkron makes assertions about the uses of typologies that would be unintelligible to a true Baconian or to his positivist descendents. His placing of the extremes of stage theories and uniqueness theories into a unified pattern

> does not at all mean that the extreme approaches are necessarily 'wrong' in any meaningful sense of the term. Since any approach of this kind ... inevitably deals not with the unmanageable and incomprehensible 'totality' of the phenomena but with sets of abstractions, different approaches yield different insights, and it is in terms of those insights that the value of an individual approach must be judged. The results need not be commensurate.
> (Gerschenkron 1968: 79)

And later: 'Historical generalizations are not universal propositions that are falsified as soon as a single black swan has been observed. Our hypotheses are not "lawlike"' (Gerschenkron 1968: 97).

Or consider the repeated images of visualizing in two pages of *Europe in the Russian Mirror*, published in 1970 (the very title and theme of the book, of course, mirrors an active vision): 'Once we *view* the industrial development of Europe *in this fashion, it appears as* a unity ...' '[C]apital disposition is only one of very many examples of *an orderly pattern* ...' Relative backwardness 'gives us first of all *an opportunity to bring some order* into the apparent chaos, to establish, that is, a morphology or typology of the development' (Gerschenkron 1970: 104, italics added). Or elsewhere, on applying it to mercantilism, 'it *conceives* of Russia as a part of Europe' (Gerschenkron 1970: 87; and 'regarded', 'arraign', 'picture', and 'conceived' on the same page). Most Kantian of all, after quoting Goethe to the same effect: 'What we call facts or reality, including Colbert or Napoleon, are just phenomena of a low degree of abstraction' (Gerschenkron 1970: 63). The scientist here is no passive observer of nature. He chooses his ways of worldmaking.

Facts passively observed constrain what can be seen, of course, and Gerschenkron drew willingly on the rhetoric of British empiricism when it suited the rhetorical situation. At the end of the passage just quoted (Gerschenkron 1970: 104) he recurs to the nature of things (as against our way of seeing them): 'the degree of backwardness becomes then a causal principle, explaining for us the nature of the process of industrial change'. Even in this, however, he is pointing to the active observer, with the phrase 'explaining *for us*'. In the sentences leading up to the assertion of how things are (as distinct from how they seem) Gerschenkron insists on the shaping of the observer, who 'sees' a 'pattern', a morphology 'temporally seen'. And the paragraph following returns to how we *see* the matter: '[T]he more backward a country, the more barren *appears* its pre-industrial landscape.... This then is ... my *picture* of European industrialization' (Gerschenkron 1970: 104).

Gerschenkron drew on the doctrines of British empiricism to attack other theories, but again with Kantian supplementation. The trouble with stage theories, he says, is that '[t]hey are not very consistent with crude empiricism, and are damaged seriously when confronted with the relevant facts as we know them' (Gerschenkron 1970: 101). He appeals here to what 'we', the scientific community, know; and to the vocabulary of 'consistent with ... empiricism' and 'confronted with the relevant facts', fragments of positivist dogma. Yet he injects the 'not very' consistent and the 'crude' empiricism, urbane ironies distancing

him from a positivism that would forget Kant. Gerschenkron was, of course, an empiricist, but a sophisticated one, who understood that scientists do not merely tally up the world's noumena.

Gerschenkron's theory, then, is by his own description 'a way of looking' at the world. The metaphor of 'substitution' is useful because it is 'a construct that ... helps to conceive Europe as a graduated unit' (Gerschenkron 1970: 108). Note that the virtue claimed is conversational. Talking this way will be helpful to the historical conversation. He speaks frequently of the theory as a classification or typology, by which he means the classification of botanical species (Gerschenkron 1970: 96), with Russia the red butterfly at one end and Britain the blue one at the other. He is hostile to mathematical or logic metaphors to describe growth. Rostow's and other theories of prerequisites are described as 'beautiful exercises in logic' (Gerschenkron 1970: 101; cf. 101 middle) which 'have been defeated by history'.

Emphatically, to repeat, Gerschenkron did not view metaphors as mere ornaments. They are the stuff of thought. He was always reflecting on the aptness of the metaphors we live by. Consider, for example, his elaborate discussion of the metaphor of 'continuity' in history (Gerschenkron 1968). He noted that the continuity in question has often been misconstrued as philosophical. The philosophical difficulty was raised by Parmenides and Zeno: if everything is perfectly continuous, change is impossible. In a manner of speaking, things are packed so tightly that they cannot move. The economist will recognize the point as analogous to an extreme form of economic equilibrium; the physicist will recognize it as analogous to the maximum entropy of physics. If human nature does not 'really' change then history will be a string of weary announcements that the more things change the more they stay the same. More to the point in economics, if the economy is 'really' in equilibrium all the time, then nothing remains to be done. Gerschenkron (1968: 12) noted that such a metaphysics would close the book of history. A history or economics that began with the Parmenidean continuum would never speak.

For the purposes of social science Gerschenkron rejects the transition from the connectedness of all change to an absence of change. True, if you squint and fit a curve then no economic change looks discontinuous in the mathematical sense; but it is wrong then to deduce that 'really' there is no change at all, or that kinks don't happen. And here Gershenkron again criticizes alternative metaphors. 'Continuity' in the strict mathematical sense, he urged, must be kept distinct from 'continuity' in the storytelling sense.

Economists have often been muddled about this philosophical distinction, drawing surprising ideological implications from it. Alfred Marshall enshrined on the title page of *The Principles* his motto '*natura non facit saltum*' (nature does not make a jump; the phrase dates back through Linnaeus and Leibnitz to Jacques Tissot in 1613). Marshall himself perhaps believed that the ability to represent behaviour with differentiable functions implies that marginalism is a good description of human behaviour. It is less sure that he believed that the lack

of jumps in nature (this on the eve of quantum physics), implies that people should not jump either, and should change society only gradually. Anyway, neither implication follows; though both have been attributed to neoclassical economics, neither is necessary for it. Much bitter controversy has assumed that neoclassical economics depends on smooth curves and in consequence must advocate smooth social policies. The peculiar alliance between discrete mathematics and Marxian economics has this origin, as does the enthusiasm of some conservative writers for continuities in economic history. Gerschenkron cursed both their houses; the social scientist should study change and continuity 'unbothered by the lovers and haters of revolutions who must find themselves playgrounds and battlegrounds outside the area of serious scholarship' (Gerschenkron 1968: 39).

The main problems of continuity and discontinuity, however, are not solvable in seminars on philosophy. They are practical problems in the uses of measurement, and must be solved in the workshop of the economic or historical scholar. When shall we say that the industrial revolution happened? Gerschenkron gives an answer confined to industry, for in common with most economic historians he regards agriculture and services as laggards in economic growth.

> In a number of major countries of Europe ... after a lengthy period of fairly low rates of growth came a moment of more or less sudden increase in the rates, which then remained at the accelerated level for a considerable period. That was the period of the great spurt in the respective countries' industrial development.... The rates and the margin between them in the 'pre-kink' and the 'post-kink' periods appear to vary depending on the degree of relative backwardness of the country at the time of the acceleration.
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> (Gerschenkron 1968: 33–4).

The level at which such discontinuity is to be observed is at choice. As Gerschenkron remarks,

> If the seat of the great spurt lies in the area of manufacturing, it would be inept to try to locate the discontinuity by scrutinizing data on large aggregate magnitudes such as national income.... By the time industry has become bulky enough to affect the larger aggregate, the exciting period of the great spurt may well be over.
>
> (Gerschenkron 1968: 34–5)

The word 'inept' is notable here, speaking of seeing as a skill. In a footnote to these sentences he remarks that 'Walt Rostow's failure to appreciate this point has detracted greatly from his concept of the take-off, which in principle is closely related to the concept of the great spurt as developed by this writer'.

The point is a good one, and applies to all questions of continuity in aggregate economics. Small (and exciting) beginnings will be hidden by the mass until well after they have become routine. Joel Mokyr has put it as a matter of arithmetic: if

the traditional sector of an economy is growing at a slow 1 per cent per annum, and starts with 90 per cent of output, the modern sector growing at 4 per cent per annum will take three-quarters of a century to account for as much as half of output (Mokyr 1985: 5). It can be called the weighting theorem (or the waiting theorem, for the wait is long when the weight is small to begin with), of which Harley's (1982) essay on cotton in the industrial revolution is a striking example.

In other words, the search for discontinuity in an aggregate time-series raises the question of the level at which we should do our social thinking, the aggregation problem. Yet Gerschenkron himself did not answer the question well, and was hoist by his own petard. For Italian industrial output he placed his 'big spurt' in the period 1896–1908, and wished to explain it with big German banks exported to Italy in the 1890s (the timing mattered because Gerschenkron was obsessed with the notion, as Federico and Toniolo have pointed out, that French or indeed Italian banks were utterly different in character from German banks; French and Italian banks were already there). Stefano Fenoaltea, once his student, anticipated the weighting theorem for the Italian case (Fenoaltea, forthcoming). Surely, Fenoaltea reasoned, the components of the industrial index – the steel output and the chemical output – are the 'real' units of economic analysis (note the similarity of this rhetoric to that advocating a micro-economic foundation for macro-economics). If the components started accelerating *before* the new banks appeared, becoming bulky only later, then the new banks could not have been the initiating force. Alas, the components did just this. They spoil Gerschenkron's bank-led story: the components accelerated not in the 1890s but in the 1880s, not after but before the banks. To paraphrase Gerschenkron on Rostow, by the time the progressive components of industry had become bulky enough to affect the larger aggregate, the exciting period was well over.

Yet the moral is still Gerschenkron's, or Kant's: that continuity and discontinuity are *tools* (a favourite word) 'forged by the historian rather than something inherently and invariantly contained in the historical matter.... At all times it is the ordering hand of the historian that creates continuities or discontinuities' (Gerschenkron 1968: 38). For the Italian case Homer nodded, but in nodding made the point. So does any choice of smoothness or suddenness in economic storytelling.

The point is that history, like economics, is a story we tell. Continuity and discontinuity are narrative devices, to be chosen for their storytelling virtues. Niels Bohr said once that 'It is wrong to think that the task of physics is to find out how nature is. Physics concerns what we can say about nature' (Moore 1966: 406). It is *our* say. We can choose to emphasize the continuous: 'Abraham begat Isaac; ... begat ... begat ... and Jacob begat Joseph the husband of Mary, of whom was born Jesus' (Matthew 1: 1–17). Or the discontinuous: 'There was in the days of Herod, the king of Judea, a certain priest named Zacharias' (Luke 1: 5). It is the same story, but its continuity is our creation, not God's.

Gerschenkron's hypothesis about European industrialization makes better sense if the language of 'hypothesis' is dropped and that of storytelling

substituted. The Bulgarian experience, for example, 'rejects' the hypothesis, because Bulgaria's rate of industrial growth 'was obviously far below what one should expect in view of the country's degree of backwardness' (Gerschenkron 1970: 126; cf. 1962, 232). He notes that Bulgaria frittered away its governmental entrepreneurship on military adventures. In the 'failures' of the story one's attention is drawn to illuminating facts. In the peroration of *Europe in the Russian Mirror* he specifically rejects the language of hypothesis testing even while using it:

> For in trying to set up interpretative models [read 'stories'] historians do not deal in universal propositions which can never be verified and can only be refuted [a direct attack on positivist dogma]. We deal in particular or existential propositions. It is the very nature of an historical hypothesis [back to positivism: read 'plot'] to constitute a set of expectations which yields enlightenment ... within a spatially and temporally limited zone. To determine the delimitations of that zone does not mean at all a refutation of a hypothesis [if 'hypothesis' is not understood in its positivistic sense], but on the contrary its reinforcement as a tool of historical understanding.
>
> (Gerschenkron 1970: 130)

The last sentence makes no sense if relative backwardness is a 'hypothesis' like the inverse square law (incidentally, the *Oxford English Dictionary* gives the method-of-science definition as its third; none of its definitions fits Gerschenkron's usage exactly; his usage plays off the third definition, but is not identical with it). If planets were attracted inversely in proportion to the *cube* of the distance between them, that would be that. There would be no sense in which such a finding 'would not necessarily detract from my approach' (Gerschenkron 1970: 130). Relative backwardness, however, is not a scientific hypothesis in the usual sense. It is a device for telling a story, like the frontier in American history or the bourgeois in revolutionary France. It can be proven wrong (in fact these two have been) if it violates the sense of reasoned adequacy.

There is, of course, nothing unscientific about a story. Plate tectonics in geology is a story, not a universal hypothesis like the inverse square law or the Shrödinger equation. Better yet, and more conformable with Gerschenkron's delight in botanical analogies, the theory of evolution is a story. Determining the delimitations of evolution does not refute the hypothesis. Anti-evolutionists refute evolution by taking the falsificationist claims of folk philosophy seriously. The scientists who have mixed bad philosophy with their science deserve what they get from the fundamentalist preachers. As 'hypothesis' (*Oxford English Dictionary* definition 3) evolution is a failure because it is in positivistic terms 'meaningless'. But by the standard of reasoned adequacy it is, of course, a spectacular and continuing success.

The question is whether one can take as seriously in economics as in, say, the criticism of novels an assertion by Peter Brooks, in his *Reading for the Plot*: 'Our lives are ceaselessly intertwined with narrative, with the stories that we tell, all of

which are reworked in that story of our own lives that we narrate to ourselves.... We are immersed in narrative' (Brooks 1985: 3). I would say yes, uncontrovertibly, for economic and other history. As the historian J.H. Hexter put it, storytelling is 'a sort of knowledge we cannot live without' (Hexter 1986: 8). This telling of stories was Gerschenkron's main project. Economists and historians have not lived without it, ever. It is no accident that European economics and the European novel were born at the same time. We live in an age insatiate of plot.

Good empirical work in economics is like good history, in being realist fiction. An historical economist such as Gerschenkron can be viewed as a realistic novelist or a realistic playwright, a Balzac or a Strindberg. His work claims to follow all the rules of actual worlds. (Well ... all the *important* rules.) But of course realism too is fictional. The evasion is similar in history: 'the plot of a historical narrative is always an embarrassment and has to be presented as "found" in the events rather than put there by narrative techniques' (White 1981: 20; cf. Megill and McCloskey 1987).

Constrained by evidence, we tell the stories about French coal mines or the battle of Borodino as economic historians or as novelists. We do not find the stories ready made. John Keegan has nicely illustrated the point in his book, *The Face of Battle*. He speaks of the 'rhetoric of battle history' (Keegan 1977: 36) as demanding that one cavalry regiment be portrayed as 'crashing' into another, a case of 'shock' tactics. Yet an observant witness of such an encounter at Waterloo reported that 'we fully expected to have seen a horrid crash – no such thing! Each, as if by mutual consent, opened their files on coming near, and passed rapidly through each other' (Keegan 1977: 149). A story is something told to each other by human beings, not something existing ready-told in the very railways or cavalry regiments or mute facts themselves.

As Piero Bolchini has pointed out, for example, Gerschenkron's telling of industrialization on the Continent uses Britain as the mirror, always present if seldom mentioned, the standard by which the story is told. The hypothetical happy family in Anna Karenina likewise serves as an anti-plot to the dismally unhappy one in the story. The anti-plot gives the target plot a meaning. The slow and voluntaristic story of British industrialization, more implied than told in Gerschenkron's writing, gives political meaning to the hurried and authoritarian story of Russia.

The story of arriving at full industrialization, again, follows the storytelling in psychology, which 'must first establish a goal state or valued endpoint.... [It must] then select and arrange events in such a way that the goal state is rendered more or less probable' (Gergen and Gergen 1986: 25f). So it is in history, most self-consciously in the great nineteenth-century exponents of the science. In the Preface to his mighty *History of the Conquest of Peru* (1843/48) William Prescott justified carrying the story beyond the adventures of Pizzaro, down to the integration of Peru into the Transatlantic empire: 'fixing the eye on this remoter point, the successive steps of the narrative will be found leading to one great result, and that unity of interest [will be] preserved which is scarcely less essential to historic than dramatic composition.'

Gerschenkron told the story of Petrine Russia by 'fixing his eye on the remoter points' in the time of Count Witte or Joseph Stalin, and on the one great result of forced industrialization, preserving thereby a unity of interest. (Paul David has noted too that his story puts more weight on individuals than do the stories by Marx or Rostow.) Gerschenkron's story, like that of Freud or Sophocles, is the story of a tragic flaw, a curse of relative backwardness damning the generations. That does not make it literature rather than science. As Gergen and Gergen (1986: 31) remark, in developmental psychology the good scientific 'narrative is likely to draw from the pool of commonly accepted narratives within the culture. To do less would fail to participate in the communal practices of making sensible accounts.'

There is more than prettiness in such matters of plot. There is moral weight. Hayden White has written that 'The demand for closure in the historical story is a demand ... for moral reasoning' (White 1981: 20). A monetarist is not morally satisfied until she has pinned the blame on the government. The historical economist says, Do not be fooled about Russian history; wake up; act your age; look beneath the surface; recognize the dismal tragedies of life. Stories impart meaning, which is to say worth. A *New Yorker* cartoon shows a woman looking up worried from the TV, asking her husband, 'Henry, is there a moral to *our* story?'

That the humanities can read the sciences should be no surprise. After all, reading is their job. I think it would have pleased Gerschenkron to see the sciences and humanities back on speaking terms this way. He too would have found the rhetorical grid better than the usual ones.

Applying such a grid, you can see, does not necessarily produce a hostile judgement. Rhetoric is not a mask to be stripped away to reveal the bare Logic and Evidence beneath. Rhetoric, like any tool, can be dishonestly used, as the best tools can be misused to greatest effect. But viewed scientifically rhetoric is merely the art of argument, ranging from index numbers to literary parallels, and from the fourth digit of accuracy to the story of industrialization. A rhetorical reading of Gerschenkron does not reveal him as a non-scientist, a mere word-spinner. Master scientists are master rhetoricians, word-spinners in no dishonourable sense. Science is rhetoric, all the way down.

The rhetoric of economic history has continued to depend on the old verities of positivism. Gerschenkron's example was not followed. For the most part one does not see economic historians nowadays crafting their statistical stories with self-conscious care, combining number and word in the style of Bloch or Heckscher or Gerschenkron. One sees them emulating modern economics instead of instructing it, pursuing this or that five-year wonder of technique from across the hall. Gerschenkron sets a different standard, worth meeting. It would improve economics and would in any case result in better economic history: examine all the evidence, not merely the evidence that fits a quantitative or anti-

quantitative epistemology; and argue the case fully aware of its fact and logic, its metaphor and story.

NOTE

1 Gerschenkron seldom required italics to make his points. All italics in quotations from his works are my own.

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