
HISTRIONICS

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C^{RITICISMS} OF my *Social and Cultural Dynamics* by Crane Brinton* fall into three classes: comments irrelevant to the validity of the theories, thrusts at "straw-men" erected by the critic, and criticisms of a few real issues.

I. Perfectly irrelevant comments consist of a lusty and repetitious enumeration of such supposed defects as: my "prolixity and repetitiousness" (my critic repeats this six times); a lack of "grace of style" (repeated by the critic five times); lack of "feeling of form" (repeated three times); emotional character of my writing (repeated nine times); wrong use of quotation marks; my irony of "the lumbering kind" (repeated twice), and the like. This sort of criticism occupies a considerable part of Mr. Brinton's article. If for the moment we grant that he is right in all these accusations, what of it? What relationship have these shortcomings to the validity of my theories? None! My work is not submitted for a prize in English composition. It does not lay claim to the virtues of elegant writing. Generally speaking, most scientific works, including the classical treatises, do not strive for grace of style and other ornaments mentioned by Mr. Brinton. Only a person who confuses *belles-lettres* with scholarly exposition applies the criteria of poetic merit to scientific treatises and the standards of these in turn to poetry. Such a perturbation as Mr. Brinton's readily results, of course, in a critical incompetency in both directions.

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The justification for these criticisms becomes more obscure when one considers that the critic himself unquestionably exhibits exactly these shortcomings in his article. As the above-mentioned rough statistics show, he is unduly repetitious and prolix, his writing is charged with emotions, and is poorly organized and rambling. His style pretends to be playfully funny, but the playfulness discloses a somewhat elephantine ineptitude. The oft-quoted *Medice cura te ipsum* can indeed be addressed to my critic.

What is still more humorous in these irrelevancies concerning my work is the fact that other, and no less competent critics, have made the following assertions to the contrary. Professor Arthur Livingston has remarked (*New York Times Book Review*, June 20, 1937): "Professor Sorokin's book is simply, clearly, and beautifully written." Another critic has said (Professor Hans Kohn in the *Survey Graphic*, August, 1937): "Professor Sorokin writes a very readable and graceful style."

Likewise in regard to the alleged prolixity, some of the judges seem to be of a different opinion. One of them states:

The large size of the work is due to the enormous mass of quantitative and factual material which the author assembled and which enables him to speak with greater authority than would otherwise be possible. (Professor C. S. Joslyn in the *Book of the Month*, July, 1937)

Other similar testimonies published by various scholars and literati could be cited to extend the list. This may be taken to imply that the validity of Mr. Brinton's objections is more than doubtful. When a reviewer fills pages with such irrelevancies, instead of taking the real issues, and when his irrelevancies are open to doubt, he is writing his own *testimonium pauperitatis*. Such criticisms mean, first, that the critic has a strong extra-scientific residue to bite the work by all means and at any cost, and second, that not being able to penetrate the surface of it, he must bite at least its shadow. If such an activity amuses Mr. Brinton I have no objection to it. So much, then, for this class of criticism.

II. The second class of Mr. Brinton's censures is also typical of picayunish criticism. Ascribing to the *Dynamics* qualities and propositions not found in the volumes, Mr. Brinton easily creates and destroys his own bogies. Self-evidently this fighting of the critic's own "straw-men" neither concerns nor affects my work.

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Here are typical illustrations of this “technique” of criticism:

H. Spencer and Pareto infuriate him—he has to the full that contempt for his predecessors—a boiling fury at the liberals—scornfulness to the fellow-scientists—a hot struggle with evolution—emotionality to the bursting point—contempt for W. James and many others. [And so on.]

Such a scornful excoriation is indeed devastating to the scientific temper! After repeated exposure to aspersions of this variety one who did not read my volumes would expect them to be filled mainly with raging, swearing, damning, scorning, boiling, furious, and frenzied emotional explosions. I must say that the work will disappoint such expectations. The books have little of this “romanticism.” It is a phantasma ascribed to the *Dynamics* by a vivid histrionic imagination. Incidentally, I have and always have had the highest respect for H. Spencer, Pareto, W. James, and for liberals and scientists in general—all of which does not hinder disagreement with them at several points.

Another sample. My work “attempts to plot the whole past, present and future of mankind”; I have a “militant insistence on my own originality.” It is needless to say that nowhere in the work can such claims be found. My critic, having attributed them to me, then proceeds to show that my theory does not have such originality and that Vico’s theory is similar to mine. Bravo! I have explicitly shown in many places (e.g., Vol. I, p. x; Vol. II, pp. 10, 33, 217, 375, 471; Vol. III, p. 154) exactly the similarity of Vico’s theory and mine, and not only of Vico’s, but also of Saint-Simon’s and those of dozens of other earlier thinkers. Thus, Mr. Brinton here appropriates to himself what I say, ascribes to me what I reject or do not say, and then with the help of my own data and arguments emerges victorious over his own straw-man.

Mr. Brinton ascribes to me further a claim of apocalyptic and astrological prophecy, makes this trait fundamental in my work, and even stresses it in the title of his article, “Socio-Astrology.” He leaves an impression with the reader that my work deals mainly with prophecies and astrologies, and that I seem to be one of the most reckless believers in a possibility of accurate forecasting of the socio-cultural processes. The truth is, first, that absurd elements in astrological and related theories are as vigorously

criticized in *Dynamics* as they are in some of my other writings; second, in our era of forecasters and planners I have been one of the few who have contended that scientific forecasting of socio-cultural phenomena is hardly possible (See especially my paper, "Is Accurate Social Planning Possible?" *American Sociological Review*, February, 1936); third, the *Dynamics* is little concerned with any kind of forecasting or prophecy. In more than 2000 pages there are hardly ten pages devoted to what I was careful to designate as *guess* or *speculation*, let alone trying to pass off prophecy or forecasting. Having found the unmistakable symptoms of a sharp change in all the compartments of our Western culture in the twentieth century, I ask: Does this sharp change or crisis mean one of the short-time spasms, or the beginning of a long-time decline of the present Sensate culture? The question is answered in the following way: "It remains to be seen" (Vol. I, p. 504); "any forecasting of the future in such a matter must be a *guess*" (Vol. II, p. 117); and I many times repeat this caution (e.g., Vol. I, p. 668; Vol. II, pp. 180, 207, etc.). As a *guess*, I am inclined to the second possibility. *Guessing*, when it is stated as *guessing* and not as scientific forecasting, is no transgression and does not mislead anybody. Passing by, I indulge in it and devote to it a few pages in all my work. Such is the real situation. Meanwhile in Mr. Brinton's description of my efforts it is made to loom as a kind of new "Socio-Astrological Apocalypse." This shows again the peculiar accuracy and the *esprit de finesse* of my critic. One may trust that he does not write his own historical works with this same accuracy and *finesse*.

His next ascription states that according to my theory all the compartments of culture change simultaneously. Mr. Brinton takes the case of music and gleefully points to the fallacy of such a theory. Marvellous procedure again! After an extensive investigation I demonstrate that the theory of simultaneous change of all the compartments of culture is wrong, and among many other evidences show this particularly in regard to the change of music as compared with that of the other arts. Now Mr. Brinton attributes to me the proposition I reject, takes my argument and data, and with their help defeats the straw-man.

Without further ado I leave it to the reader to characterize the nature of such a procedure. Only a person who did not read my work at all or

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else one who is reckless with critical integrity can resort to those kinds of tactics. It is unnecessary to give case after case from Mr. Brinton's rambling to show his indefatigable repetitiousness in employing the "straw-man technique." Since the peculiar speciousness of it hardly deserves to be pursued further, let us turn to the real issues.

III. Several of the real criticisms are of minor matters but need to be mentioned. My critic accuses me of unfairness to, and "complete incapability" of understanding, Pareto. He must forgive me for saying that in this and similar accusations he reminds me of a small boy who wishes to display his recently acquired knowledge before his elders. I had been writing about Pareto some fifteen years before Mr. Brinton was aware of his existence. And though some of these writings were translated into six or more languages, nobody, as yet, including the new proselytes of Pareto, has been able to point out any error in my characterization of Pareto's theories. If indeed there were a "complete incapability," such a blunder certainly would have been pointed out by the scholars who read, use, and quote my works. As a matter of fact, most of the writings of the latest adherents of the Paretian school have been criticized for their poor interpretations or misconceptions of Pareto, and Mr. Brinton's views may be excused as the misguided zeal of a new convert into a faith long known.

Still more naïve is his argument over "equilibrium." If Mr. Brinton only had read my paper, "Le concept d'équilibre est-il nécessaire aux sciences sociales" (*Revue intern. de sociologie*, Sept.-Oct., 1936), he would have been aware of certain implications not usually considered in connection with this concept. (Incidentally, the paper set forth a point of view which was accepted by the International Congress of Sociology, whose main convention topic was social equilibrium.) One cannot ignore the fact that the concept of equilibrium has many and diverse meanings, that it was systematically used more than a century before Pareto, and that Pareto did not evolve any new features. Any of the meanings given to this term represent a liability rather than an asset in the social sciences, in that the various fields have their own terms and conceptions which are much better fitted for purposes of analysis and description of social phenomena. In the *Dynamics* I refer to the article and warn against the use of the equilibrium concept. The term, "socio-cultural system," used by me and

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used, long before, by many sociologists, is not equivalent to Paretian or other versions of the concept of equilibrium; therefore, its use cannot be taken as an evidence of either Paretian victory or my conversion into the faith of the "equilibrists." In fact, a careful investigation of the validity of the concept will most certainly equilibrate or cool the present *cliché* for its use.

I pass by without any answer my critic's hazy statements about science, scientific methods, evolution, and other things. Here he seems to have felt himself somewhat lost, and was therefore hardly capable of formulating intelligently what he did want to say and what his sayings could mean. Hence we come to two—and the only really important—issues of the whole paper of Mr. Brinton. The first of these is that the phases of the cultures studied which I style identical, are not such at all. The thirteenth-century European culture is not similar to that of the fifth and fourth century B. C. in Greece, though I style both by the term, Idealistic.

He states:

To the plain critic, the two cultures seem about as different as cultures can be. Can anyone imagine St. Louis delivering Pericles's funeral speech? Surely the Parthenon is as unlike a thirteenth-century Gothic cathedral as it is possible for two masterpieces of architecture to be.

That is a real objection. Is it serious and decisive? If one judges the Greek and the Western cultures by their perceptive-empirical appearances, the objection seems to be crushing. But perceptually, the same chemical element, say carbon, is as different as could be, when it is given as diamond, as graphite, and as a constituent element in all organic compounds. Certainly, "to the plain critic," there is no similarity between all these carbons. And yet, chemistry teaches us that in spite of all this dissimilarity it is the same chemical element, carbon. Mr. Brinton's plain critic is wrong here in his plainness. Likewise, it is not only a conjecture, but I show it factually, that the dominant system of truth of the thirteenth century, the movement of discoveries in it, the specified forms of art, the type of movement of revolutions and war, and a number of other traits in the studied compartments of culture of the thirteenth century are essentially similar, often identical, with respective traits in the same compart-

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ments of culture of Greece of the fifth and fourth centuries B. C. And the claim is not a mere conjecture; it is sufficiently backed by and demonstrated through the factual analysis of these compartments of both cultures. In other respects, and from the naïve perceptive standpoint, the centuries may be as different as graphite, diamond, and carbon in an organic compound. What is still more important, the similarities involved in both cultures are not invented by me. They are stressed by the historians of the respective compartments: of sculpture and painting of the centuries in question; of music, ethics, and of the system of truth of these centuries.

If Mr. Brinton had taken any of the compartments studied, and had shown that these similarities were absent, that for instance the system of truth of Albertus Magnus and St. Thomas Aquinas was fundamentally different from that of Plato, Aristotle, and other leading thinkers of the fourth and fifth centuries—if he had done the same in regard to other compartments of culture analyzed by me—then his argument would have been most effective. Instead, he takes a few perceptive and singular phenomena like Pericles's oration, or Socrates's gadfly rôle, and with these he hopes to undermine my position. The argument overshoots the mark without touching it.

As a matter of fact, it returns as a boomerang to Mr. Brinton himself. Following his argument, one might say that historians are not entitled to talk of Greek culture of the fifth century because throughout this century there was only one Socrates, one Athens, and one Pericles. Also, each of these changed during the century. Historians cannot then talk of "feudal" or "city-state" or "industrial" or any other *types* of societies. The logical continuation of this line of reasoning drives one into the blind alley and all of the absurdities of the unicist conception of historical processes (which conception I dispose of in Vol. I, Ch. iv of *Dynamics*). In so far as such a unicist conception is neither factually nor logically possible, and in so far as typologizing, conceptualizing, and generalizing methods have always been used, are used, and have to be used in social sciences, the unicist, naïvely perceptive, and singularistic-nominalistic argument of my critic—until he destroys the relevant similarities of my work and many other historical treatises—goes by the board. He has to negate a great deal from his own historical science before his contention can have any force; he has

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to destroy the value of any nomographic or semi-nomographic method of cognition, and must show that the similarities established are wrong. As this is not done, even is not attempted, the argument can be dismissed in spite of its real character.

Now let us turn to the second real issue. It is presented in several aspects. First, shall we study the repeated elements of human actions, such as the physiological functions and drives, reflexes and instincts, and Paretian residues, or shall we study the forms and dynamics of cultural phenomena, such as art, science, philosophy, and other compartments treated in my work? I do not see any controversy between the two. Those who want to study the drives, reflexes, residues, and what not, may study them; those who, like myself, want to study the forms and transformations of cultural phenomena are likewise free to do so. Both fields are so vast, so important, that I do not see any reason to discourage either kind of studies. On my part, as I clearly say in my work (v. I, p. 29), I am choosing the second field. That the field is of great pertinence for the social scientist, Mr. Brinton can hardly deny. His own historical work, as well as almost the whole discipline of history, deals mainly with various aspects and fragments of cultural phenomena.

The second aspect of his argument suggests that the "art, philosophy, and culture of society is not in itself an important factor of social change, does not count heavily among the variables which determine the conditions of a given society." The real forces are Paretian 'residues' while all these forms of culture are mere derivations: *plus ça change, plus c'est la même chose*.

The third aspect of it directly concerns me and consists, in Mr. Brinton's opinion, of my failure not to see the problem of the relationships between the residues and the forms of culture.

As to the second aspect, the whole setting of the problem by Mr. Brinton appears to me very uncertain and in need of much preliminary analysis before it can be intelligently answered. For instance, what does he imply by "factor," "variable," and force of change or "of determining the condition of a given society"? It is impossible for me to enter here into a clear analysis of this fundamental problem. I can say only that the whole setting of the problem along this line seems to be faulty.

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Mr. Brinton, together with the old-fashioned school in social science, seems to think that for any change of cultural form some "force" or "external agent" is necessary. I assumed the principle of immanent change of any system which functions and is a "going concern." Just because a running automobile, a living organism, and a socio-cultural system are "going concerns" they cannot help changing immanently, even if all the external conditions remain constant. The running engine sooner or later changes merely through its own running; the living organism passes immanently from immaturity to maturity because it is a going concern; the same is true of any socio-cultural system. In such an immanent setting, any question of some external agency that starts or pushes or pulls the change becomes superfluous. Likewise, the question of the "force" or "variable" that generates the change becomes superfluous also.

Certainly, the instincts or drives or residues or wishes do not help in explaining the change. If they are constant, if *c'est la même chose*, then whence this infinite variety of social and cultural transformations? Why does not the residue of the persistence of aggregates manifest itself all the time in the same derivations or the same cultural forms? If the residues or any similar "drives" change, they cease to be constant, fall into the category of the changeable phenomena, and therefore demand an explanation of their change. It is hardly necessary to say that the Paretians, not to mention the instinctivists, reflexologists, physiologists, or partisans of the "four constant wishes," or "six constant interests," have not been able to explain through their variables even a small fraction of the major social and cultural transformations.

Next, what is meant by "the conditions of society"? Shall we exclude from it the political, economic, and social forms? Shall we omit from "the conditions of society" its art, science, religion, law, courts, jails, policemen, wars and revolutions, conflicts and solidarities, contracts and compulsions? If we shall exclude all of these, what remains among the "conditions of society"? If we do not, then all of these phenomena are mainly those of cultural forms. In that case they become components of the "conditions of society." Then to say that these various forms are not in themselves an important factor of social change or do not determine "the condition of a given society" becomes senseless. Such reasoning would oppose one part

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of society to another or to the society as a whole. It makes the part of the whole (in which this part is included) a factor or something not belonging to this whole and to this part itself. In fact, it violates the law of identity as well as that of contradiction.

In addition, the forms of culture studied in my work in no way coincide with Paretian derivations. Neither painting, buildings, economic organization, the phenomena of law, nor the greater part of any of the forms of culture studied by me are similar to Pareto's derivations, and as such cannot be confronted with the residues. If Mr. Brinton does that seriously, he misuses Pareto's residues and derivations, as well as entirely misinterprets my forms of social and cultural phenomena. The boundary line between derivations and residues is on an entirely different level from the social and cultural forms with which I dealt. For these reasons the second aspect of Mr. Brinton's argument is awkwardly set, and misuses Pareto's theory as well as mine.

As to the third aspect, here Mr. Brinton's argument is perfectly baseless. I did not overlook the problem of relationship between the culture mentality and overt actions of the members (not the residues and forms of culture, a distorted setting, as I have shown above) of such a cultural and social system. Confronting my culture mentality concept with overt action instead of residues, I have all the advantages of clearness and objectivity of the overt actions over the somewhat indefinite and introspective entity of the residues. The last part of my third volume is devoted exactly to this problem. After a proper investigation of it, I precisely formulate conclusions: the connection between the main types of cultural mentality and the character of overt actions of the participants of such a culture is loose, but tangible. In the predominantly Ideational culture the frequency of the ideational type of men and ideational actions and forms of conduct is greater, and their ideational intensity is manifested more conspicuously than in a Sensate dominant culture. And the converse likewise holds for the Sensate culture.

It is quite incomprehensible how Mr. Brinton missed the whole part of my work devoted exactly to the problem which, according to his statement, I overlooked and failed to study. Only the hypothesis that my critic

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just glanced here and there through the work and did not read it even with a minimum of care can explain this objection.

The last half-real issue is raised by his concluding pages on which he ruminatingly ponders in criticizing the prophets of doom, how such a doom can come. I can pass his rumination with one remark. He seems also to have missed another elephant in my volumes, the previously mentioned principle of immanent change. If he did not miss it, no "inner barbarians, now yellow peril, no sun spots," nor any external factor would be necessary to understand the possibility, nay, the inevitability of decline in the present form of culture. For the same reason that Mr. Brinton or any of us cannot help becoming older as time goes on, regardless of external conditions, any form of culture cannot help changing and sooner or later giving dominance to another variety. These concluding pages of my critic illustrate especially clearly his careless reading of my work and his lack of understanding of the leading principles set forth. *Sapienti sat.*