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THE PSYCHOLOGY OF POLITICS AND THE PERSONALITY SIMILARITIES BETWEEN FASCISTS AND COMMUNISTS

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To have one's writings submitted to a very detailed and exhausting critique in the pages of the *Psychological Bulletin* is a great honor; to have this happen twice is somewhat overwhelming. Before, therefore, replying to Christie's comments (1) I would like to take this opportunity of thanking both him and my earlier reviewers (6) for drawing attention to several minor misprints in *The Psychology of Politics* (4). While, as will be seen, I cannot agree with any of the major criticisms put forward, I shall always be indebted to them for their painstaking examination of the details of my book.[1]

It is curious how much alike Christie (1) and Hanley and Rokeach (6) are in their failure to deal with the logical development of the theories and experiments outlined in this book (4). Psychological theory and factorial studies agreed in showing that the interrelations of social attitudes in Great Britain required at least two orthogonal factors or dimensions for their description; these factors were labeled R (for radicalism-conservatism) and T (for tough-mindedness vs. tender-mindedness). Many theoretical and practical reasons are given why, descriptively, these two factors are superior to any of the innumerable alternative rotations which could be made, and Christie appears to agree with this when he says that it is "difficult not to appreciate the clear-cut radical-conservative axis that appears in Eysenck's data, and to agree with Eysenck that there are semantic advantages in using R and T when dealing with political parties."

Our theoretical position leads us to believe that the T factor is the projection onto the attitude field of the personality dimension of extraversion-introversion, in the sense that extraverts will have tough-minded attitudes, introverts tender-minded attitudes. The *content* of the attitudes of extraverts and introverts respectively will be determined by their position on the radicalism-conservatism axis. It would follow from this hypothesis that there should be very few, if any, *pure* T items; tender-mindedness and tough-mindedness should always appear in conjunction with either right-wing or left-wing tendencies. This is what we have found in actual fact after an examination of many hundreds of different items. It is very satisfying to find hypotheses supported in this way, yet oddly enough Christie appears to hold the opposite view. He writes "It is contended that what weakens Eysenck's position is the fact that he has no items which are relatively pure measures of T." The fact that if many such items could be found the theory which has been elaborated in *The Psychology of Politics* would be, not just weakened, but completely disproved, does not seem to occur to Christie. He blames our procedure of item selection for our failure to find *pure* T items, and says that if the writer "had analyzed the defini-

[1] Some of the points Christie makes have already been answered in my earlier reply to Rokeach and Hanley (5). The reader may like to consult this earlier paper in conjunction with the present one.

tion of tough-mindedness and then selected, invented, or modified items which appeared relevant and then factor analyzed responses to them and other items he might well have isolated a much purer dimension of 'tough—tender-mindedness.' Such a comment implies that there were such items or they might be found. . . . Whether a tough-mindedness scale could be constructed whose items are relatively independent of radicalism-conservatism or not, is an empirical question."

Having attempted for many years to do what Christie advocates, and having had several students make similar attempts, all without success, the writer believes that Christie is somewhat optimistic. Perhaps if he had himself some practical experience in carrying out work of this kind he might be less inclined to dismiss the concentrated efforts of several people over many years in this superficial fashion. It is impossible for the writer to prove a negative, i.e., to prove that such *pure* T items do not, in fact, exist; all that can be done is to carry out the search over a long enough period and wide enough field to make one's failure to find such items convincing evidence to the unprejudiced judge. Christie's critique would have gained considerably if he had shown some appreciation of the methodological position, and even more if he had actually succeeded in unearthing such items.

Granted that hitherto no pure T items have emerged, and granted also that the dimensional analysis of the attitude field requires two dimensions, it is clearly essential for the construction of a T scale to use items having reasonably high correlations with T, and which are selected in such a way that their correlations with the R scale balance out. Christie's comment on this is that "The crucial point in an interpretation of Eysenck's results is that the T scale is a somewhat better measure of R than T. The mean loading of T scale items on T is .38, on R .48." The confusion evident in this quotation appears to invalidate most of Christie's argument as far as it relates to the construction of the T scale. The crucial point is that items are selected in such a way that if we have two tough-minded items one would be a radical, the other one a conservative item. By adding the two we add the T variances and cancel out the R variances. As an example of this, let us consider an imaginary miniature scale consisting of two items. The first item relating, say, to trial marriages has a loading of +.6 on R and +.5 on T; the other item relating, say, to the death penalty has a loading of −.6 on R and +.5 on T. For the purpose of the T scale a "Yes" answer would in each case be counted one point. A person saying "Yes" to both questions would therefore get a score on the T scale of 2, a person answering "No" to both questions would get a score of zero. The fact that both items have higher correlations with R than with T does not mean that the sum of the answers is a good measure of R. A person high on R would say "Yes" to the first and "No" to the second item; a person low on R would reverse this. This point would seem too elementary to discuss in such detail, but as much of Christie's critique is based on it, it seemed desirable to clear it up. Rokeach and Hanley appear to be subject to a similar error of interpretation. If Christie were, in fact, correct in his contention that the T scale is a good measure of R, then it should correlate with the R scale. As the studies reported in *The Psychology* *of Politics* (**4**) show, no such correlations have in fact been observed.

The writer would readily admit that our first version of the T scale fell short of perfection in several respects; this was one reason why an improved version was constructed by Melvin (**9**). However, Christie is in the unfortunate position that if we *completely* accepted his criticism of the scoring system adopted, then our results would support even more strongly our own hypothesis, and go counter to his. He maintains that "by virtue of an asymmetric distribution of items combined with Eysenck's singular scoring system, a hypothetically consistent fascist is automatically made more 'tough-minded' by one point than a hypothetically consistent communist." As we have throughout found communists to be slightly less tough-minded than fascists, Christie's argument would suggest that, in fact, we should increase the communists' scores by one point, thus making them even more like the fascists than appears in our results. As Christie's main argument appears to be that communists are not tough-minded at all, and are quite unlike fascists in this respect, acceptance of his criticisms of our scoring system would, therefore, strengthen our position and weaken his.

The same is true when we look at another comment. Christie maintains that "the arbitrary system of scoring which treated zero responses as 'tough-minded' thus introduced a bias of unknown extent in the direction of making the members of the three major parties more 'tough-minded,' relatively speaking, than those of the two deviant parties." Again, even if Christie's criticism were well taken, it would merely mean that we had loaded the dice *against* our own hypothesis; making the appropriate corrections would make our results support our theory even more strongly.

Another criticism of the scoring system the writer does not understand at all. Christie maintains that "the T scale simply does not apply to communists (or at least to this sample). Comparisons of scores made by communists on a scale on which they do not respond along the continuum measured with scores by other samples are meaningless." Just what is meant by saying that a certain scale "simply does not apply" to a certain group? One might imagine that it would have zero, or at least quite low reliability for that group; yet Coulter has shown that the reliability of the T scale is higher for the communists than for fascists, or our neutral group (**2**, p. 43). Does it, perhaps, mean that our measurement of T is only a watered-down and less reliable measure of R? The reliability of the T scale for communists is higher than that of the R scale, and the two scales do not correlate. Does it, perhaps, mean that T does not correlate with other variables in the case of communists, while it does so in the case of fascists and other groups? Again, Coulter (**2**) has shown that the opposite is true, if anything. Is it that scores on the scale do not behave in conformity with firmly grounded theory? But here again, as shown in *The Psychology of Politics* (**4**) and the more recently concluded study by Nigniewitzky (**10**), to be discussed below, it is found that communists behave precisely in the predicted manner.

Is it that the T scale is irrelevant to political party structure as compared with the R scale? Here, Nigniewitzky's finding on a representative sample of the French middle-class population is relevant; he finds that the T scale, while independent of the R scale statistically, is actually *superior* to the R scale in differentiating between members of the different political parties (including the communists) (10). It is submitted, therefore, that Christie's statement is strictly meaningless. If Christie had quoted the relevant statistical findings, this fact would have become apparent immediately.

We must now turn to the problem of sampling. Christie spends a considerable amount of space in trying to show that our middle-class sample was "completely unrepresentative of the British middle class." As the writer himself has stressed this point several times, Christie's work appears to be a task of supererogation. As was pointed out in *The Psychology of Politics* (4, p. 127): "Our interest lay not in obtaining a representative cross-section of the population but in comparing different political groups. This can best be done by having the groups of equal size, thus reducing sampling errors to a minimum. If mean values are wanted for the total population, then mean values for the selected groups can be multiplied by the proportions these groups form of the total population, thus giving an adequate indication of population values." Again, Christie appears to doubt this statement:—"In view of the fact that Eysenck's basic middle-class sample is markedly unrepresentative of the British middle-classes, it would be highly dangerous to project their attitudes to obtain an estimate of the parent populations."

It may be tedious to the reader to spell out this point in detail because of its quite elementary nature, but as Christie has devoted so much space to it, his misinterpretation requires correction. If we are interested in the variance contributed to a given score by a number of factors, such as political party, sex, age, and education, then the most efficient design for giving us such information is obviously one in which all the possible groups into which these four methods of classification divide the population are represented in equal number. A representative sample of the population would be relatively inefficient, particularly when some of the groups (liberals, university-educated) comprise only a very small portion of the population. Mean values from such an analytic sample cannot, of course, be taken as representative of the whole population; we would require to correct the figures obtained for each subgroup by taking into account the proportion of people in that special group in the total population. When this is done we obtain an estimate of population parameters which is only a little inferior to one obtained from a random sample. Thus, an analytic sample is vastly superior to a random sample with respect to the analysis of the influence of different factors, and is very little, if at all, inferior to it with respect to obtaining estimates of population parameters. As our purpose was not that of obtaining population parameters but of determining the relative influence of the factors indicated, Christie's argument appears to be quite irrelevant to the facts of the situation.

It should not be assumed from this, however, that our sampling procedures are not subject to criticisms on any point. We know of no complex study in social psychology which has handled this problem with complete adequacy, and we have throughout been aware of certain weaknesses in our sampling procedures. The details have always been given in sufficient detail to enable the reader to form his own views as to the degree to which our conclusions should be modified because of these imperfections. In this our writings are in decided contrast to Christie's own critique. He seems to be quite happy to establish a point by referring to work carried out by Rokeach in which scores are given for groups of students and Vauxhall Motors workers without any mention at all of sex composition, method of sampling used, and so forth. Critics who cavil at the relatively full data presented in respect to the sampling procedures used by the writer might be expected to heed their own advice. In view of Christie's failure to give any details at all, the writer cannot take seriously the means presented, or the criticisms based on them.

It is fortunate that quite recently it has become possible to carry out a large-scale study in France, making use of a properly selected representative sample of the French middle-class population. This study was carried out by R. Nigniewitzky (10) and gave results which are of considerable relevance to Christie's remarks. Communists on the new and improved form of the T scale were found to have a mean score of 10.3; fascists to have a mean score of 10.2; communist fellow-travelers had a mean score of 10.2. The mean score of the supporters of all the other main French parties was 17.6. Communists and fascists again appear as very much more tough-minded than the democratic parties.

These results are important for several reasons. Christie takes us to task for selecting communists and fascists who were actively engaged in political work, and comparing them with people who voted for the main three parties, but were not specially active in the political world. This, he maintains, introduced a sampling bias because differences may be due to the factor of being politically active rather than to being procommunist or profascist. This argument is almost impossible to disprove because in England members of the communist party and communist adherents generally are all characterized by this strong degree of political activation; it would be practically impossible to find communists and fascists not active in this way, and if any could be found they would be extremely atypical. Conversely, the typical conservative, liberal, or socialist voter or party member, however strong his convictions, does not indulge in the same kinds of activities as does the communist or fascist. It would, therefore, be not just difficult but impossible to find conservatives, liberals, or socialists carrying out, with equal intensity, the kinds of things done by communists and fascists, and again, if such people could be found they would be extremely atypical. Christie argues "In short, to what extent are differences in attitudes between communists and major party members traceable to ideology per se and to what extent to other factors relating to political activity?" It would, indeed, be interesting to know the answer to this question, but only someone exceptionally ignorant of conditions in Britain at the moment would expect it to be possible to find the answer in this country.

There are other difficulties which make any ordinary kind of sampling procedure inapplicable in England. The number of fascists and communist party members in the whole country is usually considered to be less than 100,000; thus, it would take a sample of 300–400 people to find a single communist or fascist. To get even the relatively small number of 86 communists and fascists which formed our sample, it would require a random sample of some 25,000 people. When to this is added the secretiveness of fascist party members, who usually refuse to answer questions, and the contempt of communists for this type of work, and their consequent aversion to taking part in it, the impossibility of using orthodox methods should be even more obvious. As if all this were not enough, there is in addition the difficulty that if one were not to make party membership the criterion for acceptance of a person as being a communist or fascist, one would be left with no criterion at all. In the case of the major parties, identification was based on voting behavior. This is not applicable to the fascists as there were no fascist candidates during the election, and it is hardly applicable to the communists because communist candidates were standing only in a very small number of highly atypical constituencies. Christie condemns our method of sampling; he does not indicate how it could have been improved—even without taking into account the limitations imposed by a budget which never rose above, and frequently fell short of, the sum of 100 dollars per annum.

It is here that our French study is so important. In France, the communist party is a mass party, with sufficient members of a nonactive character to make it comparable to other parties, and to make possible orthodox methods of sampling. When this is done, as has been pointed out above, the result shows even more striking differences in the predicted direction than were found in this country. Thus, an improvement in sampling procedures, as demanded by Christie, and an improvement in the scale used do not result, as would be predicted from his criticisms, in a *lessening* of the observed differences between communists and the orthodox political parties; quite on the contrary, the differences become much wider and much more significant.

Christie might well reply that his criticisms were concerned with the studies reported in *The Psychology of Politics*, and that this new study is irrelevant. This, however, is not so. In all experiments which involve sampling, the investigator has to make certain decisions as to which factors are, and which are not, likely to influence the results, and in need of experimental control. Similarly, the reader has to decide to what extent he is willing to accept the investigators' judgment and to what extent he is prepared to reject it. Even the best stratified sampling procedure involves a decision as to the relevant variables which are to be used for the stratification. There are grounds here for legitimate disagreements. No random sampling procedure fails to encounter the problem of nonresponders; no method of handling this is beyond criticism. In studies like the ones reported in *The Psychology of Politics*, where random and stratified sampling could not be used in the orthodox manner, decisions have to be made by the investigator with which the reader may disagree legitimately. Only additional investigations can settle issues which otherwise must remain a matter of opinion. In the writer's view, the sampling methods used in *The Psychology of Politics*, while far from perfect, have adequately substantiated the hypothesis under investigation. According to Christie they have not.

The only way of deciding is not by rather pointless argument, but by further experiment.[2] It is the writer's view that the Nigniewitzky (10) experiment has settled the issue as far as the sampling controversy is concerned.

A good deal of Christie's argument is concerned with findings from American studies, which he believes contradict our own findings. He appears to believe that relationships between social attitudes and personality factors depend upon the social setting. "Any attempt to relate personality variables to political ideology without taking the social context into account is apt to be highly misleading as well as an oversimplification of some highly complex interrelationships." The reader might not guess it from Christie's comments, but this is almost precisely what the writer himself has pointed out in his book This is what he has to say. After pointing out that most of the work contained in *The Psychology of Politics* was carried out in England, he goes on to say that "results from Germany and Sweden, as well as from the U.S.A., make it seem likely that the main conclusions drawn here would apply equally well there; it would not be wise, however, to generalize too far. . . . This is particularly important when considering the personality structure of members of groups such as the fascist and communist parties. In our culture, these are minority groups; it is unlikely that conclusions based on members of such groups could be transferred without change to members of the Communist Party in the U.S.S.R., or to members of the former N.S.D.A.P. in Germany. *When we talk about communists and fascists, therefore, it is about British communists and fascists we are talking, not about their foreign prototypes.* At times the reader will undoubtedly be tempted to generalize beyond this restriction; if he does, he does so at his own peril" (italics not in original). Many of Christie's arguments and criticisms are based on assumed similarities between English and American conditions. He is free to indulge in these speculative exercises, but the writer should make it clear that they have little relevance to his own writings or views. Attempts have been made to extend our work to other countries like Spain (11), France (10), Sweden

[2] One of the criticisms made by Christie may serve as an example of the kind of point on which legitimate disagreements might arise. The writer, having found that certain controls, such as age, were uncorrelated with T in his middle-class sample, did not consider it necessary to impose these controls on his working-class sample as this would have made the investigation very much more expensive and cumbersome. Christie argues that while controls were irrelevant in the middle-class sample, there is no proof that they were irrelevant in the working-class sample, and that consequently the controls should have been retained. This is a possible point of view. It certainly would be more satisfactory if all possible sources of variation could be controlled in experiments of this kind. As this is impossible, judgments have to be made as to the relative importance of different aspects of the investigation. In the absence of any evidence to the contrary, it seemed unlikely to the writer that correlations between T on the one hand and age, etc., on the other would be so very dissimilar in a working-class group as compared with a middle-class group. Christie quotes some evidence to show that relationships between attitudinal variables are different in middle- and working-class samples, but that, of course, is quite a different point; we are here concerned with correlations between factor scores and control variables. It may be said, in parentheses, that in recent unpublished work we have found relationships between T and the various control variables to be very much the same in working-class as in middle-class samples. This does not, of course, invalidate the principle of Christie's criticism; it merely illustrates that a criticism may be abstractly legitimate without being necessarily damaging to the conclusion arrived at.

(7), Germany (3), the Near East (8), and so forth; the accumulation of facts would appear more important than the armchair theorizing in which Christie delights. The reader of these detailed reports may form his own views regarding the degree of cultural dependence of R and T.

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