

## EXCHANGE, POWER AND POLITICS (Part 1)

### I

My first main object in this article is to set out and illustrate the basic conception of "social behaviour as exchange" as it has been developed by George C. Homans in Social Behaviour: Its Elementary Forms (London, 1961) and Peter M. Blau in Exchange and Power in Social Life (New York, 1964). This is done in Section V. My second main object is to argue, with a fully worked-out example, that this conception may provide the intellectual unifying force which the exponents of numerous "approaches" to the study of politics" have all been seeking. This is taken up in Sections IX and X. My third main object is to examine with some care those parts of Blau's book which are of most direct relevance to the study of politics, and this critique will form the bulk of Part II of the article. This still leaves Sections II to IV and VI to VIII inclusive to be accounted for, and I can best do this by saying that they are devoted to describing, in an admittedly sketchy (perhaps even impressionistic) way, the intellectual setting within which the theoretical structure of "social behaviour as exchange" was developed and its relation to several other branches of social science.

### II

I can hardly imagine a more potent generator of apathy than the offer to a political scientist of yet another "approach to the study of politics". Since the early 'fifties "approaches" have come and

gone, sometimes spawning a book or two on the way, sometimes barely surviving their incarnation in print.<sup>1</sup>

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<sup>1</sup>The basic reference here is, of course, Roland Young (ed.), Approaches to the Study of Politics (Evanston, Ill., 1958, but based on papers presented in 1953-6). For another list of "approaches" see Vernon Van Dyke, Political Science: A Philosophical Analysis (Stanford Calif., 1960), Chapter \_\_\_ .

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If we are still, against the odds, to persevere, we must first know why these "approaches" failed. For the sake of brevity, I shall mention only the three which, by (I hope) common consent, at least got off the ground: the "behavioural approach", the "group approach" and the "structural-functional approach". We might suggest, without doing too much violence to the facts, that the "behavioural approach" was laid to rest by Robert Dahl in 1961<sup>1</sup>

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<sup>1</sup>"The Behavioral Approach in Political Science", American Political Science Review, LV (1961). Reprinted in Nelson W. Polsby, Robert A. Dentler and Paul A. Smith (eds.), Politics and Social Life (Boston, 1963).

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and the "group approach" seems to have perished in a mass denunciation in the American Political Science Review for 1960.<sup>1</sup>

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<sup>1</sup>See especially Stanley Rothman, "Systematic Political Theory...." APSR, LIV (1960).

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The final case, that of "structural-functional approach" is more interesting. Judging from some prominent books,<sup>1</sup>

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<sup>1</sup>Cf. the Introductions to: G.A. Almond and J.S. Coleman (eds.), The Politics of Developing Areas (Princeton, N.J., 1960); D. Apter and

H. Eckstein (eds.), Comparative Politics (Glencoe, Ill., 1963); S.H. Beer and A. Ulam (eds.), Patterns of Government (2nd Ed., New York 1962); R.C. Macridis and R.E. Ward (eds.), Modern Political Systems (2 vols., Englewood Cliffs, N.J., 1963). Significantly, the rest of the contents of these books tends not to strengthen the claims made in their Introductions.

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one might gather that all the most up-to-date people are "structural-functionalists" and that in a few years the "structural-functional" approach must succeed in sweeping all before it. The truth is, I think, rather most complicated. A large part of the answer lies in the fact that the "structural-functional" doctrine (like many others before it) has become diluted as it has spread. Robert Merton's influential essay<sup>1</sup>

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<sup>1</sup>In Social Theory and Social Structure (2nd Ed., Glencoe, Ill., 1957).

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which threw out most of the characteristics of Radcliffe-Brown's functionalism as inessential to this mode of analysis, helped the process along greatly; and, today, support for the "structural-functional" approach is sometimes equivalent to believing that one should study the consequences of one institution on more inclusive ones (hardly a distinctive mode of enquiry) and sometimes something even less tangible e.g. that one should talk of processes rather than things. The rest of the answer seems to me to be that where there is more to structural-functional analysis than this, it lacks any power to explain why things happen the way they do.<sup>1</sup>

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<sup>1</sup>This criticism has often been made in recent years. The best discussion is in my view Alvin Gouldner, "Reciprocity and Autonomy in

Functional Theory" in Llewellen Gross, Symposium in Sociological Theory (New York, 1959).

Thus, to take a fairly recent example, Gabriel Almond's "capabilities model"<sup>1</sup>

<sup>1</sup>"A Developmental Approach to Political Systems", World Politics Vol. 17, No. 2 (Jan '65), 183-215.

(a direct descendent of "functional requisite" analysis) is constantly referred to as a "coding scheme" which would enable different countries to be ranked along various continua. As he himself says, "an analysis of the capabilities of a political system does not tell us what factors affect political change or development...", and it does not appear that any further development of the same kind of analysis would do this.<sup>1</sup>

<sup>1</sup>Cf. David Easton, A Framework for Political Analysis (Englewood Cliffs, N.J., 1965), p. 89: "My approach to the analysis of political systems will not help us to understand why any specific policies are adopted by the politically relevant members in a system."

But in that case the whole enterprise seems rather pointless: if the explanation of "change or development" (and, by the same token, of their absence) lies elsewhere then refining the so-called "capabilities model" becomes a quite self-contained activity, rather like manufacturing ever-improved Monopoly-money for a game that will never be played.

What the "approaches" of the early 'fifties had in common was that they were all attempts to give the academic study of politics a certain intellectual unity by raising it above the level of common-sense description.<sup>1</sup>

<sup>1</sup>Easton's eight-point "behavioralist" credo in A Framework (p. 7)

consists entirely in the rejection of description as the be-all and end-all of the study of politics and in the demand for testable generalizations.

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They failed because they were either so vague as to leave things where they were before or so constricting as to mutilate the phenomena outside a favoured area.<sup>1</sup>

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<sup>1</sup>The "behavioral" approach falls under the former head, the "group" approach under the latter; the "structural-functional" approach as befits one so protean, falls partly under each.

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But the failure of the attempts should not derogate from the worthiness of the aim underlying them; and I want to suggest that it may now be possible to realize the aim by building on the idea of social behaviour as an exchange of values between the parties. This is in its essential an old and familiar notion, but its development into a systematic theory *related to the theory of games and to small group research* owing a lot to ~~economics and social psychology~~ dates from just the last few years.

III

In the frantic search for "approaches" almost every discipline from anthropology to zoology was ransacked; but little attention was given to ~~two quite close to home, economics and social psychology~~ *either of these studies mentioned above*. Nor is this altogether to be wondered at, if we consider what they had to offer <sup>only</sup> ten years ago. With the advantage of hindsight one might guess that ~~the extension of economics known as~~ the theory of games should have been spotted as hopeful, but this is not really so. Until Thomas Schelling's pathbreaking "Essay on Bargaining"<sup>1</sup>

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<sup>1</sup>The American Economic Review, XLVI No. 3 (1956). Reprinted in The Strategy of Conflict (Cambridge, Mass., 1960).

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there was little development of the theory of non-zero-sum games, yet most of political life refuses to fit the assumption of diametrically opposed interests among actors. Fundamentally, Schelling's innovation was to break the process of negotiation down into a sequence of moves, rather than trying to deduce an outcome directly from the initial conditions. Prior to this it could be said that certain conceivable solutions were ruled out, but this still left a range of equally possible

solutions. Which of these was the one to be actually arrived at was said to be determined by the relative "bargaining skill" of the parties -- a phrase which, unlike the Australian lady's fancy dress, concealed what it should have revealed. Ordinary economics had, of course, encountered what were in fact non-zero-sum games, for example in the analysis of "bilateral monopoly" (a single seller confronting a single buyer)<sup>1</sup>

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<sup>1</sup>Another obvious example is the situation of several firms selling (partially or completely) substitutable goods: by agreeing (explicitly or tacitly) on keeping the price up they can all benefit relatively to a price war; but each has a short-run interest in undercutting the rest. Analyses of this phenomenon prior to Martin Shubik's Strategy and Market Structure (New York, 1959), which makes use of game theory, rested on such narrow assumptions about the behaviour of firms as to have little general significance.

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but again the analysis showed only the limits within which the result must fall given that the parties would always reject a position which they could both simultaneously improve upon.<sup>1</sup>

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<sup>1</sup>My saying this must necessarily involve the rejection of the claim put forward in a number of articles by John C. Harsanyi to the effect that a satisfactory determinate solution to any bargaining situation was provided by the economist Frederik Zeuthen in a book published in 1930. (The most "popular" treatment is "Models for the Analysis of the Balance of Power in Society", pp. 442-463 of Nagel, Suppes and Tarski (eds.), Logic, Methodology and Philosophy of Science (Stanford, 1960)). The most elaborate, which includes a critique of Schelling, is "On the Rationality Postulates underlying the Theory of Co-operative Games", Journal of Conflict Resolution, V No. 2 (1961), 179-196. As set out by Harsanyi, the theory is quite simple: roughly, each party calculates at every stage in the negotiations how much worse it would be to have no agreement than to make a concession; and the party which would lose comparatively more from the breakdown of the negotiations should then make the next concession. Now although this procedure does not require "interpersonal comparison of utility" in the strict sense that would allow one to say without qualification

that A gets more enjoyment from x than B does from y, it does presuppose (as Harsanyi admits) that each party knows the utility function of the other, as well of course as knowing its own: that is to say, each must know the two scales of preference, even though one scale cannot be brought in direct relation with the other. Unless this is so, the comparison which is required at each stage between the reluctance of one side to see the negotiation fail as against making a concession and that of the other side cannot even begin to be made. But this presupposition makes the whole thing silly, for it actually rules out the possibility of anything recognizable as negotiation; the parties could simply take out their pencils and say "Let us calculate". The whole art of negotiation, as Fred Charles Iklé shows in his How Nations Negotiate (New York, 1964) is to manipulate or misrepresent one's utility function (and for maximum plausibility not to be too sure exactly where manipulation ends and misrepresentation starts) in such a way as to make it appear to the other side that they had better make most of the concessions. A party to negotiations rarely knows how important it is to the other side to secure agreement on one set of terms rather than another as against not getting agreement at all; and, as Iklé says, it often doesn't even have this information about itself!

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It is true that economists, game theorists, and even a philosopher (Professor Braithwaite) had by the mid-fifties interested themselves in the so-called "problem of fair division". In its simplest form, this may be stated as follows: if two people have to agree on the division of some money between them before either gets any, what is the "rational" or "fair" solution? This has the characteristic non-zero-sum game feature that in some outcomes both players lose relatively to what they could get in others; but the solutions, though ingenious, were hardly satisfactory. Although there were variants (cf. R. Duncan Luce and Howard Raiffa, Games and Decisions (New York, 1957), Chapter 6) the solutions all rested on seeing how much damage the players could inflict on one another by nonco-operation or bloody-mindedness and then taking this as the distributive benchmark in moving them both to a more favourable position, on the "efficient line." Such "solutions" were neither "fair" in the ethical sense (as John Rawls pointed out) nor



were they particularly likely to come about as a result of the manoeuvres of rational egoists (as Schelling pointed out). Rather, they were a sort of hybrid.

#### IV

Among sociologists and social psychologists the desire for definite, reproducible results had led by the mid-fifties to the accumulation of a formidable amount of experimental data on the behaviour of "small groups", in addition to studies of groups not specially set up for the purpose of being studied.<sup>1</sup>

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<sup>1</sup>The two best-known of the latter kind are the Hawthorne plant studies and Whyte's Street Corner Society (2nd. Ed., Chicago, 1955). In the later phase which I shall be coming on to subsequently, Blau's The Dynamics of Bureaucracy (Chicago, 1955) is of prime importance.

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But again, I do not think the seekers for "approaches to the study of politics" can be altogether blamed for overlooking this field, given its state of development at the time. Anyone reading a collection of "small group" literature up to 1954<sup>1</sup>

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<sup>1</sup>A.P. Hare, E.F. Borgatta and R.F. Bales (eds.), Small Groups (New York, 1955)

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with an eye to using its results in political analysis might be forgiven for concluding that, fascinating as much of it was, it did not seem possible to get from the numerous experimental reports a general set of ideas that could be taken away and applied elsewhere.<sup>1</sup>

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<sup>1</sup>This feeling of mental indigestion can be induced very effectively by reading through the 584 item annotated bibliography of the field in Hare, Borgatta and Bales (pp. 579-661).

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elsewhere.<sup>1</sup>

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<sup>1</sup>Such a conclusion may be unjust, but it is certainly intelligible. A deeper appraisal of the situation would probably be that there were plenty of theories, each of which offered an explanation of only one part of the data.

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In 1961 there appeared a book entitled Small Groups and Political Behaviour by Sidney Verba, which might well reinforce a cautious attitude rather than convert it into an enthusiastic one. This is not to say that Verba fails to make out his case in this impressively-documented work; but his case is the fairly modest one that "small group" studies have some relevance to Politics. Almost half the book is taken up with pointing out the relevance of "small groups" of all kinds to the working of any political system, and then arguing that the special features of experimental work on "small groups" do not preclude them from being a "valuable adjunct" to political research. The rest of the book, which takes up some of the findings of "small group" experiments on leadership, does, it seems to me, bear out the claim that this material has some relevance to politics, but not a very far-reaching relevance.<sup>1</sup>

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<sup>1</sup>One indication that Verba himself may have reached this conclusion is the lack of direct reference to "small group" studies in his later work, such as Gabriel A. Almond and Sidney Verba, The Civic Culture (Princeton, 1963).

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Two main topics are pursued, aptly summarized in the chapter headings as "Leadership: Affective and Instrumental" and "The Participation Hypothesis". The latter contains some shrewd and damaging observations on the distance between the manipulative so-called "democracy" of Kurt

Lewin's disciples and anything recognizable as such in national politics<sup>1</sup>

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<sup>1</sup>The best example of what Verba is criticizing, though it came out too late for inclusion in his book, is probably Ralph K. White and Ronald Lippitt, Autocracy and Democracy (New York, 1960).

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and thus almost constitutes a case study in how not to draw political conclusions from small-group studies. In the former part there do occur statements about politics, such as "Political systems must provide their members with both types of outputs, the instrumental and the affective." But this proposition is not derivable from the "small group" data presented, about the bifurcation of the leadership role into affective and instrumental components, and so on. Nor is the truth of the proposition (if it is true) rendered more explicable by these data, which simply allow us to conclude that the same necessity is found in certain experimental small groups.<sup>-1</sup>

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<sup>1</sup>The distinction is, after all, one of the main conceptual tools used by S.M. Lipset in Political Man ( , 1959) where it is introduced in Chapter III quite out of the blue.

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*One might*  
~~It should be willing to~~ accept Verba's claim that the "small group" findings are suggestive of questions to be asked in politics, *and still reflect that* but this is, after all, a very limited claim to make.<sup>1</sup>

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<sup>1</sup>Talcott Parsons' well-known "AGIL" model of the functional requisites for any viable social system was arrived at by reflection on some of the "small group" results referred to by Verba in this part, and the same comment seems to me to apply. Assuming that the proposition about social systems "requiring" such-and-such is an empirical

one rather than a tautology, its truth for politically-organized societies could be established only by looking at them, not by appealing to "small group" analogies.

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In Small Groups and Political Behavior Verba is seeking conclusions about leadership in small experimental groups which are to be directly relevant to the study of political behaviour. But the main significance of "small group" findings for politics lies in their important role in the development of the general theory of social behaviour as exchange—or that at any rate, is the view for which I am contending here.

V

The phrase "social behaviour as exchange" was put forward by George Homans as a label for a distinctive mode of analysis in an article published in 1958.<sup>1</sup>

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<sup>1</sup>"Social Behavior as Exchange", American Journal of Sociology LXIII (1958), reprinted in Sentiments and Activities (London, 1962).

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His own Social Behaviour and Blau's Exchange and Power both rest on this idea, which I shall briefly set out and illustrate in this section. According to Homans, in the article just mentioned, "Social behavior is an exchange of goods, material goods but also non-material ones, such as the symbols of approval or prestige." (Homans, Sentiments and Activities, p. 292). This conception is plainly a generalization from narrowly economic exchange, and propositions worked out in economics about the conditions and terms of exchange find their analogues here. After listing some, Homans concludes: "It is surprising how familiar these propositions are; it is surprising too, how propositions about the dynamics of exchange can begin to generate the static thing

we call 'group structure'...." (ibid., pp. 292-3).

Rather than spell out further in abstract terms what "social behaviour" and "exchange" mean here,<sup>1</sup>

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<sup>1</sup>I shall attempt this in Part 2.

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let me illustrate the conception in practice by giving a stock example of both Blau and Homans: the exchange of esteem and advice in a bureaucracy as described in Blau's earlier book The Dynamics of Bureaucracy.<sup>1</sup>

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<sup>1</sup>Homans uses this as a running example in developing his basic ideas in Social Behaviour (Chapters Two and Three) and then towards the end devotes a whole chapter (Seventeen) to a restatement of it in terms of his own developed theory. Blau, in Exchange and Power, not surprisingly refers to it at a great number of points and chooses it as his example when he wants to make a full-dress application of traditional economic theory to social exchange (Chapter Seven).

In studying an agency of the U.S. federal government concerned with the enforcement of a certain law, Blau found that although the agents were supposed to work independently except for consulting their common supervisor, a great deal of consultation in fact went on between agents.

A minority of agents were accepted by everyone as especially competent and most were willing to give advice to the rest. In "exchange" terms

a consultation can be considered an exchange of values: both participants gain something, and both have to pay a price. The questioning agent is enabled to perform better than he could otherwise have done, without exposing his difficulties to the supervisor. By asking for advice, he implicitly pays his respect to the superior proficiency of his colleague. This acknowledgement of inferiority is the cost of receiving assistance. The consultant gains prestige, in return for which he is willing to devote some time to the consultation and permit it to disrupt his own work." (Blau, ibid., p. 108)

But as well as a pattern of less-proficient agents consulting more-proficient agents, Blau also found a pattern of mutual consultation in pairs or trios among the less-competent agents. This can be explained within the "exchange" framework in two stages. First, more-proficient agents found the costs of being consulted soon outweighed the gains, while less-proficient agents found the converse. On the one side "the value of any one of very many consultations became deflated for experts, and the price they paid in frequent interruptions became inflated." (ibid., p. 108) On the other side, "the cost became

prohibitive, if the consultant, after the questioner had subordinated himself by asking for help, was in the least discouraging...." (ibid. p. 109) As a result of these forces operating on the supply of and demand for expert advice, the level of consultation by less-proficient of more-proficient agents stabilized at a level which left a large unfulfilled demand for advice among the less-proficient agents. It was to meet this demand (this is stage two of the explanation) that the less-proficient agents formed consulting partnerships among themselves. In "exchange" terms, they used expert advice only when there was no substitute for it, and for the rest contented themselves with a lower grade of advice which, however, had a fairly negligible cost to them (negligible because, coming from an agent of similar proficiency, it could easily be returned in kind). As one agent said, "I ask the ones I know well, because I don't feel any reluctance about asking them." (ibid., p. 109)

## VI

Whatever its validity as a method of scientific analysis may be, the idea of social exchange is surely well established in common-sense explanations of behaviour. "Men have always explained their behavior by pointing to what it gets them and what it costs them. That mine is an explanation of the same sort I claim as one of its positive advantages." (Homans, Social Behaviour, p. 13) A more significant point perhaps is that it is also already well established, in an informal way, in writings about politics. Homans can say with some justice that sociology "has ended by painting a picture of man that men

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cannot recognize" and add that "of all our many 'approaches' to social behavior, the one that sees it as an economy is most neglected, though it is the one we use every moment of our lives--except when we write sociology." (Social Behaviour, pp. 13-14) The same charge certainly will not stick in the case of politics, where (leaving aside certain recent "structural-functional" aberrations) the basic vocabulary has been one of deals, offers, bargains, interests, power and similar concepts which fit naturally into an ~~exchange~~ <sup>of rewards and costs,</sup> framework. ~~The role of a theory of social exchange would be to put this mode of analysis on a more systematic basis.~~ <sup>47a</sup> It is worth noticing, as well, that the two "approaches" to politics produced from within the discipline, the "behavioural" and "group" ones, seem to fit comfortably into an ~~exchange~~ <sup>this</sup> context. If we take the "behavioural" approach as demanding that in addition to describing institutions political scientists should try to explain them, ~~a~~ theory of elementary social behaviour lies at the heart of the programme; and the "group" approach, which obviously has great merits but was weakened by being over-extended, should be better off as an important special case of interlocking social behaviour than it ever was as a would-be ground-level conceptual framework for the whole of politics.

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Even the "structural-functional" approach, purged of organicism and reduced to the basic idea of a "system" is compatible with analysis in terms of social exchange; indeed I suspect that it can have no content without such analysis. Suppose we say that our "major and gross unit of analysis will be the political system" and define a political system as "a set of interactions, abstracted from the total-



ity of social behavior, through which values are authoritatively allocated for a society."<sup>1</sup>

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<sup>1</sup>Easton, pp. 23 and 57.

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Surely if we want to trace out causal processes among this particular set of interactions--and I would take this to be what the study of politics is about--we need a theory of social interaction in general, and this may be found in the theory of social behaviour as exchange. It is a fallacy to think that taking "the political system" as one's "major and gross unit of analysis" commits one to seeking generalizations at the system level. An economist, for example, might (in an expansive mood) say that he takes "the economic system" as his "major and gross unit of analysis", but he would hardly suppose that this committed his entire working life to looking for common factors among every economic system there has ever been, from the Ancient Britons and the Bergdama to the Soviet Union and the USA. Rather he would take this as meaning that, for any economy, he would try to show how the observed levels of money income, physical production etc., were arrived at (given certain tastes, technology, etc.) as the end result of millions of exchanges between economic actors, each exchange having ramifying effects on the terms of others. <sup>[16a]</sup> In much the same way, it seems to me plausible to suggest that taking "the political system" as our "major and gross unit of analysis" should commit us only (!) to an attempt at deriving the pattern of authoritative decisions in any given society from the conditions and processes of social exchange

Unhappily, Easton himself regards "system" analysis as entailing the search for uniformities among systems, and concludes that all one can ask is how they survive: "the primary goal of political analysis is to understand how political systems manage to persist through time" (p. 55); "it is the task of a behavioral science of politics to put kinds of questions that reveal the way in which the life processes or defining functions of political systems are protected." (p. 78) This is akin to saying that the central concern of economics should be, not how any particular economic system works, but how it comes about that any goods are <sup>ever</sup> produced and consumed at all.<sup>1</sup>

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<sup>1</sup>Oddly enough, Easton insists on asking the usual "functionalist" question but emphatically denies that the usual sort of "functionalist" answers can be given: "postulating functional requirements" is "at best theoretically trivial". (p. 105) I suspect this to be true, but if it is then this apparently means that no empirical generalizations are possible about the conditions which must be met by a political system in order to "persist". One cannot consistently assert (a) that "it is the task of a behavioral science of politics" to ask "how political systems...persist" (pp. 55, 78), (b) that no testable generalizations about (a) are possible (p. 105) and (c) that the "behavioral approach" entails a search for testable generalizations (pp. 6,7)

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## VII

In his earlier book, The Human Group (London, 1951) Homans wrote, "The elements of a synthesis are on hand. We shall only put together ideas that have been lying around for some time in the literature of the social sciences...." (p. 4). The same thing might very well be said of the fundamental notions of "social behaviour as exchange".

~~One of these sources is social psychology, which I mentioned~~

Thus, consider "small group" research again. ~~earlier with economics as a field comparatively neglected in the search for "approaches" to politics.~~ At the simplest level we can point out that experimental studies of groups underlie most of the propositions of the theory of social exchange as it has been developed so far. It is very interesting to notice a common body of research material being referred to in the footnotes of <sup>also in the cognate study by</sup> Blau, Homans and ~~the~~ Thibaut and Kelley, <sup>De</sup> Social Psychology of Groups <sup>(New York, 1959).</sup> But there is more to it than the raw data of experimental reports. Much of this research was carried out under the influence of Kurt Lewin's "field theory",<sup>1</sup>

<sup>1</sup>See for a Lewinian orientation Group Dynamics: Research and Theory edited by Darwin Cartwright and Alvin Zander (2nd Ed., London, 1960).

~~and, though this Lewinian terminology of "valences" etc. so on represent imitating the substance of a fashionable physical science, talk about "valences" in psychology, the wrong sort of abstraction, it is not in the end all that far from talk of exchanging benefits or threatening unwanted outcomes.~~ <sup>topological vocabulary seems forced,</sup> and so on is not in the end all that far from talk of exchanging benefits or threatening unwanted outcomes.<sup>1</sup>

<sup>1</sup>Thus, for example, Robert Lane used the terminology of "valences" in Political Life (Glencoe, Ill., 1959) to talk about the conditions under which a non-political group (e.g. a veterans' organization) carries its members with it if it adopts a political stand. "The first consideration, will be the valence of the group for the individuals. How important is it for them, how urgent are the needs the group satisfies and what alternatives are available?" (p. 191; italics in original). In reward-cost terms: conformity in attitude is a reward to others (though a low-grade one because, as Homans says "any fool can conform"); an individual who fails to provide this reward will lose esteem in the eyes of the others. The possible cost of conformity is a loss of integrity (sticking up for one's own opinions); whether this is greater than the cost created by losing esteem through non-conformity depends how rewarding the person finds his present position in the group and how easily he can replace this source of reward with others. But the "social exchange" framework enables us to make many

other, intelligibly connected, predictions on the same subject, many of which are stated in Chapters Five ("Influence") and Sixteen ("Status, Conformity and Innovation") of Social Behaviour.

*the theory of*  
~~Another source of "social behaviour as exchange" which is not based~~  
~~at the risk of being boring I should also mention as an intellectual source "interactionism", loosely understood. (Those who attach~~  
*acted as a spur to small group research was*  
~~importance to the question might prefer to call this Sociology.)~~

"Interactionism" in the strict sense is the doctrine formulated by Eliot Chapple and Conrad M. Arensberg that only observations of frequency, duration and initiation of interaction of individuals were to be admitted as data.<sup>1</sup>

<sup>1</sup>For tributes to the seminal influence of this idea see Whyte, Street Corner Society, pp. 286-7 and Homans, Sentiments and Activities pp. 37-8.

In a loose sense it includes modifications such as that represented by Homans' The Human Group, which proposed the now well-known basic scheme of interactions, activities and sentiments.<sup>1</sup>

<sup>1</sup>William Foote Whyte, one of the few people actually to go around calling himself an "interactionist", has endorsed the threefold scheme and defended against Chapple the inclusion of sentiments as a separate element, in "An Interaction Approach to the Theory of Organization" in Mason Haire (ed.), Modern Organization Theory (New York, 1959).

Once it is established that there are systematic connections in groups among these three elements (as it was in The Human Group) the development of ideas about social exchange to explain these connections is a natural step. In fact one might well say that the most significant difference between The Human Group and Social Behaviour is not that one is centred on interaction and the other on exchange, but (as the

titles themselves suggest) that one is about groups and the other about social behaviour.<sup>1</sup>

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<sup>1</sup>The Human Group seems to have had very little direct impact on the study of politics in spite of its wide circulation. This could be explained by intellectual conservatism within the profession, but at the same time the propositions are not stated in a form easily assimilable for application to things other than "small groups". The point made in connection with Verba's Small Groups and Political Behavior seems to apply here too: it is not so much information about small groups that is important as information obtained from small groups. Information about social behaviour, on the other hand, should (with appropriate additional premises) explain what happens both in small groups and anywhere else.

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### VIII

~~Turning now to the second of the two disciplines whose wider relevance I mentioned earlier as having been underrated, we can safely say that~~ The convergence between economics and the idea of social exchange is quite obvious. Blau and Homans both make a point of acknowledging it and I shall not labour it any further. However, both of them restrict their favourable comments to orthodox economic analysis such as Boulding's Economic Analysis (3rd. Ed., New York, 1955), a work to which Blau repeatedly refers; and both writers explicitly aim brick-bats at the theory of games. These criticisms seem to me to rest on something of a misapprehension of exactly what game theory in a general sense involves; and it seems to me that if they come to represent the received doctrine among "social exchange" theorists the result can only be to hamper future developments.

Both writers take exception to the degree of "rationality" which, they allege, game theorists impute to actors. According to Homans,

the theory of games is concerned with telling people how to maximize value (in terms of their own tastes) in the long run. But since "calculation for the long run is the exception and not the rule", Homans concludes that "the Theory of Games is good advice for human behavior but a poor description of it." (Social Behaviour, p. 81).

Blau gives a more detailed indictment.

What is explicitly not assumed here is that men have complete information, that they have no social commitments restricting their alternatives, that their preferences are entirely consistent or remain constant, or that they pursue one specific ultimate goal to the exclusion of all others. These more restrictive assumptions, which are not made in the present analysis, characterize rationalistic models of human conduct, such as that of game theory. Of particular importance is the fact that men strive to achieve diverse objectives. The statement that men select the most preferred among available alternatives does not imply that they always choose the one that yields them the greatest material profit. (Blau, pp. 18-19)

As criticisms of the theory of games these comments are about as wide of the mark as would be a criticism of "economics" which asserted that it was a subject entirely devoted to the study of perfect competition. Often the best way of opening up a line of analysis is to make extremely restrictive assumptions to begin with and relax them in a more realistic direction later.<sup>1</sup>

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<sup>1</sup>Anthony Downs' An Economic Theory of Democracy (New York, 1957) is, whatever one may think of its premises and conclusions, a beautifully organized exemplification of this procedure.

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Any lingering reluctance to accept this among practitioners of sociology and politics can only delay the arrival of these disciplines at the maturity of economics even longer than the recalcitrant nature of their subject-matter dictates. But to equate the theory of games with

all the ad hoc restrictive assumptions that have ever been made in it is simple fallacy.

Take first the "specific ultimate goal" idea to which Blau goes on to attribute "particular importance". Much game theory is worked out in terms of "utility", which is not a specific goal but a way of talking about an actor's preferences between outcomes, however those preferences may be determined. The assumption that people choose to do the thing they rank higher is identical with Blau's own assumption that "human beings choose between alternative potential associates or courses of action by evaluating the experiences or expected experiences with each in terms of a preference ranking and then selecting the best alternative."<sup>1</sup> (Ibid., p. 18)

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<sup>1</sup>Admittedly, Blau's formulation would give only ordinal utility, whereas some parts of game theory need cardinal utility--that is, the scaling of preference by collecting preferences between hypothetical lotteries. But it is easy to show that this sort of concept cannot be avoided once one starts dealing with probable outcomes rather than certain ones. Blau's ability to get by without it reveals not superior parsimoniousness of assumptions but adherence to the very primitive and restrictive assumption that nobody is ever in doubt.

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Game theorists do indeed often work in terms of money rather than utility, because it is more familiar and more directly operational. But this does not entail that money is everything or even that every value can be expressed in money terms, though the latter is<sup>a</sup> very handy assumption, as "benefit-cost" analysis shows.

The two opening counts of Blau's indictment have little more substance. It is true that the theory of games was first worked out for complete information, because this simplifies matters, but this

assumption is not inherent in the theory itself.<sup>1</sup>

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<sup>1</sup>"Complete information refers to the rules of the game. It implies that all payoff values are known." (Shubik, *op. cit.*, p. 5). "Complete" information is distinguishable from "perfect" information, which has never been assumed for all games. "The knowledge that an opponent will win if he has a royal flush constitutes part of the complete information concerning the rules of the game of poker. Whether or not he has the royal flush depends on the deal, and, since this is usually done face down, the individual player is incompletely informed about the actual cards dealt to the others." (*ibid.*, pp. 506). Shubik points out that most orthodox economics (of the kind utilized by Blau) has been based on very strong and unrealistic assumptions about information. See Shubik (*ibid.*, pp. 165-7).

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And if the actors have "social commitments restricting their alternatives" these can be represented in the utilities they assign to various outcomes. (How would Blau conceptualize such a situation, anyway, except by saying that due to "social commitments" some alternatives were ranked very low?

The only other point that Blau makes is certainly relevant to some degree but applies to exactly the same degree to his own assumptions too. It is not true that preferences must be "entirely consistent" or "remain constant" for the theory of games to apply. What is true is that the less consistent they are the less it applies, and that large random changes of tastes would be serious.<sup>1</sup>

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<sup>1</sup>One could, if it were important enough to be worthwhile, ~~quite easily~~ make the utility functions of players in one game depend in some systematic way on the outcome of the previous game.

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But aren't Blau's "preference rankings" just as vulnerable? If I am right in saying that they are the game theorists' "utilities" minus the name, they must be. If someone prefers A to B, B to C and C to A,



or prefers A to B one minute and B to A the next, there are strict limits on what you can say about him, whatever your technical apparatus and the more you try to say about him then, naturally, the more trouble you will be in. Finally, Homans' charge that the theory of games is built on an assumption of long-run maximization again rests on the confusion of an expository convenience with the nature of the theory. We must have some assumption (for some purposes at least) about the way people choose among benefits over time, but we can make their time-horizon as short as you like: they can discount the future at such a rate that no prospect more than a week off will modify their behaviour now, if that's the way you think they work,

Game theory is the study of situations <sup>behaviour in</sup> ~~in which two or more people~~ <sup>where someone believes that the</sup> ~~pay-off to him produced by his behaviour depends on what he himself does but~~ ~~find that the pay-off of each depends upon not only his own behaviour~~ ~~but that of the other(s) as well.~~ <sup>what some other person or persons do to</sup>

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<sup>1</sup>Cf. Thomas C. Schelling, The Strategy of Conflict (Cambridge, Mass., 1950), pp. 86-7 and, for other quotations, footnote 3 on page 86.

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But this seems to me an equally apt characterization of social behaviour.<sup>1</sup>

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<sup>1</sup>I shall try to justify this in Section XIII (in Part 2).

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The notion (touched on by Homans) that game theory is inherently different because it is normative is in my view a red herring. One can say either "If you want to be a person of such-and-such a kind you ought to do so-and-so," or "If you were a person of such-and-such a kind

you would do so-and-so." The same deduction of conclusions from postulates can be used to yield either statement. If game theory has tended to use the first form and "social exchange" the second this is an accident of history and not a necessity of logic. Similarly, game theory began as a mathematician's toy and has since tended to choose rigour at the price of abstraction, whereas "social exchange" developed within sociology. But these historical differences should not be allowed to obscure the fact that they both cover the same phenomena from roughly the same angle, and that there is no good reason for refusing to regard them as parts (at differing levels of generality) of a single body of theory.

#### IX

I have been doing my best in a brief compass to lend some plausibility to the notion that "social exchange" represents a basis for politics that would bring it into a closer relationship with the other social sciences. Of course, I still have not said much to suggest that it is a useful basis for politics, and this is in fact more a matter for evaluation after a few decades of experience than something subject to demonstration in a few thousand well chosen words. So far the only evidence we have is Blau's attempt to apply the theory of social exchange to certain features of institutions in Exchange and Power, and neither success nor failure in this effort could reasonably be taken as conclusive either way.

But how important is it, anyway, whether the social sciences have a common form of basic explanation or not? For that matter, is it

anything more than a questionable aesthetic preference to hope for a common pattern in analyses of politics? Some people seem to take pride in pointing to the variety of "approaches" as if it were a strength rather than a weakness. Isn't this the sensible, the liberal, attitude to take?

I think that in answering this question one has to be careful to distinguish between the division of intellectual labour and other less desirable phenomena. Nobody, I take it, would say that four men who had devoted themselves to studying, respectively, Parliament, Congress, the Dail, and the Knesset, had (simply in virtue of that fact) different "approaches to the study of politics". Nor would one say that they had different "approaches" from a man whose study was "legislatures". Differences in subject-matter and in levels of generality must necessarily involve the development of different concepts and different theories but the point is that these can all, in principle, be compatible with one another. The trouble with the usual lists of "approaches" is that many of them seem to be conceived of as rival ways of talking about the same subjects at the same level(s) of generality; this must surely be at best uneconomical and at worst self-defeating, if the object is taken to be a cumulative discipline.

At the risk of getting drawn into the aridities of the Higher Methodology, one might refer to the physical sciences for an example. The characteristic concepts used to talk about atoms are different from those used to talk about galaxies, but this is all right because it is possible to get from the one to the other.<sup>1</sup>

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<sup>1</sup>The Parsonian search for "systems" about all of which the same kind of thing could be said would correspond to physical scientists

who thought the same concepts should cover atoms and galaxies. Structural analogies are obviously a useful bonus if they occur, but they are by no means the key to the enterprise. Cf. May Brodbeck's hard-headed treatment of "models" in Llewellyn Gross (ed.), Symposium in Sociological Theory.

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Not identity of concepts but interdefinability is the important thing. No fiat can impose a greater uniformity on politics or the whole range of social sciences than their rambling subject-matter allows, but it would be far easier to see connections if everything were built up from a common basis such as social exchange. And, as the example of the physical sciences makes clear, uniformity in fundamentals is not an enemy of specialization but the condition of it. At present, every practicing social scientist has to be something of a Jack-of-all-trades because it is so difficult to dig out the relevant bits of other disciplines, with their foreign (and perhaps contradictory) theoretical under-pinnings. In particular, politics, economics and sociology have each developed their own ad hoc social psychologies and this seems to be the place to begin.

#### X

American studies of voting intention and political participation provide an illustration of the contribution that a general theory of social behaviour might be able to make to politics. They illustrate well what R. Duncan Luce has called "the dilemma of the social sciences" which, he says

is not, as some seem to think, a paucity of statistically significant correlations, but, as a glance at the journals shows, an overabundance of moderate significant ones lacking acceptable causal explanations and thus failing to suggest which relation-

ships are basic.<sup>1</sup>

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<sup>1</sup>"Analysing the Social Process Underlying Group Voting Patterns" Chapter 18 of E. Burdick and A.J. Brodbeck (eds.), American Voting Behavior (New York, 1959).

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If we start from what we believe to be the laws of elementary social behaviour, we can suggest hypotheses which would explain why the facts are what they are. Take, for instance, the finding of Voting that "within broad strata, opinion leaders are slightly higher in occupational and educational status than others."<sup>1</sup>

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<sup>1</sup>B.R. Berelson, P.F. Lazarsfeld, W.N. McPhee, Voting (Chicago, 1954), p. 117. For the evidence see pages 110-113.

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Why should this be? If we turn to Chapter Fourteen of Social Behaviour on "Authority", we find various propositions about the ability to exert above-average amounts of influence over other people which seem to have potential relevance. (Homans calls above-average influence over others "authority", without requiring any idea of "legitimacy" to be present.)<sup>1</sup>

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"Let us then define authority as follows: the larger the number of other members a single member is regularly able to influence, the higher is his authority in the group. The man with highest authority we shall call the leader." Social Behaviour, p. 286.

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Homans argues that "the higher is a man's esteem, the higher is his authority, when that authority is earned rather than acquired by appointment or inheritance." (p. 283). This is because both esteem and authority rest upon a past record of behaving in ways that other people find rewarding. Each time someone makes a suggestion to others who follow it and are gratified by the results of so doing he increases

the respect in which they hold him and also the probability that they will follow the next suggestion (advice, order etc.) that he makes. "Every time men act they change the probabilities of their future actions." (p. 285) Not only the probability of compliance within a certain range of affairs increases, but very often also the range itself increases, as the leader notches up successes. "The more often he rewards them, and the wider the range within which he does so, the more regular his authority over them and the wider its range." (P. 287)

If we are to use these propositions of Homans to explain the fact quoted from Voting we will need to postulate that opinion-leaders (people with "authority" in Homans' sense) also possess esteem. No enquiry about this was made by the authors of Voting (an illustration of the point that what counts as a strategic question depends on your theoretical apparatus) but there is some relevant evidence from another piece of research on the development of political views<sup>1</sup>

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<sup>1</sup>L. Queener, "The Development of Internationalist Attitudes", three articles in the Journal of Social Psychology, 1949, cited in H. Hyman, Political Socialization (Glencoe, Ill., 1959), pp. 161-3, 169. (The quotation given occurs on p. 162 of Hyman.) The study reported was of "long interviews on the development of internationalist-nationalist attitudes...held with 50 men, drawn mainly from the upper middle classes in New England and having a median age of 53."

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which led to the conclusion that "Mere number of attitude cues"<sup>1</sup>

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<sup>1</sup>E.g., opinions expressed by other people

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does not seem to form attitude. Mere exposure to an attitude does not even guarantee that it will attenuate the opposite attitude.<sup>1</sup>

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<sup>1</sup>Note that this contradicts the implicit and unsupported "billiard-ball" model of attitude-formation which, as Luce has argued, underlies much of the exposition in Voting.

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In none of the histories did multitudes of persons or groups holding a given attitude form respondents' attitude if their prestige was of an inferior grade to that of even a few persons or groups holding counter views."<sup>1</sup>

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<sup>1</sup>According to Hyman, Queener's "basic theory" (supported by his findings) "is that the individual imitates some model or individual who provides the cues for attitude formation. This model is not followed, however, unless he has prestige, which ultimately derives from the reward or punishment flowing from such behavior." (Hyman, p. 161) It is striking that this fruitful hypothesis was produced by the only writer whom Hyman mentions as having a reward/cost approach to behaviour. It is also striking that the hypothesis would make sense of a lot of the results quoted elsewhere by Hyman--something that Hyman himself signally fails to achieve.

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If we take as read the connection between esteem and authority (i.e. being an "opinion-leader") we now need to posit a connection between either education or occupation on one side and esteem on the other. May it not be that a man of slightly higher educational attainments than those with whom he regularly associates (calling it "educational status" partly begs the causal question) is likely to have offered more rewarding suggestions in the past than the others in the group and thus have gained esteem and authority among them? This would certainly not be true of all groups: as Homans says "followers in some groups find some of the damndest things valuable. So long as a man by hook or crook can provide his companions with these things, he is apt to win esteem and authority over them." (p. 287). But it may well be true of enough groups to explain the results with respect to educational attainments. Anyway, whether the hypothesis is true or not could be tested by asking followers about the previous

record of opinion leaders' suggestions, and seeing how far a good record seemed attributable to superior education.

"Occupational status" might be treated as a dependent variable

or it might be argued that it works separately in the same direction.

*But one might, instead or in addition, ~~explain~~ bring in a second aspect of social behaviour here, which asserts a link between status and a high level of status and a high level of influence.*

*But here one might take over some ideas from Blau to cover the specific status*

( "Status" is different from the "esteem" already mentioned in that the latter is A's own respect for B whereas the former is A's perception of the respect in which B is generally held within some wide group. ) *Both Occupational status and is an obvious*

*example of status, and educational attainment might here be regarded as a single determinant of occupational status, but if it confers status directly as well then it can be brought in as an independent factor.*

*So far we have been working on the assumption that in adopting an*

*(When discussing the connection between esteem and influence, we were)*

"opinion leader's" suggestion ~~you are~~ doing yourself some good directly; but as well as doing yourself a favour you may be looked on as doing ~~him~~ a favour <sup>to the opinion leader simply</sup> by complying with his proposal. "Willingness to comply with another's demands is a generic social reward...."

(Blau, Exchange and Power, p. 22). ~~Within the "exchange" framework we take it that you do someone a favour so as to keep up an exchange of favours with him. (Once the exchange is going it is indifferent whether we call it repayment for the last or a stimulus to the next.)~~

Is there any reason why we might expect to find most of the members of a group doing a favour for slightly higher-status members? According to Blau there is, namely that merely by associating with

them in spite of his higher status, the man rewards the members of the group. *And to keep up this state of affairs, the other members must reward him by some means.* "The fact that many people find it rewarding to associate

with superiors means that those of superior status can furnish re-



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wards, and expect a return for them, merely by associating with others of lower status." (ibid., p. 132). ~~The two explanations can be used in conjunction if we suppose that~~ ~~be compounded by saying that sometimes the follower may comply with one~~  
of the leader's suggestions although he does not expect ~~it~~ to reward him directly so as to motivate the leader to continue making suggestions, some of which he expects will be directly rewarding.

There is still the limitation of the Voting statement to slightly higher qualifications and status, and the initial limiting conditions: "within broad strata." ~~I shall suggest that both can be explained together. Now, for~~ ~~for~~ anything said so far to the contrary, ~~we~~ might expect an across-the-board finding of "the higher the status the more the influence." Why don't we? To explain this we need a further proposition about elementary social behaviour and another auxiliary hypothesis. The proposition is fully dealt with in Homans' Chapter Fifteen on "Equality". He quotes from The Human Group the proposition that "persons who interact with one another frequently are more like one another in their activities than they are like other persons with whom they interact less frequently", and adds that

the similarity in question is often a similarity in esteem or recognized status: that is, the persons concerned are social equals. By equality we do not mean the equality of all members of a group but equality within layers or strata--the rough equality with one another of members who are at the same time superior or inferior to others.... [Thus in a] study of interaction among American high school students, the students tended to receive most interaction from others who fell within their own or neighboring status-classes." (Social Behaviour, pp. 316-7)

Homans explains this phenomenon in terms of the cost involved in associating with persons of distinctly higher status and we can now suggest that the people who responses are summarized in Voting

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probably did not interact a lot with people much higher in status than themselves because they would be uncomfortable doing so. But, as Homans allows, there would still be some variations of status within interacting groups and we then suppose (for the reasons already given) that the higher status members of these groups would be the opinion-leaders.<sup>1</sup>

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<sup>1</sup>Homans himself speaks of "two tendencies: a tendency for men to express approval of, and to interact often with, others who are in some sense 'better' than they, and a tendency for men to like and interact with their equals" and he says that "the two tendencies might combine to produce a resultant tendency for men to interact with, and express liking for, others who are a little 'better' than they are themselves but not much better--to choose 'up' but only a little 'up'." (Social Behaviour, p. 327)

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The hypothesis is that the inhabitants of Elmira tended to follow those with the highest status among those they interacted a lot with. This hypothesis would be disproved if they turned out to interact a lot with people of much higher status as well but only followed those of slightly higher status.<sup>1</sup>

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<sup>1</sup>There does not appear to be any directly relevant material in Voting. However, if we accept the validity of the indices used in Chart LII on page 112 (e.g. "belonging to two or more organizations", being male, etc.) then we can say that "Opinion leaders are more active and more strategically located" (on average, than the rest of the population), which is the authors' caption for this chart. We can take this as indirect evidence for their having high esteem within their respective groups if we suppose the connection (proposed by Homans in Chapter Ten of Social Exchange) between high esteem and high interaction to be operative here.

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TO Ms.  
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I am well aware that this example is more than a little grotesque if it is simply looked on as an attempt to explain one sentence in Voting. Science relies on economies of scale and setting out so much theory to explain one little finding is as wasteful as tooling

up an entire factory to produce a single car. But it is, of course, my claim that with full production the unit costs would come down sharply: that the same body of theory, with some extentions where necessary, would explain a number of other findings and reduce them to some sort of intellectual order. And it must be remembered that it is not, in the long run, the main office of a theory to explain ex post the results of research carried out independently of it, but rather to suggest profitable lines of enquiry.<sup>1</sup>

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<sup>1</sup>Thomas S. Kuhn points out in The Structure of Scientific Revolutions (Chicago, 1962) that the physical sciences have developed by testing the conclusions generated by the current theory rather than by going after prima facie important or even easy problems. To Aristotle, and common sense, nothing could be an easier example of motion than a horse pulling a cart; to Newtonian physics (as Butterfield has commented) nothing could be more complicated. Social scientists are often too greedy: they want right at the start Newtonian answers to Aristotelian questions.

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I would hope that even my crude efforts here might be enough to suggest that a researcher operating within an "exchange" framework would have been led to ask other questions; and I can even see quite a few testable conclusions that follow from the gobbets of theory served up already.<sup>1</sup>

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<sup>1</sup>For example it follows from propositions previously set out that those who interact often with the "opinion leader" but also gave him a service he values are less likely to be influenced in their opinions by him than are others in his circle; that the correlation between education and influence does not hold in those groups whose members value ends to which education is irrelevant as a means; and that the more people a given opinion leader influences on politics the more other subjects he influences them on as well.

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An important scientific bonus is that, as well as the theory throwing light on the political phenomena, any success the theory has reacts

back by strengthening its confirmation and thus enables it to be used with greater confidence in the analysis of other areas of social life. Conversely, if the consequences deduced from the theory are not borne out by empirical findings this shows that the original formulation needs to be revised, which is equally a theoretical gain.

I do not want to weaken a good case by overstating it: although political preferences only make sense in the context of a society, we should not assume that every aspect of every party preference is covered by the theory of social behaviour as exchange. Obviously there are institutional "givens" whose roots lie in the society's history, and psychological "givens" whose roots lie (in the first instance) in each separate individual's personal history. But even leaving aside these, there is much that is still left out. Thus if a man who reads and believes that the Republicans will produce unemployment regards this as a sufficient reason for favouring the Democrats, it is probably best to leave it at that. Luce, in the article I quoted from earlier in this section, says (I think plausibly) that the main model underlying Voting is a crude one of opinion-diffusion by personal contact: "that an individual's interactions with members of his primary groups constitute the basic social mechanism for developing political decisions." (Burdick and Brodbeck, p. 333). He suggests that instead one should reckon with at least some people who try to guess the consequence of either party getting in for some social category with which they identify, and whose party preferences are comparatively impervious to casual social contacts. Though such people would be studied in terms of "social exchange" for their influence on others, it would probably be best to take their own opinions as additional "givens."