

How economists persuade

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When Kenneth Arrow was asked what he used to judge competing theories in economics he answered:

Persuasiveness. Does it correspond to our understanding of the economic world? I think it foolish to say that we rely on hard empirical evidence completely. A very important part of it is just our perception of the economic world. If you find a new concept, the question is, does it illuminate your perception? Do you feel you understand what is going on in everyday life? Of course, whether it fits empirical and other tests is also important.

(Feiwel 1987: 242)

Surprisingly the passage is quoted by Mark Blaug as demonstrating that Arrow is a Lakatosian (Blaug 1991: 505). Its prose meaning, though, is that Arrow, like us all, is a rhetorician. He seeks persuasion, through articulated introspection, through a sense of the social world, and through fully identified best linear unbiased econometric tests, too. Science is social, not solipsistic.

Science, to put it another way, is human persuasion, not mechanical demonstration. Of course the scientist will claim every time that what she is arguing is demonstrated – that is, seen, plain, shown, obvious, indubitable, exhibited wordlessly. Newton, who was very good at this sort of rhetoric, wrote: ‘what I tell ... is not Hypothesis but the most rigid consequence, not conjectured ... but evinced meditation of experiments concluded directly and without any suspicion of doubt’ (Christianson 1984: 94). So there. The scientist is always trying to persuade people that her evidence is just like the proof of the irrationality of the square root of two. If she can get people to agree that she has *demonstrated* the impossibility of monetary policy under rational expectations, she can knock off work early.

If science is mostly persuasion rather than demonstration, then it is a good idea to watch the persuasive devices. The way things are communicated does matter in economics. The mere idea that ‘people are paid what they are worth’, for example, can be uttered in various languages, resulting

in different economics. Uttered in the language of historical relations and moral indignation it is a statement in classical Marxism, comforting to would-be commissars. Uttered in the language of evolution and competition it is a statement in social Darwinism, comforting to the country club. Uttered in the language of continuous mathematics it becomes the 'marginal productivity' discussed by economics since 1900, carrying with it a rich set of images about 'production functions' and 'amounts' of labor. Uttered again in the language of discrete mathematics it becomes a branch of a new Marxist economics in the 1960s or the linear programming of the 1950s. Uttered in the language of production management, and embodied in a good novel, it becomes a fashion among American business people (Goldratt and Cox 1986). These all contain one might say 'the same basic idea' (all ideas are 'basic' when one is making such a point). And so it may be the same, crudely speaking. But the way of speaking modifies the idea, reversing for instance its political uses. 'Marginal productivity' can justify a stony *laissez faire*. 'To each according to his need' can justify a revolutionary slaughter.

The word 'communication,' though, usually evokes the image of 'sender' and 'receiver'. The sender-receiver notion oversimplifies, making argument sound like a job for the repairman. In most communication the message is not a performed slug, a mere telephone number to be read out by a computer at Directory Inquiries (once named 'Information'). Commonly the message is changed by the demands of the communication – which is to say, the presence and character of the audience, the attitudes of audience and speaker to each other, the language spoken in common, the style of the customary medium, the history of earlier and similar talk, the practical purpose to be achieved from the communication. They do not always 'distort' it (the metaphor of distortion assumes again that the preformed slug sits there ready to be found). Saying 'Too high a tax rate will bring in less revenue' to an audience of students in a price theory course makes merely a routine point about supply and demand; saying the same thing to an audience of voters in 1980 makes the Reagan revolution. The demands of communication change the message. Commonly if not always, in other words, there is no 'it' to be communicated without communication.

We need some other theory of scientific communication for economics. The sender-receiver metaphor, though an improvement on the metaphor of merely 'seeing' the demonstration, suggests that anything aside from the message slug is noise. The theory needs to be richer than a signal-to-noise ratio. The use of a certain kind of mathematical language in describing, say, the reward for labor will send along its own substantive message. What's sent is not mere prettiness (although prettiness communicates, too).

The theory of persuasion is rhetoric. Rhetoric is not what is left over after logic and evidence have done their work (see John Davis 1990b and

Peter Munz 1990). It is the whole art of argument, from syllogism to sneer. All that moves without violence is persuasion, *peitho*, the realm of rhetoric, unforced agreement, mutually advantageous intellectual exchange. It would therefore include logic and fact as much as metaphor and story. 'Logic', as logicians have been making steadily clearer in the century past, is not an unargued realm. Logic can be Aristotelian, scholastic, first-order predicate, deontic, modal, relevant, multivalued, informal, intensional, counterfactual, epistemic, paraconsistent, relevant entailment, fuzzy, and so on and so forth through the various ways that people can formalize what they are saying. The linguist and logician James D. McCawley says that 'only through arrogance or ignorance do logicians palm off any single full system of logic as unchallengeable' (McCawley 1990: 378). Likewise 'fact' is not to be determined merely by kicking stones or knocking tables. That a fact is a fact relative only to a conceptual scheme is no longer controversial, if it ever was. Kant knew it; so should we. Studies of science over the past few decades have shown repeatedly that facts are constructed by words (the studies were initiated by the biologist Fleck 1935 [1979] on syphilis).

There is nothing shameful in this logic and fact of scientific rhetoric. As Niels Bohr said '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.... We are suspended in language.... The word "reality" is also a word, a word which we must learn to use correctly' (Moore 1966 [1985]: 406; but not all people are gifted at every part of argument: Bohr, gifted at metaphor, could not follow the plots of his beloved movie westerns, and would bring someone along to whisper explanations in his ear). That is to say, appeals to experimental finding are as much a part of a broad-church definition of rhetoric as are appeals to the good character of the speaker. Mill's logic of strict implication is as much rhetoric as is the anaphora of Whitman's poetry. Wittgenstein says, 'Uttering a word is like striking a note on the keyboard of the imagination' (1945 [1958]: 6). In this definition a science as much as literature has a 'rhetoric'.

When economists look at something, say childcare, they think of markets. 'Childcare' – which to other people looks like a piece of social control, or a set of buildings, or a problem in social work – looks to economists like a stock certificate traded on the New York exchange. By this choice of metaphor they are driven to identify a demand curve, a supply curve, and a price. If the economists are of the mainstream, neoclassical kind they will see 'rational' behavior in such a market. If they are Marxist or institutionalist or Austrian economists they will see somewhat differently. But in any case the seeing will seem to them to make ordinary sense, to be the way things really truly are.

The rhetorician notes that the market is 'just' a figure of speech, though a serious rhetorician, or a serious philosopher of science, will not add the

'just', because the metaphor is a serious figure of argument. Noting the metaphors is not merely another way of saying that economics is approximate and unperfected. Economists believe that the metaphor comes from the fuzzy, humanistic side of the modernist world. A model in economics comes to be called a metaphor, in this way of thinking, if 'the statement can be tested only approximately' (thus David Gordon 1991). But the inverse square law of gravitational attraction is also a metaphor; so is Einstein's generalization. The Romantics assigned metaphor to the realm of art, distinguishing for the first time an imaginative from a scientific faculty, as though different organs of the brain. The literary critic Francis McGrath has argued that the distinction cannot be sustained (McGrath 1985). Boyle's Law shares metaphor with Shakespeare's 73rd sonnet: metaphor, McGrath argues, is as fundamental to science as to art.

Models are metaphors, that is all. So in other fields: 'the mechanistic ... the organismic, the marketplace, the dramaturgical, and the rule-following metaphors have all played a significant role in psychological research of the past decades' (Gergen 1986: 146). 'The market' is a commonplace, a *locus communis*, a *topos* – a place where economists work. The rhetorician's metaphor here is locational. In the rhetorical way of talking since the Greeks the metaphor of a 'conversation' is a *topos* for the language game across the playing fields of economics (Klamer and Leonard 1992 explore metaphors in economics more thoroughly, with reference to the now large philosophical literature; and see McCloskey 1985).

It is sometimes argued therefore that economics and other sciences, though using metaphors, use them in a different way from poets. The philosopher of science Cristina Bicchieri, for example, in a penetrating comment on my 'poetics' of economics, argues that 'A good literary metaphor should be surprising and unexpected. ... Scientific metaphors, on the contrary, are to be overused' (1988: 113, italics supplied; compare Oakeshott 1959: 528: the poet's 'metaphors have no settled value; they have only the value he succeeds in giving them').

Well, yes and no. The economist A.C. Harberger tells the story of a cocktail party at his house in the late 1950s, when Gary Becker, a brilliant student at Chicago, was working on the dissertation that became *Human Capital: A Theoretical and Empirical Analysis, with Special Reference to Education* (1964). The party was well along, but Gary as usual was sober and serious, and always, always talking economics. He came up to Harberger and remarked out of the blue, soft drink in hand, 'You know, Al, children are just like consumer durables'. It was a poetic moment, unexpected certainly to Harberger (who in fact was an expert on consumer durables, but had no idea that procreation might fit the category). True, as Bicchieri says, Becker intended the metaphor 'to be overused', which is to say, to become part of the dead metaphors of the field; and it has. A recent book on economics for gradeschoolers uses 'H' as the sign for labor. But at

the moment of creation – like a poem once alive that becomes a cliché – it was anything but dead.

To get further into the literary analysis of the scientific article we need to get down to the words. The journals became more professional in part by encouraging the growth of a technical vocabulary, fleeing the common culture. No elaborate test is necessary. The implied *reader* of an economics journal has changed to support the claim of Scientific standing of the implied *author*. The percentage of terms that a non-professional reader could understand has fallen steadily in economic journals since the 1920s.

It might be called the 'blub-blub' effect, the 'blub-blubs' being the words that only people socialized within the community of economics can understand. An interesting example of the rhetorical use of implied readers and authors is an essay in historical economics by Stephen Nicholas in 1982 in the *Economic History Review*. The implied (and actual) readers of the *Review* are not technically trained economists. Most of Nicholas' article is lucid prose accessible to such a readership. But suddenly, when he is trying to undermine the neoclassical measure of productivity change, the implied reader changes. By the mere statement of the 'assumptions' said to underlie the measure, he intends to sow doubt in the minds of all the historians and many of the economists reading. Listen as he undertakes to 'explain' the calculation:

it is assumed [note the style borrowed suddenly from mathematics, after a long time in the *persona* of the historian] that the economic unit is a profit maximizer, subject to a linear homogenous production function and operating in perfectly competitive product and factor markets. Given these limiting assumptions, the marginal productivity theory of distribution equates marginal products to factor rewards. It follows by Euler's theorem ...

(Nicholas 1982: 86)

To most of his readers he might as well have written 'it is assumed that the blub-blub is a blub maximizer, blub-blub blub-blub-blub and blub in perfectly blub and blub blub. Given these limiting assumptions, the blub blub blub blub blub blub blub. It follows by blub blub ...' The audience that can understand the argument is the audience of people who already understand it, leaving one to ask why the argument was necessary. The people who do not understand it will gain only the impression that 'limiting assumptions' are somehow involved. The apparent form of the passage is explanation; its actual and intended effect in the pages of the *Economic History Review* is to terrify the onlookers, convincing them that the neoclassical analysis makes all manner of strange assumptions (it does not, but that is not the point here).

Watching how economists argue, that is, entails watching the details of style, looking at as well as through the words, as the English professor

Richard Lanham expressed it (Lanham 1993). For example, in the modern novel the suppression of the authorial 'I' has resulted in a technique peculiar to literature, 'represented speech and thought'. Grammarians call it 'unheralded indirect speech', the French *style indirect libre*. Any page or two of Jane Austen serves, as in *Persuasion*: 'Sir Walter had taken a very good house in Camdenplace, a lofty dignified situation, such as becomes a man of consequence' (1818 [1965]: 107), Sir Walter's words 'dignified ... a man of consequence' in Austen's mouth; 'Could Anne wonder that her father and sister were happy? She might not wonder, but she must sigh that her father should feel no degradation in his change' (p. 108), Anne's words ['sigh ... no degradation'] in Austen's mouth.

The parallel suppression of the 'I' in science might be called 'represented Reality' or 'unheralded assertion' or '*style indirect inévitable*'. The scientist says: It is not I the scientist who make these assertions but reality itself (Nature's words in the scientist's mouth). Scientists pretend that Nature speaks directly, thereby effacing the evidence that they the scientists are responsible for the assertions. It's just there. The result is similar in fiction: 'We (as readers) cannot question the reliability of third-person narrators.... Any first-person narrative, on the other hand, may prove unreliable' (Martin 1986: 142). Thus Huck Finn, a narrator in the first person, misapprehends the Duke and we the readers know he does. The scientist, unlike Huck, avoids being questioned for his reliability by disappearing into a third-person narrative of What Really Happened. As Robert Solow wrote on the matter:

Personality is eliminated from journal articles because it's felt to be 'unscientific'. An author is proposing a hypothesis, testing a hypothesis, proving a theorem, not persuading the reader that this is a better way of thinking about X than that. Writing would be better if more of us saw economics as a way of organizing thoughts and perceptions about economic life rather than as a poor imitation of physics.

(Solow 1981)

The implied author of such stuff is not especially attractive. Economists write with a remote and arrogant implied author more than do many other scholars or scientists, as visitors from other fields note. In it the opponent is so obviously misled that it is incredible he got a BA in the subject.

Vivienne Brown's analysis of the style of Adam Smith's two books is a model of what can be learned from a literary approach (Brown, 1993). She notes that *The Theory of Moral Sentiments* has what the Russian literary theorist Bakhtin called a 'dialogic' character, 'in which the moral agent engages in an inner dialogue represented by the metaphor of the impartial spectator' (Brown 1993: 79). By comparison, *The Wealth of Nations*, is closer to the 'empiricist monologue' (a term popular among British sociologists of science). But she argues in a forthcoming book that even

The Wealth of Nations is not entirely 'scientific', which is to say disinterested and non-dialogic, and can even be taken, surprisingly, to criticize economic growth. This much can be read through a style, even a scientific one.

Well, so what? Suppose we economists become more self-conscious about our 'rhetoric', that is to say, about the way we argue? How will economics change?

An economist ought to feel uncomfortable answering such a question, because answering it entails knowing the intellectual future. Anyone so smart would be rich, or at minimum a Nobel laureate. But let me try out a couple of examples, one negative and the other positive.

The negative example is the rhetoric of statistical significance. Lancelot Hogben finds the first use of the word 'significance' as a statistical word to be John Venn's, in 1888, speaking of differences expressed in units of probable error:

They inform us which of the differences in the above tables are permanent and significant, in the sense that we may be tolerably confident that if we took another similar batch we should find a similar difference; and which are merely transient and insignificant, in the sense that another similar batch is about as likely as not to reverse the conclusion we have obtained.

(Venn, quoted in Hogben 1968: 325)

The argument is correct. When applied to a probability sample, significance *does* entail permanence in the sense Venn described, though permanence merely by the standard of 'tolerable confidence' agreed upon.

But a difference could be permanent without being 'significant' in any other sense. This is the main, elementary point. Few economists understand it to the degree of allowing it to affect their work. As Kenneth Arrow put it recently, when reminded of an article making the point that he published in 1959 – an article which sank without trace – the way economists use statistical significance is 'indefensible'.

For instance, the usual test of purchasing power parity fits prices at home (P) to prices abroad (P^*), allowing for the exchange rate (e): $P = a + b(eP^*) + \text{error term}$. The equation can be in levels or rates of change. A tight estimate that $b = 0.999$, 'significantly' different from 1.0, using a sample size of one million, could be used under purely statistical procedures as evidence that the theory had 'failed'. The evidence, one could say in the rhetoric of the antique positivism that rules economics, 'is not consistent with the theory'. The usage would be wrong. Common sense, presumably, would rescue the investigator from asserting that if $b = 0.999$ with a standard error of 0.00000001 we should abandon purchasing power parity, or run our models of the American economy without the world price level. Similar common sense should be applied to

findings that $b = 0.80$ or 1.30 with sample sizes of 30. The test of statistical significance can only affirm a likelihood of excessive skepticism in the face of errors arising from too small a sample, when the level of the type I error has been somehow decided upon. The test cannot say if a theory 'fails'. The test *does not* tell the economist whether a fitted coefficient is large or small in an *economically* significant sense (see McCloskey and Zecher 1984).

The mistaken notion that statistical significance is a machine for making scientific inferences can be attributed, as many good and bad features of modern statistical practice can be attributed, to R.A. Fisher. His opposition to a more rational machinery of inference was robust and sufficient. Modern econometricians think they are following statistical decision theory, and have a section on it in their textbooks, but have no place in their actual work for a loss function. They are in fact dancing to Fisher's tune. In the 1930s Jerzy Neyman and E.S. Pearson, and then more explicitly Abraham Wald, argued that actual statistical decisions should depend on substantive, not merely statistical, significance. In 1933 Neyman and Pearson wrote of type I and type II errors:

Is it more serious to convict an innocent man or to acquit a guilty? That will depend on the consequences of the error; is the punishment death or fine; what is the danger to the community of released criminals; what are the current ethical views on punishment? From the point of view of mathematical theory all that we can do is to show how the risk of errors may be controlled and minimised. The use of these statistical tools in any given case, in determining just how the balance should be struck, must be left to the investigator.

(Neyman and Pearson 1933: 296)

Wald went further: 'The question as to how the form of the weight [that is, loss] function ... should be determined, is not a mathematical or statistical one. The statistician who wants to test certain hypotheses must first determine the relative importance of all possible errors, which will depend on the special purposes of his investigation' (1930: 302). The idea of bringing cost and benefit into the scientific decision should be attractive to economists (Wald was an economist as well as a mathematician, and studied with Karl Menger in Vienna in the 1920s). But economists have not understood the idea or used it in their econometric routines. The loss function has been mislaid (cf. McCloskey 1985).

The reaction of most people to such warnings is to seize on a vague understanding of what is involved and then say, 'Oh, I don't make that mistake; nor do other first-rate economists. We know the difference between statistical and scientific significance.'

No, we do not. One can make jokes about the scientific quality of papers in the *American Economic Review* (AER), and no one would claim that it is well written. But in sober truth it is a superb magazine, and the empirical

papers in the *Review* can be taken as an upper bound. Stephen Ziliak and I read all the 182 papers in the 1980s that used regression analysis (and record again our strong impression: these are fine examples of the economist's art). Each paper was asked twenty-two questions about its use of statistical significance, yes or no or not applicable. Sometimes the theory of sampling indicates which answer is the good practice. In other cases less formal rhetoric is involved, which is to say the scientific discussion about what is meant by economic, policy, or scientific significance. When arguing over the economic significance of a tax cut or a bond issue or the minimum wage, the economists must use quantitative standards – answering questions such as 'How Large?' and 'Who Receives What Share of the Benefits?'.

Some authors, such as Kim B. Clark, show that they are aware of the substantive importance of the empirical questions they ask, and of the limits of a test of statistical significance toward getting an answer: 'While the union coefficient in the sales specification is twice the size of its standard error, it is substantively small; moreover, with over 4,600 observations, the power of the evidence that the effect is different from zero is not overwhelming' (Clark 1984: 912). And Zvi Griliches observes:

Here and subsequently, all statements about statistical 'significance' should not be taken literally. Besides the usual issue of data mining clouding their interpretation, the 'sample' analyzed comes close to covering completely the relevant population. Tests of significance are used here as a metric for discussing the relative fit of different versions of the model. In each case, the actual magnitude of the estimated coefficients is of more interest than their precise 'statistical significance'.

(Griliches 1986: 146)

Griliches understands that populations should not be treated as samples, and that statistical significance is not a substitute for economic significance. It is a pity he has not conveyed his understanding to other economists. He does not say why statistical significance is any more appropriate as a scientifically relevant 'metric for discussing the relative fit of the different versions of the model'.

The principal findings of the survey are:

- Despite the advice proffered in books on theoretical statistics, only $4\frac{1}{2}$ percent of the empirical papers in the *American Economic Review* consider the power of their tests. 1 percent examine the power function.
- 70 percent do not distinguish statistical significance from economic, policy, or scientific significance. The mistake is made by authors from Tier I and other schools alike.
- 59 percent use the word 'significance' in ambiguous ways, at one point

meaning 'statistically significantly different from zero', at another 'write to your Congressman', with nothing qualifying the difference.

- At the first use of statistical significance, typically in the 'Estimation' or 'Results' section, 53 percent do not consider anything but the size of *t*- and *F*-statistics. To put it another way, only 47 percent consider the substantive significance of estimated coefficients at the first use. About one third used only the size of *t*- and *F*-test statistics as a criterion for the inclusion of variables in future work.
- 72 percent do not ask 'How large is large?' That is, after settling on an estimate of a coefficient, 72 percent do not consider what other authors have found; they do not ask what standards other authors have used to determine 'importance'; they do not provide an argument one way or another whether the estimate $\beta = 0.9998$ is economically close to 1.0 and economically important even though statistically different from one'.
- 69 percent do not report descriptive statistics – not so much as the means of the regression variables – that would allow the reader to make a judgment about the economic significance of the results.
- Of the 107 papers using cross-sectional data, 19 percent use sampling theory to discuss a population, or they use sampling theory on a sample of convenience clearly not representative. Only two papers offered some justification.

Well, what of it? Here's what. Almost all econometric fittings have to be done over again. All of them. All the statistical work that has dropped and added variables by statistical significance needs to be redone. None of the econometrics that decides whether variable *X* is 'important' by using statistical significance has been correct, for all these years. (I warned you that this first example of why a rhetoric of economics is important would be 'negative'.) That's good news for assistant professors: all the work of your elders has been wasted, which leaves you with a brilliant career ahead redoing what they did wrong.

The positive example of what can come from recognizing that economists use a rhetoric to persuade is the rhetoric of markets. If science is a field of argument, it will occur to an economist (and occurred first to Arjo Klamer) that 'field of argument' describes the *economy* pretty well, too.

The arguments look odd in the neoclassical account of the stock market. The New York Stock Exchange, after all, is a market with numerous buyers and sellers and low transaction costs, as near to efficiency as can be hoped for. Efficient markets convey through prices all the information that a trader would want. No need to talk. Any informational advantage is reflected in price changes. Such efficiency would provide few rewards to talking if the talkless model was the whole story. The best a loquacious

trader could hope for would be the quick exploitation of minor information advantages, or a turn of luck. Efficiency gives no account of the chatter. The conventional story conjures up a silent film of people throwing darts or staring at computer screens, and typing (silently) their orders. Under neoclassical and most other economic principles the processing of the information that prices convey does not require talk.

Robert Shiller has examined the matter closely (Shiller 1989). He starts from the observed price fluctuations in the stock market, arguing that they cannot be attributed to the flow of mere information. He suggests that traders exercise judgment on their information, producing what the political philosopher Michael Oakeshott called 'knowing':

News stories or commonly noted events that remind people of the stock may make it more likely that individuals will talk about the stock. . . . The rate of spread of interest in one stock is likely to be inhibited by the spread of interest in another stock, since people can talk seriously about a limited number of stocks at a given time. Thus, for example, a big earnings announcement in a different firm may cause conversations that displace conversations about the stock. The lumpiness of media attention is also a factor inducing randomness in the behavior of diffusion traders. Opportunities to talk seriously about a given stock with others may be influenced by patterns of social interaction that may vary irregularly over time.

(Shiller 1989: 56)

Shiller does not examine the significance and consequences of selective talk directly. But he questioned participants in the market immediately after the collapses of October 1987 and 1989. His surveys (an epistemologically unsound technique, remember) suggest that the participants reacted to no particular news story. Instead, '[a]lmost all of the responses reported . . . advice of brokers and friends, or predictions others made about the future course of the market' (Shiller 1989: 387). Shiller found that in his 1987 sample the average institutional investor talked to 7.4 other people on the day of the crash and the average individual investor to 19.7 other people (Shiller 1989: 388). The talk is probably meant to reduce cognitive dissonance, as the psychologists say, getting us all to believe in the current madness by sharing it promiscuously with others. Shiller found a significant 'contagion of fear', of a sort familiar in other contexts (such as the Great Fear during the French Revolution).

Economists view talk as cheap and culture as insignificant. Yet humans are talking animals, and the animals talk a great deal in their marketplaces. Of course the economist does not have to pay attention to everything that happens in an economy. That farmers chew tobacco or paint traditional designs on their barns while dealing in corn does not necessarily have to appear in the econometrics. What would have to appear is a large

expenditure, since expenditure is the economist's measuring rod.

Two economic historians, John Wallis and Douglass North, have argued that transactions costs – that is, expenditures to negotiate and enforce contracts – rose from a quarter of national income in 1870 to over half of national income in 1970 (Wallis and North 1986: table 3.13). Their measurement is a model of how to make such calculations, and suggestive of the importance of talk in the economy. It is not precisely the measurement wanted here. Transactions costs include, for example, 'protective services', such as police and prisons, which 'talk' only in an extended sense. Literal talk is special – in particular it is cheap, as police and prisons are not – in a way that makes it analytically separate from the rest of transaction costs. Information is one part of the talk; issuing orders is another. The conveying of information and orders is well understood by economics: much of game theory is concerned one way or another with information; and production theory might be construed as the theory of one mind issuing orders.

Persuasion is the third part of economic talk. It is not well understood.

But it is startlingly big. Take the categories of employment and make an educated guess as to the percentage of the time in each category spent on persuasion (the calculation could be improved with more factual and economic detail; for instance, the workers could be weighted by salaries; the marginal product of persuasion could be considered in more detail; the occupational categories could be subdivided: I intend here only to raise the scientific issue, not to settle it). Weighted sums yield 28.2 million out of 115 million civilian employment, or about a quarter of the labor force, devoted to persuasion. One quarter of national income.

The result can be confirmed in other measures. Over half of American workers are white collar. Some do not talk for a living, but in an extended sense many do, as for that matter do many blue-collar workers and especially pink-collar workers. And of the talkers a good percentage are persuaders. The secretary shepherding a document through the company bureaucracy is often called on to exercise sweet talk and veiled threats. Or notice the persuasion exercised the next time you buy a suit. Speciality clothing stores charge more than discount stores not staffed with rhetoricians. The differential pays for the persuasion: 'It's you, my dear' or 'The fish tie makes a statement'. As Smith says (*Lectures On Jurisprudence* 1978 [1982], 'Report of 1762–63,' vi.56: 352, spelling modernized here and later), 'everyone is practising oratory ... [and therefore] they acquire a certain dexterity and address in managing their affairs, or in other words in managing of men; and this is altogether the practise of every man in most ordinary affairs ... the constant employment or trade of every man ...' Not constant, perhaps, but in Smith's time a substantial percentage and in modern times fully 25 percent.

Is the persuasive talk then 'empty', mere comforting chatter with no

further economic significance? If that was all it was then the economy would be engaging in an expensive activity to no purpose. A quarter of national income is a lot to pay for economically functionless warm and fuzzies. The fact would not square with economics. The business people circling La Guardia on a rainy Monday night could have stayed home. The crisis meeting in the plant cafeteria between the managers and the workers would lack point. Wasteful motion, or empty words, sit poorly with conventional economics. By shutting up we could pick up a \$20 bill (or more exactly a \$1,500,000,000,000 bill). That cannot be. A quarter of our working time in the marketplace is spent in persuasive converse. The conversational metaphor acknowledges the fact.

The economy, one might say from the statistics, 'rests' on persuasive talk. Yet economists are not required, I have said, to pay attention to everything the economy 'rests' on. It also rests, for example, on engineering, but in speaking of the economics of bridges and other social overhead capital an economist does not need to know about the equilibrium equations for three-dimensional rigid bodies.

In the present case, when economists can reduce some transaction to a *silent* physical action, they can properly ignore the talk, at least its effects on the equilibrium price. Adam Smith, to continue the quotation from *Lectures on Jurisprudence*, said that persuasion, 'being the constant employment of trade of every man, in the same manner as artizans invent simple methods of doing their work, so will each one here endeavour to do his work in the simplest manner. That is bartering, by which they address themselves to the self interest of the person and seldom fail immediately to gain their end.'

Of course, a sociologist would point out that the institutions of a bartering grocery store 'rest' on all manner of talk, such as the giving of orders to the junior grocery clerk to spend the next hour fronting the stock in aisles 6 through 8. The anthropologist reminds economists that 'behind' their market lies a culture, which is another talking matter. But, given the culture and the institutions (a big given, I admit), and confining attention to the immediate result, if the clerk does not like the assignment he can silently walk, exercising his option of what Albert Hirschman calls 'exit' (Hirschman 1970). He is not a slave. Likewise, a man who tries to haggle with his local grocery store about the price of a quart of milk is wasting his breath and wasting the grocer's time. If he thinks he can get a better price down the street, he can walk. If the grocer thinks the next customer will pay the price, he can ignore the haggler. The talk in such cases is not essential for the economics.

Walk or talk. To put it another way, that many transactions involve talk may be interesting but need not be a criticism of economics. The physical walking may still set tight limits on what can be charged. People bargain over houses and automobiles, talking a lot, yet no one will be talked into

selling a four bedroom apartment overlooking Central Park for \$3.57 or into buying a '77 Chevy with a little rust and 220,000 miles for \$500,000. The customer will walk away from such offers. What matters is the size of the 'gold points' within which silent trade confines the price. If the gold points are narrow, the talking does not change the deal. If they are wide, however, it may well change it.

A case of wide gold points, by definition, is pure bargaining. Two people meet in the middle of the Sahara, alone. One person has plenty of water but no food; the other plenty of food but no water. At what price will the deal take place? Obviously, it depends on the bargaining skills of the two. Economists have not made much progress in understanding situations of pure bargaining. Game theory for all its wonders has not gotten far on the matter (Fisher 1989: esp. p. 122). Perhaps, I am suggesting, the theoretical impasse arises because bargaining is talk, all the way down. As the food owner in the Saharan encounter, Arjo claims forcefully that he has a physical ailment requiring an unusual amount of both water and food. Don detects a ruse, and offers a little water in exchange for his pound of food. Arjo weeps affectingly; Don's heart softens, and he hands over half his water. Or he laughs sardonically and portions out to Arjo a tiny swig in exchange for most of his food. It depends on talk.

This unsatisfactory conclusion relates to a basic feature of speech, that it can apparently be trumped, cheaply, in a way that sweaty physical action cannot. Suppose you devised a rule that would predict the bargaining speech of lonely owners of water in the middle of Sahara. Would this permit you to extract the water at a low price? No. As economists have pointed out repeatedly, if one person is predictable, the other can exploit the predictability, which will suggest to the exploited one that he had better randomize. If you're so smart about bargaining, why ain't you rich?

A limit on calculability is a feature of any speaking. If anyone could get their way by shouting, for example, then everyone would shout, as at a cocktail party, arriving by the end of the party hoarse but without having gotten their way. The philosopher H.P. Grice affixed an economic tag to the trumping of speech conventions, 'exploitation'. The linguist Stephen Levinson puts his finger on the limits of formalization when language is involved:

'[T]here is a fundamental way in which a full account of the communicative power of language can never be reduced to a set of conventions for the use of language. The reason is that wherever some convention or expectation about the use of language arises, there will also therewith arise the possibility of the non-conventional exploitation for that convention or expectation. It follows that a purely ... rule-based account of natural language usage can never be complete....'

(Levinson 1983: 112)

A joke among linguists makes the point (the story is said to be true). A pompous linguist was giving a seminar in which he claimed that while there were languages in which two negatives made a positive (as in Received Standard English: 'I did not see nobody' – 'I saw somebody') or two negatives made a negative (standard Italian: 'Non ho visto nessuno' – 'I did not see anybody'), there are no languages in which two positives made a negative. To which a smart alec in the front row replied with a sneer: 'Yeah, yeah.' Any rule of language can be trumped cheaply for effect.

The game theorist Joseph Farrell has made a similar point in a paper of his called 'Meaning and credibility in cheap-talk games' (1988). What I call 'trumping' he calls 'neologism', and finds that games are sensitive to its use.

We could conclude that we have no satisfactory positive theory in a one-shot game [a conclusion which may explain the unpopularity of the paper with referees].... Games should be taken in context, especially when analyzing the effects of communication. Language that could not survive in equilibrium if the world were nothing but a particular game, can nevertheless affect the outcome of the game.

(Farrell 1988: 19)

Economists specialize in knowing about costs and benefits. But someone – maybe even a specialized economist – might want to learn about the speech by which people construct their stories of the cost and benefit. Maybe some useful economics can be done, or the existing economics modified. Adam Smith, as usual, put the issue well. The division of labor is the 'consequence of a certain propensity ... to truck, barter, and exchange ... [W]hether this propensity be one of those original principles in human nature, of which no further account can be given; or whether, as seems more probable, it be the necessary consequence of the faculties of reason and speech' (1776 [1981], I, ii: 25, italics supplied) *The Wealth of Nations* does not again mention the faculty of speech in a foundational role, though Smith, who began his career as a teacher of rhetoric, did remark frequently on how business people and politicians talked together. Half of his foundational formula, the faculty of reason, became in time the characteristic obsession of economists. Smith himself did not much pursue it. Economic Man, whether speaking or seeking, is not a Smithian character. It was later economists, especially Paul Samuelson, who reduced economics to the reasoning of a constrained maximizer, Seeking Man.

By contrast, Speaking Man has never figured much in economics, even among institutionalist economists. A man acts, by and for himself. That is what utility functions or institutions or social classes or property rights are about. No need to speak. Walk rather than talk. Smith would have disagreed. Towards the end of *The Theory of Moral Sentiments* (1790 [1982], VII, iv, 24: 336) he dug behind the faculty of speech (which led to the propensity to exchange, which led to the division of labor, which led to

the wealth of nations). He connected it to persuasion, which is to say, speech meant to influence others:

The desire of being believed, the desire of persuading, of leading and directing other people, seems to be one of the strongest of all our natural desires. It is, perhaps, the instinct on which is founded the faculty of speech [Smith was the sort of writer would have been well aware that he was using the same phrase here as he used in *The Wealth of Nations*], the characteristic faculty of human nature

(1790 [1982], VII, iv, 25: 336).

Compare his *Lectures on Jurisprudence*, in the passage quoted: 'Men always endeavour to persuade others ... [and] in this manner every one is practising oratory thorough the whole of his life.'

In other words, economists might explore the economics of talk. Their theory concerns walking, but the economy does a great deal of talking. No English professor or sociologist would doubt the fact. An economics confined to the faculty of reason, and ignoring the faculty of speech, creates paradoxes, as in the theory of rational expectations or the theory of games. The faculty of speech deserves some analytic attention, even from economists.

So those are two ways among many that economists can improve their persuasion, dropping statistical significance and taking up the faculty of speech. In any event, if one looks closely at the way economists argue one comes to Arrow's conclusion: economists are persuaders in a community of scientists.

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